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Control and Efficiency in the Nonprofit  
Sector Evidence from a Randomized  
Policy Experiment

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CONTROL AND EFFICIENCY IN THE NONPROFIT SECTOR EVIDENCE FROM  
A RANDOMIZED POLICY EXPERIMENT

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# Control and Efficiency in the Nonprofit Sector

## Evidence from a Randomized Policy Experiment\*

Niklas Bengtsson and Per Engström†

May 4, 2011

### Abstract

Results in behavioral economics suggest that material incentives can crowd out effort, if agents are mission-oriented rather than self-interested. We test this prediction on a sample of nonprofit organizations in Sweden. Swedish nonprofit organizations receive tax funds annually to promote global development issues through information campaigns. Traditionally, the contract with the main principal (the Swedish foreign aid agency) has been based on trust and self-regulation. We designed an experimental policy intervention, effectively replacing the trust-based contract with an increased level of monitoring from the principal, along with a threat to cut future funds if irregularities were detected. Our findings are inconsistent with (strong) motivational crowd-out. Overall, using both self-reported and observed measures of outreach, we find that the intervention improved efficiency. Graphical analysis shows that non-monitored organizations exhibit a distinct tendency to maximize expenditure; in contrast, organizations in the treatment group are more likely to return unused grants to Sida. Additionally, we find no crowding out of private contributions and no evidence of a “discouraged NGO”-syndrome.

Keywords: *Bureaucrats, NGO, Economics of psychology, Foreign aid, Randomized experiments, Hawthorne effect, Laboratory vs. field evidence, Treatment externalities, Spillover effects, Reference group contamination*

**JEL:** D03, L31, H83, O19

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# 1 Introduction

Recent developments in behavioral economics suggest that if people are motivated by intrinsic values, it might be a bad idea to use standard sticks and carrots to enforce the contracts they enter. In an inspiring laboratory study, Falk and Kosfeld (2006) find that when principals influence control over agents in altruistic acts, the agents answer with reduced prosocial behavior towards the principals.<sup>1</sup> Based on a survey to the participants, Falk and Kosfeld (2006) attribute this to a sense of distrust and a limitation of the agent’s choice autonomy; an aversion to control. Subsequent theoretic work explains that control can sanction egoistic behavior since control in itself either signals that other agents are selfish (Sliwka 2007) or that the principal itself is selfish (Ellingsen and Johannesson 2008). As a result, increased control will reduce effort and productivity.

Control aversion explains why principals and agents within public bureaucracies deliberately enter incomplete, trust-based contracts, even when some outcomes are verifiable (Besley and Ghatak 2005; Herold 2010). The Swedish nonprofit sector provides a case in point. In Sweden, projects run by the nonprofit sector are funded by governmental grants under very loose formal restrictions. Although the regulations are set by the Swedish International Development and Cooperation Agency (Sida), enforcement has been delegated to so-called framework organizations, which are chaired by members of the nonprofit sector themselves. Sida recently defended this system in an official memo, noting that “the contractual system with framework organizations is based on trust between Sida and the organizations, [and] upon our conviction that the activities and the control of these activities become more efficient through a decentralized model of responsibility, compared to a model were Sida governs the activities in detail” (Sida 2003, p. 11).<sup>2</sup>

We intervened with this trust-based relationship in the simplest of manners. Together with Sida, we designed a policy experiment on a sample of nonprofit organizations receiving tax funds to promote human development issues. At the beginning of the year, a random selection of organizations (the treatment group) were informed that their financial documentation would be subject to an additional, special audit, conducted by the financial principal itself (that is, by Sida). The organizations were also informed that they would risk losing future funds if Sida detected any irregularities.<sup>3</sup> In practice, the intervention did not require any new formal documentation; it merely informed the treated organizations that their documents would be reviewed by Sida in addition to their own trustees. Non-selected organizations (the reference group) received no information about Sida’s upcoming audit at all. After the end of the budget year, we observed all organizations’ level of outreach as well as their financial documentation, and compared these outcomes across the treatment and reference groups.

A priori, our sample appears highly susceptible to the type of motivational crowding out reviewed above. Most of the nonprofit organizations have roots in traditional social

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<sup>1</sup>Frey and Jegen (2001) and Kunz and Pfaff (2002) review the psychological research on how material incentives may affect intrinsic motivation, and Bowles and Polania-Reyes (2009) surveys the economic literature. There are also a number of closely related empirical studies by economist where some pre-dates Falk and Kosfeld (2006), e.g. Fehr and Gächter (2002), Fehr and Rockenbach (2003), Fehr and List (2004) and Dickinson and Villeval (2008). Similar incentive puzzles have been observed when donating blood (Mellström and Johannesson 2008) or participating in welfare campaigns for cancer research (Gneezy and Rustichini 2000).

<sup>2</sup>Authors’ translation.

<sup>3</sup>See the Appendix for a translation of the written information sent to the NGOs in the treatment group.

movements (Christian churches or trade unions) and in more modern missions (fair trade, sustainable development, women’s rights, etc.). Nonprofit workers are paid little (or nothing) relative their education level, which, along the lines of Besley and Ghatak (2005), suggests that they extract substantial non-pecuniary rewards from the mission itself. Additionally, many Swedish nonprofit workers are frustrated about what they perceive to be an increased professionalisation of the sector. In a recent interview, the chairman of the Swedish Red Cross was quoted saying that although control and evaluations could be motivated to some extent, too much control could “paralyze” nonprofit workers (see Section 2).

These observations are largely consistent with past literature in the field of economics of organization, promoting the idea that public servants are mission-oriented rather than driven by profit maximization (Tirole 1994). However, as of yet, the existing evidence on motivational crowding out has been constrained to game-like environments in laboratories or small-scale consumer surveys.<sup>4</sup> These studies are often framed so as to make the distinction between material and moral pay-offs as sharp as possible, but it is not obvious how well they extend to more complex and realistic policy environments. A particular concern is that laboratory experiments can interfere with agents’ natural responses to control (a so-called Hawthorne effect), and one of the main contributions of this paper is that such effects are kept to a minimum. To preserve the original policy context, all contacts with the organizations were taken through Sida representatives; the experimental design and the authors’ involvement was not mentioned to the nonprofit organizations until after the experiment. To avoid biased assessment of subjective outcomes, blind assistants and consultants were hired. Finally, to reduce reference group contamination we also set up a so-called “language convention” at Sida. All incoming phone calls from the organizations in the sample were directed to a designated contact at Sida, who was instructed not to give away any information about the experiment other than what had already been communicated through the threat of audit. The contact also documented all incoming calls, giving us a chance to evaluate the extent of rumors surrounding the intervention. As it turned out, about 12 percent of the organizations in the treatment group contacted Sida with questions, but no one from the reference group did, suggesting that few organizations in the reference group knew about the intervention – at least not to the extent that it produced inquiries.

Turning to our results, we find that expenditures among organizations who knew that they would be monitored (the treatment group) are more carefully motivated. Non-monitored organizations exhibit a very distinct expenditure maximization effect, virtually hitting the Sida-funded budget short of a few percent. In contrast, organizations in the treatment group are more likely to return unused funds to Sida. We find no evidence that the reduction in tax expenditures is achieved at the expense of prosocial behavior. In their narrative reports, organizations in the treatment group claim to have reached a higher degree of outreach relative the reference group. More importantly, the actual outreach of the projects seem to improve with increased monitoring: organizations in the treatment group are significantly more likely to be mentioned in local media compared to projects in the reference group. In particular, the intervention had a dramatic impact on the number of organizations achieving a minimum level of outreach without overspending, which suggests that non-contractible

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<sup>4</sup>In development economics, a couple of studies estimate the impact of top-down monitoring using randomized field experiments (Olken 2007; Duflo and Hanna 2005). Although methodologically similar to our study, this literature typically focus on professionals where corruption and material incentives are expected to dominate *ex ante*.

productivity (i.e. effort) increased.

One might ask whether the policy intervention successfully improved short-term outcomes while permanently damaging the long-term relationship between Sida and the non-profit sector. Previous literature has found that material incentives crowd-out intrinsic motivation after some time (Cardenas et al. 2000), and that the deterioration of intrinsic values may be irreversible (Falkinger et al. 2000). A particular concern in this context is that controlled organizations abstain from future collaboration with Sida. However, we find no evidence of a “discouraged NGO”-effect. Organizations in the treatment group were as likely to apply for new funds as the reference group. In addition, the intervention had no effect on private charity, measured as the organizations’ own contributions to the project.

The policy intervention in this study raised the stakes of underperforming, but it did not achieve anything close to a first-best contract, covering all potential, verifiable outcomes. For instance, the letter sent to the treatment group indicated that the audit would be based solely on self-reported documents (receipts, affidavits, narrative report, etc.). Despite this, the positive results from our study were not confined to cheap, self-reported outcomes. Actual outreach, as measured in media visibility, also improved, even though organizations were not formally obliged to promote their cause through this channel. The positive results were thus not realized because organizations had no choice but to follow the stipulated rules, as in Falk and Kosfeld (2006).<sup>5</sup> We conclude that material incentives in contractual relationships can increase effort even in second-best settings where effort is not completely enforceable.

One interpretation of our results is that nonprofit workers are mainly motivated by material rewards. This would explain the lack of crowding-out behavior and the tendency to react in ways predicted by conventional economic theory.<sup>6</sup> However, this raises some challenges to the very definition of mission-oriented individuals. Although some nonprofit workers in Sweden are “professional pro bono-workers”, most of them are paid far below the going market wage at their education level, and it is difficult to imagine why self-interested individuals would be attracted to the nonprofit sector in the first place. Another interpretation is that in this particular context, control might crowd *in* intrinsic motivation rather than destroy it. If Sida is perceived as something of an authority of moral judgement, it is possible that the prestige and relevance of being a nonprofit worker is enhanced when “being seen” by this particular agency. A curious example of what appears to be the opposite is reported by Besley and Ghatak (2005), who note that when George W. Bush signalled that he intended to extend the governmental support of US charity organizations, many responded with outright defiance.

The paper is outlined as follows. In the next section, the background and context is described. The policy intervention is described in Section 3, and a conceptual framework is presented in Section 4. Section 5 provides a discussion of the data and the results and Section 6 concludes.

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<sup>5</sup>In Falk and Kosfeld (2006), positive effects of control are found only when the principal completely dictates the level of reciprocity.

<sup>6</sup>As pointed out by Meier (2006) in a survey on prosocial behavior: “intrinsic motivation can only be crowded out by extrinsic incentives if people have intrinsic motivation to begin with” .

## 2 Background

### 2.1 The critique from the National Audit Office

At the onset of the experiment, there was an ongoing debate in Sweden about Sida's "control environment", spurred by a critical report from the Swedish National Audit Office, and our initiative borrowed implicit credibility from this debate. In this section, we briefly describe this policy context.

Sida channels parts of its foreign aid through nonprofit organizations. Since its inception, the financial control of these partners has been characterized by a non-trivial degree of self-regulation and trust. Sida primarily enforces its regulations through so-called framework organizations in which the nonprofit organizations are board members. This arrangement is partly motivated by tradition. When Sida was first established as the national foreign aid agency, nonprofit organizations had played a central role in lobbying for its existence, and they have been influential in forming Sweden's foreign aid policy from the start.

This "decentralized model of responsibility" was recently criticized by the Swedish National Audit Office (SNAO). A field audit (Riksrevisionen 2007) of 15 projects funded by Sida in Namibia, South Africa, Tanzania and Kenya revealed that the financial documentation among the local partners was sub-standard. According to the report, 10 out of the 15 projects contained significant errors in their financial reports and none of the projects passed without criticism. SNAO concluded that the "control environment" at Sida and the nonprofit sector was insufficient to evade threats of corruption and fraud.

Sida answered by contracting an external audit agency to re-audit the criticized projects (Eriksson and Ehrenberg 2008). Sida's consultants later dismissed 23 out of 51 alleged errors. Drawing on this re-audit, three NGOs released a joint report criticizing the selection of projects in the SNAO's audit study (Lodenius 2008). The audits were not based on random selection out of all Sida financed projects (between 2006 and 2009, more than 6000 projects received Sida support); rather, most of the projects were based on prior suspicions of irregularities. Lodenius (2008) argued that SNAO, in press releases and in public discussions, gave the impression that the projects were representative for all Sida funded projects. Lodenius (2008) went on to accuse SNAO of being biased against foreign aid and driven by a right-wing agenda.

Since the release of the SNAO report the political pressure to increase the control function of Sida has been high. Sida has taken a number of steps to increase its control environment, including that documented controls should be performed in advance of all payments and that all new contracts require revision. Sida also signalled that it was in need of credible estimates of the efficiency of control in the nonprofit sector. We approached Sida with our research proposal in November 2008.



## 3 Policy intervention

### 3.1 Sample

We study a sample of 127 nonprofit organizations.<sup>7</sup> All organizations belong to one of four umbrella organizations; LO/TCO, the Olof Palme International Center, the Swedish Mission Council, and Forum Syd. The first two organizations have links to the labor movement and the Social Democratic parti. The Swedish Mission Council consists of Christian member organizations. Forum Syd is a catch-all umbrella organization for independent organizations in Sweden. This makes Forum Syd somewhat special as they do not have permanent members, and typically do not admit funds to long-term projects. Of these four, Forum Syd is (by far) the largest framework organization.

The type of projects studied in this paper are called “information projects”. Information projects are implemented in Sweden (in contrast to the bulk of projects implemented in Africa or Latin America) with the objective of raising awareness of development issues. Activities include arranging seminars, conferences, festivals or courses. The typical nonprofit organization also attends political lobby events as part of an information project. In order to demonstrate the characteristics of the projects in greater detail we present a translation (the authors’) of very brief project descriptions of three typical projects in our sample:<sup>8</sup>

- Committed contribution: 1 944 000 SEK. Framework org: Forum Syd. NGO: The Swedish Committee for Afghanistan (SCA). Summary: SCA’s objective of the operation is to create knowledge of, commitment and aid willingness to the Afghan people and to spread SCA’s experience of development cooperation in Afghanistan. This will be achieved through media activities, to disseminate Afghanistan-nytt [member’s magazine], the website, seminars, school twinning, exhibition activities and local activities.
- Committed contribution: 201 000 SEK. Framework org: LO/TCO. NGO: ST – The Union of Civil Servants. Summary: By initiating more local and workplace-based groups and arranging lectures/debates at universities and colleges, ST increases the awareness of its international work. Trade union membership in Fairtrade, UNICEF, United Nations Association and the Global Unions will be made clearer and a better network for the internationally interested members will be developed.
- Committed contribution: 1 545 500 SEK. Framework org: The Swedish Mission Council. NGO: Christian Council of Sweden. Summary: The operation aims to bring together churches, congregations, ecumenical councils and local Christian organizations in joint action for global justice. During “Churches Global Week” these organizations are given an opportunity to a common theological and ethical treatment of issues concerning poverty reduction, sustainable development and human rights. They also participate in activities with the aim of creating public opinion and influence society.

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<sup>7</sup>6 organizations opted out before treatment however; these were omitted from the analysis. See Section 5.

<sup>8</sup>These are the official project descriptions available through Sida’s NGO database: <http://www2.sida.se/ngodatabase/>. These brief descriptions were also the information available to the assistants when assessing media outreach.

### 3.2 The contract and the threat of audit

The regulations governing the contract between Sida and the nonprofit organizations are few and relatively straightforward. For each project, at least 10 percent of the budget must be financed by the organization’s own funds. Here, “own funds” includes contributions by the organization itself, either from revenue or private gifts. Funds from other governmental sources are not counted as own contributions. The nonprofit organization commits to a level of own contributions at the stage of application. The organization cannot apply for funding for its permanent staff, and such salaries do not count as own contributions (however, consultants can be hired, making the use of paid personnel a grey area). Also, expenses on large capital investments, travel costs and production of information material must be well motivated and within the scope of the project. There are no regulations on how output should be measured or verified.

At the end of the budget year, all organizations must document their work in financial and narrative reports. The financial documentation must meet acceptable auditing standards. An authorized auditor must verify spending for large organizations. The narrative report should include motivations for deviations between actual expenditure and budgeted expenditure. The unorthodox part in this principal-agent set-up is that these documents are typically not sent to Sida, but to the designated framework organizations. Sida can request reports from the individual nonprofit organizations, but prior to our experiment, this option was rarely exercised. Thus, in practice, the formal enforcement of the contract was quite lax prior to our intervention.

The social experiment was set-up as follows. In february 2009, we randomly drew 63 organizations from the baseline sample of 127 organizations. These organizations were then contacted by letter, explaining that this year the organization had been randomly selected for a special audit by Sida (henceforth, we shall refer to this selection as the treatment group). At the end of the year, Sida performed an audit of all organizations in the baseline sample, and we collected information on media visibility. A timeline of the experiment is displayed in Figure 1.

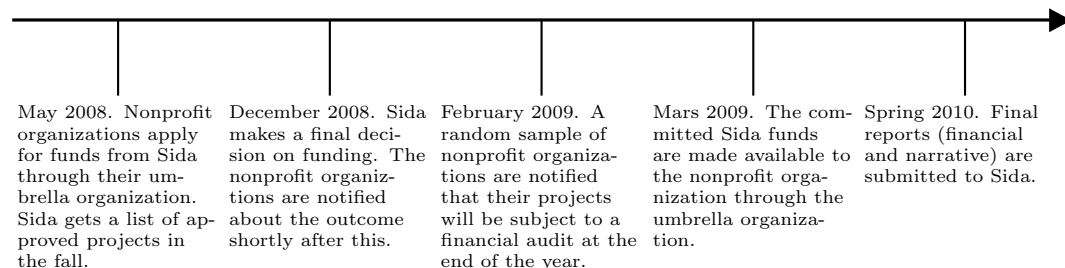


Figure 1: Timeline of Experiment.

The letter in full is appended to the paper. The letter begins with a sentence explaining that Sida “regularly” performs random audits. This is something of a half-truth, since Sida seldom requires more than a handful reports each year, and they are never random. We added this sentence in order to reduce any feelings of being singled out (although perceptions of fairness in this context is an interesting question per se, it is not the kind of mechanism we

wish to address in this paper). The second paragraph in the letter mentions the regulations governing the contract between the organization and Sida. The pages referred to contain the regulations on travel expenses, large capital investments, personnel costs and costs of material production, as well as the 10-percent minimum threshold of own contributions.

In order to isolate a natural response to a conventional audit, the intention to analyze the project's visibility in media was not mentioned in the letter. The threat of audit only referred to the organization's financial documentation and the narrative report, in keeping with standard procedures at e.g. the Swedish National Audit Office and Sida's internal control department. The letter ends with an implicit threat to cut future funds if the outcome of the audit is not satisfactory. The letter was signed by Sida and sent out to the 63 organizations in the treatment group in February 2009.

### **3.3 Assessing contamination and the Hawthorne effect**

Despite the standard wording of the letter, we expected that some organizations would try to learn more about the initiative, and in the worst case scenario, that rumors about the initiative would begin to spread to the reference group in such a way that they also perceived that the probability of audit had been increased. In the literature, this type of effects give rise to so-called Hawthorne-effects and reference group contamination.

In order to minimize the reference group contamination we imposed a so-called "convention of speech" at Sida. A speech convention is simply a set of communication guidelines used by public officials. It is often used by foreign affairs departments, since they harbor a lot of sensitive information and are in a constant state of media scrutiny. It essentially means that the department adopts rules of what kind of information will be disseminated to the public, and in what way.

During the year of implementation, the nature of the experimental design was known only by one contact at CIVSAM (the Sida department responsible for the NGO support). The rest of the group at CIVSAM was informed that a study was being undertaken. The group was instructed that all external calls regarding the intervention were to be redirected to our contact. In case someone from the treatment group made contact, the strategy was simply to repeat the content of the letter. If anyone from the reference group called, the rule was to only stress that all projects were required to submit their documentation as usual.

The contact documented all incoming calls and mails, providing us with an opportunity to test for contamination. As it turned out, 16 organizations called CIVSAM with questions or comments about the letter (about 12 percent). Out of these, 11 were independent organizations under Forum Syd. Those who contacted Sida were also larger in size. Most organizations had questions regarding some particulars of the financial report. Fortunately, all organizations that contacted Sida with questions about the letter belonged to the treatment group. The fact that no one from the reference group reacted with inquiries about the intervention suggest that reference group contamination was indeed kept to a minimum.

## 4 Conceptual framework

### 4.1 Averse to control or naive to fraud?

Finding the optimal level of control is fundamentally difficult - in particular for a business that is intrinsically hard to evaluate due to the complexity of measuring outcomes. As pointed out in Frey and Oberholzer-Gee (1997), a contractual relationship based on trust and where agents are motivated by a strong intrinsic utility from the social cause itself are likely to be susceptible to the type of psychological crowd out effects reviewed in the introduction of this paper.

Many nonprofit organizations active in the foreign aid business argue that the level of control may become too high and focused on the wrong things. Bengt Westerberg, head of the Swedish Red Cross, argued that “Controls and evaluations are good but the demands must not be so high that they spread paralysis”.<sup>9</sup> People active on the grass-root level express similar concerns. In an interview-based study of anonymous individuals active in the Swedish foreign aid business, an activist is quoted saying that control reduces the “fun” of being a nonprofit worker:

“But then it’s not so fun all of a sudden. You want to drill a well, you want exchange, you want to sit and talk - the stuff people like to do. The organization wants to show results, but for many involved it is not that fun anymore.” (Svensson 2008)

Another nonprofit worker is more explicit about the link between intrinsic motivation and control:

“The professionalisation has become a threat to spontaneous, grassroots activism. To measure result, a five year project is needed; small efforts are not rewarded anymore. [...] the ambition to measure can become an ‘engagement killer’.” (Svensson 2008)

It certainly seems less fun to be engaged in altruistic activities if someone is constantly looking over your shoulder and evaluates how the resources could have been used more efficiently, and it seems plausible that increased control could have severe detrimental effects in this type of environment. On the other hand, there is also suggestive evidence that a strong emphasis on motivational values might induce organizations to take too lightly on financial irregularities. According to the SNAO, many organizations appeared reluctant to actively work against corruption because it was deemed unnecessary given their altruistic values. For instance, when asked about the implementation of control within Christian organizations, representatives referred to their moral sentiments as a “governing factor” that “positively restrains” and “regulates” the risk of corruption and other irregularities (Riksrevisionen 2007, p. 30).

### 4.2 A simple model

In this section, we formalize the decisions of a manager for a nonprofit organization. The analysis will guide interpretation and estimation. We do not model the selection of organizations into the principal-agent contract, but focus on the outcomes that are endogenous

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<sup>9</sup>Lodenijs (2008). Our translation.

in the empirical analysis. The focus lies on the allocation of the principal's (Sida's) committed contribution into three different usages: (1) effective use within the current project, (2) unused (returned) funds and (3) diverted funds (usage that is not in line with Sida's regulations – private consumption being the most extreme case).

The objective of the model is to stylize the trade-offs involved along two different margins. First of all we assume that the manager cares about the overall mission. The overall mission is a function of the entire nonprofit sector, and not just the manager's own project. Thus, the manager recognizes that funds invested in the project have an alternative cost – if they are returned to the principal they will be redistributed to other projects within the nonprofit sector later on. When deciding how much funds to use and how much to return the manager will therefore assess the project's productivity compared to all other projects. Secondly, the manager has the option to divert funds to non-sanctioned activities that generate private rents. The tendency to do so is determined by the relative weight that the manager puts on the mission compared to his self-interest.

Consider an organization that has received a grant from Sida. Suppose the long-term outreach of the mission is given by a production function

$$g(f_i, f_s) = \sigma \ln f_i + f_s$$

where  $f_i$  denotes current use of funds by the nonprofit organization and  $f_s$  denotes the alternative use of current funds. In this context,  $f_s$  is to be interpreted as the funds returned to Sida. Returned funds are redistributed by Sida and re-invested in the nonprofit sector in future years, and so adds to the mission in the long run. The choice to return funds may be thought of as an outside option that the organization may use when the productivity of their own use of funds decreases. The parameter  $\sigma$  is therefore a measure of the manager's belief in his own project's effectiveness in comparison with the other projects sharing the same mission. We imagine that managers are self-confident (high  $\sigma$ ) at the time of application, and therefore do not plan to return funds. However, due to unexpected shocks to the project, some organizations will find it more consistent with the missionary objective to give up some funds and allow Sida to redistribute them.

The manager extracts utility from contributing to the mission, but also from expenditures that generate private rents. We denote the amount of diverted funds  $f_c$ . We can think of such side-expenditures as lunches, dinners, travels, etc.; expenditures that makes project implementation more comfortable for those involved but do not really add to the mission itself. The utility of the manager is then given by

$$v(f_c, g(f_i, f_s)) = (1 - \lambda(m)) \ln f_c + \lambda(m)g(f_i, f_s)$$

where  $\lambda(m)$  is the overall weight that the manager puts on the mission compared to private expenditures.  $\lambda(m)$  thus captures both intrinsic and extrinsic motivation. The weight is a function of the level of material incentives ( $m$ ) that the manager is exposed to. In our case the material incentives primarily consist of the level of control.

How  $\lambda(m)$  is affected by increased control is the key question of the paper. Conventional economics often maintain separability between extrinsic motivation (control) and intrinsic motivation. In that case  $\lambda(m)$  would increase in  $m$  since increased control raises the stakes of using Sida funds for private rents. However, as discussed above, the behavioral literature

argues that increased control may crowd out intrinsic motivation. If increased monitoring erodes the managers commitment to the mission, there is a risk that  $\lambda(m)$  instead decreases in  $m$ .

When solving the manager's maximization problem we need to separate between two different cases: (1) those with low  $\sigma$  that will use the option to return funds and (2) those with high  $\sigma$  that will not use this option.

The first case (low  $\sigma$ ) is given by the interior solution to the maximization of  $v(\cdot)$  subject to  $1 = f_c + f_i + f_s$  (normalizing the size of the grant to unity). This gives,

$$f_i = \sigma,$$

$$f_d = \frac{1 - \lambda(m)}{\lambda(m)}$$

and

$$f_r = 2 - \frac{1}{\lambda(m)} - \sigma.$$

The comparative statics with respect to  $\lambda(m)$  are straightforward: the efficient use of funds are unaffected by  $\lambda(m)$ , funds used for private rents decreases in  $\lambda(m)$  and returned funds increases in  $\lambda(m)$ .

The second case (high  $\sigma$ ) gives  $f_r = 0$  by assumption and the reduced maximization problem,

$$\max(1 - \lambda(m)) \ln f_c + \lambda(m) \sigma \ln f_d$$

$$s.t. f_c + f_d = 1.$$

The solution is given by,

$$f_i = \frac{\lambda(m)\sigma}{(1 - \lambda(m)) + \lambda(m)\sigma}$$

and

$$f_d = \frac{(1 - \lambda(m))}{(1 - \lambda(m)) + \lambda(m)\sigma}.$$

The comparative statics are equally straightforward in this case: used funds increases in  $\lambda(m)$  while diverted funds decreases in  $\lambda(m)$ . The breakpoint  $\sigma$ , over which case two applies, is given by,

$$\tilde{\sigma} = 2 - \frac{1}{\lambda(m)},$$

which means that an increase in  $\lambda(m)$  will increase the breakpoint and make more organizations switch to returning funds.

The model has the following implications: (1) Increased  $\lambda(m)$  will increase returned funds and reduce diverted funds for organizations with low self-confidence ( $\sigma < \tilde{\sigma}$ ) – the use of funds in the current project remains unaffected; (2) self-confident organizations (with  $\sigma > \tilde{\sigma}$ ) will react to an increase in  $\lambda(m)$  by increasing used funds at the expense of diverted funds – returned funds are unaffected; (3) Increased  $\lambda(m)$  will increase the chance that previously self-confident organizations start returning funds (since  $\tilde{\sigma}$  increases in  $\lambda(m)$ ).

In a mainstream economics model that maintains separability these are the affects of increased control, since  $\lambda(m)$  increases in  $m$ . Financial audits increase the stakes of misusing

funds so more is directed towards the mission. For a self-confident manager (high  $\sigma$ ) the effect is an increase in used funds, and an increase in the chance of returning funds, while a manager with low  $\sigma$  would instead react by returning more funds. These comparative statics would still hold qualitatively for low levels of control aversion, i.e. when the reduction in mission commitment is offset by the increase in the risk associated with misusing funds ( $\lambda'(m)$  is still positive). However, for high levels of crowding out  $\lambda(m)$  will decrease in  $m$ . In that case increased control would reduce both returned and used funds while improper use of funds would increase.

### 4.3 Empirical specification

We cannot separate  $f_c$  and  $f_i$  directly, so we observe them by proxy, through the number of irregularities and mission outreach. In contrast, the amount of returned funds,  $f_s$ , is completely observable in our data. The measurement of these outcomes will be discussed as they are included in the presentation. For outcome  $k$  and project  $i$ , we will estimate the following regression equation:

$$Y_{ik} = \alpha_k + \beta_k T_i + \theta_{\mathbf{k}} \mathbf{X}_i' + \varepsilon_{ik} \quad (1)$$

where  $T_i$  is equal to one if the project is in the treatment group; zero otherwise.  $\mathbf{X}_i'$  is a vector of predetermined controls which is included mainly to improve efficiency; since  $T_i$  is randomly assigned the controls are not required for identification (we shall report both "raw" estimates and regression-adjusted estimates). In some specifications, the outcomes will be categorized as combinations of sub-outcomes (e.g. the sum of irregularities or the average number of media hits). This is to ease interpretation and make the analysis more straightforward. An alternative approach when considering a family of hypotheses is to test for an overall treatment effect, following e.g. Kling et al. (2004), where the individual outcomes are estimated in a system of seemingly unrelated regressions (SUR). We will follow such an approach when appropriate.

## 5 Results

### 5.1 Working sample and randomization

The baseline results focus on three families of outcomes: incomplete documentation, expenditure maximization of Sida funds, observed and self-reported outreach. We will also consider two more explicit measures of crowd-out: exit from future collaboration with Sida and private donations.

Six projects were discontinued prior to treatment; three from the treatment group and three from the control group. These were excluded from the analysis, and the working sample consist of 121 organizations, 61 in the reference group and 60 in the treatment group. Some additional projects were discontinued after the intervention or failed to submit a report in time; these were kept in the sample since all post-treatment decisions are potentially endogenous.

We begin by looking at pre-determined outcomes, where we expect no difference across treatment and reference group (Table 1). Notably, there is no statistical difference between

the two groups. No pre-determined outcome is statistically different at conventional confidence levels, although outliers in expenditure creates an economically non-trivial difference when comparing funds. This difference is much less glaring when taking logs, however, and it is never statistically significant. A joint test on all predetermined outcomes using seemingly unrelated regressions confirm that the treatment group and the reference group are not statistically different across pre-determined outcomes, as expected.

## 5.2 Irregularities: missing documents

The first family of outcomes is the results from Sida’s own audit of the organizations’ documents. Sida outlined the strategy for this evaluation, but at the design stage we requested that (at least some of) the outcomes from Sida’s audit must be quantifiable. To avoid a biased assessment, we also urged Sida to use a “blind” auditor; unaware of which project belonged to the treatment group. As it turned out, Sida hired an external auditor to do the job, and complied to both our requests (the auditor was not aware of the experiment at all). The auditor was instructed to assess the quality of the self-reported documents alone and nothing else.

The auditor reported whether the documentation was unclear or missing in four specific areas: outreach, personnel costs, large investments and cost efficiency in general. These areas corresponds to the general guidelines referred to in the threat of audit. We constructed a simple measure of irregularities based on the number of missing or unclear documents across these four areas. Although “the number of missing documents“ alone hardly counts as evidence of fraud, it is a relatively exact and replicable index of the extent of financial irregularities.<sup>10</sup>

The impact of the threat of audit on irregularities is presented in the first row of Table 2. The intervention had a clear, and precisely estimated, negative effect on the number of detected irregularities. The average number of irregularities was 1.02 in the treatment group and 1.38 in the reference group. Thus, the number of missing documents was reduced by about 25 percent relative the reference mean. In the appendix (Table 6) we present individual estimates as well as the standardized treatment effect from a system of seemingly unrelated regressions (SUR).

## 5.3 Outreach

The objective of the organizations was to raise awareness of human development issues in Sweden. We assessed the organization’s success in reaching this objective using two complementary measures. The first measure is the level of outreach reported by the organizations themselves. Sida’s consultant made an evaluation of the success of each project based on the organization’s narrative report. The consultant indicated that outreach had been “high” if the the organization claimed to have reached non-members in their information campaigns.

As seen in Table 3, self-reported outreach was 60 percent in the treatment group, compared to 30 percent in the reference group, a statistically significant effect. Of course, this does not necessarily mean that most projects failed, since self-reported outreach also captures the sheer tendency to report whether the project was successful or not. But at a

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<sup>10</sup>The auditor did provide us with narrative remarks on some of the projects, but these could not be translated into quantitative variables.



minimum, the positive effect tells us that organizations did not react with outright defiance to the intervention.

To detect whether the intervention also affected observed outreach, we constructed a variable measuring the project's visibility in media. We equipped a total of seven assistants (undergraduate students) with the original project descriptions and a media search engine. The search engine covered all major local and national news outlets in Sweden (private webpages were not included). The assistants were asked whether they, based on what they found in the media database, thought the project had been implemented or not.

The impact of the threat of audit on media visibility, defined as the average number of assistants who found a media report on the project, is positive and statistically significant at conventional confidence levels. The "raw" difference is a simple t-test whereas the estimates with controls are obtained using seemingly unrelated regression across all assistants, so that the controls are allowed to have an assistant-specific impact (standard errors are clustered at the project level).

Although observed outreach is subjective we do not expect any bias to be related to the intervention *per se*, because the assistants were unaware of the selection into treatment and reference groups. Assistants do make errors however, and even non-classical measurement error in the dependent variable will dilute the estimates if the outcome is binary (Hausman et al. 1998). To get a feel of the magnitude of this problem, we decided to estimate the assistants' error probabilities using phony project descriptions. Twenty reference projects were added to the assistants' list without their knowledge, ten of which we new were reported in media and ten of which were completely made up (but in a realistic way). Over all, the assistants made few errors, but Type II errors were more likely than Type I errors: the assistants were more likely to fail to detect a project that was truly being reported in media, rather than falsely report to have found a made-up project. The error probabilities are somewhat intuitive: Type II errors occur simply because the assistants were unable to screen all articles; in contrast, a Type I error would occur if an assistant falsely reported a media hit that was not there, which would imply severe non-compliance from the assistants' part. Motivated by the finding that false positives were more common than false negatives, we estimated the impact of the intervention on the probability that at least one assistant found a positive media hit. This alternative definition yielded similar, but slightly more precise, point estimates.

## 5.4 Use of governmental grants

The evidence this far suggests that the intervention reduced irregularities by 25 percent at the same time as it improved outreach by 10-30 percentage points (depending on the definition of outreach). If productive resources increased but non-productive resources decreased, the total effect on the total use of governmental grants is ambiguous. We will therefore begin this section with a graphical presentation of the total use of Sida grants.

Data on expenditure of Sida grants are retrieved from the organizations' own annual reports. The use of governmental grants is defined as total expenditure net of reported private contributions. Although the amount of Sida funds committed to the project was set in advance, organizations need not use up all grants. Moreover, organizations can shuffle grants between years before the project is completed, which is why the annual expenditure

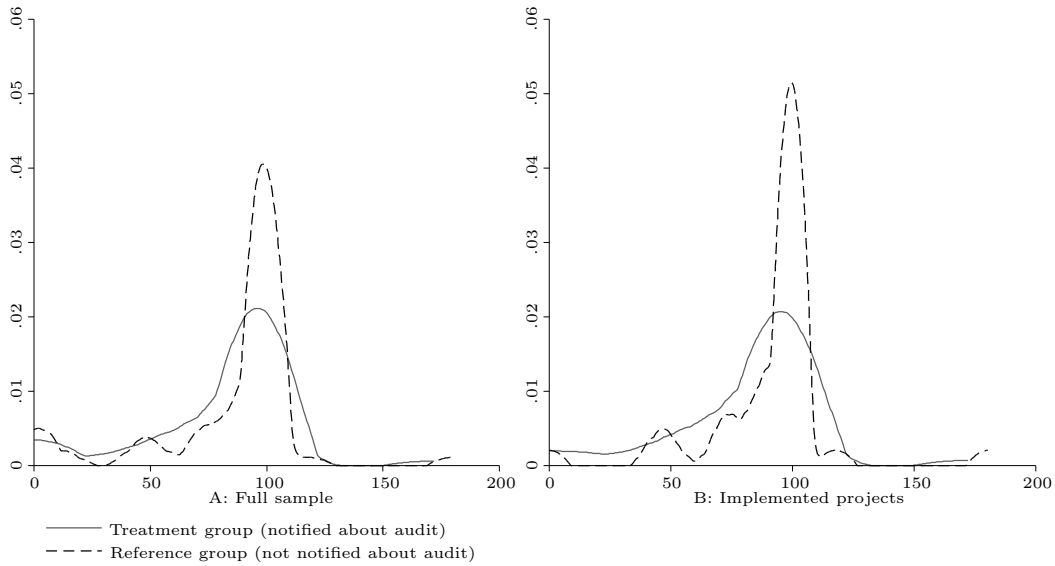


Figure 2: Expenditure maximization among nonmonitored organizations vs. monitored organizations. The figure displays the probability density functions (kernel estimates) of the expenditure of Sida funds in 2009 (in percent). The sample in Panel B is restricted to organizations with a minimum level of media visibility (a media report detected by at least one out of seven assistants). The expenditure shares are calculated by dividing total expenditure minus private contributions by the annual Sida contribution. Expenditure of Sida funds can exceed 100 percent for multiple-year projects since past contributions can be used.

of grants sometimes exceed the annual contribution. However, at the end of the project time frame, unused grants must be returned to Sida (provided they are truthfully reported).

Figures 5.4 depict non-parametric (kernel) density functions of annual expenditure of governmental grants relative the size of these grants. A value of 100 percent implies that all governmental grants were used; less than 100 percent indicates that organizations report a surplus in unused governmental grants, and more than 100 percent implies that organizations used grant commitments from previous or future years. This means that surpluses reported by multiple-year projects are not necessarily returned to Sida. However, most of the organizations (83 percent) were on their final year in 2009. Thus, for the bulk of the organizations, the figures represent completed expenditure, which means that the surpluses are returned to Sida.

The graphical evidence reveals a distinct tendency to maximize annual expenditure and use up all funds among the reference organizations. In contrast, organizations that had been informed about the upcoming audit more often report a surplus to Sida. Notably, most of the action appears to take place around the budget threshold, and the treatment effect seems to pertain to implementing organizations (defined by at least one media hit out of the 7 assistants). Without monitoring, implementing organizations typically report to have spent all of their grants (Figure 2.B).

In Table 4, we perform formal tests of bunching around the budget threshold using different bandwidths. We also test for the tendency to report a surplus in spending. On the full sample, the overall average impact on spending is negative but imprecise. However, we argue that average expenditure is not a particularly informative measure of central tendency in this case, given the skewness of the distribution of used funds. In contrast to average

expenditure, median expenditure is about 99.5 percent in the reference group, compared to 93.2 percent in the treatment group. This is a statistically significant difference, and it tells us that most organizations in the reference group virtually used up the entire budget despite having a lower outreach. Estimates of the probability of reporting a surplus tell a similar story: organizations that knew that they were subject to a more extensive audit were more moderate in their expenses.

The graphical evidence in Figure 2.B suggests that implementing organizations are particularly likely to maximize expenditure in the absence of control, suggesting some complementarity between irregular and regular expenditures at baseline. Arguably, unproductive expenditure items are more glaring in the absence of productive expenditure. For instance, some organizations report expenditures on dinners and gifts. Clearly, dinner expenditures would look really bad had they been the *only* expenditures, but can, under a sloppy control device, look quite regular and pass unnoticed if they are reported as complements to a seminar or workshop (from an auditor’s viewpoint, this is indeed the very definition of “motivated” side-expenditures). This reasoning implies that irregular expenditures are used to inflate the real project costs, but only to the extent that there *are* any real project costs.

It is instructive to think about the variation in expenditure around the budget threshold using the stylized model presented in section 4.2. At low  $\lambda$ , self-confident managers (high  $\sigma$ ) do not return any funds at all. Rather, self-confident but self-interested managers direct the funds into the project, but with a high level of irregular spending (diverted funds). Under the conventional economic prediction, control will increase  $\lambda$  and the model implies that some of the self-confident organizations will start returning funds instead of misusing them (Implication 3).

To formally test how the threat of audit affected the tendency to inflate expenditures associated with a successful project implementation, we created a variable equal to one if the organization had implemented the project *without* maximizing expenditure. On this margin, we see a dramatic difference between the treatment group and the reference group (Table 5). The estimates are insensitive to how project implementation or expenditure maximization are defined. Virtually no organization in the reference group report a successful implementation of the project while returning funds. When defining project implementation based on a minimum level of media visibility, the treatment effects are slightly smaller but still significant.

## 5.5 Crowd-out

We now ask whether the intervention crowded out participation and private charity. Private contributions are reported by the organizations themselves. The tendency to re-apply for new funds for the upcoming year (2010) is observed in Sida’s registers. Both of these outcomes answer, in different ways, whether the intervention promoted short-term outcomes while permanently damaging the nonprofit sector’s ability to raise private charity and activism.

The results are presented in Table 7. Private contributions to the project are very close to the stipulated threshold of ten percent in both groups. The median share of private contributions is 10.7 percent in the reference group and 10.4 percent in the treatment group; a small and statistically insignificant difference. That the stipulated threshold is already

binding for many organizations suggest that the leeway to adjust this particular margin downwards is slim.

Since the nonprofit organizations bring effort rather than financial resources to the table, the tendency to reapply for new funds seems like a more promising indicator of motivational crowd-out. But this margin tells a similar story – there is no tendency that monitored organizations are discouraged from applying for new funds. The probability that an organization receives new funds in 2010 is about 50 percent in both groups.

## 6 Conclusion

We study the effect of a policy intervention that increases control over nonprofit organizations by replacing a trust-based enforcement mechanism with one where the principal monitors the organizations directly. We do this through a randomized policy experiment, allowing us to estimate causal effects of the intervention under relatively weak assumptions. The study is motivated by recent laboratory evidence linking material incentives and control to inefficiencies, suggesting that control might crowd out intrinsic motivation and make agents less willing to contribute to the public good. We argued that the Swedish nonprofit sector provides an ideal sample when evaluating the field relevance of these mechanisms. Nonprofit workers have an explicit mission, they are motivated by intrinsic values, and in this context, the relationship with the sponsor has traditionally been based on a nontrivial degree of trust. Yet, we find no evidence of motivational crowd-out. Rather, the intervention appears to have had a positive effect on effort, as the outreach of the nonprofit organizations improved and the use of governmental funds decreased.

The experiment was designed so as to keep the original policy context as undisturbed as possible, and we deliberately abstained from introducing new policy objectives to the subjects. The wording of the threat of audit and the instructions given to the contact person at Sida aimed to minimize the Hawthorne effect and treatment externalities. Indeed, it is unlikely that the organizations in the treatment group knew that they were subjects in an experiment and that they would eventually be compared to a reference group. Supplementary results indicate that the reference group, in turn, did not know at all about the experiment.

Since notions about trust and reciprocity were embedded in the contractual relationship from the start, and not part of the experimental “frame”, we are able to estimate the response to control among intrinsic-motivated agents in their natural habitat. This might be key to understanding why we find positive and mission-consistent effects, contrasting previous results in laboratory games – the altruistic objectives were not imposed by the research design. Another striking difference is that the principal in our study is not neutral *ex ante*. Rather, Sida is something of a moral authority in the Swedish nonprofit sector. It is perceivable that the reputation of the principal interacts with the impact of control, potentially even to the extent that material incentives crowds in effort, so that control makes the agents even more willing to impress the principal. We still have more to learn about how such interactions manifest themselves in similar contractual arrangements in public bureaucracies.

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## A Appendix



Figure 3: Letter sent to the treatment group in February 2009. Translation from Swedish to English by authors.

Table 1: Predetermined variables in treatment and reference group.

	Treatment	Reference	Difference	Standard error
Comitted public funds 2009	389	317	72	99
Log of Sida funds 2009	12.01	11.80	0.21	0.24
Project budget	535	444	91	128
Log of project budget	5.47	5.34	0.13	0.23
Received funds in 2008	0.650	0.705	-0.055	0.086
Sida funds 2008	295	219	76	106
Independent organization	0.650	0.557	0.093	0.089
Start year	1986	1987	-0.39	2.33
Time frame	1.4	1.4	0.01	0.11
Observations	60	61	121	121
Test of joint significance				
- chi-square (9 restrictions)	8.00			
- p-value	0.5344			

Notes: Funds and budget figures in thousand SEK. Start year contains one missing value. Independent organization equal to one if organization belonged to the Forum Syd umbrella organization, zero otherwise.

Table 2: The impact of the threat of audit on detected irregularities (missing documentation).

	Levels		Differences	
	Treatment	Reference	Without controls	With controls
Number of irregularities	1.017	1.377	-0.360*** (0.135)	-0.405*** (0.128)
Number of irregularities excluding unclear outreach	0.850	1.049	-0.199** (0.098)	-0.256*** (0.090)
Observations			121	

Standard errors in parenthesis (robust when using controls). The regression adjusted estimates are obtained using linear probability models. In these models, controls are log of funds, square of log of funds, log of budget, square of log of budget, framework organization, years of operation (dummies for missing value and censored data), number of other projects run by the organization, time frame of the project, and a dummy indicating whether the organization received support from Sida in 2008. Number of irregularities span from 0 to 4 (0 to 3 when excluding unclear outreach). See Table 6 for further details.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 3: The impact of the threat of audit on outreach among Swedish nonprofit organizations. Shares.

	Levels		Differences	
	Treatment	Reference	Without controls	With controls
High self-reported outreach	0.567	0.279	0.288*** (0.087)	0.273*** (0.087)
Media visibility (average)	0.531	0.403	0.128** (0.062)	0.109** (0.052)
Media visibility (minimum level)	0.850	0.689	0.161** (0.076)	0.130** (0.063)
Observations	60	61	121	121

Standard errors in parenthesis (robust when using controls). Controls are log of funds, square of log of funds, log of budget, square of log of budget, framework organization, years of operation (dummies for missing value and censored data), number of other projects run by the organization, time frame of the project, and a dummy indicating whether the organization received support from Sida in 2008. Average media visibility is equal to the number of assistants who found a positive media hit, divided by the total number of assistants (seven). The minimum level of media visibility is equal to one if at least one assistant found media coverage of the project.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$



Table 4: Expenditure maximization of governmental grants among nonprofit organizations: Monitored (treatment) vs. Non-monitored (reference).

	Levels		Differences	
	Treatment	Reference	Without controls	With controls
Expenditure of grants (share of total)				
Mean	0.801	0.850	-0.050 (0.061)	-0.071 (0.051)
Median	0.926	0.995	-0.073*** (0.0188)	-0.047** (0.0214)
Organizations hitting the budget, by interval				
±1%	0.267	0.393	-0.127 (0.085)	-0.115 (0.082)
±2%	0.300	0.475	-0.175** (0.088)	-0.176** (0.082)
±3%	0.333	0.525	-0.191** (0.089)	-0.199** (0.084)
±5%	0.383	0.557	-0.174* (0.090)	-0.188** (0.083)
Organizations reporting a surplus, by size of surplus				
> 1%	0.617	0.475	0.141 (0.090)	0.143* (0.087)
> 2%	0.600	0.443	0.157* (0.090)	0.171** (0.086)
> 3%	0.583	0.426	0.157* (0.091)	0.170** (0.087)
> 5%	0.567	0.426	0.140 (0.091)	0.152* (0.087)
Observations	60	61	121	121

Standard errors in parenthesis. Controls are log of funds, square of log of funds, log of budget, square of log of budget, framework organization, years of operation (dummies for missing value and censored data), number of other projects run by the organization, time frame of the project, and a dummy indicating whether the organization received support from Sida in 2008. Median expenditure estimates are obtained using least absolute deviation (LAD).

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 5: The probability of implementing the project without maximizing expenditure: Monitored (treatment) vs. Non-monitored (reference).

	Levels		Differences	
	Treatment	Reference	Without controls	With controls
Project implementation defined as a high self-reported outreach				
Surplus				
> 1%	0.350	0.066	0.284*** (0.070)	0.298*** (0.070)
> 2%	0.333	0.049	0.284*** (0.067)	0.305*** (0.065)
> 3%	0.317	0.033	0.284*** (0.064)	0.304*** (0.063)
> 5%	0.300	0.033	0.267*** (0.064)	0.287*** (0.063)
Project implementation defined as outreach in media				
Surplus				
> 1%	0.533	0.295	0.238*** (0.088)	0.231*** (0.086)
> 2%	0.517	0.262	0.254*** (0.086)	0.259*** (0.085)
> 3%	0.500	0.262	0.238*** (0.086)	0.243*** (0.085)
> 5%	0.483	0.262	0.221** (0.086)	0.225*** (0.085)
Observations	60	61	121	121

Table displays the joint probability of implementing the project *and* reporting a surplus in the budget. The last two columns display raw differences and regression-adjusted difference with controls. Controls are log of funds, square of log of funds, log of budget, square of log of budget, framework organization, years of operation (dummies for missing value and censored data), number of other projects run by the organization, time frame of the project, and a dummy indicating whether the organization received support from Sida in 2008.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 6: Supplementary results: the impact of the threat of audit on irregularities. Seemingly unrelated regression (SUR) estimates over components in the auditor’s report.

	Levels		Differences	
	Treat-ment	Ref-erence	Without controls	With controls
Personel costs (missing/unclear )	0.050	0.115	-0.065 (0.050)	-0.084 (0.053)
Investment costs (missing/unclear )	0.017	0.049	-0.033 (0.033)	-0.049 (0.033)
Cost-benefit analysis (missing/unclear )	0.783	0.885	-0.102 (0.067)	-0.123** (0.057)
Outreach (missing/unclear )	0.167	0.328	-0.161** (0.078)	-0.149** (0.074)
Average over group outcomes				
- estimate			-0.120***	-0.135***
- chi-square			7.26	9.98
- p-value			0.0071	0.0016
Observations	60	61	121	121

All outcomes are binary, and equal to one if the activity was reported unclearly or not at all (as measured by Sida’s designated auditor). Controls are log of funds, square of log of funds, log of budget, square of log of budget, framework organization, years of operation (dummies for missing value and censored data), number of other projects run by the organization, time frame of the project, and a dummy indicating whether the organization received support from Sida in 2008. The average over group outcomes is a joint test of whether the average treatment effect is equal to zero.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 7: Crowd-out: The impact of increased monitoring on private charity and the probability of applying for new funds.

	Levels		Differences	
	Treat-ment	Ref-erence	Without controls	With controls
Private contributions (share of expenditure)				
Mean	0.136	0.161	-0.025 (0.026)	-0.007 (0.022)
Median	0.108	0.104	-0.004 (0.007)	-0.005 (0.004)
Private contributions more than 10 percent	0.600	0.639	-0.039 (0.089)	-0.012 (0.088)
Re-applied for funds 2010	0.517	0.525	-0.008	-0.052
Observations	60	61	121	121

First two outcomes are shares, the last two outcomes are binary. Controls are log of funds, square of log of funds, log of budget, square of log of budget, framework organization, years of operation (dummies for missing value and censored data), number of other projects run by the organization, time frame of the project, and a dummy indicating whether the organization received support from Sida in 2008. Standard errors in parenthesis.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

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