The Marginal Rate of Corruption in Public Programs*

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Abstract

Optimal fiscal policy depends on the marginal benefits of public spending. In developing countries corrupt officials often embezzle funds, so optimal policy should reflect marginal corruption. We analyze marginal corruption in the context of a statutory wage increase in India's employment guarantee scheme. Strikingly, workers received none of the increase even though initially they were on average overpaid. The data are inconsistent with theories of "voice" in which the threat of complaints limits corruption, but consistent with "greasing the wheels" theories in which (a) corruption undoes price interventions and (b) the market bounds how much rent officials can extract.

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1 Introduction

A core principle of public finance is that the marginal costs and benefits of social spending should be equated.[1] In the standard approach the marginal benefits of spending are simply the marginal per-dollar benefits of the activity being financed. This implicitly assumes, however, that money allocated by the government reaches its intended use. In many countries this is not the case: substantial sums "leak out" due to corruption.[2] Recent research has documented this for countries as diverse as Brazil, India, Indonesia, and Uganda, with estimates of leakage ranging from 18%-87%.[3] Olken (2006) emphasizes that corruption drives up the effective costs of redistribution, so that governments anticipating a high leakage rate will optimally choose a low level of transfers.

Specifically, optimal redistribution depends on the *marginal* rate of leakage, or the amount of the marginal dollar spent that does not reach its intended use. Here difficulties arise. Even if the planner can measure current average leakage rates, it is unclear what information these contain about marginal rates. For example, suppose 50% of a transfer is currently being diverted. Marginal leakage could be 50% if transfers are shared proportionally with beneficiaries, or 0% if officials take a fixed cut, or 100% if officials pocket all but a fixed amount. Understanding how to distinguish among such possibilities is an open problem.

This paper provides the first empirical analysis of marginal rates of corruption. We study India's largest welfare program, the National Rural Employment Guarantee Scheme (NREGS), which entitles every rural household to up to 100 days of paid employment per year. The scheme covers 850 million people – India's entire rural population – and costs roughly 1% of India's GDP. Statutory wages are set by state governments, but the local officials who implement the scheme do not always pay workers the wages to which they are entitled. A central policy question is therefore how actual wages vary with the statutory wage.

We examine this question using data from an original survey of 1,938 households in the eastern state of Orissa, who were listed in official records as having participated in the NREGS between March and June of 2007. We collected data on all spells of NREGS work done by these households and compared these to the corresponding official micro-

[1] The appropriate measure of marginal costs is actively debated; one tradition emphasizes the distortionary costs of taxation while another sees these as a separable redistributive issue (Kaplow, 2004; Kreiner and Verdelin, 2009).

[2] We use "leakage" throughout to refer to theft of public funds as opposed to, for example, the dissipation of benefits through deadweight losses or mis-targeting of benefits to the non-poor.

[3] For example, Reinikka and Svensson (2004) estimate that on average 87% of a block grant intended for primary schools in Uganda was diverted by local officials. India's Planning Commission estimates that 58% of the subsidized grains allocated to the Targeted Public Distribution System are diverted (Programme Evaluation Organization, 2005). Olken (2006) places a lower bound of 18% on the fraction of rice diverted from Indonesia's OPK program. See also Chaudhury et al. (2006), Olken (2007), and Ferraz et al. (2010).

data. The statutory wage due to participants changed from Rs. 55 to Rs. 70 half-way through this study period, allowing us to estimate marginal leakage along the program's main margin of adjustment.

Figure 1 summarizes our main result. It plots the evolution of wages paid during our study period, distinguishing between wages paid according to official records and wages actually received by program participants. The statutory wage increase is clearly reflected in official reports. What is striking is that *none* of the wage increase was passed through to workers. Thus while average leakage on this margin prior to the policy change was close to 0%, marginal leakage was 100%. This point is (unsurprisingly) re-affirmed by regression estimates. To ensure that the result is not driven by a contemporaneous, offsetting negative shock we also estimate specifications that take as a control group villages in which *official* records do not reflect the wage change, because of distance-related communication lags. We find no significant differences; if anything wages are differentially lower in the "treated" villages (Figure 2).

What is it about the nature of corruption that leads margins and averages to diverge so substantially and in this particular direction? In the second part of the paper we examine alternative interpretations. We obtain two main results: our data are inconsistent with a class of theories based on "voice," but consistent with and at least partially explained by an alternative theory based on "exit" (Hirschmann, 1970).

By "voice" we refer to the idea that rule-bending is kept in check by the threat that the victim may complain. This mechanism appears to play an important role in some settings but not others. For example, Reinikka and Svensson (2004) argue that variation in Ugandan communities' ability to complain explains variation in average leakage rates, while Olken (2007) finds that facilitating complaints had a limited impact on corruption in Indonesian road projects. The NREGS includes formal provisions for a complaint-handling mechanism, but how effectively these function in practice is an empirical question.

Our data suggest they function poorly. First, complaint-based theories predict positive average leakage but also positive marginal pass-through; intuitively this is because the value of complaining rises with the value of the benefit being denied. This is contrary to our main result, however. Respondents' themselves say that voice plays a limited role: while 36% of participants reported having experienced problems while working, only 7% said that they had or would deal with a problem by complaining to higher-up authorities. Twenty-two percent said they would do nothing at all, citing the costs of complaining (53%) and the low probability of success (37%).

It could be that while complaints are ineffective for the typical worker, they do matter for some. In particular, lack of awareness of the wage change could be a constraint. We do not find any evidence, however, of higher pass-through among workers who knew about the change (72% of work spells in our sample). This is inconsistent with a dynamic story in which wages converge to the statutory ones once people learn about the policy change. We also find no evidence of higher pass-through among workers who live closer to the government offices where complaints are (ostensibly) heard, suggesting that travel costs are not the limiting factor. Interestingly, we do find some evidence of positive pass-through in the 36% of villages in which an NGO is active. This suggests that NGOs (or factors correlated with their presence) may facilitate voice, though not enough to be detectable in the aggregate.

The difficulty of reconciling voice-based models with our data raises a puzzling question: if officials are not afraid of getting in trouble for underpaying, then why do they pay at all? A variety of forces could be at work. We test one interpretation built on the seminal insight of Leff (1964) and Huntington (1968) that corruption can "grease the wheels" of the economy, undoing distortionary policies. The NREGS is distortionary in the sense that it should act as a wage floor; corrupt officials could undo this effect, however, by simply paying workers the market wage rather than the statutory wage. Hiring at the market wage, as opposed to not hiring at all, makes sense for officials because they benefit indirectly from worker participation. (As we discuss below, this is because officials often over-report the amount of work done on NREGS projects; getting at least some work done may reduce their risk of getting in trouble for doing so.) To summarize, the "exit" hypothesis predicts that program wages are determined not by laws but by the market.[4]

To test this prediction we turn to data on workers' outside options. Ninety-six percent of respondents said that the private labor market was the outside option relevant for them, so the exit hypothesis amounts to saying that local labor market conditions determine program wages. We implement this test using variation in villages' relative endowments of land and labor. Although de jure wages should be the same everywhere, de facto they are substantially and significantly higher in villages that are land-abundant and labor-scarce. This suggests that the threat of exit to the private sector at least partly explains NREGS wages. Interestingly, these results show that the fact that workers were paid the right wage on average before the shock is misleading: it masks the fact that workers in some areas were overpaid while others were underpaid. In general, neither the pre-shock nor the post-shock equilibrium involved systematic compliance with the rules.

A potential caveat to this result is that factor endowments could matter not because they affect wage offers but because they affect who accepts offers from a given wage distribution, and thus who appears in our sample of NREGS participants (Heckman, 1979). We present three pieces of evidence to rule this out. First and most importantly, participation rates and wages are correlated with factor endowments in the same direction, inconsistent

[4]This idea is related to papers that emphasize participation constraints as a determining factor of equilibrium bribe levels, such as Svensson (2003) and Hunt (2007).

with selection. Second, while selection models predict that factor endowments shift the lower end of the NREGS wage distribution, we find that they shift the upper end. Third, we use survey data on respondents' reservation wages to obtain selection-corrected point estimates; these turn out to be essentially identical to the uncorrected ones.

Our analysis of marginal leakage is part of a recent effort to adapt public economic theory for use in developing countries.[5] Anticipating the threat of corruption is essential: as Banerjee et al. (2009) argue, "it is impossible to understand policy without understanding corruption." Workfare is a relevant case given the many schemes in place worldwide (Subbarao, 2003), to say nothing of the 11% of the world's population covered by the NREGS itself. Earlier analyses of workfare could fruitfully be extended to the case of imperfect enforcement we study (Ravallion, 1987; Ravallion et al., 1993; Basu et al., 2009). More broadly, the fact that the corruption literature has documented voice and exit constraints in other contexts suggests that these concepts may be generally useful for understanding marginal leakage.

The rest of the paper is organized as follows: Section 2 describes the NREGS setting, and Section 3 describes the data collected. Section 4 presents our main empirical results on marginal leakage; Section 5 examines the voice hypothesis in greater depth, and Section 6 the exit hypothesis. Section 7 concludes.

2 Contextual Background

India's National Rural Employment Guarantee Scheme is a central pillar of welfare policy in rural India. Launched in 2005, it extends to every rural household the right to up to 100 days of paid employment on government projects per year.[6] The rationale for the work requirement is to induce self-selection of the poor into program participation (Besley and Coate, 1992). The NREGS is a fiscal behemoth; the central government's budget allocation for fiscal year 2010-2011 is Rs. 401 billion ($8.9 billion), 3.6% of government expenditures, or 0.73% of 2008 GDP.[7] Total program expenditures are higher as state governments are responsible for a share of the cost (25% of the cost of materials).

From the point of view of a worker, the process of NREGS participation begins with an application for a job card. The job card lists household members and contains empty spaces for keeping records of their subsequent employment and compensation. Households obtain jobcards at either their local Gram Panchayat or block/sub-district office

[5] Examples include Keen (2008), Gordon and Li (2009), Olken and Singhal (2010) and Pomeranz (2010) on taxation and Atanassova et al. (2010) on poverty-targeting.

[6] This 100-day limit is rarely binding; almost no one in our sample reached it, and our understanding from work in Orissa and Andhra Pradesh is that in practice no one is denied work for having attained the limit.

[7] Costs: `http://indiabudget.nic.in/ub2010-11/bh/bh1.pdf`. Expenditures: `http://indiabudget.nic.in/ub2010-11/bag/bag3.htm`. GDP: `GDP:http://mospi.nic.in/4_gdpind_cur.pdf`.

(the lowest and second-lowest units in the Indian administrative hierarchy, respectively). Jobcard in hand, workers from the household can apply for spells of up to 15 days of work. The officials who receive the work application are legally obligated to provide the worker with employment on a project located within 5 km of the worker's home.

The projects undertaken through the NREGS are typical of rural employment generation schemes – road construction and irrigation earthworks predominate. The administration of these projects is the responsibility of the Gram Panchayat (GP), whose key figures are the elected Sarpanch and the appointed Panchayat Secretary. The day-to-day supervision of projects is typically delegated to a Village Labor Leader (a GP employee) or to a Junior/Assistant Engineer in the relevant state department. The use of private contractors to execute projects is prohibited but occurs nevertheless.

Workers receive either a fixed wage per day or a piece rate per unit of output (e.g. per cubic foot of soil excavated) depending on the feasibility of measuring output. In either case participation and implied compensation are recorded on an official muster roll. The paper muster rolls are periodically submitted to the local block office, where the data are entered into a national database. The state and national governments advance funds to the panchayats to compensate workers and replenish these funds on the basis of the records entered into the database. Most of the workers in our study received their wages in cash from panchayat officials, though a few were paid through a bank or post office account and efforts are underway to increase the use of banks for wage payment.

These wages are the principle margin of adjustment within the NREGS, and our focus will be on marginal leakage with respect to changes in the statutory wage. NREGS wages have changed frequently since the program's inception because of the way in which the scheme is financed: the wage bill is paid by the central government, but wage rates are state-specific and determined by state governments. This gave state politicians strong incentives to raise statutory wages, and most did so repeatedly. This paper examines the impacts of an increase in the minimum daily wage in the eastern state of Orissa from Rs. 55/day to Rs. 70/day on 1 May, 2007.

We focus on impacts on leakage from the labor budget, which by law must be at least 60% of total expenditure and in practice is often substantially higher. The officials who implement the NREGS can steal from the labor budget in two ways: they can underpay workers, and they can over-report the number of person-days of work done. For example, if a worker works for 10 days and is owed Rs. 55 per day the official might report that he worked for 20 days and pay him Rs. 50 per day, earning $(20-10) \times 55$ from over-reporting and $10 \times (55-50)$ from under-payment.

Under-payment (theft from beneficiaries) and over-reporting (theft from taxpayers) are monitored in distinct ways. Underpaid workers can in principle access a formal grievance redressal process. The first point of appeal is the Program Officer, a block-level role typically filled by the Block Development Officer (BDO); further appeals go to the district Programme Coordinator, a role played by the District Collector. Both the BDO and the Collector are appointed bureaucrats from the state or national administrative service. According to the guidelines these officials should accept grievances on standardized forms and issue a receipt for each accepted form to the petitioner, allowing them to follow up. Given that the eventual basis for decisions might be the official's word against the beneficiary's, however, it is an empirical question whether this system actually provides workers with a cost-effective means of resolving problems.

Workers have less incentive to monitor over-reporting because the program's budget is not fixed; a rupee stolen through over-reporting does not mean a rupee less for them. Over-reporting is therefore monitored primarily from the top down. The NREGS Operational Guidelines call for internal verification of works by officials (100% works audited at the block level, 10% by district level monitors, and 2% by state level monitors). (Ministry of Rural Development, 2008) In practice we found that block and district officials use the NREGS's management information system (MIS) to track aggregate quantities of work done on various projects and compare these to technical estimates or to their own intuitions about how much work should be necessary. In some cases they kept progress photographs of worksites which they could use to assess the veracity of the muster roll data. This practice gives local officials some incentive to induce workers to participate, if only to make the over-reporting they do engage in less obvious.[8]

3 Data Collection

The NREGS is unusually amenable to an audit study because program micro-data are, by law, available online to the public (`http://NREGS.nic.in`). Data available from jobcards include the roster of individuals within each household with their names, genders, and ages. These records can be matched to muster roll information on each spell of work performed, which includes the individual who worked, the project worked on, number of days worked, and amount earned. Muster rolls do not explicitly state whether a spell was compensated on a daily wage or piece rate basis; we can infer this, however, since the few allowed daily wage rates are round numbers that would rarely occur by chance under a piece rate scheme.[9]

In order to construct a sample frame we downloaded (in January 2008) all muster roll information for the period March-June 2007, i.e. two months before and after the

[8]Officials face a low but positive probability of getting caught and punished. A coalition of non-governmental organizations that monitor NREGS in Orissa has reported numerous instances of officials being caught and suspended by their District Collectors (`http://groups.google.co.in/group/oregs-watch`).

[9]These are Rs. 55, 65, 75, and 85 prior to the wage change, and Rs. 70, 80, 90 and 100 afterwards. The higher rates are for skilled categories of laborers and are rarely applied.

statutory wage change on 1 May 2007.[10] We sampled work spells from the official records for Gajapati, Koraput, and Rayagada districts in Orissa.[11] We then sampled 60% of Gram Panchayats within our study blocks, stratified by whether or not the position of GP chief executive was reserved for a woman or ethnic minority.[12] Finally, we sampled 2.8% of work spells in these panchayats, stratifying the sample by panchayat, whether the project worked on was implemented by the block or panchayat government, whether the project was a daily wage or piece rate project, and whether the spell began before or after 1 May 2007. This yielded a sample of work spells and an implied sample of 1938 households whom we set out to survey.

Like much of central India, our study area experiences frequent conflict. Sources of violence include the activity of the Naxals (armed Maoist insurgents), disputes between mining conglomerates and the local tribal population, and tensions between evangelical Christian missionaries and right-wing Hindu activists. We attempted to sample around areas known to be experiencing conflict, but in the end were unable to attempt to reach 439 of our 1938 households without exposing our enumerators to unacceptable risks. The main issues were conflict between locals and a mining company in Rayagada and a polite request by the Naxals to not enter certain areas of Koraput. Of the remaining 1499 we were able to reach or confirm the non-existence/permanent migration/death of 1408 households. In order to determine whether an individual/household that was included in the official records was actually non-existent or dead or no longer lived in the village, we asked surveyors to confirm their status with 3 neighbors who were willing to supply their names on the survey. Households who do not match these stringent standards are excluded from our analysis.

Given these omissions, an important issue is the extent to which the spells of work we analyze are representative of the frame we sampled from. Table 1 provides summary statistics from the official records for the universe of spells in our study region, our initial sample, and the subset of spells included in our analysis. As one would expect, values for the frame and the initial sample are essentially identical. Reassuringly, differences between the initial sample and the analysis sample are also small and statistically insignificant, with one exception: the fraction of spells performed by members of a Scheduled Caste or Scheduled Tribe is 0.79 in the initial sample and 0.77 in the analysis sample and this difference is significant ($p = 0.05$). This likely reflects the fact that violence was

[10]We waited until January to ensure that all pertinent muster roll information had been digitized and uploaded – by law this should take place within two weeks after work is performed, but in practice delays of several months are common. As a consistency check we also downloaded the same data again in March 2008 and verified that it had not changed.

[11]We restricted ourselves to blocks (sub-districts) that border the neighbor state of Andhra Pradesh. Our companion paper uses additional data from AP as a control for trends in Orissa, but since almost all work in AP is compensated on a piece rate basis we do not use it here.

[12]Chattopadhyay and Duflo (2004) find that such reservations affect perceived levels of corruption.

concentrated in tribal areas. There is no evidence of differential selection by the key spell characteristics (wage rate and date) we study below.

We interviewed respondents about their NREGS participation and in particular about spells of work they did between March 1, 2007 and June 30, 2007. We also collected data on household demographics, socio-economic status, awareness of NREGS rules and of the wage change, labor market outcomes, and political participation. Table 2 provides demographic information on the households in our sample. Notably, only 821 of $1,408$ households reported ever doing any work on the NREGS.

Given the lag between the study period and our survey, imperfect recall might be anticipated. The NREGS was a new and very salient program, however, and spells of work were likely to be memorable and distinct compared to other employment. Moreover, since participants do not necessarily get paid what they are owed and often not on time, they are likely to keep track of how much they worked and what they received. To prompt respondents' memory we asked about work on specific NREGS projects with detailed descriptions, for example "Imp[rovement]. of Road from Brahmin street to DP Camp at Therubali". We also trained enumerators to use standard techniques for enhancing recall, such as providing major holidays as reference points. Consequently, we obtained information on wages received for 99% of the spells in our sample and data on at least the month in which work was done for 93% of spells. We do not find significant differential recall problems over time: in a variety of specifications including location fixed effects and individual controls such as age and education, subjects' estimated probability of recalling exact dates increases by only 0.7%–2.2% per month and is not statistically significant. We will return to the issue of recall after presenting our main results below.

Another potential measurement issue is related to collusion between officials and respondents; perhaps beneficiaries claim to be getting what they are due while in fact they are splitting their earnings with officials. A priori this seems unlikely; for example, our review of newspaper articles and NGO reports of corruption did not turn up reports of officials getting into trouble for under-payment. In any case, our data are themselves inconsistent with the collusion hypothesis. On the quantity margin we observe huge discrepancies between the amount of work households report doing and the amounts officials report were done. Similarly on the wage margin workers often report being paid less than they think they were owed. Most importantly, collusion would imply that workers' reported earnings change in response to changes in the statutory wage, which we will see below is not the case.

Survey interviews were framed to minimize other potential threats to the accuracy and veracity of respondents self-reports. We made clear that we were conducting academic research and did not work for the government, to discourage respondents from claiming fictitious underpayment. None of the interviewed households have income close to the taxable level and will have ever paid income taxes, so there are no tax motives for underreporting.

4 Estimating Marginal Leakage

We turn now to estimating the proportion of the marginal dollar of program expenditure that does not reach the intended beneficiaries.

Some notation may help clarify the nature of the exercise. Consider an NREGS worker who is entitled by law to a statutory wage $\overline{w}$ but receives a (potentially distinct) wage w. If $w < \overline{w}$ then there is leakage in the form of underpayment. Leakage may also occur through over-reporting the number of days of work done; let $B(\overline{w})$ represent the amount of money stolen per work-day through this or other channels. Then the overall average rate of leakage is

$$AL = \frac{\overline{w} + B(\overline{w}) - w}{\overline{w} + B(\overline{w})} = 1 - \frac{w}{\overline{w} + B(\overline{w})} \tag{1}$$

or total leakage divided by total expenditures. Marginal leakage with respect to an increase in the statutory wage $\overline{w}$ is the change in leakage as a fraction of the change in total expenditure, or

$$ML = \frac{1 + B'(\overline{w}) - \frac{\partial w}{\partial \overline{w}}}{1 + B'(\overline{w})} = 1 - \frac{\frac{\partial w}{\partial \overline{w}}}{1 + B'(\overline{w})} \tag{2}$$

Thus to estimate marginal leakage with respect to a change in $\overline{w}$ we generally need to estimate two things: the effect on recipients' actual earnings $\left(\frac{\partial w}{\partial \overline{w}}\right)$ and on total program expenditures $(1 + B'(\overline{w}))$.[13] If wage pass-through is zero, however, then mechanically marginal leakage can only be 100%.

Figure 1 illustrates our most important result: prior to 1 May wages paid are on average similar to wages reported and to the statutory wage (Rs. 55), but none of the wage increase passed through to workers. Table 3 provides a more formal statistical analysis of pass-through. In columns I-IV observations are spells of daily-wage work reported in the official records, while in columns V-VIII they are spells of daily-wage work as reported by the corresponding households.[14] Note that the official records include all reported spells including fictitious spells by worker, non-worker, and "ghost" households.[15] Columns

[13]This concept of marginal leakage would remain appropriate for policy callibration if changes in $\overline{w}$ affected participation as well, since the marginal participant obtains zero surplus.

[14]We categorize a spell of work as occurring on the day it began, so that a spell which overlapped 1 May would be attributed to the "pre" period. As a robustness check we also dropped overlapping spells (3% each of official and actual spells) and obtained essentially identical results.

[15]We also ran regressions on the restricted set of official spells associated with households that reported doing some work, and obtained essentially identical results. Note that directly matching a particular actual spell to a particular official spell is difficult due to the large number of fictitious officially reported spells.

I-III show that the official wage jumps up significantly after 1 May and that this jump is abrupt enough to be distinguishable from a quadratic trend (Column II) and widespread enough to be distinguishable from panchayat fixed effects (Column III).[16]

One interesting feature of Figure 1 is that while the average wage paid according to official records increases sharply after 1 May, it does not increase all the way to Rs. 70, the new minimum wage. The reason for this is that some panchayats (43% of panchayats with daily wage projects) continued paying the older, lower wage rates even after 1 May. The fact that some panchayats did not even claim to be paying higher wages is something of a puzzle as it means they were leaving rents on the table. The most plausible explanation is that some panchayats did not immediately learn about the wage change. (Consistent with this view, Columns I and II of Table 5 show that the post 1 May increase is roughly Rs. 3 larger in panchayats below median travel time from the block office and from the collector's office. These variables are described in more detail below.) This suggests that it may be informative to treat "unaware" panchayats as a control group when we look at wages actually received by workers. Column IV of Table 3 differentiates between panchayats that ever reported paying a new, higher wage during May or June (the "aware" panchayats) from those that did not; tautologically, the increase in official wages is concentrated in those that are aware.

Columns V-VIII mirror Columns I-IV but with wages actually paid to surveyed households as the outcome. If marginal leakage were equal to the pre-shock average leakage rate we would expect to see actual wages increase by the same amount as official wages. In contrast, and exactly as one would expect from Figure 1, wages are lower after 1 May (Column V). This decrease simply reflects an overall downward trend in wages (Column VI), and this trend is itself largely a compositional effect that disappears when we control for village fixed effects (Column VII).

In Column VIII we differentiate between panchayats that did or did not ever implement the statutory wage change. This lets us test for the possibility that some other factor determining wages changed discretely at the same time as the statutory wage did, offsetting what would otherwise have been a positive effect. If this were the case we would expect to see an increase in wages in panchayats that implemented the policy change *relative* to those that did not. This is not the case, however: the differential effect is negative and statistically insignificant. Figure 2 presents this difference-in-difference graphically: it shows that the actual wages in implementing panchayats parallel those in non-implementing panchayats, while official wages diverge sharply after 1 May. In sum there is strong evidence of 0% pass-through, or 100% marginal leakage.

Given that our survey was conducted well after our study period, it is worth investi-

[16] We use months as the time trend variable for comparability to the household-reported spells data, for which specific start days within months are not always available due to limited recall. Results for the official data are similar using day-of-year trends, however.

gating whether recall problems might be attenuating the estimates in Table 3. Suppose that the wage increase was in fact passed through, at least to some workers, but that they misremembered how much they earned on different spells. Then we would expect to see average actual wages between Rs. 55 and Rs. 70 both before and after the shock, with some attenuated upward trend. None of our estimates match this pattern, however. Going further, we can isolate workers who worked only after the shock, and thus could not have confused their post-shock earnings with those from earlier spells. In fact these workers report receiving slightly *lower* post-shock wages than those who worked both before and after the shock (Rs. 52 vs Rs. 55). One might also worry that respondents confuse NREGS wages with prevailing market wages, but in our data at least 76% of workers report NREGS wages different from market wages, depending on the measure of market wages used.[17] Finally, we will see below that our wage data are strongly correlated with cross-sectional variation in factor endowments and with time-series variation in the statutory wage within villages with active NGOs. These results suggest that our data are accurate enough to pick up effects where they do in fact exist.

As an indirect check for wage pass-through we can also examine effects on participation. If wages did in fact increase then, ceteris paribus, participation should have increased as well; this test has the advantage that participation is presumably easier to recall than the details of payments. Consistent with the direct evidence, Column IV of Table 7 reports that participation was weakly lower after 1 May even after controlling for a trend. (The construction of the dependent variable is discussed in detail in Section 6.1.1 below.)

One final concern is that Figure 1 correctly summarizes the wage dynamics during our study period, but that these are temporary. Might the fact that workers were on average paid the statutory wage prior to 1 May suggest that they will eventually receive the new, higher wage? The argument is, in fact, not this simple. The fact that workers were paid the statutory wage on average during the pre-period is somewhat misleading since, as we will see below, workers in some parts of our study area were paid substantially less, while workers in other areas were paid substantially more. In short, our data are not consistent with the view that the equilibrium prior to 1 May was one of adherence to the statutory wage. This in itself suggests that we should not necessarily expect the new equilibrium to converge to adherence.

We can also directly test for dynamics due to learning. In the aggregate there is clearly no evidence of learning; we also show below that the 72% of workers who were aware of

[17]We asked about market wages separately for men and women and for particular tasks such as road construction and planting/harvesting of rice. The average market wages for men for road construction are almost exactly the same as the average daily wage for men on NREGA works (Rs. 58.6 vs Rs. 57.5), yet 76% of work spells were paid NREGA daily wages distinct from the market wages for road construction reported by the same respondents. Results using other categories of work or wages for women are even more discrepant.

the wage change when we surveyed them earned the same wages as their less-informed counterparts. Our data are thus hard to reconcile with a story in which recipients receive their due once information about the policy change has disseminated.

Recall from Equation 2 that the absence of wage pass-through implies that, regardless of the exact amount by which expenditures increased, marginal leakage cannot be other than 100%. Of course, one can also estimate the amount by which expenditures increased. We have done this in earlier work; our best estimate is that total expenditure per dollar received by recipients increased from \$4.08 to \$5.03. (Niehaus and Sukhtankar, 2010) Expenditures thus increased substantially without any benefit for participants.

5 Is Voice a Binding Constraint?

The stark divergence between average and marginal under-payment seen in Figure 1 seems surprising – but what exactly does it imply about the underlying model of corruption? In the following two sections we examine how well different classes of theories explain this core finding.

We begin with the class of theories in which corruption is kept in check by the threat of victims complaining, an idea we refer to as "voice." Voice appears to be an important determinant of corruption in some contexts but not in others. For example, Reinikka and Svensson (2004) argue that variation in communities ability to complain explains cross-sectional variation in leakage from their school block grants, while Olken (2007) finds that providing community members access to anonymous complaint boxes and inviting them to public audit meetings had only limited effects on corruption in Indonesian road-building projects. It is thus an empirical question whether the NREGS' complaint-handling process described above plays a significant role in practice.

Our pass-through results suggest that it does not. The reason is that, holding fixed the wage w a worker is receiving, the value of complaining increases with the statutory wage $\overline{w}$ he hopes to recover. An official concerned about complaints would therefore have to pay more to forestall them. For example, if the worker can complain at cost c and the complaint is succesful with probability π then he will complain unless $\pi\overline{w}+(1-\pi)w-c \leq w$, so that the official must pay at least $\overline{w}-\frac{c}{\pi}$. If this constraint binds then the worker's wage w should increase one-for-one with $\overline{w}$. The fact that none of the wage change was passed through thus suggests that complaints have little bite.

To better understand how the complaint-handling process was working in practice we asked participants, "Do you feel you were treated fairly at the job site? Or did you have any problems at work?" (to which 36% responded that they had had problems) and then "If you did have any problems, or if a problem were to arise in the future, what would you do about it?" While this is a broad question that does not specifically refer to issues of under-payment, it should shed some light on workers' approach to dealing with wage issues.

The great majority of respondents told us that if they had problems they would either do nothing (22%), or take up the issue with local panchayat officials or village elders (74%), the same officials responsible for implementation of the NREGS to begin with. Only 7% of all workers (and only 13% of workers who had actually experienced problems) said they would appeal at the Block or District levels, which are the entities designated by NREGS guidelines for dealing with grievances (Table 4). Among those who said they would do nothing, the main reasons stated were that complaining would be in vain (37%) and that complaining would be too time-consuming or take too much effort (53%). Ten percent indicated fear of retribution as the main deterrent.

These responses are consistent with what Das and Pradhan (2007) report based on their fieldwork in Orissa:

> "One must apply to the BDO, then to the district collector, and then only to the state level authorities and the CM's office. But, this is precisely where people face a problem. Their applications are stone-walled, by the simple absence of any officials to receive these applications. If there are officials present, they refuse to give receipts, which makes it difficult for the applicants to follow up. In any case the tribal villages are at least an hour's walk away in majority of the cases from the block head office. There are little [sic] options, with the poor public transport, which can cover only a partial distance because of the paucity of roads."

This account is useful both because it underscores the difficulties facing a typical worker and also because it suggests dimensions along which voice may vary. In particular, workers who live closer to the relevant government offices should have more effective voice. To test this conjecture we use data on distances and travel times from our survey of village elders. The average village in our sample is 17km from the corresponding Block office and 38km from the District office, and average estimated round-trip travel times are 3 hours and 5 hours, respectively.[18] We have already seen that panchayats located closer to block and district offices saw larger increases in their officially reported wages (Table 5, Columns I and II). Columns V and VI show, however, that the same is not true for wages actually received. Actual wage changes are insignificantly different in panchayats close to block offices and if anything significantly lower in those located close to district offices.

Another dimension along which voice might vary is information. The literature on information and accountability has shown that information is a binding constraint in

[18]These are times using whatever (possibly costly) means of transport the respondent would use. At a typical walking speed of 3 mph the average round-trip travel times would be 7 hours and 16 hours, respectively.

some contexts. (Reinikka and Svensson, 2005; Besley and Prat, 2006; Ferraz and Finan, 2008) If this were true in our setting then we should see higher pass-through for better-informed workers. Seventy-two percent of the work spells in our sample were done by households that knew that there had been a change in the daily wage rate, and of these 81% were done by households that correctly identified the new wage as Rs. 70 per day. Individual workers claimed to be underpaid on 31% of work spells but overpaid on only 3%.[19] Yet Column IX of Table 5 shows that there is no significant tendency for workers from households that learned of the wage change to receive differentially higher wages after 1 May, as one would expect if awareness were sufficient. Note that while awareness is clearly endogenous, the most natural biases (aware individuals are also more influential in other ways) would tend to inflate this coefficient, not bias it towards 0.

Finally, voice might vary with the likelihood that a given complaint succeeds. While this is a difficult dimension to measure, one plausible proxy is the presence of an active non-governmental organization (NGO) in a village. NGOs in Orissa have formed a loose coalition devoted to monitoring NREGS implementation and ensuring that participants obtain their entitlements; at least one NGO is active in 36% of the villages in our sample. Columns III and VII examine whether the effects of the policy change were different in these villages. Interestingly, while we find no differential effects on officially reported wages, we do find a significant positive effect on wages actually received in villages with an active NGO. This is consistent with the idea that NGOs help program participants hold government accountable. This is not the only reasonable interpretation, since having an NGO may be correlated with many other unobservable variables. At a minimum, however, the result establishes that there exists some such variable that improves accountability.

6 Is Exit a Binding Constraint?

Voice seems to play a minor role at best in disciplining NREGS officials. This raises a puzzling question: if officials are not afraid of getting in trouble for underpaying workers, then why do they pay them at all?

In principle a variety of forces could be at work. For example, elected officials such as the sarpanch may pay attractive wages to improve their re-election prospects; more generally, officials may employ workers in exchange for other favors. Wage rates would then be determined by the value of votes or favors. In this section we test the hypothesis that program wages are determined, at least in part, by market wages. This idea builds on the classic argument of Leff (1964) and Huntington (1968) that corruption "greases the wheels" of the economy by undoing distortions introduced by government intervention in the market. The NREGS as legislated is clearly distortionary, as it should act as a floor

[19] Claiming to be underpaid is strongly positively correlated with actually being underpaid.

on wages. Leff and Huntington would predict that corruption will tend to close the gap between market and statutory wages. This view seems consistent with our conversations with NREGS participants, who openly discussed negotiating with program officials until they received a satisfactory wage.[20]

Of course, this "exit" hypothesis does not explain why officials care about exit. Why not simply let the worker leave, claim that he worked, and pocket his entire remuneration? Some of this undoubtedly happens. One reason it may not always be an optimal strategy, however, is that hiring a worker to do *some* work makes it less risky to over-report a good deal more. For example, it may seem safer to claim that it took 150 person-days to dig a hole in the ground when there actually is a hole in the ground that took 50 person-days to dig than when there is no hole at all. More generally, paying a worker to do some work may increase the amount one can safely over-report by more than enough to make it profitable.

For a formal illustration, let $\underline{w}$ be the worker's reservation wage, i.e. the least he would be willing to accept in order to do NREGS work. (One naturally thinks of $\underline{w}$ as the wage he could earn in the private sector, but in principle it could simply be his valuation of leisure.) If the worker works then the official increases the amount of remuneration he claims on the muster rolls by at least $\overline{w}$ and possibly by a further $B(\overline{w})$ in additional over-reporting, so that the pair's total surplus from reaching agreement is $\overline{w}+B(\overline{w})-\underline{w}$. When this is negative the worker will work in the private sector; when positive, the official hires the worker at a wage of at least $\underline{w}$. In this view the statutory wage may have little effect on the worker's realized wage w while the market conditions that determine $\underline{w}$ play a key role. Note also that if $\overline{w} < \underline{w} < \overline{w} + B(\overline{w})$ then the equilibrium involves *over*-payment: the official finds it profitable to hire workers even at wages above the statutory one.

One intruiging piece of circumstantial evidence consistent with this hypothesis is visible in Figure 1. During the first month of the study period the mean wage received by households is actually higher than the mean wage reported in official records. This gap is driven by a large number of observations from Gajapati district where both prevailing market wages and households' reported NREGS wages are relatively high. NGOs working in this area have reported that officials do in fact overpay workers to induce them to participate precisely because this creates scope for further theft in the form of over-reporting.

More generally, if the exit constraint binds then variation in workers' outside options should be positively related to the NREGS wage realizations we observe. Implementing this test requires a measure of variation in those outside options. Private-sector employment, rather than leisure, appears the be the relevant outside option: when asked what

[20]See also Svensson (2003), Bertrand et al. (2007), Hunt (2007), and Olken and Barron (2009) for evidence on bargaining between citizens and corrupt officials in other contexts.

they would have done if the NREGS wage were below their reservation wage, 96% of respondents indicated some other form of work as opposed to only 4% who said they would have waited for a better wage. Higher private sector wages should therefore lead to higher NREGS wage realizations.

A naive approach to testing this hypothesis would be to regress NREGS wages and participation on private sector wages. The direction of causality would be unclear, however; indeed the standard view of employment guarantee schemes is that they act as a binding floor on private sector wages. To circumvent this simultaneity issue we exploit variation in local factor endowments. If a village endowed with cultivatable land T and labor L produces output $Y = F(T, L)$ then the competitive real wage will be $\underline{w} = F_L(T, L)$; assuming decreasing returns to labor and land-labor complementarity this wage will be decreasing in the labor endowment and increasing in the land endowment.

We matched our survey data to records from the 2001 Census on the stock of cultivatable land and the total population at the Gram Panchayat level.[21] Unlike contemporaneous market wages these quantities were pre-determined prior to the launch of the NREGA in 2005, so there is no concern about reverse causality. Relative factor endowments also vary substantially in our data. This need not imply variation in reservation wages, since the effects could be offset by variation in other unmeasured factors. Reservation wages will vary, however, if there is variation in location-specific consumption amenities that compensate for real wage differentials, or if labor mobility is limited. The chief concern is that that factor endowments are correlated with other determinants of worker's bargaining power; we will check the sensitivity of our results to controlling for a battery of variables that one would expect to capture such variation.

Table 6 reports estimates of the relationship between factor endowments and wages. All specifications include month fixed effects and thus implicitly control for any effects of the statutory wage change. As a preliminary we first examine in Column I the relationship between factor endowments and workers' reservation wages, i.e. the lowest wage for which they would be willing to accept NREGS work (we describe this variable in more detail below). Consistent with the hypothesis that factor endowments affect the marginal product of labor, we find that reservation wages are significantly higher in relatively land-abundant panchayats and lower in labor-abundant ones. In Columns II-V we show that this also holds for NREGS wages, consistent with the view that NREGS wages respond to variation in workers' labor market opportunities. A 10% increase in cultivatable land is associated with a Rs. 0.7 higher NREGS wage, while a 10% increase in population is associated with a Rs. 0.9 lower NREGS wage. In Column III we include worker-level proxies for bargaining power; we find that men, non-minorities, and workers paid

[21] We define cultivatable land as the sum of "irrigated farmland", "unirrigated farmland", and "cultivatable waste".

through banks receive significantly higher wages, reflecting either stronger bargaining power or better outside options. In Column IV we control for village-level predictors of bargaining power such as the presence of NGOs, and in Column V we include both control sets. The coefficients on land and population remain strongly significant across all specifications and fall by at most 30% relative to the uncontrolled model.

6.1 Are Wages Selected or Affected?

One caveat to interpreting the factor endowment patterns in Table 6 is that they could reflect either causal effects (shifts in the distribution of wages offered to potential workers) or selection effects (shifts in the distribution of reservation wages). This section provides three tests to distinguish the causal view from the selection view. First and most importantly, we measure the labor supply response to factor endowments and show that NREGS participation generally moves in the *same* direction as NREGS wages, rather than in the opposite direction as would be required to generate selection. Second, we examine the impacts of factor endowments on the entire wage distribution and show they are concentrated in the upper end, the opposite of what at least the simplest selection stories would predict. Finally, we extend the classic selection-as-misspecification framework (Heckman, 1979) and show how participants' reservation wages can be used to test for selection and to obtain consistent estimators of effects on wage offers.

6.1.1 Impacts on Participation

As a first test we examine whether variation in factor endowments moves NREGS participation in the opposite direction as NREGS wages, as would be necessary for our results to arise due to selection. To measure effects on participation we shift from analyzing the data at the spell level to analyzing it at the panchayat-day level. We construct panchayat-day series on days of work done and average wage paid on daily wage spells as follows: if a spell involved d days of work done and took place between a start date and an end date that are D days apart then we attribute d/D of the spell to each day in that interval. We then take for each day a weighted average of the wages paid on spells that overlap that day, with weights equal to the d/D ratios for those spells.

Columns I-III of Table 7 report the estimated impacts of factor endowments on NREGS program outcomes using this method of aggregation. All specifications include month fixed effects to absorb any impact of the statutory wage change; columns IV and VIII show, however, that we cannot detect any significant impacts of the wage change on participation.[22] Column I shows that the relationship between factor endowments and

[22] This is of some independent interest as it suggests that there were few workers whose reservation wages $\underline{w}$ fell between the old wage (Rs. 55) and the new one (Rs. 70), since they would have been selected in. It may also be, however, that officials face short-run quantity constraints in hiring due to the nature of project

NREGS wages found in Table 6 still holds after restructuring the data. Could this be due to selective participation? Columns II and III show that the supply of person-days to NREGS projects moves in the *same direction* as wages. It cannot be the case, therefore, that accepted NREGS wages are higher in land-abundant villages because fewer people accept low values from a fixed distribution of NREGS wage offers. (The difference between the two columns is that II restricts the sample to days on which we observe some work to allow for comparability with Column I.)

One might worry that this test is under-powered since, due to the fact that our sample is small relative to the number of panchayat-days in our study period, 48% of observations have no recorded work done. Columns V-VIII of Table 7 replicate the analysis using data aggregated at the panchayat-month level, for which 37% of observations have no recorded work. We obtain similar results: factor endowments have significant impacts on NREGS wages and same-signed effects on participation, with the (insignificant) exception of population in Column VI.

6.1.2 Distributional Impacts

As a second check on selection we estimated quantile-regression analogues of the models in Table 6. If the distribution of NREGS wage offers did not respond to factor endowments, so that the estimated mean impacts in Table 6 are entirely the result of workers with low NREGS wage offers selecting out, then we would expect to see impacts concentrated in the lower quantiles of the distribution. Figure 3 plots the estimated coefficients on our two factor endowment measures from a series of quantile regressions at each decile, including month dummies. Evidently the effects are concentrated in the upper, not lower, end of the distribution.

6.1.3 A Test and Correction using Reservation Wages

For a third approach we exploit data on workers' reservation wages at the time they worked. We show how incorporating this variable into the standard selection bias framework (e.g. Heckman (1979)) yields a direct test for selection and in addition allows us, under tenable assumptions, to back out selection-corrected point estimates of the structural relationship of interest. We present the argument non-parametrically, though given our sample size we will estimate a parametric version.

To characterize the selection problem let s be any variable predicted to affect program wage offers w. We will treat s as a scalar for expositional purposes but in practice this will be a vector of factor endowments and other controls. The model determining wage

planning.

offers and reservation wages is

$$w = f(s) + u \tag{3}$$
$$\underline{w} = g(s) + v \tag{4}$$

where s is assumed independent of (u, v) and we are interested in estimating $f(\cdot)$. A selection problem arises because we observe $(w, \underline{w})$ only if $w \geq \underline{w}$ so that the worker chooses to work on the NREGS project. Let $d = 1(w \geq \underline{w})$ indicate this event. The conditional expectation of w given s in the selected sample is

$$\mathbb{E}[w|s, d = 1] = f(s) + \mathbb{E}[u|u \geq g(s) - f(s) + v] \tag{5}$$

The second term measures selection bias; for example, if $f'(s) = 0$ but $g'(s) > 0$ it may appear as if s raises wage offers when in fact it does not.

Further conditioning on the reservation wage does not eliminate this bias:

$$\begin{aligned} \mathbb{E}[w|s, \underline{w}, d = 1] &= f(s) + \mathbb{E}[u|u \geq \underline{w} - f(s), \underline{w}] \\ \frac{\partial}{\partial s}\mathbb{E}[w|s, \underline{w}, d = 1] &= f'(s)(1 - h_1(\underline{w} - f(s), \underline{w})) \end{aligned} \tag{6}$$

where $h(x, \underline{w}) \equiv \mathbb{E}[u|u \geq x, \underline{w}]$ and $h_1 > 0$ is the derivative of h with respect to its first argument. It does, however, yield a test of the null $f' = 0$: if s is a significant predictor of w in the selected sample after conditioning on $\underline{w}$ then we can reject the null that it does not influence w in the population.

To point-identify f' we need to pin down h_1. Under the additional assumption that (u, v) are independently distributed the distribution of u is independent of $\underline{w}$, $h(\underline{w} - f(s), \underline{w}) = \overline{h}(\underline{w} - f(s))$, and $h_1(\underline{w} - f(s), \underline{w}) = \overline{h}'(\underline{w} - f(s))$ is identified by variation in $\underline{w}$ conditional on s. Then

$$f'(s) = \frac{\frac{\partial}{\partial s}\mathbb{E}[w|s, \underline{w}, d = 1]}{1 - \frac{\partial}{\partial \underline{w}}\mathbb{E}[w|s, \underline{w}, d = 1]} \tag{7}$$

is identified. The independence assumption amounts, in our context, to assuming that we have included in s all the variables that influence both wage offers and reservation wages. We cannot test this directly, but we can assess how reasonable it is by examining how sensitive estimates of $\overline{h}'$ are to expanding the set of variables included in s.

Given sample size we use linear approximations $f(s) = \beta s$ and $\overline{h}(x) = \gamma x$, in which case consistent estimators can be obtained by running the reduced-form regression

$$E[w|s, \underline{w}, d = 1] = \pi_s s + \pi_{\underline{w}} \underline{w} \tag{8}$$

and then calculating $\hat{\beta} = \frac{\hat{\pi}_s}{1-\hat{\pi}_{\underline{w}}}$. Our empirical measure of reservation wages is subjects' response to following question: "Think about when you requested work. What is the lowest daily wage you would have been willing to work on NREGS for at that point?". Answers to this question correspond to realizations of $\underline{w}$ in our model. Importantly, these are reservation wages and not market wages: they should therefore serve as sufficient statistics for *all* factors driving selection into NREGS participation, including both the attractiveness of other work and of leisure, for example.

Unfortunately we only asked this question once per NREGS participant, not per spell of work. To minimize measurement error in $\underline{w}$ we restrict ourselves to the sample of workers who did exactly one spell of work, for whom there is no ambiguity. Results are similar if we use the full sample and impute the same reservation wage for each spell of work done by workers who worked more than once. In our restricted sample 89% of workers report receiving a wage at least as high as their reservation wage; the other 11% may represent measurement error or may have been subject to unanticipated hold-up.

Table 8 implements our approach. Panel A simply shows that the uncorrected results reported in Table 6 do not change when we use our new, restricted estimation sample. In Panel B we re-estimate the same models but include worker's reservation wages as an additional control. While biased, these estimates let us reject the null that factor endowments do not influence wage offers (Equation 6). As expected the point estimates are smaller than those in Panel A, but they remain economically meaningful and strongly significant. In addition the estimated coefficient on the reservation wage is stable across control sets, which suggests that factor endowments are the major determinants of wage offers and hence that independence of the error terms in Equations 3 and 4 is a reasonable approximation. Panel C presents selection-adjusted estimates under this maintained assumption. The estimates are similar (and in the case of population somewhat larger) than the uncorrected estimates in Panel A and are strongly significant. They corroborate earlier pieces of evidence that labor market conditions have a causal effect not only on wage realizations but on wage offers.[23]

In summary, officials appear to price NREGS jobs to market, rather than setting a binding floor on market wages. This is consistent with the "greasing the wheels" view of corruption, which argues that corruption arises naturally when governments attempt to move factor allocations away from competitive equilibrium.

[23] One alternative approach to incorporating reservation wage data is to estimate truncated Tobit models. However, for a large proportion (60%) of the work spells in our data the wage received exactly equalled the reservation wage. This is exactly what our hypothesis (that officials pay people their reservation wages) predicts, but it cannot be fit well by a truncated model with a smoothly distributed latent variable. We have also fit censored Tobit models and obtained strongly significant estimates roughly 20% larger than those presented here.

7 Conclusion

Marginal rates of corruption or "leakage" are an important input into policy-making. We provide the first empirical analysis of marginal leakage, working in the context of a large social protection program, India's National Rural Employment Guarantee Scheme. We find that marginal leakage with respect to an increase in the statutory daily wage due to workers was 100%: none of the wage increase was passed through to workers, even though on average they were if anything over-paid prior to the change. The policy implications of the analysis are thus starkly opposite those one would draw from examining averages alone.

Our main estimates and a number of supporting pieces of evidence are inconsistent with a class of theories based on "voice," in which the threat of complaints to higher-ups determines equilibrium levels of corruption. The data do support a theory based on "exit" in which officials price jobs to market (rather than setting the market price), consistent with the "greasing the wheels" view of corruption. (Leff, 1964; Huntington, 1968) The threat of exit need not be workers' only source of bargaining power vis-a-vis officials, however, and a better understanding of their negotiations would be valuable.

Our analysis was motivated by the question of optimal redistribution. It is intuitive to think that a planner who anticipated marginal leakage of 100% would never increase the statutory wage. We do not know, however, the ultimate incidence of the rents that accrue to NREGS implementing officials. Some may find their way into the pockets of political superiors in the form of payments for plum jobs or collusive bribes to prevent exposure; some may be returned to local voters as campaign spending.[24] Understanding the distribution of rents in political and bureaucratic hierarchies is another frontier for research in the political economy of developing countries and complementary to work on marginal leakage.

The result that NGOs may lower marginal leakage is one of the few bright points in the otherwise gloomy picture we have presented. There are many plausible reasons that NGOs might matter: for example, they might provide literate advocates who better understand how to navigate the bureaucracy, or have better access to the press than individual participants, or serve a coordinating function among the workers. Understanding whether and how NGOs function in this sort of environment would be a valuable step towards understanding accountability in local government more generally.

[24] Ferraz and Finan (2011) show that political incentives matter for corrupt behavior.

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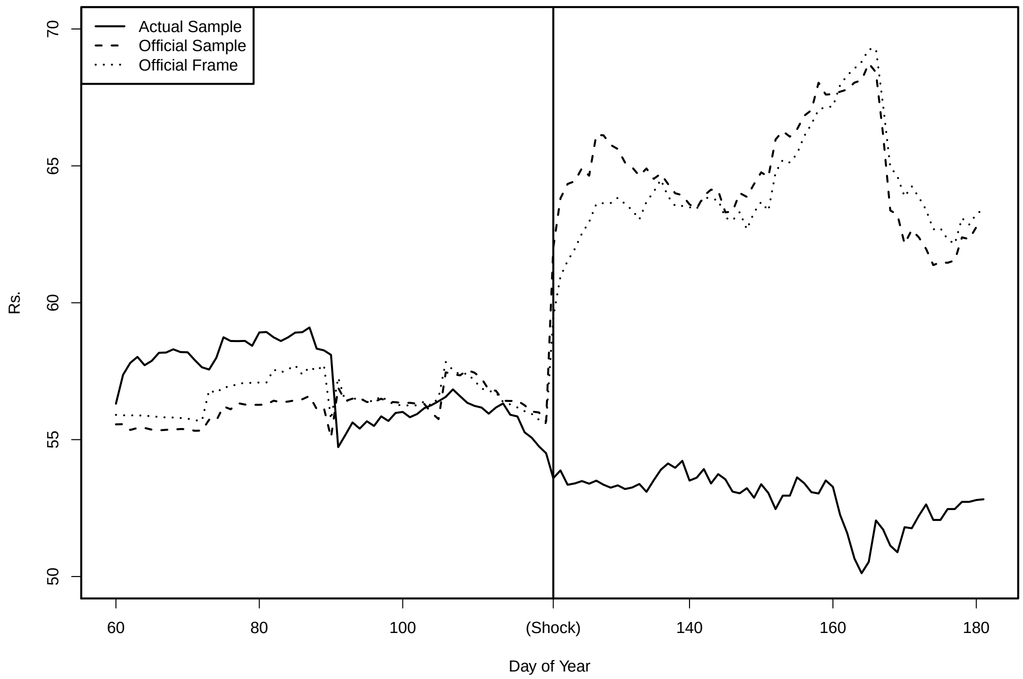
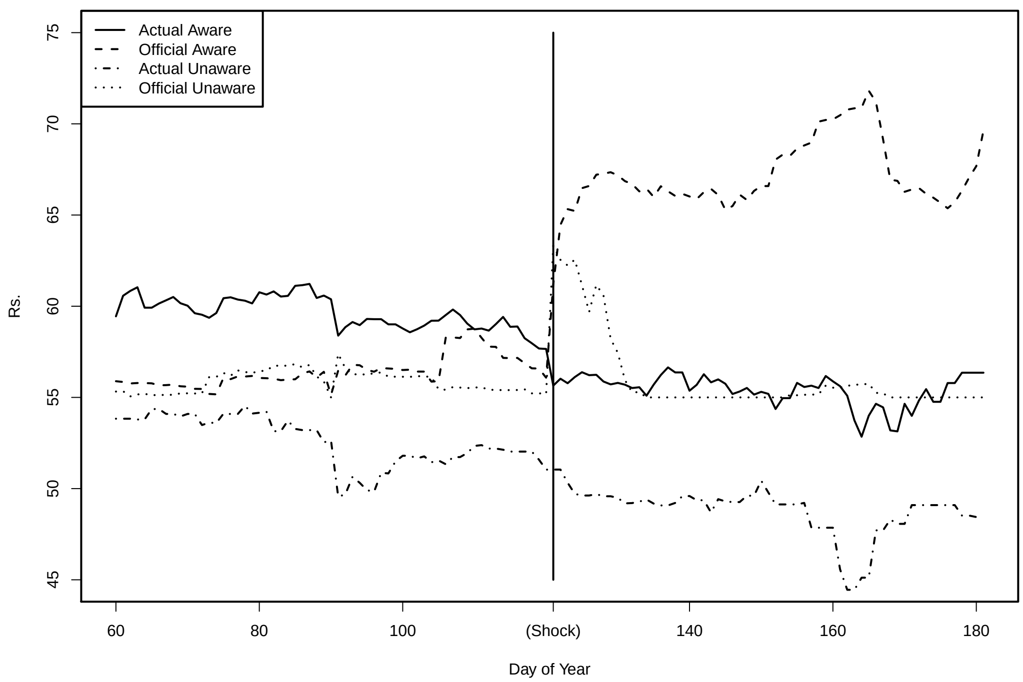
Figure 1: Daily Wage Rates Paid



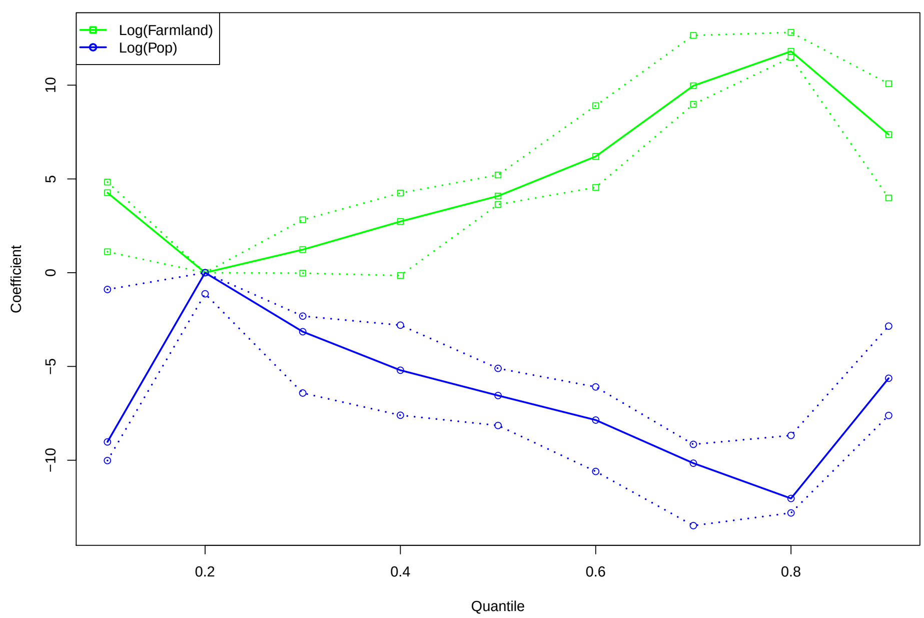
Plots daily series of the average wage rate paid on daily wage work-spells in Orissa over the study period. The Actual Sample series is constructed from household surveys, the Official Sample from official records for the corresponding households, and Official Frame from the universe of official records from which that sample was drawn. Day 60 corresponds to March 1st, 2007, the start of the study period; day 121 to May 1st, 2007, the date of the statutory wage change; and day 181 to June 30, 2007, the end of the study period.

Figure 2: Daily Wage Rates Paid by Awareness of Wage Change



Plots daily series of the average wage rate paid on daily wage work-spells in Orissa over the study period. Aware refers to panchayats that actually implemented the wage change after May 1st, and Unaware to those panchayats that did not. The Actual series are constructed from household surveys, while the Official series come from official records for the corresponding households. Day 60 corresponds to March 1st, 2007, the start of the study period; day 121 to May 1st, 2007, the date of the statutory wage change; and day 181 to June 30, 2007, the end of the study period.

Figure 3: Factor Endowments Shift the Upper Portion of the Wage Distribution



Plots coefficients from quantile regressions of NREGS wage received on factor endowments, controlling for month dummies. For example, the points at $x = 0.5$ correspond to the coefficients from a median regression of wages on log cultivatable land, log population, and month dummies. The dotted lines denote quantile-wise 95% confidence intervals.

Table 1: Characteristics of Spells in Universe, Sample, and Reached Sample

	All Spells			Sampled Spells			Reached Spells			
Variable	N	Mean	SD	N	Mean	SD	N	Mean	SD	p-value
Age	111,109	37.60	14.93	7,123	37.37	13.60	4,791	37.55	13.28	0.33
Male	111,057	0.54	0.50	7,123	0.54	0.50	4,791	0.54	0.50	0.67
SC/ST	111,109	0.78	0.41	7,123	0.79	0.41	4,791	0.77	0.42	0.05
Post	111,172	0.40	0.49	7,126	0.43	0.49	4,794	0.42	0.49	0.57
Spell Length	111,172	11.13	2.92	7,126	11.14	3.01	4,794	11.09	3.14	0.33
Wage Spell	111,172	0.83	0.37	7,126	0.83	0.38	4,794	0.84	0.36	0.20
Daily Rate	111,172	63.48	17.24	7,126	64.37	20.34	4,794	63.90	18.92	0.30

Notes:

1. Reports summary statistics at the work spell level using official records for (a) the universe of spells sampled from, (b) the initial sample of work spells we drew, and (c) the work spells done by households we were ultimately able to interview.
2. The last column reports the p-value from a regression of the variable in question on an indicator for whether or not the observation is in our analysis sample (conditional on being in our initial sample), with standard errors clustered at the panchayat level.
3. "SC/ST" stands for "Scheduled Caste/ Scheduled Tribe", historically discriminated minorities. "Post" is an indicator equal to 1 for the period after May 1, 2007, the date of the wage change. "Wage Spell" refers to a spell done on a daily wage project (as opposed to a piece rate project).

Table 2: Characteristics of Interviewed Households

Variable	NREGA Participants N	NREGA Participants Mean	NREGA Participants SD	Non-Participants N	Non-Participants Mean	Non-Participants SD
Demographics						
Number of HH Members	812	4.94	1.88	498	4.65	2.18
BPL Card Holder	815	0.77	0.42	497	0.76	0.43
HH Head is Literate	803	0.30	0.46	501	0.23	0.42
HH Head Educated Through Grade 10	819	0.04	0.19	502	0.04	0.20
Awareness						
Knows HH Keeps Job Card	806	0.84	0.37	476	0.89	0.31
Fraction of Amenities Aware Of	810	0.24	0.21	494	0.20	0.21
HH Head has Heard of RTI Act	821	0.02	0.13	501	0.01	0.09
Primary Income Sources						
Self-employed, agriculture		45%			36%	
Self-employed, non-agriculture		18%			19%	
Agricultural Labor		11%			13%	
Non-agricultural Labor		21%			21%	
Other		5%			11%	

This table characterizes the households successfully interviewed in Orissa, split between those who worked on an NREGS project between March 1st and June 30th, 2007 and those that did not. "BPL" stands for Below the Poverty Line, a designation that entitles households to certain government schemes other than NREGS. "Literate" means able to sign one's name. The amenities meant to be provided at NREGS worksites include water, shade, first aid, and child care. We asked respondents to name amenities without prompting. "RTI Act" stands for the Right to Information Act, a national freedom of information act passed in 2005.

Table 3: No Passthrough of Statutory Wage Change

	Official Wages				Actual Wages			
Regressor	I	II	III	IV	V	VI	VII	VIII
Post 1 May	8.19	5.76	6.66	-1.54	-3.23	-0.83	-0.49	-2.60
	(1.02)***	(1.83)***	(1.30)***	(0.69)**	(1.18)***	(1.94)	(0.88)	(1.23)**
Month		2.38				-8.42		
		(3.91)				(4.46)*		
Month^2		-0.12				0.79		
		(0.46)				(0.54)		
Post * Panch. Aware				12.33				-0.81
				(1.17)***				(1.98)
Panch. Aware				-0.10				7.58
				(0.94)				(1.64)***
Panchayat FEs	N	N	Y	N	N	N	Y	N
N	4037	4037	4037	4037	1009	1009	1009	1009
R^2	0.20	0.21	0.47	0.34	0.02	0.03	0.46	0.12

Notes:

1. Each column reports a separate regression. Each observation is a spell of daily-wage work, and the outcome is the wage paid as reported by interviewed households (Columns V-VIII) or in the corresponding official records (Columns I-IV).
2. "Post 1 May" is an indicator equal to 1 from 1 May 2007 onwards. "Aware" is an indicator equal to 1 if the panchayat ever reported paying a wage in $\{70, 80, 90, 100\}$ during May or June 2007, indicating that they became aware of the statutory wage change.
3. Robust standard errors clustered by panchayat are reported in parenthesis; statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4: Actual or Planned Responses to Unfair Treatment are Local

	% Agreeing		
Action	All Workers	W/ Problems	W/Out Problems
Write a letter to MLA/MP	0.1%	0.3%	0.0%
File a complaint with the BDO	7.4%	12.3%	4.6%
File a complaint at the Panchayat office	35.9%	40.9%	33.3%
Speak to village elders/ward members	39.0%	31.0%	43.5%
Nothing	21.7%	16.3%	24.6%

Notes:

1. Reports the percentages agreeing with the given responses to the question “If you did have any problems, or if a problem were to arise in the future, what would you do about it?”
2. Percentages sum to more than 1 because multiple responses were allowed. 12% of respondents did not provide any answer and are not included in the tabulation.
3. “MLA” refers to the elected Member of the Legislative Assembly, the state legislature; “MP” refers to the elected Member of Parliament, the national legislature. “BDO” is the Block Development Officer, the first level of oversight over the Panchayat (village) officials.

Table 5: Heterogeneity in Wage Passthrough

	Official Wages				Actual Wages				
Regressor	I	II	III	IV	V	VI	VII	VIII	IX
Post 1 May	7.35	7.18	8.56	6.56	-3.44	-0.99	-5.44	-2.09	-3.68
	(1.20)***	(1.28)***	(1.12)***	(1.78)***	(1.45)**	(1.32)	(1.62)***	(2.38)	(2.42)
Post * Near to BDO	3.18				0.44				
	(1.62)**				(2.63)				
Near to BDO	-0.61				0.73				
	(0.59)				(2.09)				
Post * Near to Collector		3.12				-6.37			
		(1.48)**				(2.22)***			
Near to Collector		-0.64				5.90			
		(0.54)				(1.60)***			
Post * NGO Active			0.19	-0.05			5.69	5.49	
			(1.76)	(1.73)			(1.93)***	(1.94)***	
NGO Active			-0.06	0.00			-2.90	-2.51	
			(0.63)	(0.63)			(1.78)	(1.76)	
Post * Worker Aware									2.02
									(2.62)
Worker Aware									-7.80
									(1.40)***
Month				1.12				-1.91	
				(0.66)*				(1.01)*	
N	3614	3627	3646	3646	860	860	860	860	1009
R^2	0.28	0.28	0.27	0.27	0.02	0.06	0.03	0.04	0.10

Notes:

1. Each column reports a separate regression. Each observation is a spell of daily-wage work, and the outcome is the wage paid as reported by interviewed households (Columns V-IX) or in the corresponding official records (Columns I-IV). The sample is smaller than in Table 3 because of missing village-level data.

2. "Post 1 May" is an indicator equal to 1 from 1 May 2007 onwards. "Near to BDO" is an indicator equal to one if the travel time from the village to the Block Development Office is below the median for villages in the sample, and "Near to Collector" is an analogous indicator for travel time to the Collector's office. "Any NGO" is an indicator equal to one if any NGOs are active in the village. "Worker Aware" indicates whether the worker's household knew the daily wage changed.

3. Robust standard errors clustered by panchayat are reported in parenthesis; statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 6: Factor Endowments Affect NREGS Wage Realizations

	Reservation Wage	NREGS Wage			
Regressor	I	II	III	IV	V
Log(Farmland)	8.94	6.78	5.25	6.59	4.81
	(2.07)***	(1.27)***	(1.03)***	(1.44)***	(1.06)***
Log(Population)	-8.72	-9.18	-7.96	-9.02	-7.23
	(2.84)***	(1.96)***	(1.72)***	(1.98)***	(1.58)***
Male			1.93		1.75
			(0.74)***		(0.74)**
Literate			0.94		0.67
			(0.92)		(0.95)
Paid via Bank			6.13		5.42
			(1.02)***		(1.03)***
Scheduled Caste			-2.62		-2.88
			(1.68)		(1.67)*
Scheduled Tribe			-3.31		-4.32
			(1.29)**		(1.19)***
Backward Caste			-10.14		-11.89
			(1.45)***		(1.52)***
Near to BDO				1.07	0.45
				(1.21)	(1.00)
Near to Collector				3.17	2.33
				(1.21)***	(1.17)**
NGO Active				-0.24	0.35
				(1.16)	(0.93)
Month FEs	Y	Y	Y	Y	Y
N	988	988	981	841	837
R^2	0.13	0.15	0.22	0.18	0.25

Notes:

1. The unit of observation is a spell of NREGS wage work. The outcome variable is the worker's reservation wage in Column I and the NREGS wage paid in Columns II-V.
2. Robust standard errors clustered by panchayat are reported in parenthesis; statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 7: Factor Endowments Affect Participation and Wages Similarly

	Daily				Monthly			
Regressor	Wage	Days	Days	Days	Wage	Days	Days	Days
Log(Farmland)	7.53	0.39	0.23		7.61	7.97	6.89	
	(1.97)***	(0.21)*	(0.14)		(1.76)***	(5.79)	(4.24)	
Log(Population)	-10.24	-0.27	-0.15		-9.23	-2.03	-4.45	
	(3.15)***	(0.34)	(0.25)		(3.05)***	(9.48)	(7.59)	
Post 1 May				-0.22				-5.07
				(0.22)				(6.63)
Month				-0.06				-2.59
				(0.09)				(2.79)
Month FEs	Y	Y	Y	N	Y	Y	Y	N
RHS Mean	53.5	1.7	0.9	0.9	54.1	41.4	26.3	26.3
N	7186	7186	13786	13908	287	287	452	456
R^2	0.17	0.04	0.04	0.01	0.18	0.05	0.05	0.02

Notes:

1. Each column reports a separate regression. An observation is a panchayat-day in Columns I-IV and a panchayat-month in Columns V-VIII. The outcome variable is the average wage paid on NREGS work spells in the given panchayat-period in Columns I and V, and the number of person-days of work done on NREGS projects in Columns II, III, IV, VI, VI, and VII. Columns I, II, V, and VI restrict to observations for which the number of person-days is positive (and thus wages are observed); Columns III, IV, VII and VIII include all possible observations.

2. Robust standard errors clustered by panchayat are reported in parenthesis; statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 8: Factor Endowments Affect NREGS Wage Offers

	Regressor	I	II	III	IV
Panel A: Partial Model					
	Log(Farmland)	5.95	4.97	5.88	4.74
		(1.40)***	(1.26)***	(1.55)***	(1.37)***
	Log(Population)	-9.31	-8.67	-8.71	-7.71
		(2.2)***	(2.12)***	(2.22)***	(2.02)***
	Month FEs	Y	Y	Y	Y
	Controls	-	Individual	Village	Both
	N	762	738	655	636
	R^2	0.10	0.15	0.11	0.15
Panel B: Full Model					
	Log(Farmland)	3.88	3.40	3.86	3.24
		(1.19)***	(1.1)***	(1.36)***	(1.18)***
	Log(Population)	-7.70	-7.38	-7.22	-6.60
		(2.03)***	(2.00)***	(2.01)***	(1.87)***
	Reservation Wage	0.33	0.30	0.33	0.31
		(0.03)***	(0.03)***	(0.04)***	(0.04)***
	Month FEs	Y	Y	Y	Y
	Controls	-	Individual	Village	Both
	N	762	738	655	636
	R^2	0.23	0.25	0.24	0.26
Panel C: Structural Parameters					
	Log(Farmland)	5.75	4.85	5.80	4.68
		(1.86)***	(1.62)***	(2.13)***	(1.75)***
	Log(Population)	-11.41	-10.53	-10.85	-9.54
		(3.10)***	(2.92)***	(3.13)***	(2.76)***

Notes:

1. Each column of Panels A and B reports a separate regression. The unit of observation in each regression is a spell of NREGS wage work; the outcome variable is the wage received. Panel C reports structural parameters derived from the estimates in Panel B as described in Section 6.1.3.
2. Individual controls include gender, literacy, caste, and whether paid via bank. Village controls include an indicator equal to one if the travel time from the village to the Block Development Office is below the median for villages in the sample, an analogous indicator for travel time to the Collector's office, and an indicator equal to one if any NGOs are active in the village.
3. Robust standard errors clustered by panchayat are reported in parenthesis in Panels A and B. The standard errors in Panel C were derived from the estimates and standard errors in Panel B using the delta method, so that the implied t-tests correspond to linearized Wald tests. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$