The Marginal Rate of Corruption in Public Programs*

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Abstract

The marginal benefits of public spending are a key determinant of optimal fiscal policy. In developing countries public funds are often stolen by corrupt officials, so optimal policy should reflect marginal corruption. This paper provides the first theoretical and empirical analyses of marginal corruption. We analyze the effects of a large statutory wage increase in India's national employment scheme. Strikingly, none of the increase was passed through to workers even though initially most were, if anything, overpaid. Theory and supporting evidence suggest that this divergence between margins and averages occurs because the threat of exit to the private sector, and not the threat of complaints, is workers' main source of bargaining power. Officials price program jobs to market, consistent with the "greasing the wheels" view of corruption.

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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, Olken's point implies that 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 important unaddressed problem.

This paper provides the first theoretical and empirical analyses 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 key policy question is therefore how actual wages vary with the statutory wage.

We first show formally how marginal and average rates of corruption can diverge. We model wage determination as a bargaining problem between a worker and an official. Even within our simple framework any rate of average leakage is consistent with a wide range of marginal leakage rates, including 0% and 100%. The key to pinning down marginal

[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).

rates of leakage more precisely is to understand the sources of workers' bargaining power. Workers can respond to under-payment either by complaining to superior government officials or by leaving for private-sector employment. In the former case marginal leakage is lower than average leakage, while in the latter it is higher. The important difference is that in the former case the value of the worker's outside option increases with the statutory wage, while in the latter case it does not. We call the worker's two sources of bargaining power "voice" and "exit" in deference to Hirschmann (1970), who introduced these categories of response to government failure.

We assess our framework 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-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 most important margin.

Figure 1 summarizes our most important 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%.[4] 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.

A high rate of marginal leakage suggests that, for most NREGS workers, exit and not voice is the important source of bargaining power. We examine each of these constraints directly. Survey responses do indicate a lack of voice: 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%). The majority of respondent households were aware of the wage change, but wages did not increase differentially for aware workers. We find no evidence of wage pass-through in villages located close to government headquarters, as would be the case if travel costs were the main constraint on the exercise of voice. We find significant pass-

[4]Our overall estimate of average leakage before the shock, including all measured forms of theft, is 75%. We discuss distinct margins of corruption below.

through only in villages in which NGOs are active, suggesting that NGOs (or factors correlated with their presence) may facilitate voice.

If exit is the binding constraint then NREGS wage realizations should be positively correlated with the value of workers' outside options. Ninety-six percent of respondents said that the private labor market was the outside option relevant for them. We therefore test whether market wage variation due to differences in local factor endowments affects NREGS wage realizations. Although de jure wages should be the same everywhere, we find that they are substantially and significantly higher in villages that are land-abundant and labor-scarce. We provide three pieces of evidence to show that this reflects a causal effect on wage offers, and not a selection effect. First, participation rates and wages are correlated with factor endowments in the same direction, inconsistent 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. In a third approach we exploit survey data on respondents' reservation wages. We show how to obtain a valid test for effects on wage offers and, under tenable assumptions, consistent selection-corrected point estimates. Our test rejects the null of no causal effect and our corrected estimates are essentially the same as the uncorrected ones.

To summarize, the data imply that officials price jobs to market rather than bounding the market price. This is the first evidence we are aware of in support of the classic thesis of Leff (1964) and Huntington (1968) that corruption tends to "grease the wheels" of the economy by (partially) undoing distortionary policies. It implies that general-equilibrium analyses of employment guarantees could fruitfully consider the case where market wages determine program wages, as well as the other way around (Ravallion, 1987; Ravallion et al., 1993; Basu et al., 2009). Finally, we note that pricing-to-market is a generically regressive mechanism, in the sense that beneficiaries in places where markets wages are high to begin with receive higher NREGS wages.

Our analysis is most directly relevant for the 11% of the world's population covered by the NREGS and for participants in the many similar workfare schemes worldwide (Subbarao, 2003). More broadly, the insights that margins and averages can diverge sharply and that bargaining theory can explain marginal leakage are relevant for many settings. One indication of this is the fact that earlier work has already used either voice or exit to interpret cross-sectional variation in corruption. For example, Reinikka and Svensson (2004) argue that variation in Ugandan communities' ability to complain explains differences in the share of central government transfers they obtain. Meanwhile Svensson (2003) emphasizes participation constraints that must be satisfied in order for Ugandan firms to be willing to pay bribes for permits to operate. Similarly Hunt (2007) shows how variation in households demand for publicly provided services affects bribe rates.

The paper also fits within a broader, nascent effort to adapt public economic theory for use in developing countries: for example, Keen (2008), Gordon and Li (2009), Olken and Singhal (2010) and Pomeranz (2010) on taxation and Atanassova et al. (2010) on poverty-targeting.[5] Banerjee et al. (2009) capture the basic premise when they argue that "it is impossible to understand policy without understanding corruption."[6]

The rest of the paper is organized as follows: Section 2 describes the NREGS setting, and Section 3 models marginal leakage in that setting. Section 4 describes the data collected. Section 5 presents our main empirical results on marginal leakage along with supporting evidence on the relevance of voice and exit. Section 6 presents tests and a correction for selection effects in wage regressions. 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. 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), or 0.73% of 2008 GDP, and total expenditures are higher as the states are also responsible for a share of the cost.[7]

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 (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

[5]For theoretical analyses of investment in state capacity see Besley and Persson (2009, 2010).

[6]Work by Ravallion et al. (1993) on Maharashtra's Employment Guarantee Scheme is most topically related; they examine the effects of a piece rate increase on officially recorded participation figures, but do not measure leakage.

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

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.

We focus here on corruption in 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.

The creators of the NREGS worried that workers might be under-paid, and the official guidelines specify 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. Whether this system functions well enough in practice to provide workers with a cost-effective means of resolving problems is an empirical question that we will examine below.

To measure marginal leakage we need an exogenous source of variation in the benefits due to program participants. In our setting the wage (or piece rate) offered to participants is the key policy parameter. The wage rates paid to NREGS participants 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 wages are state-specific and determined by state governments. This gave state politicians strong incentives to raise statutory wages, and most did. We study 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.

3 Understanding Marginal Leakage

This section develops a model of NREGS wage determination to understand what restrictions theory can place on marginal leakage. The model is intentionally as parsimonious as possible while remaining true to the key features of the NREGS context. Simplicity is a virtue here since a primary goal is to demonstrate how weak the connection between average and marginal leakage may be.

One particularly important simplification is our exclusive focus on daily wage underpayment. We abstract from leakage on other margins both for conceptual and for pragmatic reasons. Conceptually, because we will ultimately find no evidence of pass-through our overall estimate of marginal leakage will be 100% regardless of how leakage on other margins changes. Appendix A makes this point formally, re-deriving our main results in a more general setting with multiple leakage margins. Pragmatically, over-reporting and theft on piece rate projects respond in subtle ways to the wage change that are dealt with in a separate paper, to which we refer the interested reader (Niehaus and Sukhtankar, 2010). The key fact to keep in mind about over-reporting is that it contributes substantially to average leakage, which we estimate at 75% overall.

Turning then to wages, consider the following game between the government, a single official, and a single worker. The government moves first, announcing a statutory wage $\overline{w}$ to be paid to the worker if he participates. Monitoring is weak, however, so that the implementing official can potentially pay some lower wage $w \leq \overline{w}$. The value of w is determined through negotiations with the worker. This is consistent with our own conversations with participants, who openly discussed negotiating with program officials until they obtained a wage they were satisfied with.[8]

We assume that w must satisfy two constraints. First, the worker must receive at least a reservation wage $\underline{w}$ equal to the monetized value of his next-most-preferred use of time. The most natural interpretation is that $\underline{w}$ is the wage he could earn in the private sector, but it might also be his value of leisure. Second, if the worker is underpaid he can file a complaint with the official's superiors. While a complaint process exists on the books, in practice complaining is costly and may not succeed.[9] To capture this we suppose that complaining costs $c \geq 0$ and succeeds with probability $\pi \in [0, 1]$, implying

[8] 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.

[9] Observers have expressed skepticism about the complaint process. Based on their fieldwork in Orissa, Das and Pradhan (2007) say: "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."

that the worker will complain unless $\pi\overline{w}+(1-\pi)w-c\leq w$, or $w\geq\overline{w}-\frac{c}{\pi}$. The expected costs of a complaint for the official are large enough that he does not want to pay a wage that violates this constraint. Combining these constraints yields the worker's reservation payoff

$$r\equiv\max\left\{\underline{w},\overline{w}-\frac{c}{\pi}\right\} \tag{1}$$

If $\underline{w}>\overline{w}$ the surplus from cooperation is negative and the parties optimally agree to part ways.[10] If $\underline{w}<\overline{w}$ the bargaining problem is to choose a wage $w\in[r,\overline{w}]$, which amounts to choosing a division of the residual surplus. We assume an asymmetric Nash bargaining solution, which allows for special cases such as those in which the official or the worker makes a take-it-or-leave-it offer but also for intermediate divisions of bargaining power. If the Nash bargaining weights of the worker and the official are α and $1-\alpha$, respectively, then the Nash solution is

$$w=(1-\alpha)r+\alpha\overline{w} \tag{2}$$

The worker's wage is increasing in his bargaining weight α and in the value of his reservation payoff r, which we will refer to collectively as his sources of bargaining power.[11]

Average and Marginal Leakage. The average level of leakage is $\frac{\overline{w}-w}{\overline{w}}$, which equals the official's surplus, $(1-\alpha)(\overline{w}-r)$ divided by total outlays $\overline{w}$:

$$\begin{aligned} AL &\equiv 1-\frac{w}{\overline{w}} \\ &= (1-\alpha)\left(1-\frac{r}{\overline{w}}\right) \end{aligned} \tag{3}$$

As one would expect, average leakage decreases with with the worker's bargaining weight and the value of his reservation payoff. Marginal leakage, on the other hand, is

$$\begin{aligned} ML &\equiv 1-\frac{\partial w}{\partial\overline{w}} \\ &= (1-\alpha)\left(1-\frac{\partial r}{\partial\overline{w}}\right) \end{aligned} \tag{4}$$

which depends on the *level* of the worker's bargaining weight but the *derivative* of his reservation payoff with respect to the statutory program wage.

Optimal Redistribution. Consider a planner deciding on a statutory wage $\overline{w}$. The planner places a value $\lambda^w>1$ on the worker's surplus $w-\underline{w}$ and a value $\lambda^o<\lambda$ on the official's surplus $\overline{w}-w$; to simplify things let $\lambda^0=0$ and $\lambda\equiv\lambda^w$, but note that marginal leakage will matter provided $\lambda^w\neq\lambda^0$. The total cost of public funds is $C(\overline{w})$

[10] This constraint will be relaxed (without substantively changing matters) if the official obtains an additional benefit beyond $\overline{w}$ from the worker's participation. See Appendix A.

[11] Note that we implicitly assume the official can commit to a wage. If this were not the case then the worker would anticipate that regardless of what he promises the official will only ever pay him enough to keep him from complaining, in which case the results for that case apply.

with $MC(\overline{w}) \equiv C'(\overline{w})$.[12] The planner solves

$$\max_{\overline{w}} \lambda(w(\overline{w}) - \underline{w}) - C(\overline{w}) \tag{5}$$

An interior solution (one in which the program continues to operate) must satisfy $\lambda \frac{\partial w(\overline{w})}{\partial \overline{w}} = MC(\overline{w})$, or

$$\lambda(1 - ML) = MC(\overline{w}) \tag{6}$$

which depends on ML. Note that an analogous condition would hold if we modeled a group of workers with a distribution of reservation payoffs since the marginal participant derives 0 surplus for participating.

Predicting Marginal Leakage. Any factor that differentially affects the levels and margins of r, the worker's reservation payoff, will tend to induce separation between average and marginal leakage. The relevant factors here are the effective costs $\frac{c}{\pi}$ of complaining, and the reservation wage w. When $\frac{c}{\pi} < \overline{w} - \underline{w}$ the operative constraint on negotiations is the worker's threat of complaining; in this case average leakage is $(1-\alpha)\frac{c}{\pi}$ while marginal leakage is lower: in fact, 0. Intuitively, in this case the official must pay the worker at least enough to make him indifferent between complaining and not. Since that amount increases one-for-one with $\overline{w}$, marginal pass-through of wage increases to the worker is complete.

Now consider the case where $\frac{c}{\pi} > \overline{w} - \underline{w}$. In this case the costs of complaining are high enough that inducing participation, rather than preventing complaints, is the binding constraint on negotiations. Average leakage in this case is $(1-\alpha)(1-\frac{\underline{w}}{\overline{w}})$ while marginal leakage is higher at $(1-\alpha)$. If the market wage is close to the statutory program wage then average leakage may be small even as marginal leakage approaches 100%. For example when $\alpha = 0$ the official must leave the worker just enough surplus to induce him to participate, but can keep any residual difference $\overline{w} - \underline{w}$.

Combining these two cases, we see that for any observed level $AL \in [0,1]$ of average leakage the model is consistent with any marginal leakage ML in $0 \cup [AL, 1]$, or in other words that average leakage on its own contains little information about marginal leakage. Enriching the model with additional features would further attenuate the relationship. For example, one natural extension would be to allow for an unknown degree of asymmetric information about the worker's reservation wage, which gives the worker information rents and (one can show) may reduce marginal leakage. On the other hand, the model illustrates the value of auxiliary information on the determinants of the worker's reservation payoff. For example, if we know that c/π is large relative to $\underline{w}$ so that complaining is not relevant in equilibrium then then we can predict that $ML \geq AL$.

Effects of Labor Market Conditions. The differential effects of $\overline{w}$ by regime are

[12] Whether $C' = 1$ or $C' > 1$ is irrelevant for our argument.

mirrored by differential effects of the worker's outside option $\underline{w}$. Notice that $\frac{\partial w}{\partial \underline{w}} = 1 - \alpha$ whenever $\overline{w} - \frac{c}{\pi} \leq \underline{w} < \overline{w}$ and 0 otherwise. Intuitively, if the worker's option to exit for the private sector is the binding constraint then we should see changes in the reservation wage pass through to changes in the program wage (or lead to exit for the private sector if the new reservation wage is higher than $\overline{w}$). If on the other hand the threat of complaining is the binding constraint then the program wage already exceeds the reservation wage and changes in the latter will not affect the former. The parameter values for which reservation wages affect program wages are thus precisely the ones for which marginal leakage should exceed average leakage, implying that reservation wage variation can be used to help identify the policy parameters of interest.

The broader point of this exercise is that understanding the sources of program participants' bargaining power vis-a-vis officials is important for predicting marginal leakage. Our distinction between the threat of complaints and the threat of exit to the private labor market echoes the seminal work of Hirschmann (1970) on "voice" and "exit" as responses to organizational or governmental failings. It also synthesizes earlier theoretical frameworks developed to explain cross-sectional variation in corruption, which have typically emphasized either voice or exit exclusively.

4 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.[13]

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 statutory wage change on 1 May 2007.[14] We sampled work spells from the official records for Gajapati, Koraput, and Rayagada districts in Orissa.[15] We then sampled 60% of

[13]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.

[14]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.

[15]We restricted ourselves to blocks (sub-districts) that border the neighbor state of Andhra Pradesh. Our

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.[16] 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 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

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.

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

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.

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. Finally, there is no reason to believe that respondents would under-report corruption for fear of reprisals, since they could not have known how many days they were reported as having worked or wages they were meant to have received in the official data.

5 Estimating and Interpreting Marginal Leakage

5.1 Estimating Marginal Leakage

We turn now to estimating marginal leakage. Our main result is evident from Figure 1: prior to 1 May wages paid are very similar to wages reported and to the statutory wage (Rs. 55), but none of the wage increase passed through to workers, implying that marginal leakage was 100%.

Table 3 provides a more formal statistical analysis of pass-through. In columns V-VIII observations are spells of daily-wage work as reported by interviewed households, while

in columns I-IV they are those reported in the corresponding official records.[17] Columns 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).[18]

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 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, and the estimated effect of the shock is very close to 0 after accounting for trends or village effects.

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

[17] 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. 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.

[18] 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.

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 investigating 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.[19] 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.

5.2 Interpretation: Is Voice a Binding Constraint?

The fact that marginal leakage exceeds average leakage suggests that voice is not a binding constraint on corruption in the context of the NREGS. In the language of our model, it implies that the costs of complaining c are large and/or that the perceived probability π of a complaint succeeding is low.

To better understand how participants perceive the complaint process we asked them, "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

[19]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.

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 designed by NREGS guidelines for dealing with grievances (Table 4). Among those who said they would do nothing, the main reasons stated where that complaining would be in vain (37%) and that complaining would too time-consuming or take too much effort (53%). Ten percent indicated fear of retribution as the main deterrent.

One measurable and potentially important component of the costs of complaining is the cost of traveling to the nearest government office to file a grievance. We have 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.[20] There is of course substantial variation around these means; if travel costs are an important constraint on voice then we should see a relationship between distance and wage pass-through. 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.

While variation in the probability of successfully complaining is harder to measure, one plausible proxy is the presence of an active non-governmental organization (NGO) in the 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, which is encouraging.

A final potential limitation on the effectiveness of complaints is that in the short run some workers may not have learned about the wage change.[21] We asked respondents

[20]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.

[21]Reinikka and Svensson (2005), Besley and Prat (2006), and Ferraz and Finan (2008) show that providing information can help reduce corruption in settings where lack of information is a binding constraint.

what wages they were owed and whether they knew of any changes in the statutory wage. 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%.[22] 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.

5.3 Interpretation: Is Exit a Binding Constraint?

Given that NREGS workers appear to have little effective voice, we next examine an alternative recourse: exit. One interesting piece of circumstantial evidence supporting the exit 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 overpay workers to induce them to participate because this creates scope for further theft in the form of over-reporting (as in the model extension in Appendix A). Over-paid workers cannot complain, so for this sub-sample exit is unambiguously the binding constraint.

This example also suggests a more systematic test: if the exit constraint binds then variation in workers' outside options should be positively related to the NREGS wage realizations we observe. This could arise because officials respond to good outside options with higher wage offers, as in our model, but it would also arise if the distribution of wage offers stays fixed and workers accept only offers above the reservation wage. In either case the key to constructing a test is to measure variation in outside options. Private-sector employment, rather than leisure, appears the be the relevant outside option: when asked what 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

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

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.[23] 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 vary substantially, due either to historical demographic shocks (if workers are immobile) or variation in location-specific amenities that compensate for real wage differentials (if workers are mobile). The chief concern is that that this variation is correlated with other determinants of worker's bargaining weight α (see Equation 2). The fact that none of the statutory wage increase was passed through to workers implies $\alpha = 0$; we will also check the sensitivity of our results to controlling for a battery of variables that one would expect to capture variation in α.

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 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.

In addition to supporting the exit view of NREGS wage determination these results have interesting distributional implications: evidently the NREGS is regressive across labor markets, paying out higher wages in markets where wages were higher to begin with.

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

Reinikka and Svensson (2004) also find regressive leakage in funding flows to Ugandan schools attributable to variation in voice rather than exit.

6 Are Wages Selected or Affected?

Table 6 shows that the mean wage received by NREGS participants is higher in communities with factor endowments favoring labor. There are two potential interpretations of this result: it could reflect shifts in the distribution of wages offered to potential workers (a causal effect) or shifts in the distribution of reservation wages (a selection effect). Either interpretation is consistent with our main point that the participation constraint (and not the voice constraint) binds. Separating the two interpretations is nevertheless important for two reasons. First, if officials do price program jobs to market this would provide direct evidence in support of the Leff-Huntington thesis that corruption may "grease the wheels" of an economy that would otherwise be distorted by government intervention (here, the imposition of a wage floor). Second, understanding wage-setting can inform theoretical work on the general-equilibrium effects of employment guarantees (Ravallion, 1987; Ravallion et al., 1993; Basu et al., 2009).

This section provides three tests to distinguish the causal view from the selection view. First and most directly, 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 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.[24] Column I shows that the relationship between factor endowments and 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.

6.2 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.

6.3 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,

[24] 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 planning.

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 *offers* and reservation wages is

$$w = f(s) + u \tag{7}$$

$$\underline{w} = g(s) + v \tag{8}$$

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

$$\mathbb{E}[w|s, d = 1] = f(s) + \mathbb{E}[u|u \geq g(s) - f(s) + v] \tag{9}$$

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:

$$\begin{aligned} \mathbb{E}[w|s, \underline{w}, d = 1] &= f(s) + \mathbb{E}[u|u \geq \underline{w} - f(s), \underline{w}] \\ \frac{\partial}{\partial s}\mathbb{E}[w|s, \underline{w}, d = 1] &= f'(s)(1 - h_1(\underline{w} - f(s), \underline{w})) \end{aligned} \tag{10}$$

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

$$f'(s) = \frac{\frac{\partial}{\partial s}\mathbb{E}[w|s, \underline{w}, d = 1]}{1 - \frac{\partial}{\partial \underline{w}}\mathbb{E}[w|s, \underline{w}, d = 1]} \tag{11}$$

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

$$E[w|s, \underline{w}, d = 1] = \pi_s s + \pi_{\underline{w}} \underline{w} \tag{12}$$

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 10). 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 7 and 8 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.[25]

[25]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.

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.

7 Conclusion

Marginal rates of corruption or "leakage" are an important input into policy-making. Building on the seminal ideas of Hirschmann (1970), we provide a bargaining framework for conceptualizing marginal leakage in which beneficiaries' outside options play a key role. When citizens have effective "voice" they become residual claimants on transfers and marginal leakage will be lower than average leakage. When citizens' threat of "exit" is the binding constraint the corrupt official is residual claimant and marginal leakage is higher than average.

We then provide the first empirical analysis of marginal leakage, working in the context of an important 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. Auxiliary evidence suggests that this is because the threat of exit to the private sector, and not the threat of complaining to higher-ups, is the binding constraint on wage underpayment. These results imply that officials price jobs to market rather than setting the market price, suggesting that corruption tends to "grease the wheels" of the economy by undoing distortionary policies.(Leff, 1964; Huntington, 1968)

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.[26] Understanding the distribution of rents in political and bureaucratic hierarchies is another key 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

[26]Ferraz and Finan (n.d.) show that political incentives matter for corrupt behavior.

and how NGOs function in this sort of environment would be a valuable step towards understanding accountability in local government more generally.

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A Wage Bargaining with Over-reporting (Not for Publication)

In this appendix we briefly demonstrate that if the official can extract additional rents $B(\overline{w})$, $B' > 0$ when the worker participates then (i) the main conclusions of the simpler model in the text still hold, and (ii) the worker may sometimes be overpaid. The idea behind B is that the more actual work is performed the more the official can *over-report* the amount of work done. Niehaus and Sukhtankar (2010) study over-reporting in detail and in particular document $B' > 0$.

As before the worker's reservation payoff is $r \equiv \max\left\{\underline{w}, \overline{w} - \frac{c}{\pi}\right\}$. The surplus generated by reaching agreement is now $\overline{w} + B(\overline{w}) - \underline{w}$ and the worker's equilibrium wage is $w = (1-\alpha)r + \alpha(\overline{w} + B(\overline{w}))$. Note that if $\overline{w} < \underline{w} < \overline{w} + B(\overline{w})$ then the equilibrium involves the worker participating at a wage higher than the statutory one. Average leakage is the fraction of total government expenditure $\overline{w} + B(\overline{w})$ that does not reach the beneficiary, or

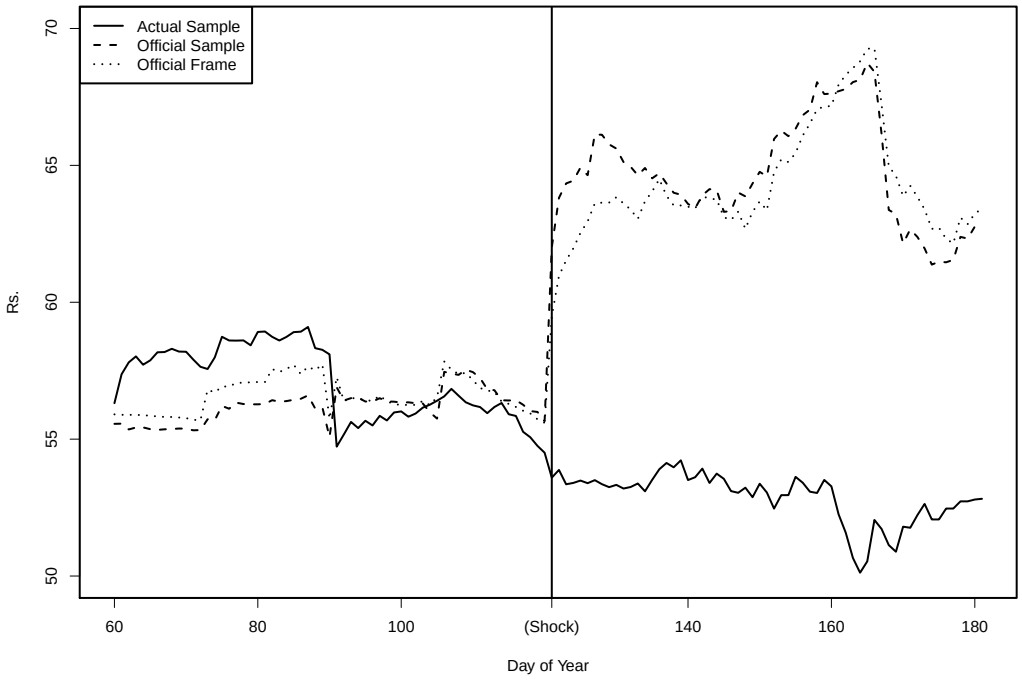
$$AL = \frac{\overline{w} + B(\overline{w}) - w}{\overline{w} + B(\overline{w})} = (1-\alpha)\left(1 - \frac{r}{\overline{w} + B(\overline{w})}\right) \tag{13}$$

Marginal leakage is the change in leakage generated by a change in $\overline{w}$ divided by the change generated in total outlays, or

$$ML = \frac{\frac{\partial}{\partial \overline{w}}\left[\overline{w} + B(\overline{w}) - w\right]}{\frac{\partial}{\partial \overline{w}}\left[\overline{w} + B(\overline{w})\right]} = (1-\alpha)\left(1 - \frac{\frac{\partial r}{\partial \overline{w}}}{1 + B'(\overline{w})}\right) \tag{14}$$

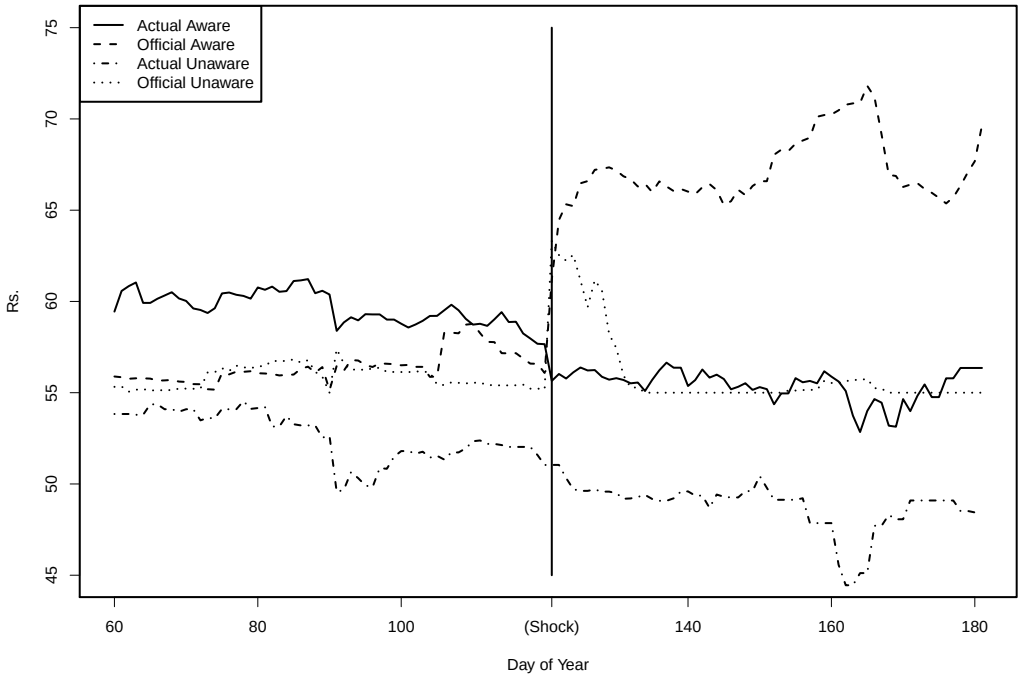
Note in particular that if $\frac{\partial w}{\partial \overline{w}} = 0$ then marginal leakage is 100%, as before. If on the other hand passthrough is positive then calculating marginal leakage necessarily involves calculating $B'(\overline{w})$.

Figure 1: Daily Wage Rates Paid



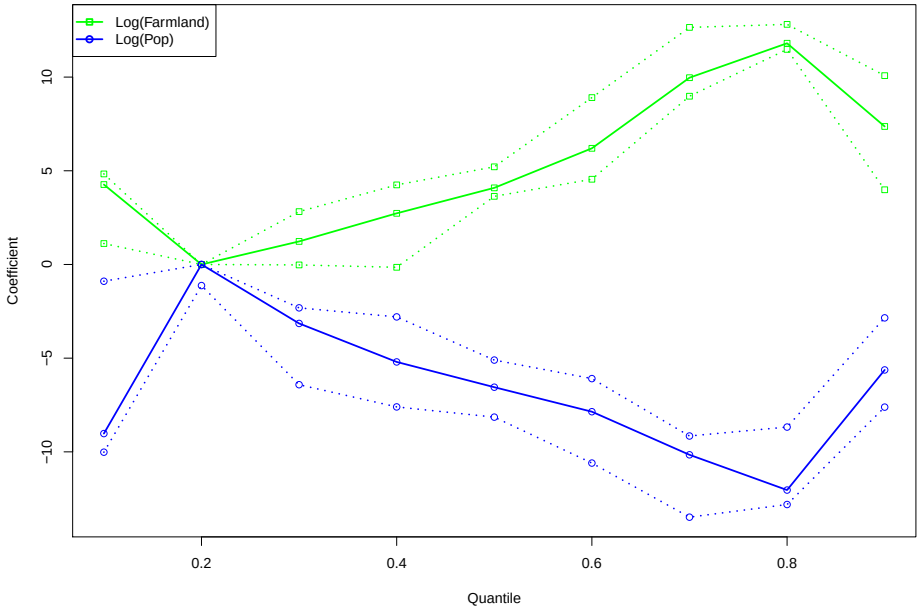
Plots daily series of the average wage rate paid on daily wage work-spells in Orissa over the study period. The Actual Sample series is constructed from household surveys, the Official Sample from official records for the corresponding households, and Official Frame from the universe of official records 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

	NREGA Participants			Non-Participants		
Variable	N	Mean	SD	N	Mean	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	-1.1
	(1.20)***	(1.28)***	(1.12)***	(1.78)***	(1.45)**	(1.32)	(1.62)***	(2.38)	(2.70)
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	2.12
			(1.76)	(1.73)			(1.93)***	(1.94)***	(2.97)
NGO Active			-0.06	0.00			-2.90	-2.51	2.18
			(0.63)	(0.63)			(1.78)	(1.76)	(1.60)
Post * Worker Aware									
Worker Aware									
Month				1.12				-1.91	-1.48
				(0.66)*				(1.01)*	(0.93)
N	3614	3627	3646	3646	860	860	860	860	860
R^2	0.28	0.28	0.27	0.27	0.02	0.06	0.03	0.04	0.15

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 6.3.
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: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$