The Marginal Rate of Corruption in Public Programs: Evidence from India*

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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 theories of "exit" in which outside options in the private sector limit how much rent officials can extract.

JEL codes: D61, D73, H11, H53, I38, K42, O10

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1 Introduction

Public finance teaches us to equate the marginal costs and benefits of social spending.[1] "Marginal benefits" here usually mean the marginal per-dollar benefits of the activity being financed. This interpretation implicitly assumes, however, that money allocated reaches its intended use. In many countries this is not the case, as substantial sums "leak out" due to corruption.[2] Research documenting leakage in Brazil, India, Indonesia, Uganda, and elsewhere has estimated average rates of diversion ranging from 18%-87%.[3]

Olken (2006) argues that governments anticipating a high leakage rate may optimally choose a low level of social spending. In practice this could play out in two ways. On the extensive margin, policy-makers may simply shut down extremely "leaky" programs. On the intensive margin, the optimal level of funding allocated to each program will depend on the *marginal* rate of leakage: that is, the amount of the marginal dollar spent that does not reach its intended use. For example, all else equal a planner will allocate less to a program that loses 50% of every additional dollar to graft than one that loses 40%.

Implementing this idea raises a practical challenge: corruption is notoriously difficult to measure, and even if a planner can measure average leakage rates it is unclear what information these contain about marginal rates. For example, suppose an audit reveals that 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. Distinguishing among such possibilities is an open problem.

This paper presents what we believe to be 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 roughly 11% of the world's population and costs roughly 1% of India's GDP. State governments frequently revise the statutory wages they offer via the NREGS; since implementing officials do not always pay workers what they are due, however, it is unclear how these reforms influence wages actually paid.

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

[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, 2012).

[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. (2012).

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 main margin of adjustment.

Figure 1 summarizes our main result. It plots the evolution of wages during our study period, distinguishing between those reported in official records and by actual participants. The official series clearly increases in response to the reform on 1 May. What is striking is that *none* of the wage increase was passed through to workers. Thus while average leakage from wage payments prior to the policy change was close to 0%, marginal leakage was 100%.[4]

To ensure that the result is not driven by a contemporaneous negative shock we also estimate specifications that take as a control group villages in which *official* records do not reflect the wage change, possibly because of communication delays. We find no significant differences; if anything wages are differentially lower in the "treated" villages (Figure 2). There is also no evidence to suggest that these wage dynamics are transitory. Even before the change most workers were paid a wage different from the statutory wage (i.e. there is substantial heterogeneity around the average), and this pattern continues to hold after the change. Moreover, as we discuss below, although a majority of workers knew of the wage change, aware workers did not see any earnings growth.

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. Research to date suggests that this mechanism plays 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. Whether the NREGS' complaint-handling mechanisms constrain corruption on the margin thus bears investigation.

Our data suggest these mechanisms have little bite. 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

[4]Total theft from the labor budget, including wage under-payments, over-reporting of days worked, and payments to ghost workers, went from 75% to 80% (Niehaus and Sukhtankar, forthcoming).

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 not consistent with a story in which wages converge to the statutory ones once people learn about the policy change in the long run. 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 based on the idea that market forces provide an important check on corruption. If officials value worker participation in the scheme, but workers have outside options in the private sector, then the private sector wage will limit underpayment. In short, this "exit" hypothesis predicts that program wages are determined not by laws but by the market.[5]

We test this conjecture using data on workers' outside options. Ninety-six percent of respondents said that the private labor market was the outside option relevant for them, which suggests that local labor market conditions should influence program wages. To minimize concerns over reverse casuality we test this prediction using variation in villages' relative endowments of land and labor. Although de jure wages should be the same everywhere, we find that de facto they are substantially and significantly higher in villages that are land-abundant and labor-scarce. This suggests that outside options in the private sector at least partly explain NREGS wages. Interestingly, these results imply the fact that workers were paid the statutory wage on average before the shock is misleading, masking the fact that workers in some areas were paid more than the statutory wage ("overpaid") while others were paid less ("underpaid").[6]

[5]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). It also parallels the thesis of Leff (1964) and Huntington (1968) that corruption undoes distortionary policies and thus "greases the wheels" of the economy. In our context, the NREGS is designed to act as a distortionary wage floor, but corruption pulls program wages (and hence, labor allocations) back towards their competitive values.

[6]We use "underpaid" and "overpaid" throughout to refer to the relationship between what workers received and what they were legally entitled to.

A potential caveat to this result is that factor endowments might affect not wage offers but who accepts these offers and thus appear in our sample (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 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.

We see our analysis of marginal leakage as part of recent efforts to adapt public economic theory for use in developing countries where, as Mullainathan et al. (Forthcoming) argue, "it is impossible to understand policy without understanding corruption."[7] Our findings apply most directly to the analysis of workfare schemes, which are common worldwide (Subbarao, 2003) and frequently studied (Ravallion, 1987; Ravallion et al., 1993; Basu et al., 2009). More broadly, our analysis underscores the value of identifying which constraints on rent extraction bind. Shleifer and Vishny (1993) have argued persuasively that competition within government can play an important role; our results suggest that outside options in the private sector may be similarly important.

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 entitles every rural household to up to 100 days of paid employment on government projects per year.[8] 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 was Rs. 401 billion ($8.9 billion), 3.6% of government expenditures, or 0.73% of 2008 GDP.[9] Including state expenditures (25% of the cost of materials) the total outlay is yet higher.

Participation in the program takes place in discrete "spells" of employment. Program

[7] Work in this genre includes Keen (2008), Gordon and Li (2009), Olken and Singhal (2010) and Pomeranz (2012) on taxation and Niehaus et al. (forthcoming) on poverty-targeting.

[8] 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.

[9] 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.`

guidelines state that adult members of registered households can apply for spells up of to 15 days of work at any time. Applications are submitted to the local Gram Panchayat or block office, the lowest and next-lowest units in the administrative hierarchy. Officials are legally obligated to provide the applicant 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), a Junior/Assistant Engineer in the relevant state department, or – contrary to program guidelines – to a private contractor.

Depending on how feasible it is to measure output, workers receive either a fixed wage per day or a piece rate per unit of output (e.g. per cubic foot of soil excavated). In either case participation and compensation are recorded on a paper muster roll. These documents are periodically delivered to the local block office where the data are entered into an electronic 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.

Because eligibility for the NREGS is already universal in rural areas, wages are the principle margin for adjustment. We therefore focus 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. Note that we define both of these behaviors as corruption, in the sense that they violate the official program rules, regardless of what the corrupt agents do with their illicit gains. The policy implications of corruption may of course vary depending on how diverted funds are used. We discuss several specific hypotheses below in concluding.

Under-payment and over-reporting are monitored in different 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. By rule these officials should accept grievances on standardized forms and issue receipts so that petitioners can follow up. How effectively this system functions in practice is an open question.

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. One potential check on over-reporting is the internal verification of works recommended by program guidelines, which call for audits of 100% of projects by block officers, 10% by district offiers, and 2% by state officers (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 and compare these to technical estimates or to their own intuitions about how much work should be necessary. In some cases we observed officials tracking work through periodic photographs of worksites. The upshot is that implementing officials face a low but positive probability of being caught and punished.[10]

3 Data Collection

NREGS 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. Data available from muster rolls include information on each spell of work performed including the identity of the worker, 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 unlikely to occur by chance under a piece rate scheme.[11]

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

[10]For documentation see for example the reports of OREGS-Watch, a coalition of NGOS monitoring implementation in Orissa (`http://groups.google.co.in/group/oregs-watch`).

[11]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; they appear in 6.5% of all spells in our official data in roughly equivalent proportions before and after 1 May.

statutory wage change on 1 May 2007.[12] We sampled work spells from the official records for Gajapati, Koraput, and Rayagada districts in Orissa.[13] 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.[14] Finally, we sampled 2.8% of work spells in these panchayats, stratifying by panchayat, implementing agency (block or panchayat), payment scheme (wage or piece rate), and wage regime (pre or post 1 May 2007). We then set out to survey the 1,938 households appearing in this sample.

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 send enumerators into the villages of 439 households without exposing them 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 parts of Koraput. Of the remaining 1,499 households we were able to either interview or confirm the non-existence/permanent migration/death of 1408 households. To verify non-existence, death, or migration we required enumerators to obtain the names of 3 neighbors willing to vouch for this fact. We exclude from the analysis any households not meeting these stringent standards.

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. The lone exception is that we interviewed the households associated with slightly fewer spells performed by members of a Scheduled Caste or Scheduled Tribe (79% of the intial sample, 77% of the analysis sampel, $p = 0.05$). 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

[12]We waited until January to ensure that all muster roll information had been digitized and uploaded; by law the data should be entered within two weeks after work is performed, but longer delays are common in practice. As a consistency check we downloaded the same data again in March 2008 and confirmed that it had not changed.

[13]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.

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

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 we anticipated imperfect recall. At the same time, the NREGS was a salient new program, and spells of work were likely to be memorable and distinct relative to traditional employment. Moreover, because NREGS payments are uncertain and often delayed, participants seem to keep better track of what they are owed than, say, a US worker with direct deposit. To further 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.

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 $(1 + B'(\overline{w}))$.[15] 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. If anything actual wages received appear to decline over time, though this pattern is largely compositional. Note also that during March 2007 workers were on average *overpaid*; we return to discuss this pattern in Section 5 below.[16]

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. 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 (not reported). Note that in order to obtain an unbiased measure of the change in total program outlays in response to the policy reform we use a representative sample of official spells, including fictitious spells attributed to worker, non-worker, and "ghost" households. As a result the sample of official spells is substantially larger than the sample of actual spells, the difference being attributable to over-reporting. As a sensitivity check we also ran analogous regressions on the restricted set of official spells associated with households that reported doing a strictly positive amount of work, and obtained essentially identical results (not reported).[17]

Columns I-III show that the official wage jumps up significantly after 1 May and that

[15]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.

[16]See Section 6.1.1 for details on the construction of the time series in Figure 1. Note that official wages vary both because of the 1 May policy change and also to a lesser degree because of variation in the proportion of workers reported as having received a "skilled" wage. The overall proportion of spells paid such wages is low (6.5% of all spells).

[17]Directly matching actual and official spells is infeasible because of the many fictitious official spells. For example, a worker may report having worked for one week sometime in May while the records state he worked for the 1st, 2nd, and 4th weeks of May; in such cases any spell-to-spell mapping is inevitably somewhat arbitrary.

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] Note also that, consistent with 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 a puzzle as it means they were leaving rents on the table. The most plausible explanation is that some panchayats did not learn about the wage change immediately. Consistent with this view, we show below that the post 1 May increase is substantially larger in panchayats below median travel time from the block office and district office (Table 5). 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.[19]

[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 using day-of-year trends are similar.

[19] An alternative explanation for non-implementation of the wage reform might be that some panchayats faced cash flow management problems. We believe this would if anything encourage them to claim to be paying the higher wages, since reported wage payments are reimbursable claims. That said, regardless of the exact reason for non-implementation, the tests presented here are inconsistent with the view that the

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.[20] 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.

Another potential measurement concern is that officials might pressure or bargain with workers to report a rosy view to "outsiders." If this were the case then we would expect an upward bias in our measure of pass-through as workers report receiving the new, higher wages when in fact they are not. The fact that we find no pass-through is thus inconsistent with this hypothesis. On this point note also that the large gaps we find between the *quantities* of work reported by officials and by workers imply that collusion is limited, if present (see Niehaus and Sukhtankar (forthcoming)).

Finally, we can also test indirectly for wage pass-through by examining 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 and less vulnerable to collusion 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

apparent lack of pass-through is due to some offsetting negative shock affecting all panchayats similarly.

[20] 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 new, higher wage? The argument is not this simple. The fact that the mean average pre-period wage is very similar to the statutory pre-period wage appears to be something of a coincidence since, as we will see below, there was substantial variation around that mean. 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 wages continue to fall, not rise towards their new statutory level, during our study period (Figure 1). Our ability to measure dynamics over longer horizons is limited by the fact that most NREGS work in Orissa stops once the kharif season begins; 82% of all work done in our study area during 2007 was completed by the end of our study period. We can, however, conduct two tests. First, we re-estimated the specifications in Table 3 on an augmented sample of work spells including those conducted after the end of our study period. This sample is representative of earnings trends among those reported as having worked during our study period. The estimates are similar in all respects to those reported here (results available on request). Second, we can test for differential changes in earnings during our study period for workers better-informed about the wage change. We show below that the 72% of workers who were aware of 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 model in which recipients receive their entitlements once information about the policy change has disseminated.

The absence of wage pass-through implies that, regardless of the exact amount by which expenditures increased, marginal leakage cannot be other than 100% (Equation 2). In earlier work we also estimated the increase in total outlays, and our best estimate is that total expenditure per dollar received by recipients increased from $4.08 to $5.03. (Niehaus and Sukhtankar, forthcoming) In short, expenditures increased substantially without any apparent benefit for participants.

5 Is Voice a Binding Constraint?

The stark divergence between average and marginal under-payment is surprising. In the following two sections we examine what it implies about the underlying model of corruption, and in particular how well two different classes of theory fit the results.

We begin with theories in which corruption is constrained by the threat of victims complaining, an idea we refer to as "voice." Earlier work suggests that voice matters 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.

In our context, the lack of wage pass-through provides little prima facie support for the voice hypothesis. The problem 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. The amount an official must pay to forestall complaints thus increases with $\overline{w}$ as well. To make this point concrete, suppose a worker can complain at expected cost c and the complaint is successful with probability π then the official must pay at least $\overline{w} - \frac{c}{\pi}$ to prevent a complain. In this simple model, a binding constraint complaint has the counterfactual implication that the worker's wage w should increase one-for-one with $\overline{w}$.

To better understand how complaints work 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.[21] 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 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 relative to the statutory wage 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.

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,

[21] 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.

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

the result establishes that there exists some such variable that improves accountability.[23][24]

6 Is Exit a Binding Constraint?

That fact that voice does not appear to constrain corruption on the margin raises an interesting question: if officials are not afraid of underpaying workers more, 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 workers outside options, and in particular by market wages. This view seems consistent with our conversations with NREGS participants, who openly discussed negotiating with program officials until they received a satisfactory wage.[25]

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

[23]We also tested for but did not find evidence of differentially higher pass-through in panchayats in which the position of sarpanch was reserved by law for a woman or ethnic minority.

[24]An additional caveat to this result is that, unlike the other results, it loses statistical significance once we control for district fixed effects.

[25]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.

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 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.[26] 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

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

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 through banks receive significantly higher wages. We note that, while only suggestive, the latter fact is consistent with the hypothesis that financial sector development is critical for the design of fiscal policy (Gordon and Li, 2009). 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.[27]

Finally, in column VI we estimate an instrumental variables model based on the exclusion restriction that factor endowments affect NREGS wages only through their effect on reservation wages. We estimate that a Rs. 1 increase in a worker's reservation wage increases his NREGS wage by Rs. 0.84, and we cannot reject the null that this coefficient is equal to 1.

6.1 Are Wages Selected or Affected?

One caveat to our factor endowment findings is that they could reflect either causal effects on the wages *offered* to workers or selection effects on wages *accepted*. We provide three tests to further distinguish these views. First and most importantly, NREGS participation responds to factor endowments in the *same* direction as NREGS wages, opposite the pattern that would generate selection bias. Second, the impacts of factor endowments on the entire wage distribution are concentrated in the upper end, opposite what selection stories predict. Finally, a new estimator that exploits participants' reservation wages

[27]We we include district fixed effects in these regressions the factor endowment effects are smaller in magnitude but remain strongly significant. The coefficient on bank payment remains positive but loses significance, however, and should be interpreted with this caveat in mind.

within a selection-as-misspecification framework (Heckman, 1979) yields estimates very similar to our baseline ones.

6.1.1 Impacts on Participation

We first test a necessary condition for selection bias: do factor endowments move NREGS participation in the opposite direction as NREGS wages? To do so 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 an average of the wages paid on spells that overlap that day, weighted by these d/D ratios.

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, though columns IV and VIII show little evidence of such effects.[28] Column I shows that relative labor scarcity is still associated with higher NREGS wages, as in Table 6, after restructuring our data. Columns II and III show that this is unlikely to be due to selective participation, as participation moves in the same direction as wages. Concretely, it cannot be the case that accepted NREGS wages are higher in land-abundant villages because fewer people accept low values from a fixed distribution of wage offers. (The columns differ only in that II restricts the sample to days on which we observe some work, making it more comparable with Column I.)

One might worry that this test is under-powered since, with few sampled households per panchayat, 48% of panchayat-days have no participation reported. Columns V-VIII of Table 7 show that we obtain very similar results when we aggregate the data and conduct the analysis at the panchayat-month level, for which fewer (37%) observations have no participation reported.

6.1.2 Distributional Impacts

As a second check on selection we estimated quantile-regression analogues of the models in Table 6. We use these to test the null hypothesis that wage offers are drawn from some fixed c.d.f. F that is invariant to factor endowments. One can show that if factor endowments simply truncate the distribution of *accepted* offers then

[28] This is of independent interest as it suggests either that there were few workers whose reservation wages $\underline{w}$ fell between the old wage (Rs. 55) and the new one (Rs. 70), or that officials face short-run quantity constraints in hiring due to the nature of project planning.

(a) They strictly increase the lowest quantile, have no effect on the highest quantile, and have a decreasing impact on average across quantiles of the accepted wage distribution.

(b) If F is not too concave then they have a monotone decreasing effect on higher quantiles of the accepted wage distribution.

A formal derivation is available on request. Figure 3 plots the estimated coefficients on our two factor endowment measures from a series of quantile regressions at each decile, including month dummies. The effects of both factors are concentrated in the upper, not lower, end of the distribution.

6.1.3 A Test and Correction using Reservation Wages

Our third approach exploits data on workers' reservation wages to achieve set or point identification. Given our sample size we present the argument parametrical, though the non-parametric extension is straightforward and available on request. 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. Following Heckman (1979), let wage *offers* and reservation wages be determined by

$$w = \beta s + u \tag{3}$$

$$\underline{w} = \gamma s + v \tag{4}$$

where s is independent of (u, v). We wish to estimate β but observe $(w, \underline{w})$ only if $w \geq \underline{w}$. The conditional expectation function in the selected sample is

$$\mathbb{E}[w|s, w \geq \underline{w}] = \beta s + \mathbb{E}[u|u \geq (\gamma - \beta)s + v] \tag{5}$$

which implies that OLS estimates of β are biased unless $\gamma = \beta$. We consider instead the reduced-form augmented regression function

$$E[w|s, \underline{w}, w \geq \underline{w}] = \pi_s s + \pi_{\underline{w}} \underline{w} \tag{6}$$

In Appendix A we establish two results concerning this specification. First, while $\pi_s \neq \beta$ generally, $\beta = 0$ implies $\pi_s = 0$. The practical implication is that we can reject the null that s does not influence wage offers in the population if we find that it is a significant predictor of offers in the selected sample, *after controlling for* $\underline{w}$. Second, under the structural assumption that u and v are independently distributed we can identify β as $\beta = \frac{\pi_s}{1-\pi_{\underline{w}}}$. In other words, we can recover the effect of s on wage offers in the population by running a regression of wage realizations in the selected sample on s and the reservation wage, and then scaling up the former coefficient by one minus the later.[29]

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 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 7). 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.[30]

[29] The independence assumption amounts 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 can assess how reasonable it is by examining the sensitivity of our results to expanding the control set.

[30] Truncated Tobit models are often used to address selection. For a large proportion (60%) of the work spells in our data, however, the wage received exactly equalled the reservation wage. This mass point is exactly what our "exit" hypothesis predicts, but is inconsistent with 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. We study India's National Rural Employment Guarantee Scheme, a large social protection 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 slightly over-paid prior to the change. The policy implications of the analysis are thus sharply different from those one might infer from examining averages.

These estimates, along with corroborating pieces of evidence, are inconsistent with theories of corruption in which the threat of complaints, or "voice," is binding at the margin. The data are consistent with theories in which officials price jobs to reflect the value of workers' outside options in the private labor market. Of course, this "exit" threat need not be workers' only source of bargaining power vis-a-vis officials, and a deeper understanding of their negotiations would be valuable.

Our analysis was motivated by the question of optimal redistribution. While it is intuitive to think that wage increases are never optimal in the face of 100% marginal leakage, we do not know the ultimately incidence of the rents extracted by NREGS 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.[31] 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.

There are two bright points in the otherwise gloomy picture we present. First, NGOs may lower marginal leakage. There could be many mechanisms – they may provide literate advocates who better understand how to navigate the bureaucracy, or may serve a coordinating function among workers. Understanding what NGOs do in this sort of environment may help us understand accountability in local government more generally. Second, workers paid through bank accounts earn more. In ongoing work we are evaluating the causal impact of electronic payment technologies on NREGS corruption.

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

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A Identification using Reservation Wages

Consider the model from Section 6.1.3 and let $d = 1(w \geq \underline{w})$. The derivative of the conditional expectation of w given s and $\underline{w}$ in the selected sample is

$$\frac{\partial}{\partial s}\mathbb{E}[w|s,\underline{w},d=1] = \beta(1 - h_1(\underline{w} - \beta s, \underline{w})) \tag{7}$$

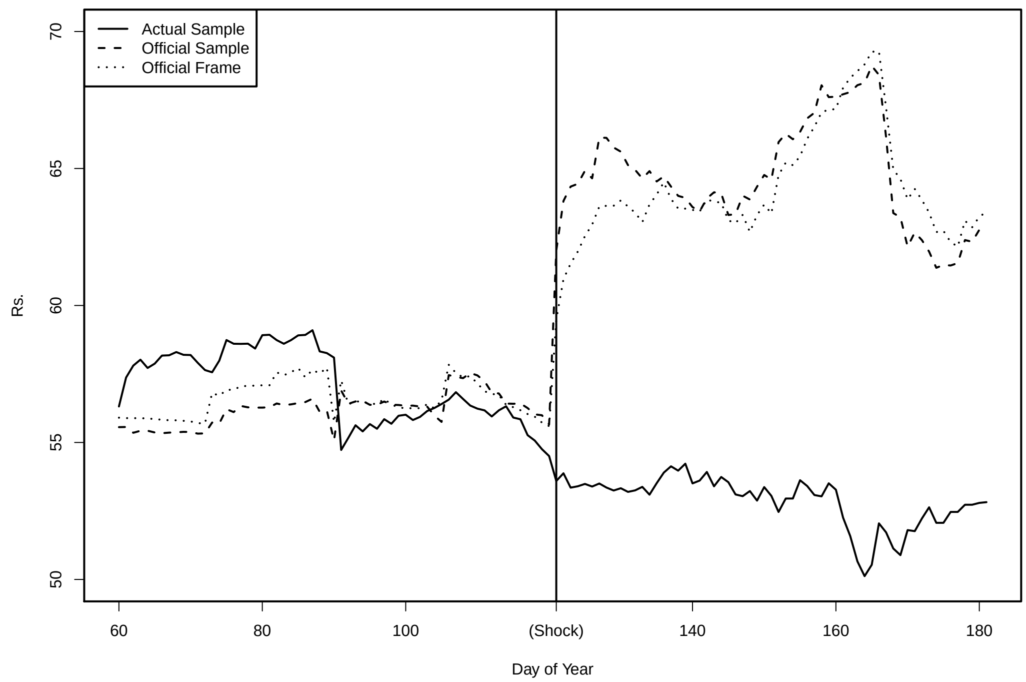
where $h(x,\underline{w}) \equiv \mathbb{E}[u|u \geq x, \underline{w}]$. This immediately implies that $\pi_s \neq \beta$ generically, but also that $\beta = 0 \Rightarrow \pi_s = 0$, establishing the first point.

Now suppose u and v are independently distributed. In this case the distribution of u is independent of $\underline{w}$ so that $h(\underline{w} - \beta s, \underline{w}) = \overline{h}(\underline{w} - \beta s)$. $\overline{h}'(\underline{w} - \beta s)$ is then identified by variation in $\underline{w}$ and we can write

$$\beta = \frac{\frac{\partial}{\partial s}\mathbb{E}[w|s,\underline{w},d=1]}{1 - \frac{\partial}{\partial \underline{w}}\mathbb{E}[w|s,\underline{w},d=1]} \qquad \frac{\pi_s}{1-\pi_{\underline{w}}} \tag{8}$$

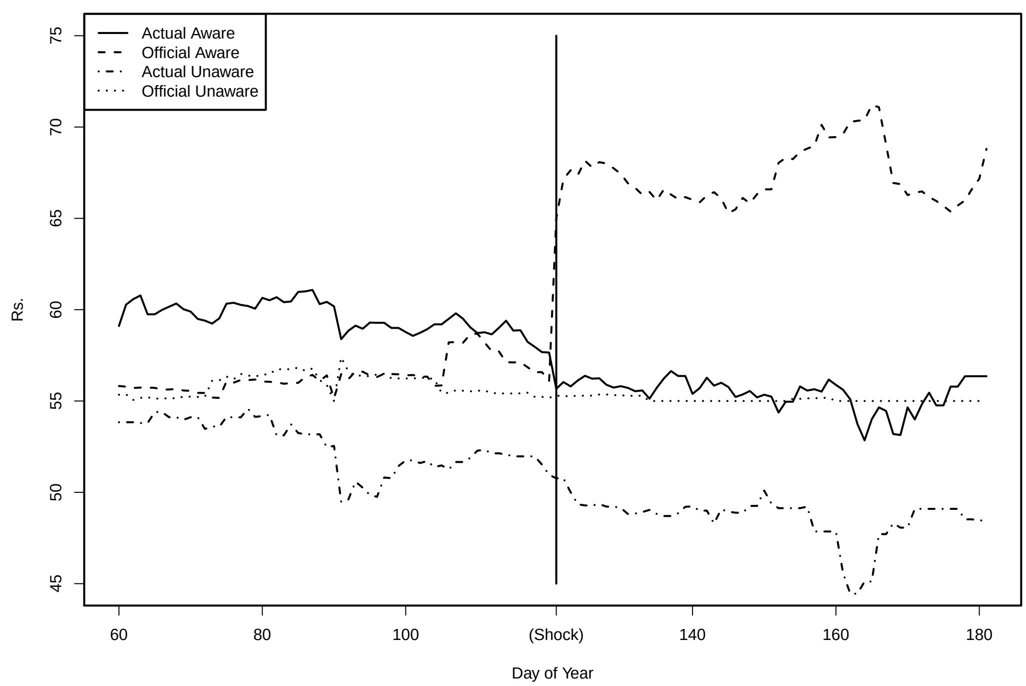
where the second equality follows from the linear functional form imposed in (6.

Figure 1: Daily Wage Rates Paid



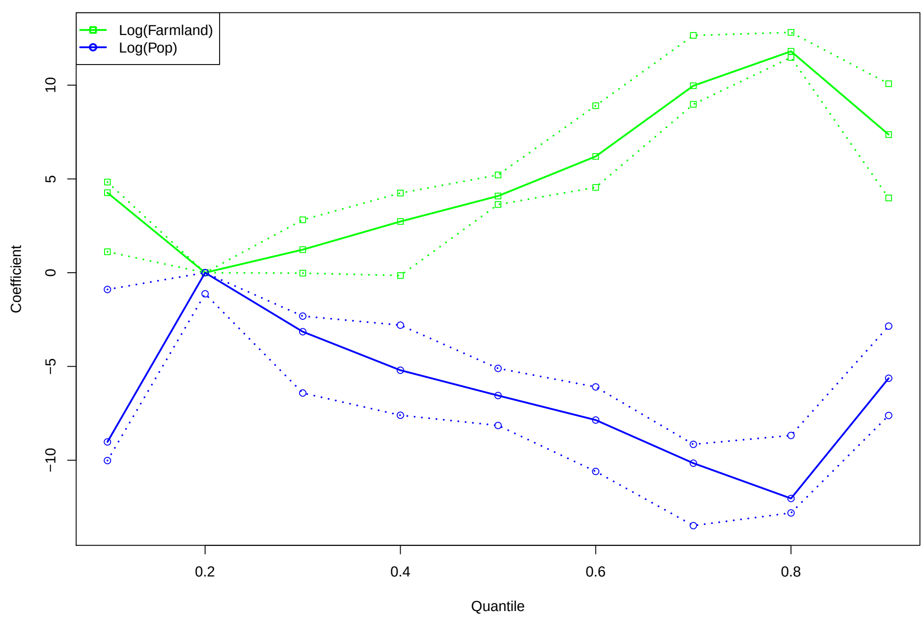
Plots daily series of the average wage rate paid on daily wage work-spells in Orissa over the study period. The Actual Sample series is constructed from household surveys, the Official Sample from official records for the corresponding households, and Official Frame from the universe of official records 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	Mean	SD	Non-Participants 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	-0.92	-3.23	-0.83	-0.49	-2.85
	(1.02)***	(1.83)***	(1.30)***	(0.35)***	(1.18)***	(1.94)	(0.88)	(1.28)**
Month		2.38				-8.42		
		(3.91)				(4.46)*		
Month^2		-0.12				0.79		
		(0.46)				(0.54)		
Post * Panch. Aware				10.82				-0.41
				(1.32)***				(1.97)
Panch. Aware				1.28				7.42
				(0.90)				(1.65)***
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	0.33	0.02	0.03	0.46	0.11

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	VI
Reservation Wage						0.84
						(0.16)***
Log(Farmland)	8.78	7.17	5.69	7.26	5.46	
	(2.04)***	(1.42)***	(1.23)***	(1.66)***	(1.32)***	
Log(Population)	-9.05	-10.62	-9.53	-10.35	-8.68	
	(2.94)***	(2.18)***	(2.03)***	(2.34)***	(2.01)***	
Male			1.59		1.34	
			(0.78)**		(0.82)	
Literate			0.85		0.68	
			(0.96)		(1.04)	
Paid via Bank			5.95		5.47	
			(1.12)***		(1.25)***	
Scheduled Caste			-2.54		-3.02	
			(1.77)		(1.80)*	
Scheduled Tribe			-3.32		-4.6	
			(1.55)**		(1.43)***	
Backward Caste			-9.50		-11.31	
			(1.71)***		(1.88)***	
Near to BDO				0.92	0.37	
				(1.41)	(1.24)	
Near to Collector				2.72	1.82	
				(1.49)*	(1.51)	
NGO Active				0.01	0.56	
				(1.20)	(1.04)	
Month FEs	Y	Y	Y	Y	Y	Y
N	975	975	945	829	806	975
R^2	0.12	0.14	0.18	0.16	0.21	-

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-VI. Estimation is via OLS in Columns I-V and via GMM in Column VI.
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: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$