Corruption Dynamics: The Golden Goose Effect*

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Abstract

Theories of corrupt behavior imply that parameter changes have dynamic incentive effects very different from their static ones. We study these dynamic effects in one of the world's largest public transfer programs, India's National Rural Employment Guarantee Act. We uncover large-scale embezzlement along multiple margins: theft from beneficiaries and theft from taxpayers. Using exogenous changes in statutory wages, we then test a simple, dynamic model of rent extraction. We find evidence for a "golden goose" effect: other things equal, when expected future opportunities for rent extraction are high, officials extract less rent today in order to preserve tomorrow's opportunities. This behavioral response tends to stabilize levels of corruption in the face of external shocks. Our results imply that evaluations of policy experiments should take dynamics seriously.

JEL codes: D73, H53, J30, K42, O12

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1 Introduction

Development scholars view corruption as a leading cause of persistent poverty in less developed countries. Corruption hampers states' ability to redistribute wealth and can transform progressive fiscal schemes into regressive ones (Reinikka and Svensson 2004, Olken 2006). It also imposes social costs by undermining policies meant to address market failures (Bertrand, Djankov, Hanna, and Mullainathan 2007). Consequently the World Bank (2006) ranks the fight against corruption as a top priority for poverty reduction.

Motivated by these practical concerns, research has focused on the design of incentives for corruptible officials, generating a series of predictions. One key idea is that auditing can deter corruption (Becker and Stigler 1974), an idea borne out in the field (Nagin, Rebitzer, Sanders, and Taylor 2002, Olken 2007). A related view is that transparency can increase discipline (Reinikka and Svensson 2005, Ferraz and Finan 2008). Another line of work examines the effects of market structure (Shleifer and Vishny 1993, Ades and Di Tella 1999, Olken and Barron 2009) – for example, theory predicts that increasing competition between government service providers will drive down equilibrium bribe rates.

This paper examines the robustness of comparative statics for corruption in a dynamic setting. Taking dynamics seriously is important because changes to parameters governing corruption tend to have offsetting static and dynamic effects. For example, consider decreasing the probability that an agent will be audited. As is well-known, the static effect of this change is to make illicit behavior more attractive. But the same argument applies in the future as well, so that future illicit rents will increase. This in turn raises the continuation value to the agent of keeping his job; if job loss is a potential punishment, this makes illicit behavior today *less* attractive. We call this dynamic effect the "golden goose" effect: officials wish to preserve the goose that lays the golden eggs (not kill it, as did the deplorably myopic farmer in the fable).

We formally derive golden goose effects and then test for them empirically. The latter task presents several challenges. First, consistently measuring corruption at all is difficult, as usual. Second, we need to observe not only present corruption but also some factors shaping agents' expectations about future corruption opportunities. Finally, we require a clearly exogenous source of variation in these factors.

India's National Rural Employment Guarantee Act (NREGA) meets these criteria. Launched in 2005, this federal Act mandates state-level anti-poverty schemes on a grand scale: it extends to every household in rural India a legal guarantee of 100 days of paid employment per year. It imposes no eligibility restrictions; participants need only be willing to work. It far exceeds all previous workfare schemes in scope and ambition, including the well-known state-level program in Maharashtra (Ravallion, Datt, and Chaudhuri 1993). It is also a volatile political issue in India, with much of the debate centering on how much corruption exists and how much is tolerable.

For our purposes the NREGA offers several attractive features. First, we have access to completely disaggregated official records – names and addresses of participants and details of their employment and compensation. By conducting our own corresponding survey of (alleged) beneficiaries we can construct credible measures of corruption. Second, we can observe corruption evolving over time, which allows us to construct measures of forward-looking expectations. Third, exogenous changes over time in statutory program wages provide a credible source of identification. We test our model using a wage change in the eastern state of Orissa in May of 2007, using the neighboring state of Andhra Pradesh as a control in some specifications. Finally, we observe corruption on two distinct kinds of employment projects: some on which workers receive a daily wage, and others on which they are paid piece rates. Piece rate projects were not directly affected by revisions to the daily wage schedule. As we explain below, this particular institutional feature helps us to separate dynamic golden goose effects from static price effects.

We find that prices do matter. When statutory daily wages increase, officials "over-report" more work on wage projects — that is, they invent more fictitious days of work and pocket the reimbursement for those days. Overall we estimate that the cost to the government per dollar received by beneficiaries increased from from \$4.08 to \$5.03 as a result of the wage increase.

However, we also find two forms of evidence for golden goose effects. First, we find that theft on piece rate projects, which are not directly affected by the wage shock, declines in response to it. Second, we find that theft of all kinds is differentially lower after the wage increase in the areas which subsequently executed the most daily wage projects — in other words, in the areas where the shock had the greatest impact on future rent expectations. We can rule out a broad class of alternative explanations by comparing the effects of future rent expectations to past rent realizations; we only find a robust role for the future. We also test directly for confounding changes in monitoring intensity and find no evidence for such changes. These results all suggest that golden goose effects are at work, and the point estimates imply that they are quantitatively important. Back-of-the-envelope calculations imply that the wage increase raised theft by approximately 70% less than it would have had it not affected future rent expectations.

Golden goose effects necessitate care in the design and evaluation of policy. This is particularly true where policy-makers hope to learn about the merits of a project by piloting it, since pilots generate different dynamic incentives than commitments to implement a project in perpetuity. For example, the golden goose view predicts that a one-shot distribution of disaster relief will be diverted with greater rapaciousness than a comparable long-term distribution scheme. Conversely, a crackdown on corruption will have larger effects if it is expected to be temporary. These considerations imply that there is real value in tracking the performance of interventions over long time periods.

Golden goose effects relate to several other lines of research, most notably the concept of the efficiency wage (Mookherjee and Png 1995, Banerjee 1997, Acemoglu and Verdier 2000, Cadot 1987, Besley and McLaren 1993), which is a rent promised to officials to induce their good behavior. Our results suggest, rather ironically, that illicit rents can be as effective as licit ones for this purpose. They can therefore be interpreted as an indirect test for the efficiency wage effect, which has proved difficult to test for directly (Di Tella and Schargrodsky 2003). Golden goose effects apply to politicians who risk losing elections (Ferraz and Finan 2009) as well as to bureaucrats who risk their jobs; consequently they influence models of electoral discipline (Barro 1973, Ferejohn 1986, Persson, Roland, and Tabellini 1997, Ahlin 2005), where they limit the extent to which voters can control rent extraction by politicians. Finally, golden goose effects provide a potential alternative explanation for "displacement effects" such as those documented by Yang (2008).

The rest of the paper is organized as follows. Section 2 describes the context and operational details of the NREGA in greater detail. Section 3 develops a simple contextualized model of corruption, derives predictions driven by the golden goose effect, and discusses potential confounding interpretations. Section 4 describes our empirical strategy for estimating these effects; Section 5 presents the main results and also tests for several potential confounds. Section 6 concludes.

2 Contextual Background on the NREGA

India's National Rural Employment Guarantee Act is a landmark effort to redistribute income to the rural poor. It was a central tenet of the ruling United Progressive Alliance's campaign platform when it won elections in 2004. It has been controversial since its inception. Supporters hoped that it would usher in large-scale changes in the countryside, bringing relief to those left behind by India's surging GDP growth. Detractors claimed that it would be a sop for the "babu raj," the petty elite of the countryside; one former Chief Economic Advisor called it an "expensive gravy train."[1]

Some numbers are helpful to get a sense of the NREGA's ambitious scope. The program was launched in February 2006 in the poorest 100 districts in India and as of April 2008 covers the entire country (604 rural districts). The total proposed budget allocation for the 2008-2009 fiscal year is Rs. 160 billion,[2] or approximately US$ 2.5 billion, which is 6.6% of the total government plan expenditures and 0.4% of 2007 GDP.[3] Since the program is still relatively new in many parts of the country and since full implementation and participation generally take some time, it is likely the program will ultimately cost substantially more. Based on estimates of likely demand, program architect Jean Dreze has projected an annual cost of Rs. 400 billion (US$10 billion) at scale (Dreze 2004).

Another distinguishing feature of the NREGA is that it is an unrestricted entitlement program: every household in rural India has a right to 100 days of paid employment per year. Implementing officials are not to turn anyone away and have no leeway to categorize applicants as eligible or not, since there are no eligibility requirements. This distinguishes the scheme from many of its predecessors, in which the supply of benefits was fixed. It also contrasts with several well-known theoretical analyses of corruption which focus on the fixed-benefits case and the ways in which a bureaucrat can extract rents by controlling the allocation of benefits (e.g. Banerjee (1997), Acemoglu and Verdier (2000)). Many common corruption situations fall into the fixed-benefits paradigm — for example, paying a bribe to obtain a government contract — but ours does not. We therefore present in section 3 a simple model better adapted to understanding corruption in a demand-driven program. The model seeks to capture the essence of the opportunities for corruption that the NREGA affords, which we describe next.

2.1 Statutory Operational Procedures

Each operational program cycle begins before the start of a fiscal year, when local governments at the panchayat and block[4] levels plan a "shelf" of projects to be undertaken during the upcoming year.[5] The particular types of project allowed under the NREGA

[1] As cited in an article by Jean Dreze in *The Hindu*, `http://www.thehindu.com/2007/03/09/stories/2007030902701000.htm`

[2] See `http://indiabudget.nic.in/ub2008-09/eb/vol2.htm`. The central government proposes to contribute Rs. 144 billion, which must by law be at most 90% of total expenditure, the rest of the funding coming from the states.

[3] Expenditure figures: `http://indiabudget.nic.in/ub2008-09/bag/bag3.htm`. GDP figures: `http://mospi.nic.in/4_gdpind_cur.pdf`

[4] A *Gram Panchayat (GP)* or simply panchayat is the lowest level of administration in the Indian government, comprising of a group of villages which elect a chief executive - *Sarpanch* - to run the GP. A block is an intermediate level of government between GPs and districts.

[5] There seems to be little scope for endogenous gaming of project implementation. Technically, village assemblies suggest projects which are then approved by panchayats at various levels. In practice, the Block

are typical of rural employment projects: road construction and earthworks related to irrigation and water conservation predominate. Projects also vary in the payment scheme they utilize. NREGA workers can be paid either on a daily wage or a piece rate basis depending on the practicality of measuring output. In practice, all work on a particular project is generally compensated in the same manner (see Figure 1). Consequently there are identifiable daily wage projects and piece rate projects, an institutional feature which we exploit in our empirical analysis below.

To obtain work on a project, interested households must first apply for a *jobcard*.[6] The jobcard contains a list of household members, some basic demographic information, and blank sheets for recording work and payment history. In principle, any household can obtain a jobcard for free at either the panchayat or block administrative office.

Jobcards in hand, workers can apply for work at any time. Each application for work specifies the desired length of the spell, up to 15 days. The applicant must be assigned to a project within 15 days after submitting the application, but has no influence over the choice of project, except that it must be located within 5 km of his or her home. Applicants who do not receive work within this time period are eligible for unemployment benefits.

At the work sites the implementing officials record attendance (in the case of daily wage projects) or measure output (in the piece rate case). These records should be duplicated in the job card which the workers keep and in muster rolls kept by the officials. At the end of each pay period the officials pay the workers out of money advanced by higher levels of government. Wherever possible these payments are supposed to be automated through branch banks or their rudimentary substitutes, the post offices. However, most workers in our study area receive their wages in cash (if at all).

In the meantime the official paper records are transported to the local block office, where they are digitized and entered into a nation-wide database. Central and state governments then reimburse the local officials for expenses incurred on the basis of these records. The official micro-data are also made publicly available through a web portal maintained by the central Ministry of Rural Development (`http://nrega.nic.in`). This aggressive transparency measure was included in the NREGA with the explicit intent of checking corruption.

2.2 Golden Eggs: Opportunities for Rent Extraction

No doubt the perspicacious reader of the preceding section has already spotted several opportunities for illicit gain. Observing theft along several of these margins is what enables us to construct sharp tests of our agency model. We focus on theft from project spending, and in particular on theft from the labor budget. This is required by law to exceed 60% of total spending, and in fact we find that theft in this category is so extensive that even if all of the 40% allocated to materials were stolen, the labor budget would still

Development Officer suggests the shelf and it gets approved by the District Panchayat. Conversations with BDOs indicate that the shelf of works and the order in which those are to be executed are typically set in advance.

[6]Since each household is limited to 100 days of employment per year the definition of a household is important. In NREGA guidelines a household is "a nuclear family comprising mother, father, and their children, and may include any person wholly or substantially dependent on the head of the family". (Ministry of Rural Development 2008)

be the larger source of illegal rents.[7]

Theft from the labor budget comes in two flavors. First, officials can under-pay workers for the work they have done. For example, a worker who worked for 10 days on a daily wage project when the statutory minimum wage was Rs. 55 per day might receive only Rs. 45 per day in take-home pay. We call this "theft from beneficiaries". Second, officials can over-report the amount of work done when they send their reports up the hierarchy, which we call "theft from taxpayers". Continuing our example, the official might report that the worker had worked for 20 days rather than 10. His total rents would then equal $55 \cdot 20 - 45 \cdot 10 = 650$ rupees, the sum of the two sorts of theft.

Conceptually this decomposition into theft from beneficiaries and from taxpayers applies to both daily-wage and piece-rate spells. However, the former case is substantially easier to decompose empirically. In our survey we asked wage-workers how many days they worked in addition to the total they were paid, which combined with knowledge of the official wage rate lets us establish how much they should have been paid. To measure how much piece-rate workers should have been paid, we would need them to have known and be able to recall the amount of earth they moved, volume of rocks they split, etc. Unsurprisingly, most do not know these figures. Consequently in our theoretical and empirical work we treat theft on piece rate projects as unitary, keeping in mind that it includes theft both from beneficiaries and from taxpayers.

Aside from redistributive implications, theft from beneficiaries and from taxpayers clearly differ in how they are monitored. Underpaid workers may know they are underpaid and the question is whether there is anything cost-effective they can do about it. In practice this means complaining to someone. The right person to complain to may be near at hand, but may also be as far away as district headquarters. The level of access to a sympathetic ear is therefore important for understanding theft from beneficiaries. But workers with little leverage and rational expectations may expect to be held up and prefer not to work on NREGA projects at all. We examine this issue formally in Section 3 below.

On the other hand, program beneficiaries have little incentive to monitor theft from taxpayers. This is an important consequence of the NREGA's demand-driven nature: a rupee stolen through over-reporting does not mean a rupee less for the poor in the village. Therefore over-reporting must be monitored from the top down by higher-level officials. Officials at the block and district level can use the NREGA's management information system (MIS) to see aggregate quantities of work done on various projects and compare these to technical estimates or to their own intuitions about how much work should be necessary. In some areas we visited, higher-tier officials also receive digital photographs of the works at various stages of completion, which they can use to assess the plausibility

[7] Many other forms of corruption may occur. First, project selection might be influenced by garden-variety bribes and favors. Next, households occasionally have to pay to obtain a jobcard or to have a photograph taken for their jobcard. We asked about this in our household survey and found that such charges are small (averaging Rs. 10 conditional on being positive) and uncommon (17% report paying positive amounts). This is sensible given that (1) a jobcard is an entitlement and not receiving a jobcard is a relatively verifiable event; (2) households can apply to either the panchayat or the block office, which potentially creates bribe-reducing competition (Shleifer and Vishny 1993); (3) the NREGA places no limit on the number of participants, so that there is less scope for "greasing the wheel" forms of corruption, wherein corruption mainly improves efficiency by getting around cumbersome red tape or regulations (Huntington 1968, Leff 1964); and (4) officials can in fact extract far greater rents by actively encouraging participation and then stealing in other ways.

of expenditure reports. However, it is unclear what incentives higher-level officials have to exert effort monitoring their subordinates for corruption. The levels of over-reporting that we document below suggest that these incentives are weak.

What of punishment? The NREGA Operational Guidelines (Ministry of Rural Development 2008) – a 194 page document created by the Government of India – has very little to say on punishing errant officials, and the one paragraph in the entire document that mentions punishment says nothing specific:

> The State Government will ensure speedy action against the concerned officials/ nonofficials for misappropriation of funds, frauds, incorrect measurement, false entries in the muster rolls and other irregularities of a serious nature, resulting in the leakage of Government/public funds/resources and the denial of entitlements to workers. The State Government will also take appropriate steps to prevent such irregularities. [sic]

In practice, while some officials responsible for implementing the NREGA are elected and others appointed, all seem to face the same potential penalty if caught stealing: suspension or removal from office. A loose online coalition of non-governmental organizations that monitor NREGA in Orissa called OREGS-Watch produces numerous daily reports, which occasionally pertain to officials being caught and suspended. In these instances, the common factor is incontrovertible proof brought straight to the office of the District Collector (the chief administrative officer in the district), whereafter the guilty official is immediately suspended and the stolen money often recovered.[8] These instances, however, are rare when compared to the steady stream of reports on misappropriation of funds and complaints made to higher authorities.[9]

2.3 Who Pays the Piper? The Political Economy of Wage-Setting

Our empirical work exploits for identification an increase in statutory program wages in Orissa which, we argue, was exogenous from the point of view of local officials. The particular wage change we study was in fact part of a broad pattern of wage hikes in many states, a phenomenon which arose due to special features of the NREGA's funding pattern.

By law the central (federal) government foots most of the NREGA's massive bill: 100% of the unskilled labor budget, and 75% of the materials budget (defined to include the cost of skilled labor) (Ministry of Law and Justice 2005). However, the task of setting wages and piece-rates was left in the hands of the states. This provision was likely intended to allow flexibility to adapt program parameters to local labor market conditions. But it also created a powerful incentive for state politicians to raise their minimum wages, since their citizens would benefit and the central government would pay. Most states have duly raised their minimum wages.[10] A particularly valuable feature is that many states

[8] A typical example of such a case that was documented is available at http://www.dailypioneer.com/indexn12.asp?main_variable=BHUBANESWAR&file_name=bhub6%2Etxt&counter_img=6.

[9] The first half of 2008 saw the murder of several activists who had been working to expose corruption, giving the impression that the penalties for corruption are less than the penalties for exposing corruption.

[10] The central government has attempted to limit this behavior in two ways: by requiring states to use the same rates under NREGA as they do on projects which they pay for themselves, and by increasing the norm for number of hours in a workday from 8 to 9.

(including Orissa) raised their wage rates and piece rates at different times. As a result the policy shocks only directly impacted a subset of the projects on each panchayat's "shelf". In the next section we explain how this feature generates sharp tests of the golden goose effect.

3 A Dynamic Model of Rent Extraction

Like most corrupt officials, those who siphon money from the NREGA must worry about the likelihood of being caught and punished. Following the seminal work of Becker and Stigler (1974), a large literature has explored the implications of this constraint. One familiar result is that efficiency wages may help deter misbehavior. Here we draw out an additional and closely related implication: the expectation of *illicit* future rents may also deter current rent extraction.

The model depicts a corrupt agent who trades off rent extraction against the risk of exposure and job loss. Our driving assumptions are that the chance the official is caught and punished increase in the amount of corruption he engages in, and that the penalty for being caught is dismissal. Golden goose effects – wherein officials are cautious about corruption now in order to preserve future opportunities for rent extraction – arise naturally in any such setting. We adapt the model to our context by explicitly modeling the distinct forms of corruption that we measure empirically: over-reporting on daily wage projects, under-payment on daily wage projects, and aggregate theft on piece-rate projects. The combination of the standard theory with multiple theft margins and an exogenous wage rate shock allows us to develop testable predictions, which we then take to the data in Sections 4 and 5.

Time is discrete. An infinitely-lived official and a group of N infinitely-lived workers seek to maximize their discounted earnings stream:

$$u_i(t) = \sum_{\tau=t}^{\infty} \beta^{\tau-t} y_i(\tau) \tag{3.1}$$

where $y_i(\tau)$ are the earnings of agent i in period τ. Additional players with identical preferences wait in the wings to replace the official should he be fired.

In each period exactly one NREGA project is active. We abstract from simultaneous ongoing projects primarily to simplify the exposition; it is also true, however, that most of the panchayats in our sample have either one or zero projects active at all times during our study period. Let $\omega^t = 1$ indicate that the active project at time t is a wage project, and $\omega^t = 0$ that it is a piece rate project. We represent the "shelf" of projects as an infinite stochastic stream of projects: at the beginning of each period a random project is drawn from the shelf with

$$\phi \equiv \mathbf{P}(\omega^t = 1 | \omega^{t-1}, \omega^{t-2}, \ldots) \tag{3.2}$$

We suppose that all agents know ϕ but do not know exactly which projects will be implemented in the future. At the cost of a small loss of realism, this approach ensures that the dynamic environment is stationary and also greatly simplifies the expression of comparative statics. It also permits a close analogy between the model and our empirical work, in which the fraction of future projects that are daily wage (a measure of ϕ) plays a key role.

Each worker inelastically supplies one indivisible unit of labor in each period. We will interpret a unit flexibly as either a day (in the case of daily wage projects) or as a unit of output (in the case of piece-rate projects). Labor may be expended on an NREGA project or in the private sector, where worker i can earn $\underline{w}^t$ ($\underline{r}^t$). Let n^t (q^t) be the number of days (output units) supplied to the project when $\omega^t = 1$ ($\omega^t = 0$), and let and w_i^t (r_i^t) be the wage (piece-rate) that participating worker i receives. This need not equal the statutory wage $\overline{w}$ (the statutory piece rate $\overline{r}$).

NREGA wages and employment levels emerge from bargaining between the official and the workers. We model bargaining as an ex-ante agreement between each worker and the official on participation and remuneration, and assign bargaining power to the official. Play within each period evolves as follows:

1. The official offers each worker i employment on the NREGA project at wage w_i^t (rate r_i^t).
2. Workers choose either to work on the NREGA project or in the private sector.
3. The official reports a number of days worked $\hat{n}^t$ (quantity of work done $\hat{q}^t$) to his superiors, collects compensation, and remunerates the workers.

By modeling bargaining as an ex-ante event we implicitly assume that officials can commit to paying the wages they promise, which is reasonable given the ongoing nature of the interaction between local officials and workers. Appendix A shows that our bargaining assumptions uniquely generate two key features of our data: NREGA wages actually received by program participants move one-for-one with local market wages, but are unaffected by changes in the statutory wage.

Participation n^t and the average participant's wage w^t (piece rate r^t) are predetermined once the official chooses how much work $\hat{n}^t$ to report. If the current project is a wage project, official's period t rents will be

$$y_o^t(\omega^t = 1) = \underbrace{(\overline{w} - w^t)}_{\text{under-payment}} n^t + \underbrace{(\hat{n}^t - n^t)}_{\text{over-reporting}} \overline{w}$$

and analogously if it is a piece-rate project,

$$y_o^t(\omega^t = 0) = \underbrace{(\overline{r} - r^t)}_{\text{under-payment}} q^t + \underbrace{(\hat{q}^t - q^t)}_{\text{over-reporting}} \overline{r}$$

We present the components of piece-rate theft for expositional purposes only; in our data we observe only their sum, i.e. $\hat{q}^t\overline{r} - q^t r^t$.

Over-reporting the amount of work done puts the official at risk of being detected by a superior and removed from office. The probability of detection on daily wage projects is $\pi(\hat{n}, n)$, with $\pi(n, n) = 0$ for any n, $\pi_1 > 0$, $\pi_2 < 0$, and $\pi_{11} > 0$ for all n; the last condition ensures an interior equilibrium amount of over-reporting. We also assume that if $n > n'$ then $\pi((n + x), n) \leq \pi((n' + x), n')$. This condition ensures that officials weakly prefer to have more people work on the project; it would be satisfied if, for example, the probability of detection depended on the total amount of over-reporting or on the average rate of over-reporting. The probability of detection on piece rate projects is $\mu(\hat{q}^t, q^t)$ with entirely analogous properties. If an official is caught we assume that he is removed from office before the beginning of the next period and earns some fixed outside option normalized to zero in every subsequent period. In practice corrupt officials are sometimes suspended rather than fired; modeling this would affect our results only quantitatively.

In stating and proving comparative statics statements we will make use of the recursive formulation of the official's decision problem.

$$\begin{aligned}
\overline{V}(\overline{w},\phi) &\equiv \phi V(\overline{w},1,\phi) + (1-\phi)V(\overline{w},0,\phi) \\
V(\overline{w},1,\phi) &\equiv \max_{\hat{n}} \left[(\overline{w}-w^t)n^t + (\hat{n}-n^t)\overline{w} + \beta(1-\pi(\hat{n},n^t))\overline{V}(\overline{w},\phi)\right] \\
V(\overline{w},0,\phi) &\equiv \max_{\hat{q}} \left[(\overline{r}-r^t)q^t + (\hat{q}-q^t)\overline{r} + \beta(1-\mu(\hat{q},q^t))\overline{V}(\overline{w},\phi)\right]
\end{aligned}$$

Here $V(\overline{w},1)$ is the official's expected continuation payoff in a period with a daily wage project, $V(\overline{w},0)$ is his expected continuation payoff in a period with a piece rate project, and $\overline{V}(\overline{w})$ is his expected continuation payoff unconditional on project type. The variables w^t, n^t, r^t and q^t are written as constants since — as we show in Appendix A — they are invariant to the main parameters of interest, $\overline{w}$ and ϕ.

3.1 The Effects of a Statutory Wage Shock

How should a rent-maximizing, forward-looking official respond to an increase in the statutory daily wage? Intuition suggests that he should increase over-reporting, for the simple reason that the temptation to do so has increased. Our first result shows that this is true only under an inelasticity condition:

Proposition 1. *Over-reporting $\hat{n}^t - n^t$ on daily wage projects is increasing in $\overline{w}$ if*

$$\frac{\overline{w}}{\overline{V}}\frac{\partial \overline{V}}{\partial \overline{w}} < 1 \tag{3.3}$$

and decreasing otherwise.

Proof. All proofs are deferred to Appendix B. □

Why is this prediction ambiguous? Higher statutory wages have two offsetting effects. The first is the familiar price effect: a higher wage increases the benefit from over-reporting. The second is a golden goose effect: a higher wage raises the value of future over-reporting, which in turn increases the importance of keeping the job. Proposition 1 states that the former effect dominates only if the elasticity of future benefits with respect to the wage is sufficiently small.

The tension captured by Proposition 1 is a general feature of dynamic agency models of corruption. Any increase in the "scope" for rent extraction — new opportunities, lower costs, weaker monitoring — will have a direct tendency to increase rent extraction. But if agents expect the increase in scope to be permanent then it will also raise the importance they attach to remaining in a rent-extracting position. If the punishment for illicit rent extraction involves loss of future opportunities to extract rents, this generates an incentive to *reduce* current extraction.

While it cleanly illustrates this tension, Proposition 1 also implies that over-reporting of daily wage work is not an ideal outcome variable with which to identify the golden goose effect, due to the confounding price effect. To obtain a stronger test we need to observe rent extraction along a different margin, one not directly affected by the wage increase. Theft from piece-rate projects satisfies this requirement. A myopic profit-maximizing official would steal the same amount from piece-rate projects regardless of the daily wage. But a forward-looking official should reduce theft in response to a daily wage shock:

Proposition 2. *Total theft from piece-rate projects* ($\hat{q}^t\overline{r} - q^t r^t$) *is decreasing in* $\overline{w}$.

Here we obtain an unmitigated golden goose effect. A higher statutory wage has no effect on current rent-extraction opportunities for a bureaucrat managing a piece-rate project. It does, however, increase expected future rent extraction opportunities, just as in the daily wage case.

3.2 Differential Effects by Project Shelf Composition

Propositions 1 and 2 make predictions about the effects of a statutory wage shock, which we test in Section 5 below. However, we have available only inter-temporal variation in the statutory wage with which to implement these tests. While the results of these tests are insensitive to non-parametric controls for time trends, it would be reassuring to derive additional tests that exploit cross-sectional variation in the intensity of the wage shock.

Our model suggests a natural approach to doing so. Golden goose effects operate through increasing the continuation value $\overline{V}$ of remaining in office. Since the wage change directly affects only those future periods in which the project is a wage project, it is natural to expect that this effect should be stronger where the fraction of future projects that will be daily wage projects, ϕ, is larger.

In fact this is not strictly necessary. It is possible that changing ϕ changes $\overline{V}$ directly, independent of $\overline{w}$, which generates additional golden goose effects whose impacts on wage sensitivities are difficult to sign. However, this second set of effects vanish when the equilibrium rents extracted from daily wage and piece rate projects are the same, since in this case there is no direct behavior effect of changing ϕ.

Proposition 3. *Suppose that for some* $(\overline{w}, \phi)$ *we have* $y_o(1) = y_o(0)$*. Then*

$$\frac{\partial^2(\hat{n} - n)}{\partial\overline{w}\partial\phi} < 0$$

and

$$\frac{\partial^2(\hat{q}^t\overline{r} - q^t r^t)}{\partial\overline{w}\partial\phi} < 0$$

evaluated at $(\overline{w}, \phi)$.

We verify below that equilibrium rents from daily wage and piece rate projects are similar, and then test these predictions directly.

3.3 Confounding Explanations

While the predictions above permit us to test our model, they are not necessarily unique to that model. We describe here three alternative mechanisms that could generate similar effects and then highlight a common implication of these mechanisms which we can use to distinguish them from golden goose effects.

One potential confound involves the "production function" for corruption. We believe that the bulk of corruption in our setting simply involves writing one number on paper instead of another. Suppose, however, that this requires the use of some scarce input that can be shifted across time (e.g. effort). Then the wage shock would induce officials to optimally re-allocate this input across time, giving rise to patterns similar to those we predict.

Second, if officials care about things other than consumption then the wage shock might have income effects. The expectation of large future rents would lower the expected relative marginal utility of income now, leading to lower corruption.

Finally, empirical tests could potentially be sensitive to issues of time aggregation. In our empirical work we treat the day as the basic unit of time, but monitoring might be based on less frequent observations. This would mechanically imply that officials expecting to steal more tomorrow would steal less today, since the probability of detection would depend on the sum of today's report and tomorrow's.

The key difference between the golden goose effect and each of these mechanisms is that while the former is purely forward-looking, the latter are all time-symmetric. For example, if officials who plan to expend a lot of effort stealing tomorrow steal less today, then officials who have expended a lot of effort yesterday should also steal less today. Similarly, if officials who expect large future income shocks care less about income today, then so should officials who have already received large income shocks. Likewise, if monitoring probabilities are based on weekly or monthly aggregates then corruption today should on average be negatively related to both corruption tomorrow and corruption yesterday.

In contrast to these time-symmetric factors, golden goose effects are purely forward looking. We exploit this distinction between the past and the future in our empirical work below to construct more refined tests of our model.

4 Empirical Approach

The main reason that micro-empirical studies of corruption are scarce is that corruption defies objective, consistent measurement across multiple settings (Innovative exceptions include Reinikka and Svensson (2004), Olken (2006), and Olken (2007)). However, the public availability of official NREGA records allows us to measure corruption by checking these records against independent survey results. Our basic approach to testing the implications of the model described above is to compare official data on wages paid and days worked to results from an original survey of (alleged) program beneficiaries. Identification comes from a wage shock - Orissa changed its minimum daily wage from Rs. 55 to Rs. 70 effective May 1st, 2007.

4.1 Official Data

All official data pertaining to the operation of the program are publicly available online through a central website (`http://nrega.nic.in`). Data are available at the level of the individuals within households, including names, ages, and addresses, the unique household jobcard number, the number of days each household member worked, which projects they worked on, and how much they were paid. The sheer quantity of micro-data available is staggering. One can download an individual jobcard, or a two-week muster roll on a particular project.[11]

[11]Whereas we are reasonably confident that the official records accurately reflect the data passed on to the government by lower level officials, it is possible that some of the records are compromised in a way that is not necessarily duplicitous. For example, for convenience you might record that Jane Doe worked even though it was John Doe - but in fact there was no wrong-doing. If the Janes are aware of this we would learn it during the survey. We also survey other household members and ask if they worked, even if they are

The official data also contained details on the projects undertaken, including the nature of the project and the names of officials responsible for implementing it. While the designation of a project as a daily wage versus a piece rate project is missing, it is easy to infer whether a project pays daily wages or piece rates, since there are only a few allowed daily wage rates,[12] As Figure 1 shows, almost all projects pay exclusively daily wages or piece rates.

While official guidelines call for data to be uploaded within two weeks of work being done, longer delays of up to a few months are common in practice. To allow time for all the relevant data to be uploaded, we used as our sample frame the official records for the states of Orissa and Andhra Pradesh as downloaded in January 2008, six months after our sample period. As a cross-check we also downloaded the official records a second time in March 2008. We found that the records for Orissa remained essentially unchanged, but that a large number of new observations appeared for Andhra Pradesh. These new observations were spread uniformly across space and time and so do not appear to have resulted from delays in processing records for specific panchayats or projects. They do, however, raise some concerns about the appropriateness of the sample frame used for Andhra Pradesh. Consequently we focus primarily on the Orissa data, using AP outcomes as controls only in Table 5.

4.2 Sampling Design

Our main sampling frame was the list of officially recorded NREGA work spells during the period March 1st, 2007 to June 30th, 2007 in Gajapati, Koraput, and Rayagada districts in Orissa. Within these districts, we restricted our attention to blocks at the state border. We sampled 60% of the Gram Panchayats within the study blocks, stratified by whether the elected seat of the GP chief executive (the *Sarpanch*) was reserved for women, since previous work on local government in India by Chattopadhyay and Duflo (2004) suggests that these reservations may affect levels of corruption. Having set the target sample size at 2000, we sampled work spells in the randomly selected panchayats,[13] stratifying by:

- Panchayat
- Whether the project was implemented by block or panchayat
- Whether project was wage or piece-rate project
- Whether work spell was before or after the daily wage shock

This procedure yielded a total of 1938 households (fewer than 2000 because some households had multiple reported spells during our study period). We asked all adult members of the sample household about their work on NREGA projects during our sample period, in case work done by one member of the household was mistakenly or for convenience sake assigned to someone else in the household, and also to gauge the extent of this kind of misreporting which may not be entirely malicious.

not listed as having worked, so we would find some of the Johns if this is going on.

[12] These are Rs. 55, 65, 75, and 85 prior to the wage change, and Rs. 70, 80, 90 and 100 afterwards. We designate a project as daily wage if more than 95% of the wages paid are these amounts.

[13] In a given panchayat, we sampled X percent of work spells reported as done during the sample period, where X was the sample size for the state divided by the number of work spells done

4.3 Survey Results and Sample Description

Figure 2 summarizes our attempts at locating officially listed program participants during the months of January and February 2008 in Orissa. Given the sensitive nature of the survey, and the dangers inherent in surveying in a region beset with Maoist insurgents, conflict between mining conglomerates and the local tribal population, and tensions between evangelical Christian missionaries and right-wing Hindu activists, our surveyors were asked not to enter villages if they felt threatened in any way.[14] Hence, out of the original sample of $1,938$ households, we were unable to make attempts to reach 439, mainly due to an incident which caused tensions between a mining company and locals in Rayagada and a polite request by Maoists to not enter certain areas of Koraput. The official records for the sample we were unable to reach are no different than the official records for the sample we were able to reach, which assuages concerns about selection bias.

Of the 1499 households we did attempt to reach, we managed to reach or confirm the non-existence/permanent migration/death of 1408 households. In order to determine whether a household that was included in the official records was actually non-existent or no longer lived in the village, we asked surveyors to confirm the non-existence or permanent migration date with 3 sets of neighbors who were willing to supply their names on the survey. Households who match these stringent standards are included in the analysis as fictitious. We also discovered some "ghost" workers – those who supposedly had worked in the official records but who had actually died before the sample period began – who are also included in the analysis as fictitious. We exclude from the analysis 91 households who were away temporarily or refused the survey, or were households for whom we do not have conclusive evidence of the migration date or fictitious nature.

Fictitious households were not the only way through which officials inflated labor records. Of the 1328 households in which we completed interviews, only 821 confirmed having a household member who worked on an NREGA project during the period we asked about. Those households that actually worked on NREGA are very similar to those that did not, the only potential differences being that NREGA participants came from slightly larger households (4.94 members versus 4.65), and were more likely to claim being self-employed in agriculture (45% versus 36%). Awareness of amenities that NREGA projects should provide to workers (drinking water, shade, first aid, and a creche) is also slightly higher for those who worked. In general, the sample is extremely poor and uneducated, compared either to averages across India or Orissa, with 77% of households below the poverty line and only 27% of household heads being able to write their names (the definition of "literate" in India). (See Table 1)

[14]A number of people have been threatened, beaten, and even murdered for investigating NREGA corruption, including an activist killed in May 2008 in one of our sampled Panchayats. See, for example, an article in the Hindu describing the dangers facing NGO activists working on NREGA issues: `http://www.thehindu.com/2008/05/22/stories/2008052253871000.htm`. For an account of an armed Maoist attack on a police armament depot in a neighboring district see `http://www.thehindu.com/2008/02/17/stories/2008021757890100.htm`. For an account of Christian-Hindu tension see `http://news.bbc.co.uk/2/hi/south_asia/7486252.stm`.

4.4 Variable Descriptions and Empirical Specifications

We ask respondents retroactively about spells of work[15] they did between March 1, 2007 and June 30, 2007, including when they worked on each separate spell, the number of days worked, the wages they received, the amount of work done in case it was a piece rate project, and what project they worked on.[16] While one might be worried about recall issues as the survey was conducted over six months later, we are confident that this does not pose a large problem, for several reasons. First, NREGA is a new and very salient program, and spells of work on this program are likely to be memorable and distinct as opposed 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. Next, we designed the survey carefully to prompt memory (e.g. using major holidays as reference points) and trained surveyors to jog respondents' memories. Finally, recall problems will only bias our results if recall is affected by the treatments we examine, and there is little reason to believe it will be.

We believe other potential threats to the accuracy and veracity of respondents self-reports of NREGA work to be minimal. We made clear to respondents that we were conducting academic research and did not work for the government to discourage them from claiming fictitious underpayment. In fact most respondents reported that they had been paid what they were owed. Conversely, the structure of the NREGA gives officials little need to secure worker's collusion in their over-reporting. All a worker could possible supply would be a signature, which has little relevance when most people cannot write their own name anyway. There is also 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 in the official data. Finally and most importantly, there is no reason to think any of these concerns would lead to differential biases (which would affect our results) and not just level ones (which would not). We confirm this conjecture below by showing that the wage shock had no effect on the self-reported variables we use in our analysis.

Our analysis of the survey data includes spells that contain information on at least the month of the spell, the number of days worked, and the wages received. We impute start or end dates if unavailable,[17] and construct time-series of survey reports of work done and wages paid by aggregating data at the panchayat-day level for the sample period.[18] Similarly, we construct time-series of the official data by aggregating official reports of work done and wage paid *of only those households who we interviewed or confirmed as*

[15]A spell of work is defined as an uninterrupted period of employment on a single project, not including holidays.

[16]In addition to asking about work spells, we ask questions about household demographics and socio-economic status, on awareness of NREGA rules and the wage changes, on labor market outcomes and outside options, and political participation and beliefs. In addition to the survey of program participants, we also asked a separate questionnaire to village elders with questions on labor market conditions and agricultural seasons in the village.

[17]We distribute days worked equally over the month if neither start nor end date are available, and equally in the period between the start date and end date if the number of days worked is less than the period between the start and end dates.

[18]The alternative method would be to match individual spells from the official data to those reported by respondents. However, this is difficult given the number of over-reported spells in the official data, and missing information in the survey data. Our method assembles the same information in a much simpler manner.

fictitious over the sample period.

Given these outcome variables, we then test propositions 1 and 2 from the model above in a simple econometric framework. Our exogenous shock is the wage change in Orissa, which we code as a simple dummy variable for observations including and after May 1, 2007. While plotting the raw time-series of days worked we noticed substantial periodicity in both the actual and official reports of days worked, and sharp drops which are explained by major public holidays (see Figure 4). To control for these factors, and also any spurious correlations over time, we include various time trends and an indicator for major public holidays. Moreover, given that the opportunity for corruption only exists when a project is ongoing, we include non-parametric controls for number of days of work actually done ($DayCat$). We also include district fixed effects (δ) in certain specifications. In all specifications, we cluster standard errors by both panchayat and day. Hence, for outcome Y in panchayat p at time t, we have:

$$Y_{pt} = \beta_0 + \beta_1 Shock_t + Time_t^{'}\gamma + DayCat_{pt}^{'}\phi + \delta_p + \epsilon_{pt} \tag{4.1}$$

Identification rests on the assumption that unobserved factors affecting the optimal amount of theft are orthogonal to the shock $Shock_t$ after controlling for general time trends.

We can relax this identifying assumption by using data from the neighboring district of Vizianagaram in Andhra Pradesh to control for unobserved time-varying effects common to the geographic region under study. This approach is, however, subject to several caveats. First, we can only utilize it when estimating models of piece-rate theft, since essentially all projects in Andhra Pradesh are piece rate. Second, as noted above a substantial number of new observations appeared in the official Vizianagaram records between January 2008, when we culled our initial sample, and March 2008. These observations are distributed evenly across space and time and so appear unlikely to introduce any bias; nevertheless we can only conjecture as to their origin. Finally, Andhra Pradesh made two revisions to its schedule of piece rates during our sample period, the latter of which took effect on March 25th, 2007. Because of its proximity to the daily wage change in Orissa this shock limits the value of Andhra Pradesh as a control.

Keeping these limitations in mind, we estimate

$$\begin{aligned} Y_{pt} = \beta_0 + \beta_1 ORshock_t * OR_p + \beta_2 APShock1_t * AP_p + \beta_3 APShock2_t * AP_p \\ + \beta_4 ORshock_t + \beta_5 APShock1_t + \beta_6 APShock2_t + OR_p \\ + Time_{pt}^{'}\gamma + DayCat_{pt}^{'}\phi + \delta_p + \epsilon_{pt} \end{aligned} \tag{4.2}$$

The coefficient of interest in this specification is β_1, the differential effect of the post-shock period $ORshock_t$ on corrupt behavior in Orissa, indicated by OR_p. We control for a variety of time and state-specific time trends.

Proposition 3 suggests a final test of the golden goose hypothesis, wherein higher continuation values of staying in office – proxied here by larger fractions of daily wage projects post-shock – lead to lower corruption now. To test this prediction we need an appropriate empirical analogue to ϕ, the probability that a future project in our model is a daily wage project. For each panchayat-day observation, therefore, we calculate the fraction $FwdWageFrac$ of project-days in the upcoming two months that are daily wage project-days. We define a "project-day" as a day on which a particular project is running, and define a project as running if work on that project as been reported in the past and will be reported in the future. The $FwdWageFrac$ aims to measure variation in proportion of daily wage projects on the panchayat's "shelf" of projects, which by rule should have been planned in advance of the wage change. As a precautionary measure we also test below whether the composition of projects itself responds to the wage shock and find that it does not.

We could attempt to test Proposition 3 directly by interacting $FwdWageFrac$ with the wage shock and estimating

$$Y_{pt} = \beta_0 + \beta_1 \text{Shock}_t + \beta_2 Shock_t * FwdWageFrac_{pt} + \beta_3 FwdWageFrac_{pt} + Time_t^{'}\gamma + DayCat_{pt}^{'}\phi + \delta_p + \epsilon_{pt} \quad (4.3)$$

and testing $\beta_2 < 0$. However, mechanisms other than the golden goose effect might also generate this interaction effect. As discussed above, a key shared feature of these mechanisms is that they imply that *past* opportunities for corruption should matter in a manner similar to *future* opportunities. To test this notion we construct $BkWageFrac$, the backwards-looking analogue to $FwdWageFrac$. This measures for every panchayat and every day the fraction of project-days in the preceding two months that were daily wage. We then estimate

$$Y_{pt} = \beta_0 + \beta_1 \text{Shock}_t + \beta_2 Shock_t * FwdWageFrac_{pt} FwdWageFrac_{pt} + \beta_3 Shock_t * BkWageFrac_{pt} + \beta_4 FwdWageFrac_{pt} + \beta_5 BkWageFrac_{pt} + Time_t^{'}\gamma + DayCat_{pt}^{'}\phi + \delta_p + \epsilon_{pt} \quad (4.4)$$

The coefficients of interest here are β_2 and β_3. If our model is correct we should see $\beta_2 < 0$ with no prediction about β_3. If, on the other hand, one of the time-symmetric mechanisms is at work we should see $\beta_2 \simeq \beta_3 < 0$.

5 Results: The Golden Goose Effect

5.1 Preliminaries: Wages, Quantities and Rents

We first verify that our "treatment" – the exogenous wage shock – actually worked. Figure 3 shows this clearly – the average rate paid on daily wage projects as officially reported shoots up to close to Rs. 70 after the change on May 1st. The figure also shows that the average actual wage rate paid has little to do with the statutory wage; whereas it seems to decline over the study period, once compositional effects are removed it is quite flat,[19] and does not rise after the wage increase. Underpayment of daily wages is driven by something else, which we find is the going market wage (results not reported, available on request).

The fact that actual wages are irresponsive to the statutory wage suggests that actual labor supply should also be independent of the wage shock and that it is appropriate to control for real labor supply when predicting the reports that officials submit. To reassure ourselves that this is the case we report in Table 2 the results of regressions of real piece rate and daily wage labor supply on time-varying predictors, including the wage shock.

[19] Gajapati district has higher daily market wage rates, and there are fewer observations from Gajapati in our sample over time.

As expected the coefficients on the wage shock indicator are small and insignificant across all specifications.

Finally, before implementing the test of the golden goose hypothesis defined by Proposition 3 we must first check that equilibrium rents from daily wage projects are similar to those from piece rate projects. By dividing the total theft in the two categories of projects by the number of actual days worked on those projects, we find that the rate of theft per day worked is very similar post-shock; Rs. 228 per actual day worked in daily wage projects as opposed to Rs. 254 in piece rate projects.[20]

5.2 Over-reporting of Days Worked in Daily Wage Projects

We begin our core analysis by examining the reported number of days worked on daily wage projects. Figure 4 shows the evolution of this figure and the corresponding amount of real labor supplied over time. The absence of a clear effect of May 1st mirrors the ambiguous nature of Proposition 1, which states that the effect of a wage shock depends on the elasticity of future rents with respect to the wage. It is difficult to tell from the figure whether over-reporting went up or down after the wage change.

Table 3 reports that the effect of the wage shock on over-reporting of daily wage days is positive; however, none of the three specifications has a significant coefficient on the wage shock. Column 1 presents the basic specification, which includes a third-order polynomial in day of month and an indicator variable for major holidays in order to control for the monthly periodicity and sharp dips evident in the figure above, in addition to the linear time trend reported in the table. We also include non-parametric controls for the number of days of work actually done, since opportunities for corruption only exist when a project is ongoing. Column 2 adds higher order polynomials to the time trend to the basic specification, while column 3 adds district fixed effects to the specification in column 2. The differences in the specifications do not make any difference to the coefficient on the wage shock, which remains positive but short of statistical significance. Might this reflect a countervailing force to the price effect of the increase in daily wage?

Columns 4-6 in Table 3 suggest that this is the case. They show that the direct effect of the wage shock is indeed positive and significant at the 10% level. However, there is a strongly significant negative interaction between the wage shock and the forward-looking fraction of daily wage projects, as predicted by Proposition 3. Morever, the interaction between the shock and the backward-looking fraction of wage projects is positive and insignificant, except weakly in Column V. This is inconsistent with the class of models that generate time-symmetric effects of corruption opportunities, but consistent with the golden goose mechanism whereby only future opportunities depress current theft.

5.3 Theft in Piece Rate Projects

As opposed to the predicted effect on over-reporting of daily wage days which was ambiguous, proposition 2 suggests that the effect on theft in piece rate projects is unambiguously negative. Since the wage shock only affects daily wage projects, current opportunities for

[20] These figures are scaled to reflect misreporting of days worked as daily wage projects when in fact they were designated as piece rate projects in the official data. In general, this kind of misreporting is rare: 82% of spells are reported correctly, whereas 15% of piece rate spells are reported as daily wage spells.

theft from piece rate projects are unchanged, and the expected future benefits cause officials to be more cautious. Figure 5 shows the evolution of the official and actual payments in piece rate projects over the sample period. A decline in theft is evident immediately after the wage shock, as the officially reported payments fall while the actual payments rise.

Regression analysis confirms the visual evidence. Table 4 presents the same specifications as in table 3, with the total reported payments on piece rate projects as the dependent variable. The effect of the wage shock is negative in all three specifications in columns 1-3 and is significant at the 10% level. The magnitude of the coefficient – about Rs. 70 per day – is also economically meaningful compared to the average theft per panchayat-day observation prior to the shock was Rs. 264 (conditional on there being some theft).

Columns 4-6 in Table 4 report tests of Proposition 3. As with daily wage over-reporting we find a negative differential effect of the shock in panchayats with a relatively high fraction of daily wage projects upcoming, and again we estimate a positive coefficient on the interaction between the shock and past project composition. These results are not, however, significant at conventional levels. Power is limited due to the relative infrequency of piece-rate projects in Orissa (see Figure 1). Nevertheless, the point estimates are consistent with our model predictions and inconsistent with time-symmetric interpretations.

We can potentially improve the power of our tests and rule out time-varying confounds by using Andhra Pradesh as a control. Table 5 reports estimates of Equation 4.2, the differences-in-differences specification. The Orissa-specific effect of the daily wage shock in Orissa is negative, larger than the first-differences estimate, and significant across all specifications. Column I is the base model; Column II introduces finer-grained locality fixed effects, Column III adds non-linear time controls, and Column IV allows for state-specific trends. While these estimates are subject to the concerns noted above, they are entirely supportive of the golden goose hypothesis.

5.4 Interpreting Magnitudes

The numbers mentioned above give some sense of the economic importance of golden goose effects: for example, the estimated minus Rs. 70 effect of the wage increase on per-day theft from piece rate projects is large in comparison to the pre-shock average of Rs. 264 stolen per day. One way to provide a more comprehensive sense of magnitudes is to use the point estimates from both piece rate and daily wage regressions to construct a counterfactual: by how much would theft have increased following the wage shock absent golden goose effects? We can then compare this counterfactual to the observed increase and quantify the "dampening" effect of expectations.

To operationalize this idea we use the coefficients from Column IV of Table 3 and Column I of Table 4, our base specifications. Note that using the single-difference estimates for piece rate theft rather than the difference-in-differences is conservative as the latter are larger. We estimate the actual increase in theft attributable to the wage change for each observation as the product of the coefficient on the shock indicator (and in the daily wage regressions, its interactions with project composition) with the corresponding regressors. We then sum this quantity over all observations to obtain an estimate Δ_{actual} of the total increase in theft attributable to the shock. For the daily wage regressions (in which the outcome variable is number of days reported) we then multiply this figure by the average daily wage reported in the official data after the shock. To construct a counterfactual $\Delta_{counter}$ we perform a similar calculation but omit the contributions of the piece rate regressions and the forward-looking interaction term in the daily wage regressions. This corresponds loosely to a situation in which officials believed (incorrectly) at all times that all future projects would be piece rate projects, so that the wage shock had only direct effects on their behavior. Our estimates imply that the dampening effect $\frac{\Delta_{counter}-\Delta_{actual}}{\Delta_{counter}}$ was approximately 73%, or in other words that the increase in the daily wage raised theft by 73% less than it would have had it not affected officials future rent expectations.

Are golden goose effects of this magnitude plausible? Direct calibration of our model is not possible since we observe only some of the illicit revenue streams which a corrupt official would lose if suspended or removed from his job. In principle, however, large golden goose effects are entirely possible even when the empirical probability of detection and dismissal is low, as in our context. This is because lax monitoring itself has golden goose effects: it lowers the chance of detection today, but also lowers the chance of detection in the future and thus raises the continuation value of holding onto one's job. By way of illustration, if a perfectly patient ($\beta = 1$) official only supervises wage projects ($\phi = 1$) and over-reports a fixed number $\hat{n} - n$ of days per period, then the sensitivity of his continuation value to the daily wage

$$\frac{\partial \overline{V}}{\partial \overline{w}} = \frac{\hat{n} - n}{\pi(\hat{n}, n)} \tag{5.1}$$

becomes very large as the probability of detection π falls. Because of this lengthening of the effective time horizon, lax monitoring can *increase* rather than decrease the sensitivity of rents to a daily wage change and thus drive large golden goose effects.

5.5 Is Project Composition Affected?

The analysis above treats the composition of projects (ϕ in our model) as exogenous. Officially the shelf of projects is fixed in advance at the start of the fiscal year (March 2007) and payment schemes are determined by project characteristics — for example, whether it involves digging a ditch where it is easy to measure output, as opposed to watering plants where it is not. However, if officials had scope to reclassify projects they might be tempted to convert piece rate projects into daily wage.[21] This would introduce subtle biases into the estimates reported above. The most obvious bias actually pushes against our main results: panchayats more likely to convert projects to daily wage after the shock would presumably be generally more likely to steal after the shock, which would generate positive bias in estimates of forward interaction effects. Nevertheless, we ask whether project composition appears to respond to the change in daily wages.

Prima facie this does not appear to be the case: 74% of projects were daily wage before May 1st compared to 72% after. Table 6 presents a formal test where we regress $FwdWageFrac$ on an indicator for the shock along with time controls. The point estimates are insignificant and correspond to a 0.05 standard deviation change in project

[21] In principle one might examine project composition before the announcement of the wage change, between its announcement and the implementation, and after its implementation. In practice, the earliest official announcement of the wage change was a Government of Orissa notice dated April 25th to take effect May 1st, so that the announcement and the change itself were almost concurrent.

composition. These results corroborate the testimony of block-level officials that the shelf of projects and payment schemes is pre-determined. They are also natural given that changing the designation of project is a more readily observable form of corruption than over-reporting.

5.6 Is Monitoring Affected?

One final concern is that the intensity with which officials were monitored by their supervisors changed around the same time as the daily wage change. Official notifications and instructions regarding the wage change did not include any provisions regarding monitoring, and officials and the block and panchayat level do not appear to have implicit incentives to monitor linked to the amount of corruption (for example, it is not the case that a detecting official earns a reward proportional to the amount the detected official stole). Nevertheless, we would like hard evidence to reassure us that the changes in corruption we have documented are not related to changes in monitoring.

To do this we exploit data from our village-level survey on the most recent visit to each village by the Block Development Officer (BDO) and the District Collector, the two officials responsible for monitoring panchayat-level behavior. Such visits are generally infrequent: in fact, 38% of panchayats in our Orissa sample had never had a BDO visit and 76% never had a Collector visit. For those panchayats which did receive a visit since the beginning of the NREGA in 2005 we can test whether the likelihood of a visit went up after May of 2007.

Let t be the month in which a given panchayat was last visited by an official. We suppose that the probability of the panchayat receiving a visit is independent (but not identical) across months, as would be the case under optimal monitoring in many plausible scenarios. Call $p(\tau|\theta, d)$ be the probability that a panchayat in district d receives a visit at time τ. We suppose that p has the logarithmic form

$$p(t|\theta, d) = \frac{\exp\{\delta_d + \gamma 1(t \geq t^*) + f(t)\}}{1 + \exp\{\delta_d + \gamma 1(t \geq t^*) + f(t)\}} \tag{5.2}$$

Under our independence assumption the probability that the panchayat's last visit was at time t is

$$f(t|\theta, d) = p(t|\theta, d) \cdot \Pi_{\tau=t+1}^{T}(1 - p(\tau|\theta, d)) \tag{5.3}$$

Similarly, the probability that a panchayat did not receive a visit since the beginning of the NREGA is

$$\Pi_{\tau=\underline{t}}^{T}(1 - p(\tau|\theta, d)) \tag{5.4}$$

where $\underline{t}$ is the NREGA start date. We estimate this model via maximum likelihood for both BDOs and Collectors and for various specifications of p, in each case testing the null $\gamma = 0$.[22]

Table 7 reports the results. The estimate of γ is positive but small and insignificant for BDOs; for collectors it is positive and insignificant when controlling linearly for time and is actually significantly negative when controlling for a quadratic in time. We conclude

[22]In a small number of panchayats respondents could only remember the year, and not the month, of the most recent visit by an official. We allow these observations to contribute to the likelihood function by simply calculating the probability that the most recent fell in the given year. Our results are insensitive to omitting these observations.

that there is no evidence of an increase in monitoring intensity associated with the change in the daily wage.

6 Conclusion

Corrupt agents typically risk losing or being suspended from their jobs if they are caught stealing. We show that these incentives generate a "golden goose" effect: as the value of continuing in office increases due to increased opportunities for rent extraction, officials are wary of losing their positions, and attempt to reduce the probability of getting caught by stealing less now. This mechanism tends to stabilize levels of corruption in response to exogenous shocks.

We test for the golden goose effect using panel data on corruption in India's National Rural Employment Guarantee Act, exploiting an exogenous increase in program wages to construct formal tests. Officials in charge of implementing the program face opportunities for theft from the labor budget in projects which remunerate beneficiaries in different ways – either paying workers daily wages or piece rates. When the opportunities for theft from daily wage projects increase due to an exogenous increase in the statutory minimum daily wage, we are able to track officials' responses to these new opportunities. We find that theft on piece rate projects goes down in response to the wage increase. In addition, we also find reduced over-reporting of days worked on daily wage projects in areas where the proportion of future daily wage projects is higher. These findings support our central hypothesis that the potential to earn illicit future rents serves as a deterrent to current rent extraction. Rough calculations based on the point estimates imply that this effect is economically significant, reducing the increase in corruption generated by the wage change by approximately 70%.

Nevertheless, our findings suggest that raising statutory program benefits may not generate much additional benefit for NREGA participants. Insights from the literature on the the "industrial organization" of corruption, which has recently gained prominence among economists (Shleifer and Vishny 1993, Ades and Di Tella 1999, Barron and Olken 2007), could be of use here. For example, some NREGA participants receive lower wages than they are entitled to, and these wages might rise if the workers could choose among competing state-reimbursed employers. However, far more money leaks from the program in the form of reimbursements to officials for work that was never done at all. As this form of corruption harms only taxpayers, controlling it through competition may be more difficult.

Looking beyond the NREGA, golden goose effects may help explain several puzzles about corruption. For example, they explain why officials facing low chances of detection may nevertheless steal modest amounts: with lax enforcement the value of remaining in office is high. They also explain why efforts to reduce corruption may have less effect than one-shot models predict, and analogously why expanding opportunities for corruption may raise equilibrium levels of corruption less than feared. These insights can provide guidance to planners thinking about how to limit corruption. Ultimately the success of interventions like the NREGA – and from the perspective of impoverished citizens in developing nations their next meal – depends on the ability of governments to control corruption. Understanding whether officials prefer to preserve the golden goose or kill it now may help in achieving this goal.

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A Alternative Models of Bargaining

We sketch the implications of two alternative models of bargaining: one in which wages are determined before workers work, and another in which wages are determined afterwards. The thrust of the argument is simply to establish that under ex-ante bargaining wages are driven primarily by workers' outside options, while under ex-post bargaining it is the statutory wage and the workers' ability to complain about receiving less than the statutory wage that matters. The former prediction closely matches our empirical findings (not reported) while the latter does not, which we take as justification for focusing on ex-ante bargaining in the bulk of our analysis.

A.1 Ex-Ante Bargaining

Under ex-ante bargaining the worker has two potential sources of bargaining power. First, if the offer he receives is less attractive than employment in the private sector at the market wage $\underline{w}$, he can decline it. Second, if he is offered less than the statutory wage $\overline{w}$ he can complain to a higher-up official and, if the complaint succeeds, receive unemployment benefits. In practice, however, these are never paid in Orissa: state-wide we are aware of only 2 cases in which workers receiving unemployment benefits.[23] We therefore focus on the market wage as the worker's relevant outside option.

We solve the daily wage case; the piece-rate case is analogous. We suppose, as in the main text, that play evolves within each period as follows:

1. The official offers each worker i employment on the NREGA project at wage w_i^t.
2. Workers choose to work either on the NREGA project or in the private sector.
3. The official reports a number of days worked $\hat{n}^t$ to his superiors and collects compensation.

Clearly each worker i participates if $w_i^t \geq \underline{w}^t$ and otherwise does not. Suppose the official offers a wage schedule such that $\tilde{n} < N$ workers work on the project and then reports $\hat{n}$ days of work to his superiors. Let j be a worker who does not work on the project under this offer profile. Amending the offer profile so that $w_j^t = \underline{w}^t$ induces a change in the official's rents which is at least as great as

$$(\overline{w} - \underline{w}^t) + (\pi(\hat{n}, \tilde{n}) - \pi(\hat{n}+1, \tilde{n}+1))\overline{V}(\overline{w}, \phi) \tag{A.1}$$

since it is feasible for the official to increase his report to $\hat{n}+1$. This expression is positive when $\overline{w} \geq \underline{w}^t$ and $\pi(\hat{n}, \tilde{n}) \geq \pi(\hat{n}+1, \tilde{n}+1)$ as assumed. Thus the unique equilibrium is for the official to offer all workers employment at the market wage. Intuitively, adding an extra worker to the project allows the official to gain a weakly positive additional rent from under-paying him, and does not reduce the expected rents attainable by over-reporting. It is thus in everyone's best interest to employ all willing workers.

Ex-ante bargaining thus yields an equilibrium in which workers' wages are determined entirely by the market wage $\underline{w}$, and are insensitive to the statutory wage $\overline{w}$. In unreported results (available on request) we show that both of these predictions are born out in the data. NREGA wages closely track the market wage (instrumented with spatially and temporally varying features of the local agricultural economy), and do not respond at all to the change in the statutory wage.

[23] This may be because the Chief Minister of Orissa stated that unemployment benefits would be paid out of the pockets of the supervising officials.

A.2 Ex-Post Bargaining

Under ex-post bargaining the market wage $\underline{w}$ becomes irrelevant since the worker has already forfeited the opportunity to work in the market by the time bargaining takes place. His only source of bargaining power is the threat to complain if too severely underpaid. We suppose, therefore, that the worker can file a complaint at some (monetized) cost c which will succeed with probability ψ, in which case he recovers $\overline{w} - w_i^t$ and the official is fired.

As before we solve the daily wage case, without any loss of generality. Play evolves within each period as follows:

1. Workers choose to work either on the NREGA project or in the private sector.
2. The official pays each worker who participates on the project a wage w_i^t.
3. Each worker chooses whether or not to complain.
4. If he has not been fired, the official reports a number of days worked $\hat{n}^t$ to his superiors and collects compensation.

Clearly each worker i will complain if and only if $w_i^t \leq \overline{w} - \frac{c}{\psi} \equiv w^*$. The official will therefore pay each worker either w^* or 0. If n workers work and the official pays w^* to $0 \leq \tilde{n} \leq n$ workers he earns

$$(n - \tilde{n}) \cdot (\overline{w}) + \tilde{n} \cdot (\overline{w} - w^*) + (1-\psi)^{n-\tilde{n}} \cdot \max_{\hat{n}} \left[(\hat{n} - n)\overline{w} + \beta(1 - \pi(\hat{n}, n))\overline{V}(\overline{w}, \phi) \right] \quad \text{(A.2)}$$

This is convex in $\tilde{n}$, so the official will either pay all his workers or none of them. In our data officials obtain the bulk of their rents through over-reporting rather than through under-payment, which suggests that it will generally be worth the official's while to pay his workers w^*. Consistent with this intuition, the vast majority of the workers in our survey do receive a positive wage. We focus, therefore, on the case where all workers receive $w^t = w^* = \overline{w} - \frac{c}{\psi}$.

Ex-post bargaining thus yields an equilibrium in which wages are determined by the statutory wage and the effective costs of complaining, and are unresponsive to local market conditions. Both of these conditions are counterfactual, as discussed above.

B Proofs

We use throughout the fact (demonstrated above in Appendix A) that participation (n^t, q^t) and remuneration (w^t, r^t) are fixed and exogenous with respect to the parameters $(\overline{w}, \phi)$.

B.1 Proof of Proposition 1

The official's problem during daily wage periods is

$$\max_{\hat{n}} \left[(\overline{w} - w^t)n^t + (\hat{n} - n^t)\overline{w} + \beta(1 - \pi(\hat{n}, n^t))\overline{V}(\overline{w}, \phi) \right] \quad \text{(B.1)}$$

The posited attributes of π ensure that this problem has an interior solution satisfying the Kuhn-Tucker condition

$$\overline{w} = \beta \pi_{\hat{n}}(\hat{n}, n^t)\overline{V}(\overline{w}, \phi) \quad \text{(B.2)}$$

Differentiating with respect to $\overline{w}$ yields

$$\frac{\partial \hat{n}}{\partial \overline{w}} = \frac{1 - \beta \pi_{\hat{n}} \frac{\partial \overline{V}}{\partial \overline{w}}}{\beta \pi_{\hat{n}\hat{n}} \overline{V}(\overline{w}, \phi)} \tag{B.3}$$

Substitution back the first-order condition (B.2) yields

$$\frac{\partial \hat{n}}{\partial \overline{w}} = \frac{1 - \frac{\overline{w}}{\overline{V}} \frac{\partial \overline{V}}{\partial \overline{w}}}{\beta \pi_{\hat{n}\hat{n}} \overline{V}(\overline{w}, \phi)} \tag{B.4}$$

from which (and $\pi_{\hat{n}\hat{n}} > 0$) the result is apparent.

B.2 Proof of Proposition 2

The official's problem during piece rate periods is

$$\max_{\hat{q}} \left[(\overline{r} - r^t) q^t + (\hat{q} - q^t)\overline{r} + \beta(1 - \mu(\hat{q}, q^t))\overline{V}(\overline{w}, \phi) \right] \tag{B.5}$$

The posited attributes of μ ensure that this problem has an interior solution satisfying the Kuhn-Tucker condition

$$\overline{r} = \beta \mu_{\hat{q}}(\hat{q}, q^t) \overline{V}(\overline{w}, \phi) \tag{B.6}$$

Since $(\overline{r}, r^t, q^t)$ are fixed we know that $\hat{q}^t \overline{r} - q^t r^t$ moves with $\hat{q}^t$. Differentiating with respect to $\overline{w}$ yields

$$\frac{\partial \hat{q}}{\partial \overline{w}} = \frac{-\beta \mu_{\hat{q}} \frac{\partial \overline{V}}{\partial \overline{w}}}{\beta \mu_{\hat{q}\hat{q}} \overline{V}(\overline{w}, \phi)} \tag{B.7}$$

Since by assumption $\mu_{\hat{q}\hat{q}} > 0$ it is sufficient to show $\frac{\partial \overline{V}}{\partial \overline{w}} > 0$. Applying the envelope theorem, we obtain

$$\begin{aligned}
\frac{\partial \overline{V}}{\partial \overline{w}} &= \phi \frac{\partial V(\overline{w}, 1, \phi)}{\partial \overline{w}} + (1 - \phi) \frac{\partial V(\overline{w}, 1, \phi)}{\partial \overline{w}} && \text{(B.8)} \\
&= \phi \hat{n} + \beta[\phi(1 - \pi(\hat{n}, n^t)) + (1 - \phi)(1 - \mu(\hat{q}, q^t))] \frac{\partial \overline{V}}{\partial \overline{w}} && \text{(B.9)} \\
&= \frac{\phi \hat{n}}{1 - \beta[\phi(1 - \pi(\hat{n}, n^t)) + (1 - \phi)(1 - \mu(\hat{q}, q^t))]} && \text{(B.10)} \\
&> 0 && \text{(B.11)}
\end{aligned}$$

B.3 Proof of Proposition 3

First, note that by the envelope theorem

$$\frac{\partial \overline{V}}{\partial \phi} = \frac{y_o^t(1) - y_o^t(0)}{1 - \beta[\phi(1 - \pi(\hat{n}, n^t)) + (1 - \phi)(1 - \mu(\hat{q}, q^t))]} \tag{B.12}$$

and thus $\frac{\partial \overline{V}}{\partial \phi} = 0$ when $y_o^t(1) = y_o^t(0)$. Considering the first-order conditions (B.2) and (B.6), it is apparent that this further implies that $\hat{n}$ and $\hat{q}$ are invariant to ϕ.

$\hat{n}$ and $\hat{q}$ invariant to ϕ implies

$$\frac{\partial}{\partial \phi}\left[\frac{\partial \hat{n}}{\partial \overline{w}}\right] \propto -\frac{\partial}{\partial \phi}\left[\frac{1}{\overline{\overline{V}}}\frac{\partial \overline{V}}{\partial \overline{w}}\right] \tag{B.13}$$

$$\frac{\partial}{\partial \phi}\left[\frac{\partial \hat{q}}{\partial \overline{w}}\right] \propto -\frac{\partial}{\partial \phi}\left[\frac{1}{\overline{\overline{V}}}\frac{\partial \overline{V}}{\partial \overline{w}}\right] \tag{B.14}$$

We showed above that

$$\frac{\partial \overline{V}}{\partial \overline{w}} = \frac{\phi \hat{n}}{1-\beta[\phi(1-\pi(\hat{n},n^t)) + (1-\phi)(1-\mu(\hat{q},q^t))]} \tag{B.15}$$

and since $\overline{V}$ can be written

$$\overline{V} = \frac{\phi y_o(1) + (1-\phi)y_o(0)}{1-\beta[\phi(1-\pi(\hat{n},n^t)) + (1-\phi)(1-\mu(\hat{q},q^t))]} \tag{B.16}$$

we have

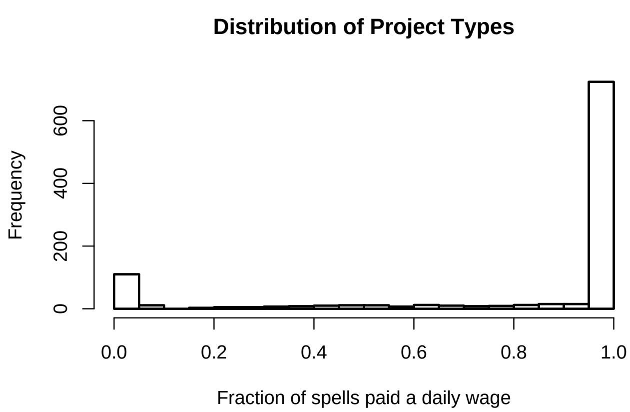
$$-\frac{\partial}{\partial \phi}\left[\frac{1}{\overline{\overline{V}}}\frac{\partial \overline{V}}{\partial \overline{w}}\right] = -\frac{\partial}{\partial \phi}\left[\frac{\phi \hat{n}}{\phi y_o(1) + (1-\phi)y_o(0)}\right] \tag{B.17}$$

Then using again the fact that $y_o(1) = y_o(0)$ and that both are invariant to ϕ (since $\hat{n}$ and $\hat{q}$ are invariant to ϕ) we have

$$-\frac{\partial}{\partial \phi}\left[\frac{1}{\overline{\overline{V}}}\frac{\partial \overline{V}}{\partial \overline{w}}\right] = -\frac{\hat{n}}{\phi y_o(1) + (1-\phi)y_o(0)} < 0 \tag{B.18}$$

As before, $(\overline{r}, r^t, q^t)$ fixed imply that $\hat{q}^t\overline{r} - q^t r^t$ moves with $\hat{q}^t$.

Figure 1: Distribution of Project Types



Plots distribution of projects in study panchayats by the fraction of spells of (reported) work done that were daily wage spells. Work spells are coded as daily wage spells if the payment per day is one of the statutory daily wages. (Orissa implements four different daily wages for varying skill levels.)

Figure 2: Survey Results

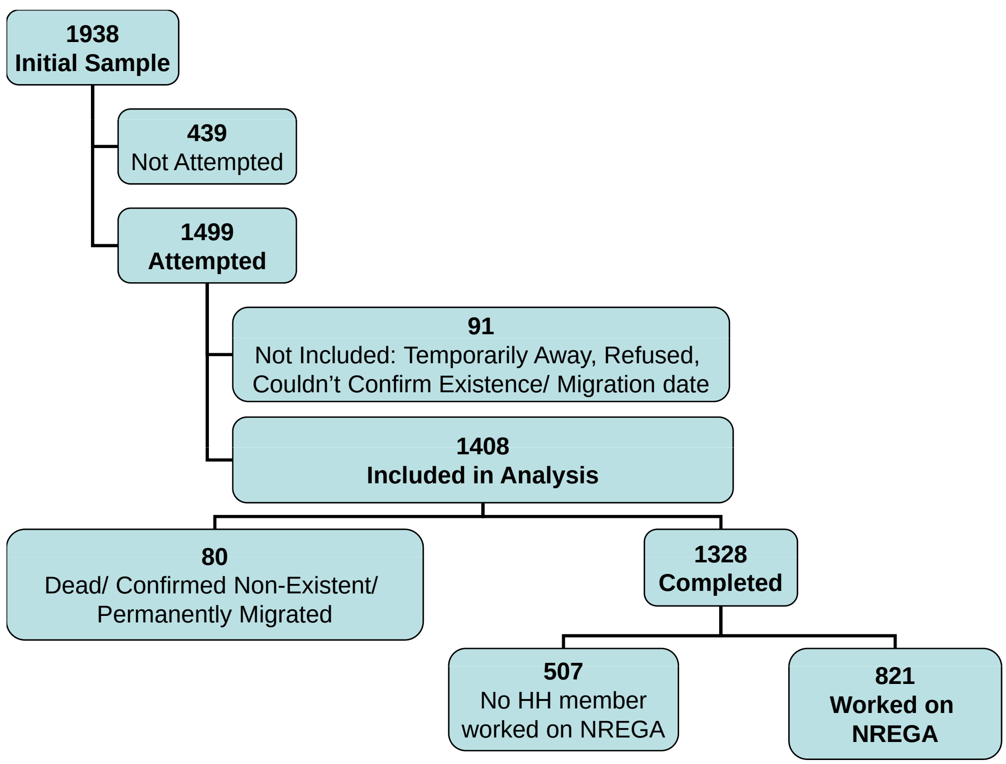
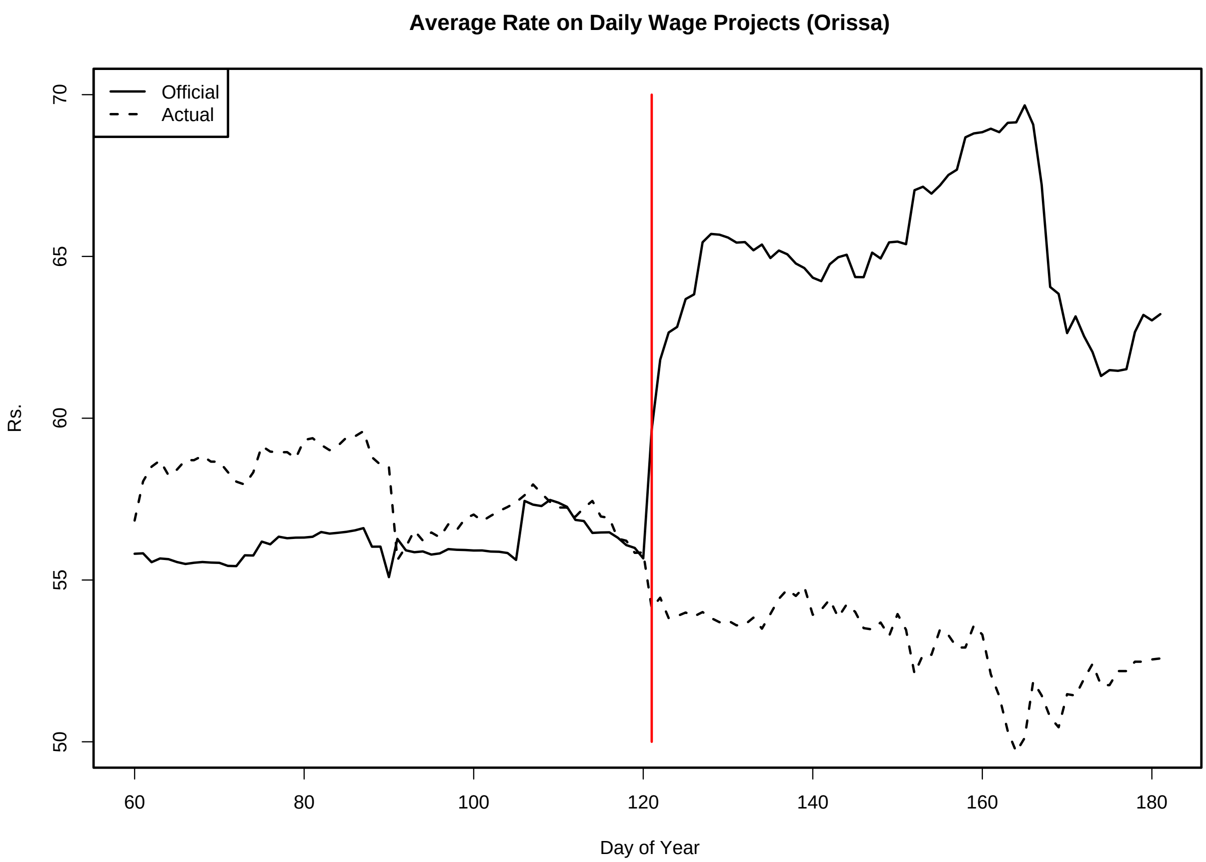
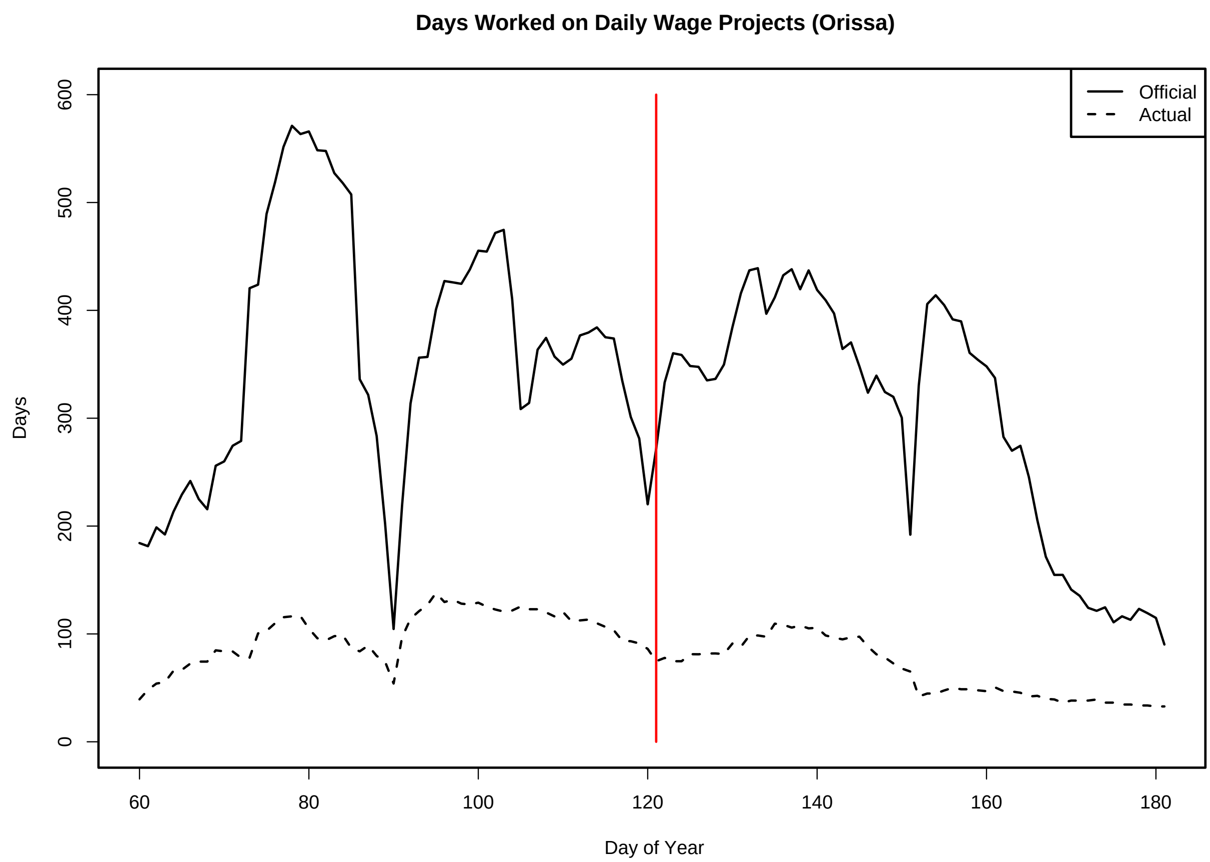


Figure 3: Daily Wage Rates Paid



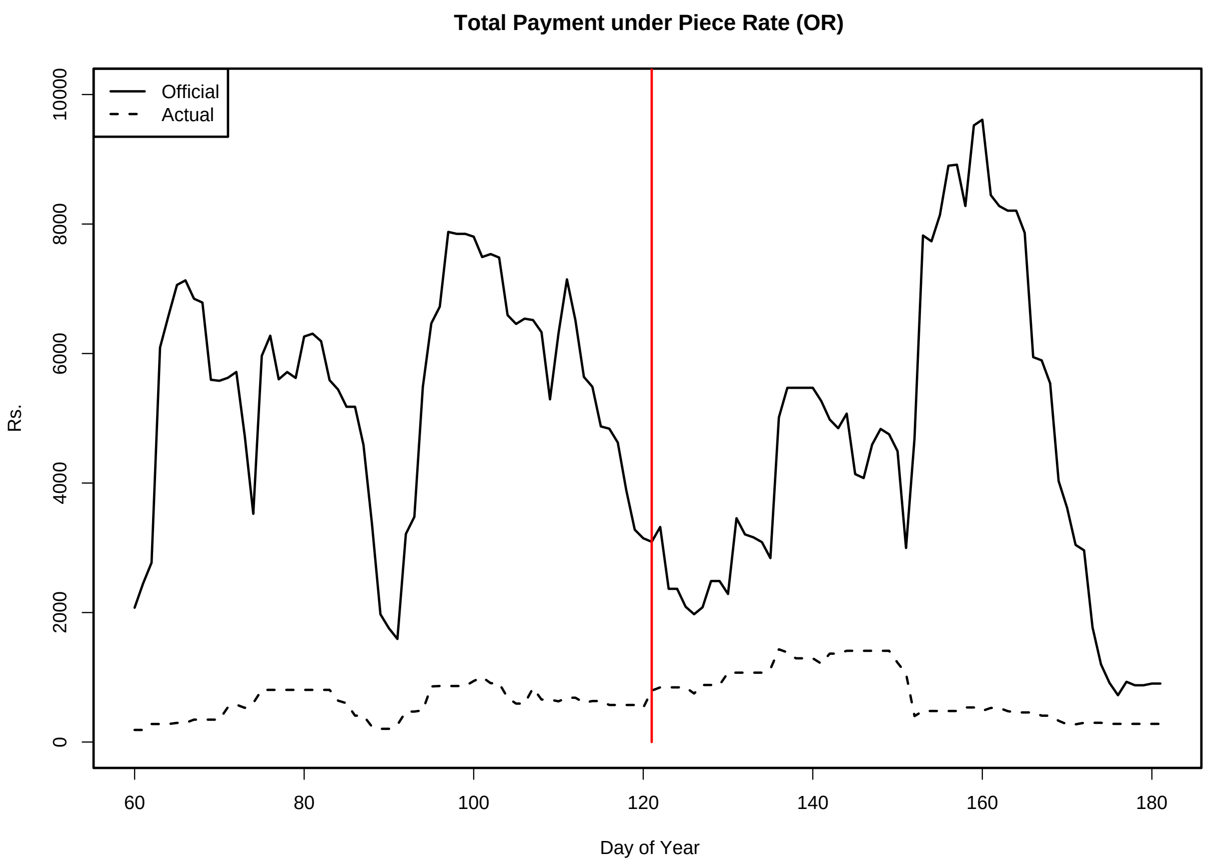
Plots a daily series of the average wage rate paid in daily wage projects in Orissa over the study period, according to official records and survey data. Day 60 corresponds to March 1st, 2007, the start of the study period; day 121 to May 1st, 2007, the date of the wage shock; and day 181 to June 30, 2007, the end of the study period.

Figure 4: Daily Wage Days Worked



Plots a daily series of the average number of days worked in daily wage projects in Orissa over the study period, according to official records and survey data. Day 60 corresponds to March 1st, 2007, the start of the study period; day 121 to May 1st, 2007, the date of the wage shock; and day 181 to June 30, 2007, the end of the study period.

Figure 5: Payments in Piece Rate Projects



Plots a daily series of the average amount of theft in piece rate projects in Orissa over the study period, by subtracting the wages paid as reported by survey respondents from officially reported wages. Day 60 corresponds to March 1st, 2007, the start of the study period; day 121 to May 1st, 2007, the date of the wage shock; and day 181 to June 30, 2007, the end of the study period.

Table 1: Sample Description

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	0.3	0.46	501	0.23	0.42
HH Head Educated Through Grade 10	819	0.04	0.19	502	0.04	0.2
Awareness						
Knows HH Keeps Job Card	812	0.84	0.37	498	0.89	0.31
Number of Amenities Aware Of	815	0.96	0.85	497	0.78	0.82
HH Head has Heard of RTI Act	803	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 describes attributes of the household survey sample that was successfully interviewed in Orissa. The sample is split between households who confirm that they worked on an NREGA project between March 1st and June 30th, 2007 – 821 households (NREGA Participants) – and those that did not – 507 households. "BPL" stands for Below the Poverty Line, a designation that entitles one to several government programs, although makes no difference for NREGA work. The definition for literacy used by the Indian government is whether one can sign her name (instead of placing a thumbprint). The amenities meant to be provided at the worksite in NREGA projects are – amongst others – water, shade, first aid, and a creche/child care. We ask respondents to name amenities without prompting. "RTI" stands for the Right to Information, a broad freedom of information act passed by the Indian government in 2005.

Table 2: Wage Shock Effects on Real Labor Supply

Regressor	DW I	DW II	DW III	PR I	PR II	PR III
Shock	-0.143	-0.134	-0.19	0.071	0.07	0.06
	(0.222)	(0.223)	(0.214)	(0.085)	(0.085)	(0.081)
Day	0	-0.001	0.047	0	0	0.013
	(0.004)	(0.004)	(0.011)***	(0.001)	(0.001)	(0.005)**
Day^2			-0.024			-0.006
			(0.005)***			(0.003)**
Season 1	0.414	0.405	0.161	0.095	0.075	0.03
	(0.23)*	(0.221)*	(0.229)	(0.088)	(0.082)	(0.078)
Season 2	0.1	0.258	0.159	-0.004	0.058	0.012
	(0.249)	(0.235)	(0.248)	(0.092)	(0.108)	(0.095)
District FEs	N	Y	N	N	Y	N
N	12810	12810	12810	7320	7320	7320
R^2	0.023	0.061	0.041	0.005	0.026	0.016

This table presents regressions of actual days of work done in daily wage (columns 1-3) and piece rate projects (columns 4-6) in a given panchayat-day. The variable Day represents a linear time trend. The variable Day^2 (Day^3) has been rescaled by the mean of Day (the mean of Day^2). Season 1 refers to main agricultural season in the panchayat, and Season 2 to the secondary agricultural season. Robust standard errors – multi-way clustered by panchayat and day – are presented in parenthesis. All columns include the following standard controls: non-parametric controls for number of days of wage work actually done, a third-order polynomial in the day of the month, and an indicator for major holidays. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3: Wage Shock Effects on Daily Wage Over-reporting

Regressor	I	II	III	IV	V	VI
Shock	1.18	1.185	1.141	1.87	1.76	1.805
	(0.798)	(0.799)	(0.8)	(0.983)*	(0.984)*	(0.995)*
Shock * FwdWageFrac				-3.236	-3.208	-3.082
				(1.413)**	(1.36)**	(1.423)**
Shock * BkWageFrac				2.638	2.829	2.532
				(1.616)	(1.581)*	(1.609)
FwdWageFrac				3.385	3.442	3.325
				(1.209)***	(1.174)***	(1.211)***
BkWageFrac				-0.477	-0.606	-0.406
				(1.195)	(1.242)	(1.209)
Day	-0.03	-0.028	0.022	-0.034	-0.034	0.029
	(0.013)**	(0.013)**	(0.048)	(0.016)**	(0.015)**	(0.052)
Day^2			-0.026			-0.033
			(0.023)			(0.025)
Holiday	-0.577	-0.602	-0.667	-0.664	-0.695	-0.759
	(0.278)**	(0.27)**	(0.252)***	(0.33)**	(0.322)**	(0.305)**
District FEs	N	Y	N	N	Y	N
Real Labor, Seasons	Y	Y	Y	Y	Y	Y
N	12810	12810	12810	10651	10651	10651
R^2	0.09	0.097	0.098	0.12	0.134	0.121

The dependent variable in this table is the number of days worked by panchayat-day on daily wage projects as reported officially. "Shock" is an indicator equal to 1 on and after May 1, 2007. "FwdWageFrac" is the proportion of daily wage project-days in the panchayat in the future. "BkWageFrac" is the proportion of daily wage project-days in the panchayat in the past. The variable Day represents a linear time trend. The variable Day^2 (Day^3) has been rescaled by the mean of Day (the mean of Day^2). All columns include the following standard controls: non-parametric controls for number of days of wage work actually done, a third-order polynomial in the day of the month, and an indicator for major holidays, the actual number of days worked on daily wage projects, an indicators for major agricultural seasons. Robust standard errors – multi-way clustered by panchayat and day – are presented in parenthesis. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4: Wage Shock Effects on Piece Rate Reports

Regressor	I	II	III	IV	V	VI
Shock	-73.877	-74.651	-74.108	-88.39	-92.553	-87.442
	(39.662)*	(39.86)*	(39.867)*	(59.419)	(60.216)	(59.822)
Shock * FwdWageFrac				-44.251	-41.217	-46.593
				(95.667)	(96.018)	(98.075)
Shock * BkWageFrac				72.098	78.151	73.813
				(63.808)	(69.431)	(65.329)
FwdWageFrac				-88.086	-86.423	-86.962
				(60.23)	(63.532)	(60.491)
BkWageFrac				-57.038	-55.327	-58.731
				(59.524)	(62.365)	(59.553)
Day	0.525	0.596	0.826	0.559	0.596	-0.35
	(0.722)	(0.736)	(2.026)	(0.844)	(0.833)	(2.377)
Day^2			-0.156			0.473
			(1.058)			(1.284)
Holiday	-5.898	-6.484	-6.304	-9.033	-9.834	-7.783
	(11.87)	(11.932)	(11.461)	(13.034)	(13.246)	(13.461)
District FEs	N	Y	N	N	Y	N
Real Labor, Seasons	Y	Y	Y	Y	Y	Y
N	7320	7320	7320	6312	6312	6312
R^2	0.046	0.05	0.046	0.078	0.085	0.078

The dependent variable in this table is the total amount paid by panchayat-day on piece-rate projects as reported officially. “Shock” is an indicator equal to 1 on and after May 1, 2007. “FwdWageFrac” is the proportion of daily wage project-days in the panchayat in the future. “BkWageFrac” is the proportion of daily wage project-days in the panchayat in the past. The variable Day represents a linear time trend. The variable Day^2 (Day^3) has been rescaled by the mean of Day (the mean of Day^2). All columns include the following standard controls: non-parametric controls for number of days of wage work actually done, a third-order polynomial in the day of the month, and an indicator for major holidays, the actual number of days worked on piece rate projects, an indicators for major agricultural seasons. Robust standard errors – multi-way clustered by panchayat and day – are presented in parenthesis. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 5: Effects on Piece Rate Reports using Andhra Pradesh as a Control

Regressor	I	II	III	IV
OR Shock * Orissa	-151.265	-151.379	-150.583	-117.684
	(44.319)***	(44.174)***	(44.417)***	(47.393)**
AP Shock 1 * AP	-5.408	-6.168	-17.924	-108.348
	(37.874)	(37.888)	(34.015)	(63.028)*
AP Shock 2 * AP	184.959	185.027	184.74	156.921
	(53.762)***	(53.646)***	(53.668)***	(52.177)***
OR Shock	64.789	65.517	82.4	48.748
	(44.5)	(44.31)	(43.368)*	(45.764)
AP Shock 1	32.894	34.226	-15.737	87.993
	(27.944)	(27.924)	(29.101)	(39.116)**
AP Shock 2	-42.856	-42.603	-76.578	-27.796
	(26.445)	(26.289)	(24.871)***	(24.084)
Day	1.493	1.463	6.7	
	(0.591)**	(0.592)**	(2.253)***	
Day^2			-3.863	
			(1.7)**	
Day * OR				0.968
				(0.589)
Day * AP				1.971
				(0.741)***
FEs	State	District	State	State
Real Labor, Seasons	Y	Y	Y	Y
N	17446	17446	17446	17446
R^2	0.114	0.114	0.116	0.114

The dependent variable in this table is the total amount paid by panchayat-day on piece-rate projects as reported officially; these regressions now include data from both Orissa (OR) and Andhra Pradesh (AP). "OR Shock" is an indicator equal to 1 on and after May 1, 2007. "AP Shock 1" is an indicator equal to 1 on and after March 5, 2007, while "AP Shock 2" equals 1 on or after April 25, 2007. "FwdWageFrac" is the proportion of daily wage project-days in the panchayat in the future. "BkWageFrac" is the proportion of daily wage project-days in the panchayat in the past. The variable Day represents a linear time trend. The variable Day^2 (Day^3) has been rescaled by the mean of Day (the mean of Day^2). All columns include the following standard controls: non-parametric controls for number of days of wage work actually done, a third-order polynomial in the day of the month, and an indicator for major holidays, the actual number of days worked on piece rate projects, an indicators for major agricultural seasons. Robust standard errors – multi-way clustered by panchayat and day – are presented in parenthesis. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 6: Wage Shock Effects on Project Composition

Regressor	I	II	III
Shock	0.021	0.019	0.021
	(0.023)	(0.023)	(0.023)
Day	0	0.001	-0.002
	(0.001)	(0.001)	(0.003)
Day^2			0.001
			(0.001)
District FEs	N	Y	N
Real Labor, Seasons	Y	Y	Y
N	12103	12103	12103
R^2	0.016	0.024	0.017

This table presents regressions of the "FwdWageFrac" indicator, or the proportion of future project-days in a panchayat that are daily wage. "Shock" is an indicator equal to 1 on and after May 1, 2007. The variable Day represents a linear time trend. The variable Day^2 (Day^3) has been rescaled by the mean of Day (the mean of Day^2). All columns include the following standard controls: non-parametric controls for number of days of wage work actually done, a third-order polynomial in the day of the month, and an indicator for major holidays, the actual number of days worked on piece rate projects, an indicators for major agricultural seasons. Robust standard errors – multi-way clustered by panchayat and day – are presented in parenthesis. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 7: ML Estimates of Changing Audit Probabilities Over Time

Regressor	BDO	BDO	Collector	Collector
Shock	0.049	0.07	0.105	-1.597
	(0.304)	(0.322)	(0.482)	(0.753)**
Koraput	-3.007	-2.996	-4.769	-4.854
	(0.179)***	(0.187)***	(0.276)***	(0.274)***
Gajapati	-4.771	-4.761	-5.742	-5.83
	(0.242)***	(0.246)***	(0.39)***	(0.389)***
Rayagada	-3.872	-3.862	-5.425	-5.51
	(0.168)***	(0.174)***	(0.284)***	(0.283)***
t	0.082	0.082	0.048	0.147
	(0.017)***	(0.018)***	(0.024)*	(0.038)***
t^2		0		0.007
		(0.001)		(0.002)***

This table presents maximum likelihood estimates of the probability of a visit by government officials – Block Development Officers (BDO) and District Collectors – to the panchayat. "Shock" is an indicator equal to 1 on and after May 1, 2007. "t" and "t^2" are time trends. Koraput, Rayagada, and Gajapati are indicators for the three study districts in Orissa. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$