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Abstract

Theoretical work on disciplining corrupt agents has emphasized the role of promised future rents (e.g. efficiency wages) but not of illicit future rents. Yet when opportunities for future rent extraction increase, agents should extract less rent today in order to preserve those opportunities. We study this "golden goose" effect in the context of a statutory wage increase in India's employment guarantee scheme, comparing official micro-records to original household survey data to measure corruption. We estimate large golden goose effects that reduced the total impact of the wage increase on theft by roughly 64%.

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

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1 Introduction

Disciplining corrupt officials is a key governance challenge in developing countries. In an influential early analysis Becker and Stigler (1974) argued that, if there is some chance of detecting and dismissing corrupt agents, then the problem can be mitigated by promising them an efficiency wage. Intuitively, agents have an incentive to cheat less today in order to improve their chances of earning a wage premium or a pension tomorrow. Subsequent research has maintained this emphasis on contracts that promise future rents.[1][2]

In contrast, the rents agents expect to gain from future *corrupt* activities have not played a prominent role, primarily because such rents do not appear in one-shot models of corruption. This paper takes a more dynamic approach, modelling the tradeoff that corrupt agents face between extracting rents today and improving their chances of surviving to extract rents tomorrow. We call this the "golden goose" effect: agents wish to preserve the goose that lays the golden eggs (not kill it, as did the deplorably myopic farmer in the fable).[3] We show theoretically how incorporating the tradeoff between current and future rents can dampen or reverse standard comparative statics because of a generic tendency for static and dynamic effects to offset each other.

To assess the empirical relevance of golden goose effects we use data from India's largest rural welfare program, the National Rural Employment Guarantee Scheme (NREGS). The scheme entitles every rural household in India to up to 100 days of paid employment per year, provided only that they are willing to do manual labor. We obtained disaggregated official records on participation, including the names and addresses of participating households, the duration of each spell of employment and the amount of compensation paid. We then independently surveyed a sample of these (alleged) beneficiaries in order to compare the amounts of work actually done and payments actually received to the official records. The gap between official and actual payments – which includes both over-reporting of days and under-payment of wages – is the primary form of corruption we study.[4]

Testing for golden goose effects requires an exogenous source of variation in anticipated rent-extraction opportunities. We exploit a policy shock: a 1 May 2007 increase in the

[1] See Cadot (1987), Andvig and Moene (1990), Besley and McLaren (1993), Mookherjee and Png (1995), and Acemoglu and Verdier (2000), among others. Becker and Stigler's (1974) model is a multi-period one but they examined a contract that entirely eliminates illicit rents. As we discuss below, the literature on electoral discipline is an important exception.

[2] A distinct literature following Holmstrom (1999) models the career concerns of a worker who wishes to influence future perceptions of his ability. Although the worker in this environment is forward-looking, shifts in the attractiveness of future employment do not affect incentives for effort today because they do not affect the relationship between effort today and perceived ability.

[3] Our usage thus differs from that of McMillan (2001), who uses the term "golden goose" to refer to ex-ante investments by individuals that a government cannot commit not to hold up ex-post. Commitment will not be an issue in our setting.

[4] On the importance of measuring corruption directly, rather than using perceptions, see Olken (2009).

statutory wage due to program participants in the state of Orissa. A higher statutory wage meant more lucrative corruption opportunities for officials, since they received more money for every fictitious day of work reported. Importantly, the wage reform was enacted by policy-makers well removed from the officials we study, making it plausibly exogenous. Because the wage increase was specific to the state of Orissa we can also use data from the neighboring state of Andhra Pradesh as a control in some specifications.

The theoretical framework in Section 3 shows that the effects of a wage change include both a static price effect and a dynamic golden goose effect. The price effect is straightforward: when officials receive more money for every day of wage work they report, they have stronger incentives to over-report participation. If the wage increase were temporary this would be the only effect. Following a permanent change, however, officials also anticipate a more lucrative future, and this dynamic effect tends to make them more conservative. The net effect on daily wage over-reporting is thus ambiguous.

To separate out golden goose effects we exploit an additional institutional feature of the NREGS: roughly 30% of the projects in our sample operated on a piece rate basis, rather than a daily wage one. Different projects used different payment schemes because piece rates could not be implemented on projects where worker output is hard to measure. Crucially, the list of projects to be implemented had been fixed in advance of the 1 May 2007 wage change and piece rate schedules were not revised along with the daily wage, so this reform should not have directly affected piece rate projects. However, many officials who were managing piece rate projects at the time of the wage change also had daily wage projects planned for the near future and should thus have anticipated an increase in future rents. This effect should also have been stronger in proportion to the share of upcoming projects that were daily wage. The model thus predicts that the wage increase should (1) reduce theft from piece rate projects, and (2) differentially reduce corruption in villages with more daily wage projects upcoming.

We take these predictions to panel data on corruption before and after the policy shock in 215 panchayats (villages). The data suggest that prices do matter: when statutory daily wages increase, officials report more fictitious work on wage projects. Overall, the daily wage increase from Rs. 55 to Rs. 70 (combined with secular trends) increased the cost to the government per dollar received by beneficiaries from \$4.08 to \$5.03. We also find evidence broadly consistent with golden goose effects. First, theft on piece rate projects in Orissa declined after the shock, both in absolute terms and relative to neighboring Andhra Pradesh. Second, both daily-wage over-reporting and piece rate theft fell differentially (the former significantly) in villages which subsequently executed a higher share of daily wage projects. While some of the point estimates are imprecise, so that magnitudes should be interpreted cautiously, they imply large golden goose effects. Rough calculations suggest that the wage increase raised theft by approximately 64% less than it would have had it been temporary.

To separate our interpretation from other substitution mechanisms we test for *time-symmetry*. Intuitively, most substitution mechanisms imply that the effects of future rent expectations should be similar to the effects of past and current rent realizations. For example, if the marginal value of rents is decreasing so that officials become "satiated" then both past and future windfalls should decrease current rent extraction. Empirically we find a consistent negative relationship with future rent-extraction opportunities, but an inconsistent relationship with past rent-extraction opportunities. We also analyze data on visits by superior officials to directly rule out changes in monitoring intensity as a confound.

While our theoretical framework is tailored primarily to our empirical setting, we also use it to highlight some broader implications of the golden goose effect. We examine, for example, the impact of increasing the probability that an agent will be audited. While the direct effect of this change is to make the behavior subject to audit less attractive, this in turn implies that expected future rents decrease, lowering the continuation value to the agent of keeping her job and making other forms of illicit behavior more attractive. Dynamic considerations may thus provide an alternative explanation for displacement effects such as those documented by Yang (2008).

Golden goose effects also inform the interpretation of policy pilots, since temporary pilots and permanent implementation generate different dynamic incentives. For example, distributing welfare benefits once does not generate dynamic disincentives for theft, but distributing them repeatedly does. A pilot may therefore appear to perform artificially poorly. Conversely, a one-shot crackdown on corruption does not affect future rent expectations and may thus be more effective than a program of perpetual audits. Understanding dynamics is thus important for interpreting the literature on monitoring (Di Tella and Schargrodsky 2003, Nagin, Rebitzer, Sanders and Taylor 2002, Olken 2007) and of transparency more generally (Reinikka and Svensson 2005, Ferraz and Finan 2008).

Our empirical results contribute to several other strands of research. They indirectly support the efficiency wage hypothesis, which also hinges on the role of future rent expectations but has proven difficult to test directly – Di Tella and Schargrodsky (2003) being the notable exception. They are consistent with theories of electoral discipline in which voters must allow politicians some future rents in order to maintain control over them (Barro 1973, Ferejohn 1986, Persson, Roland and Tabellini 1997, Ahlin 2005, Ferraz and Finan 2009), given that some NREGS officials are locally elected. Finally, they add to the mounting evidence on the costs of corruption, which include constraints on redistribution (Reinikka and Svensson 2004, Olken 2006), the creation of new market distortions (Sequeira and Djankov 2010) and the inability to remedy existing ones (Bertrand, Djankov, Hanna and Mullainathan 2007).

The rest of the paper is structured as follows: Section 2 describes the NREGS context, Section 3 lays out the theoretical framework, Section 4 describes data collection and estimation equations, Section 5 presents results, and Section 6 concludes.

2 Contextual Background on the NREGS

India's National Rural Employment Guarantee Scheme is a landmark effort to redistribute income to the rural poor. 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 2010-2011 fiscal year is Rs. 401 billion (US$ 8.9 billion), which is 0.73% of 2008 GDP.[5] It is likely that the steady-state cost will be higher as implementation is still incomplete in many parts of the country. The following discussion describes the program as it was implemented during our study period; some of the procedures described may have changed, as has the official name (now called the "Mahatma Gandhi National Rural Employment Guarantee Act").

2.1 Statutory Operational Procedures

Each operational program cycle begins before the start of a fiscal year, when local governments at the Gram Panchayat (GP or panchayat, lowest level of administration in the Indian government, comprising of a group of villages) and block (intermediate level of government between GPs and districts) levels plan a "shelf" of projects to be undertaken during the upcoming year. The particular types of project allowed under the NREGS 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: NREGS workers can be paid either on a daily wage or a piece rate basis depending on the practicality of measuring output. Our conversations with low-level officials in Orissa indicated that the decision about how to pay workers is generally made on a project-by-project basis and by officials at the block level. Empirically it is the case that all the work done on any particular project is generally compensated in the same manner (see Figure 1). Consequently there are identifiable daily wage projects and piece rate projects. While according to statute the project shelf should be proposed by village assemblies (Gram Sabhas), in practice higher up officials at the Block and District level suggest and approve the shelf.

A key feature of the NREGS is that it is an unrestricted entitlement program: every household in rural India has a right to 100 days of paid employment per year, with no

[5] Costs: `http://indiabudget.nic.in/ub2010-11/bh/bh1.pdf`. GDP: `http://mospi.nic.in/4_gdpind_cur.pdf`. The central government must by law contribute at most 90% of total expenditure, the rest of the funding coming from the states.

eligibility requirements.[6] To obtain work on a project, interested households must first apply for a *jobcard*.[7] The jobcard contains a list of household members, some basic demographic information, and blank spaces 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. The applicant must be assigned to a project within 15 days after submitting the application, if not they are eligible for unemployment compensation. Applicants have no influence over the choice of project.

At the work sites the panchayat officials record attendance (in the case of daily wage projects) or measure output (in the piece rate case). They record this information both in workers' jobcards and in muster rolls which are sent to Block offices and digitized. The state and central governments reimburse local governments on the basis of these electronic records. Most workers in our study area received their wages in cash from the panchayat administration, although efforts to pay them through banks are under way. As a transparency measure, all the official micro-data on payments have been made publicly available through a web portal maintained by the central Ministry of Rural Development (`http://nrega.nic.in`).

2.2 Implementing Officials

The officials in charge of implementing the program are mainly appointed bureaucrats at the block (Block Development Officers, Junior Engineers, Assistant Engineers) and panchayat (Panchayat Secretary, Field Assistants, Mates, etc) levels, with the exception of the elected chairman of the Gram Panchayat (the "Sarpanch"). The work of these officials is overseen by district level program officials, including the District Collector. While officials can be fired, suspended, or removed from their jobs for misconduct, Article 311(2) of the Indian constitution says that no civil servant can be dismissed without an official enquiry, which makes it difficult to fire someone outright. Suspensions and transfers into backwater jobs, however, are common punishments (Das 2001).

Because our analysis revolves around forward-looking optimization it is useful to understand bureaucratic tenure in these jobs. Tenure is typically short, primarily because transfers are used as a disciplinary tool and as a way for political parties to bestow favors. Iyer and Mani (2009) document that the district-level Indian Administrative Service (IAS) officers who oversee local officials stay in a job for a year and a half on average, and since they often move with their staff this implies that the tenure of lower-level officials is

[6] Officials thus do not have an opportunity cost of allocating work to workers, as in Banerjee (1997).

[7] Since each household is limited to 100 days of employment per year the definition of a household is important. In NREGS 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)

at least as short. In Gujarat, Block Development Officers keep that post for an average of sixteen months (Zwart (1994), p 94). Given the small but significant pay differential between private sector and public sector jobs at this level (Das 2001) and the short tenure, local public officials often seek opportunities for extracting rents.

2.3 Rent Extraction, Monitoring and Enforcement

Officials' opportunities for illicit gain include control over project selection; bribes for obtaining jobcards and/or employment; and embezzlement from the materials and labor budgets. We focus on theft from the labor budget, which we can cleanly measure. The labor budget 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 be the larger source of illegal rents.[8]

Theft from the labor budget comes in two conceptually distinct forms. First, officials can under-pay workers for the work they have done (theft from beneficiaries). Second, officials can over-report the amount of work done when they send their reports up the hierarchy (theft from taxpayers).[9]

A key difference between theft from beneficiaries and theft from taxpayers lies in the way they are monitored. Underpaid workers who know they are underpaid could either complain to someone at the block or district headquarters or simply leave for the private sector. (We examine these mechanisms in more detail in Niehaus and Sukhtankar (2010).) On the other hand, workers have little incentive to monitor over-reporting: because the program's budget is not fixed, a rupee stolen through over-reporting does not mean a rupee less for the workers. Realistically, then, over-reporting must be monitored from the top down. The NREGS Operational Guidelines (Ministry of Rural Development 2008) call for both top-down monitoring, via internal verification of works by officials (100% works audited at the block level, 10% by district level monitors, and 2% by state level monitors), and bottom-up monitoring via Gram Sabhas (village meetings), local Vigilance and Monitoring Committees, as well as bi-annual "social audits" done by civil society. In practice we saw that block and district officials use the NREGS's management information system (MIS) to track aggregate quantities of work done on various projects and compare

[8]We also found that bribes paid to obtain jobcards are uncommon (17% report paying positive amounts) and small (averaging Rs. 10 conditional on being positive). This is not surprising 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 NREGS places no limit on the number of participants, so officials actually have positive incentives to sign up participants.

[9]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. 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.

these to technical estimates or to their own intuitions about how much work should be necessary.

Officials caught cheating face a low but positive probability of getting punished. Program guidelines call for "speedy action against [corrupt] officials" but do not lay out specific penalties. In practice the most likely penalty is suspension or transferal to a less desirable job; for elected officials it is loss of office.[10] The strength of enforcement in Orissa is difficult to quantify; the Chief Minister at one point claimed to have initiated action against nearly half the Block Development Officers in the state, but some of this is likely political posturing.[11] A more reliable source may be the records of OREGS-Watch, a loose online coalition of non-governmental organizations that monitor NREGS in Orissa; their reports note numerous instances of officials being caught and suspended (`http://groups.google.co.in/group/oregs-watch`). The common pattern in these cases was incontrovertible proof brought to the office of the District Collector, followed immediately by the suspension of the guilty official and in some cases by the recovery of the stolen funds. In one case in Boudh district, for example, the offending official was caught within two weeks of the misdemeanor, the money recovered and the official suspended.[12] Andhra Pradesh has systemized the process of social audits, creating a quasi-government "Society" for Social Audits (`http://www.socialauditap.com`) that conducts door-to-door verification of muster rolls, which has succeeded in recovering over Rs. 130 million in stolen funds.

2.4 Wage-Setting

Our estimation strategy below exploits an increase in statutory program wages in the eastern state of Orissa in 2007. Such wage hikes were common due to the incentives generated by the NREGS's funding pattern. The central (federal) government pays 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 states set wages and piece-rates. This provision – possibly intended to allow flexibility to adapt program parameters to local labor market conditions – creates strong incentives for state politicians to raise wage rates, benefiting their constituents at the central government's expense. We study the effects of a change in the statutory daily wage in Orissa from Rs. 55 to Rs. 70. This change was announced on April 28th, 2007 and went into effect on May 1st, 2007. Two key features of this policy change are that it did not directly affect payments

[10]The theoretical predictions of our model will not depend qualitatively on whether the punishment is suspension, transfer, or permanent dismissal. Similarly, some degree of collusion between local officials and their monitors would not change the qualitative predictions.

[11]`http://www.orissadiary.com/Shownews.asp?id=6201`

[12]`http://www.dailypioneer.com/59458/Action-taken-after-study-finds-fake-muster-roll-in-Boudh.html`.

on piece rate projects and that it was specific to Orissa and did not affect neighboring Andhra Pradesh.

3 Dynamic Rent Extraction

Following the seminal work of Becker and Stigler (1974), a large theoretical literature has studied the use of dismissal threats to motivate corruptible agents. Much of this literature, however, studies one-shot corruption games. In this section we develop a fully dynamic model of rent-extraction in order to draw out the role that illicit future rents play in shaping the agent's decision-making. 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. We will show how combining standard theoretical elements with these margins yields testable predictions about the effects of a statutory wage change.

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 NREGS 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 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. We treat ϕ as exogenous here since de jure it should be predetermined for our study period, but we will also check in our empirical work that it does not respond to the wage change.

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 NREGS 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 $\overline{r}$).

NREGS wages and employment levels emerge from bargaining between the official and the workers. As we discuss in a companion paper (Niehaus and Sukhtankar 2010), participants NREGS wages (w_i^t) and their participation choices (n^t) appear to be determined by the prevailing market wage rate $\underline{w}^t$ in the village and *not* by the statutory NREGS rate $\overline{w}$. Thus while in principle labor supply n^t depends on the official's wage offers $\{w_i^t\}$ we ignore this dependence since $w_i^t = \underline{w}^t$ for all (i, t). We further simplify matters by abstracting from time variation in the market wage, so $\underline{w}^t = \underline{w}$ and $n^t = n$.

Participation n and the average participant's wage w (piece rate r) are thus 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

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

and analogously if it is a piece-rate project,

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

The official can report up to $\overline{n} > n$ work-days, where $\overline{n}$ is the number of registered workers in his village. Over-reporting puts the official at risk of being detected by a superior and removed from office. The probability of detection on daily wage projects is $\pi(\hat{n}, n)$. We assume that $\pi(n, n) = 0$ for any n so that there is no penalty for honesty, while $\pi_1 > 0$ and $\pi_2 < 0$ so that the probability of detection increases as the gap between $\hat{n}$ and n widens. We also assume that π is such that the official's problem has an interior optimum. Finally, we 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 $\mu(\hat{q}^t, q)$ for $q \leq \hat{q} \leq \overline{q}$ and has analogous properties. If an official is caught he is removed from office before the beginning of the next period and earns a continuation payoff normalized to zero. In practice corrupt officials are sometimes suspended rather than fired; modeling this would affect our results only quantitatively.[13][14]

The recursive formulation of the official's objective function is

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

where $V(\overline{w}, 1)$ 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.

We first derive the official's response to a *temporary*, one-period change in the statutory daily wage. These are not testable predictions, since the wage change we study below was a permanent one. Rather, because they coincide with the predictions a static one-period model would deliver, they help highlight the consequences of modeling dynamics.

Proposition 1. *A one-period increase in the statutory daily wage $\overline{w}$ increases over-reporting on daily wage projects ($\hat{n}^t - n$) and has no effect on theft from piece rate projects ($\hat{q}^t \overline{r} - qr$).*

These results are immediate (and hence the proof is omitted) because the official's continuation value $\overline{V}(\overline{w}, \phi)$ is unaffected by a temporary wage change. Given this, the wage change acts like a pure price shock for officials managing daily wage projects: the value of over-reporting a day of work goes up, while the cost is unaffected. Consequently over-reporting increases. As for officials managing a piece-rate project, neither the costs nor the benefits of stealing have changed.

When the statutory wage changes permanently this generates additional dynamic effects working through changes in the official's continuation value $\overline{V}(\overline{w}, \phi)$. This can potentially reverse the model's predictions for daily wage over-reporting depending on the elasticity of future rents with respect to $\overline{w}$:

Proposition 2. *Over-reporting $\hat{n}^t - n$ on daily wage projects is increasing in $\overline{w}$ if $\frac{\overline{w}}{\overline{V}} \frac{\partial \overline{V}}{\partial \overline{w}} < 1$ and decreasing otherwise.*

[13] Officials may also leave their posting for more benign reasons – a bureaucrat may be reassigned or a politician's term may expire. Modeling this possibility would yield additional predictions: a bureaucrat near the end of his term may have weaker incentives to avoid detection, as suggested by Olson (2000). Campante, Chor and Do (2009) provide a complementary analysis of the effects of exogenous changes in the probability of job retention. Unfortunately our data do not include variation in tenure, and so for simplicity we omit it from the model as well.

[14] We model π as independent of the daily wage and other program parameters. In our context, incentives for monitoring are poorly defined and in particular not linked to other program parameters. In Section 5.5 we directly test for effects of $\overline{w}$ on monitoring and do not find any evidence of a relationship. Of course, monitoring intensity may respond to incentives for corruption in important ways in other settings.

Proof. Proofs are deferred to Appendix A. □

This comparative static is ambiguous because the positive price effect is at least partially offset by a negative golden goose effect: a higher wage raises the value of future over-reporting, which in turn increases the importance of keeping one's job. The former effect dominates only if the elasticity of future benefits with respect to the wage is sufficiently small.[15]

While it illustrates the tension between static and dynamic effects, Proposition 2 does not yield a refutable prediction. One way to obtain a test is to examine the effects of a permanent wage change on forms of rent extraction that are not directly affected, such as theft from piece-rate projects. A higher statutory wage has no effect on current rent-extraction opportunities for a bureaucrat managing a piece-rate project, but does increase expected future rent extraction opportunities, discouraging theft:

Proposition 3. *Total theft from piece-rate projects* $(\hat{q}^t\overline{r} - qr)$ *is decreasing in* $\overline{w}$.

Of course, one could also imagine mechanisms through which different kinds of corruption complement each other. For example, successful embezzlement might require fixed investments such as paying a superior officer to look the other way; in this case, an increase in the returns to one form of corruption might lead to an increase in other forms as well. Ultimately it is an empirical question whether alternative forms of corruption are substitutes or complements.

We can construct an additional test by exploiting cross-sectional variation in the intensity with which the wage change affects official's future rent expectations. Since the wage change only affects rents in future periods during which a wage project is running, one might expect to see differentially stronger effects in places with more future wage projects upcoming (higher ϕ). As it turns out things are not quite this simple: if piece rate and daily wage projects are not equally lucrative then there may be additional sources of treatment heterogeneity working through these "wealth effects". If the rents from piece rate and daily wage projects are approximately the same, however, we get the prediction one intuitively expects:

Proposition 4. *Restrict attention to any closed, bounded set of parameters* $(\phi, \overline{w}, \overline{r}, \underline{w}, \underline{r})$. *Then for* $|y_o(1) - y_o(0)|$ *sufficiently small,*

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

In our empirical work we will first verify that equilibrium rents from daily wage and piece rate projects are similar, and then test this prediction.

[15] In fact, one can go further and construct examples (available on request) in which the *total* amount stolen per period decreases.

Propositions 2 - 4 characterize corruption's response to a daily-wage change. While these will be the relevant comparative statics for our empirical work, it is also worth understanding what the model has to say about changes in other, less context-specific parameters. In particular, the probability of being audited is a key parameter in most models of corruption. To illustrate the effects of auditing intensity in a dynamic setting, let $\pi(\hat{n}, n) = \gamma\tilde{\pi}(\hat{n}, n)$ where γ measures the probability a daily-wage project is audited and $\tilde{\pi}$ the probability of conviction conditional on an audit.

Proposition 5. *A one-period increase in the audit probability γ decreases over-reporting on daily wage projects ($\hat{n}^t - n$) and has no effect on theft from piece rate projects ($\hat{q}^t\overline{r} - qr$). A permanent increase in γ decreases over-reporting on daily wage projects if and only if $\frac{\gamma}{\overline{V}}\frac{\partial\overline{V}}{\partial\gamma} > -1$ and if so the effect is strictly smaller than the effect of a one-period increase; a permanent increase in γ also increases theft from piece rate projects.*

(The derivation exactly parallels earlier proofs and hence is omitted.) Notably, increasing the probability of an audit will always lower corruption on the *audited* activity by less than a static model would predict, and will increase corruption on other non-audited activities. This illustrates the generic nature of the tension between static and dynamic effects. It also implies that the right interpretation of empirical evidence on auditing depends on whether the audits were perceived to be temporary or permanent.

3.1 Confounding Explanations

Some of our framework's testable implications could also be generated by alternative substitution mechanisms. 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.[16]

[16]Past theft might also matter if officials were worried about being forced to repay old debts; in this case officials who had enjoyed better rent-extraction opportunities in the past should be more conservative in the present.

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 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. We will exploit this distinction below, showing that the differential effects of the wage change are time-asymmetric.

4 Empirical Approach

4.1 Official Data

To test the theoretical predictions in Section 3 we adopt an audit approach, comparing official micro-data on wage payments and program participation to original household survey data collected from the same (alleged) beneficiaries. The official data we use are publicly available on a central website (`http://nrega.nic.in`). Data available at the level of the individual worker include names, ages, addresses, caste status, and unique household jobcard number. Data available at the level of the work spell include number of days worked, name and identification number of the project worked on, and amount paid. Descriptive information on the nature of the projects and the names of the officials responsible for implementation are also available. It is straight-forward to infer whether a project paid daily wages or piece rates because there are only a few allowed daily wage rates.[17] (Figure 1)

An important point regarding the official records is that the 100-day-per-household constraint essentially never binds. During fiscal year 2006-2007 only 4% of jobcards in our study area in Orissa are recorded as having reached 100 days, and all panchayats had a significant number of jobcards with less than 100 days – on average 95% of the cards in the panchayat, and at a minimum 22%.

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 study period to allow time for all the relevant data to be uploaded. As a cross-check we also downloaded the official records a second time in March 2008. We found that the records for Orissa remained

[17] 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. The higher wages are paid for slightly higher-skilled work; these are very rare occurrences, and the overwhelming majority of wages reported paid are Rs. 55 and Rs. 70.

essentially unchanged, but that the number of work spells recorded for Andhra Pradesh had increased by roughly 10%. 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, generate some uncertainty about the appropriateness of our AP sample frame, and so we will emphasize the Orissa data and use AP as a control only in Table 6.

We sampled from the list of officially recorded NREGS 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 border with AP. We sampled 60% of the Gram Panchayats within study blocks, stratified by whether the position of GP chief executive had been reserved for women. (Chattopadhyay and Duflo (2004) find evidence suggesting that reservations affect levels of corruption.) Within these panchayats we sampled 2.8 percent of work spells, stratified by Panchayat, by whether the project was implemented by the block or the panchayat administration, by whether the project was a daily wage or piece-rate project, and by whether the work spell was before or after the daily wage shock. This yielded a total of 1938 households. We set out to interview all adult members of these households about their NREGS participation, so that our measures of corruption would not be affected if work done by one member was mistakenly reported as having been done by another. Details on survey results and a sample description are in Appendix B.

4.2 Survey Content

We asked respondents retroactively about spells of work they did between March 1, 2007 and June 30, 2007. A spell of work is a well-defined concept within the NREGS: it is an uninterrupted period of up to two weeks employment on a single project. For each spell we asked subjects the dates during which they worked, the number of days worked, what project they worked on, whether they were paid on a piece rate or daily wage basis, what payment they received, and in the case of piece rate projects what quantity of work they did. While recall of most of these variables is good, recipients have difficulty recalling the quantity of work done on piece rate projects – the amount of earth they moved, volume of rocks they split, etc. Consequently in our empirical work we treat theft on piece rate projects as unitary ($\hat{q}^t\overline{r} - q^t r^t$ in terms of the model). In addition to the survey of program participants, we also administered a separate questionnaire to village elders with questions on labor market conditions, agricultural seasons and official visits in the village.

While imperfect recall could potentially be a concern given the lag between the study period and our survey, results were encouraging. We obtained information on at least the month in which work was done for 93% of the spells in our sample. We do not find significant differential recall problems over time: in a variety of specifications including location fixed effects and individual controls such as age and education, subjects' estimated probability of recalling exact dates increases by only 0.7%–2.2% per month and is not statistically significant. Since our main tests exploit discrete time-series changes while controlling for smooth trends, these patterns should not introduce bias. Subjects' recall was facilitated by the fact that the NREGS was a new and salient program, and spells of work were likely to be memorable and distinct compared to other employment. Subjects are also more likely to keep track of their participation and compensation given that they do not necessarily get paid what they are owed or on time. Finally, we designed the survey instrument and trained enumerators to jog respondents' memories: for example, using major holidays as reference points.

Survey interviews were framed to minimize other potential threats to the accuracy and veracity of respondents self-reports. We made clear that we were conducting academic research and did not work for the government, to discourage them from claiming fictitious underpayment; in the end most respondents reported that they had been paid what they thought they were owed. None of the interviewed households have income close to the taxable level and will have ever paid income taxes, so there are no tax motives for underreporting. Conversely, officials had little need to secure workers' collusion in their over-reporting. A worker could only supply a signature, which has little relevance when most people cannot write their own name. 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 issues would lead to differential biases (which would affect our results) and not just level ones (which would not). Niehaus and Sukhtankar (2010) confirms that the wage shock had no effect on the self-reported variables we use in our analysis.

4.3 Empirical Specifications

Our empirical analysis includes all spells of work from our survey data 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, 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 fictitious* over the sample period.

[18]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.

Our first empirical strategy is to regress officially reported outcomes $\hat{y}_{pt}$ for panchayat p and day t on actual outcomes y_{pt} as reported by participants, an indicator $Shock_t$ for the post 1 May period, and a number of time-varying controls summarized by T_t including a polynomial in day-of-year to capture long-term trends, a polynomial in day-of-month to capture periodicity, and an indicator for major holidays. In some specifications we allow time trends to vary before and after the shock. Finally, we include indicators for political reservations R_p and in some specifications district fixed effects $\delta_{d(p)}$ to capture variation in program implementation across locations:[19]

$$\hat{y}_{pt} = \beta_0 + \beta_1 y_{pt} + \beta_2 Shock_t + T_t^{'}\gamma + R_p^{\prime}\zeta + \delta_{d(p)} + \epsilon_{pt} \tag{4.1}$$

Note that if $\hat{y}_{pt}$ were correlated one-for-one with y_{pt} then this approach would be equivalent to using $\hat{y}_{pt} - y_{pt}$ as the dependent variable, while if not our approach is less restrictive. We have also implemented the more restrictive approach, however, and the results are if anything stronger (see Table 7 and the discussion in Section 5.4). Identification in (4.1) rests on the assumption that unobserved factors affecting $\hat{y}_{pt}$ are orthogonal to $Shock_t$ after controlling for the other regressors.

To relax this assumption we also exploit data from the neighboring district of Vizianagaram in Andhra Pradesh to control for unobserved time-varying effects common to the geographic region under study. There are, however, several caveats. First, we can only implement this strategy when studying 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 after we selected our sample. 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 for high-frequency confounds, although it may still be useful for low-frequency ones. Keeping these limitations in mind, we estimate

$$\begin{aligned}\hat{y}_{pt} = \beta_0 + \beta_1 y_{pt} + \beta_2 ORshock_t * OR_p + \beta_2 APShock1_t * AP_p + \beta_3 APShock2_t * AP_p \\ + \beta_4 ORshock_t + \beta_5 APShock1_t + \beta_6 APShock2_t + OR_p \\ + T_t^{'}\gamma + R_p^{\prime}\zeta + \delta_{d(p)} + \epsilon_{pt}\end{aligned} \tag{4.2}$$

where OR_p (AP_p) indicates panchayats in Orissa (Andhra Pradesh). The coefficient of interest in this specification is β_2, the differential change in corruption in the post-shock period in Orissa.

To test for the differential effects of the wage change predicted by Proposition 4 we

[19] Key political positions in some villages are reserved by law for women and/or ethnic minorities.

need an empirical analogue to ϕ, the probability that a future project in our model is a daily wage project. Given that many of the panchayats in our data only implement wage projects, one simple way to do this is to partition the set of panchayats into those that do and do not ever run piece-rate projects and estimate:

$$\hat{y}_{pt} = \beta_0 + \beta_1 y_{pt} + \beta_2 \text{Shock}_t + \beta_3 Shock_t * AlwaysDW_{pt} + \beta_4 AlwaysDW_{pt} + T_t^{'}\gamma + R_p^{\prime}\zeta + \delta_{d(p)} + \epsilon_{pt} \quad (4.3)$$

for daily-wage outcomes. Our model predicts $\beta_2 > 0$ while $\beta_3 < 0$. We can also apply a similar idea to piece-rate outcomes, replacing $AlwaysDW$ with $AlwaysPR$.

Of course this approach, while transparent, does not completely isolate the differential response attributable to *future* daily-wage projects. To do this we must define, for every panchayat and every day, the proportion of upcoming work that is daily-wage. We accomplish this by (1) defining a "project-day" as a day on which a particular project is running, where a project is running if work on that project as been reported in the past and will be reported in the future, and then (2) calculating for each panchayat-day observation the fraction $FwdWageFrac$ of project-days in the upcoming two months that are daily wage project-days.[20] Figure 4 plots the distribution of $FwdWageFrac$ in our sample. Given the existence of clear mass points at 0 and 1 we adopt a flexible approach, binning the data into three categories: one where $FwdWageFrac = 0$ (the omitted category), one where $0 < FwdWageFrac < 1$ ($FdwSome$), and one where $FwdWageFrac = 1$ ($FdwAll$).[21] We then allow the effects of the wage change to vary across these categories:

$$\hat{y}_{pt} = \beta_0 + \beta_1 y_{pt} + \beta_2 \text{Shock}_t + \beta_3 Shock_t * FdwAll_{pt} + \beta_4 FdwAll_{pt} + \beta_5 Shock_t * FdwSome_{pt} + \beta_6 FdwSome_{pt} + T_t^{'}\gamma + R_p^{\prime}\zeta + \delta_{d(p)} + \epsilon_{pt} \quad (4.4)$$

Note that a key goal in constructing these forward-looking measures is to capture variation in the *proportion* of daily wage projects on the panchayat's "shelf" of projects without also including endogenous variation in the *amount* of work reported. This is the reason that we focus on whether projects are ongoing, rather than the number of person-days of work purportedly done. We show below that the $FwdWageFrac$ variable is indeed uncorrelated with the wage shock. It is also important to note that if it *were* endogenously related to the wage change we would expect the resulting bias to work against us rather than for us: panchayats that increased their corruption most in response to the shock

[20] A two-month window seems reasonable relative to our study period and to the typical tenure of appointed officials (12-16 months). We also obtain qualitatively similar results using one- and three-month windows (Table 7).

[21] We have also estimated more restrictive models in which $FwdWageFrac$ enters linearly and obtained qualitatively similar results (available on request).

would be the most likely to switch to wage projects, generating a positive bias on the interaction term.

The main outstanding concern about the specification above is that it does not rule out time-symmetric substitution mechanisms such as income effects. To examine whether *past* opportunities for corruption matter in the same way as *future* opportunities, we construct bins based on an analogous measure $BkWageFrac$ of the fraction of project-days in the *preceeding* two months that were daily wage and estimate:[22]

$$\begin{aligned}\hat{y}_{pt} = \beta_0 + \beta_1 y_{pt} + \beta_2 \text{Shock}_t + \beta_3 Shock_t * FdwAll_{pt} + \beta_4 Shock_t * BdwAll_{pt} \\ + \beta_4 Shock_t * FdwSome_{pt} + \beta_5 Shock_t * BdwSome_{pt} \\ + \beta_6 FdwAll_{pt} + \beta_7 BdwAll_{pt} + \beta_6 FdwSome_{pt} + \beta_7 BdwSome_{pt} \\ + T_t^{'}\gamma + R_p^{\prime}\zeta + \delta_{d(p)} + \epsilon_{pt} \quad (4.5)\end{aligned}$$

Our model predicts $\beta_3 < 0$ with no prediction about β_4, while if time-symmetric mechanisms are important then we should see $\beta_3 \simeq \beta_4 < 0$.

Table 2 presents summary statistics of the main variables used in our regressions.

5 Results: The Golden Goose Effect

5.1 Preliminaries: Wages, Quantities and Rents

We begin with a series of preliminary tests of the main identifying assumptions. First we verify that the policy change was actually implemented; Figure 2 shows this. The average rate officially reported as being paid on daily wage projects stays fairly constant near Rs. 55 up until May 1st and then jumps up sharply thereafter. Interestingly it does not immediately or permanently reach the new statutory wage of Rs. 70, because not all panchayats implemented the change – some continued to claim the old rates after May 1st, presumably because they were not informed about the change.[23] Figure 2 also shows that the wage rate actually received by workers was unaffected by the shock; it appears to trend slightly downwards, but this effect is largely compositional and vanishes once we control for district fixed effects.[24]

[22]Note that the correlation between $FwdWageFrac$ and $BkWageFrac$ is 0.75 within district, 0.6 within blocks, and 0.11 within panchayats; the results must be interpreted with these high correlations in mind.

[23]This interpretation suggests an additional test: all our predictions should hold only in panchayats that actually implemented the wage change. We pursued this strategy, but unfortunately there are insufficiently many non-implementing panchayats for us to precisely estimate the difference.

[24]It is intriguing that during the first month of our study period the average wage received by workers actually exceeded the average wage claimed by officials. The discrepancy is driven by a large number of observations from Gajapati district where prevailing market wages are relatively high. Anecdotally, officials in these areas overpay workers to execute projects so that they can then over-report the amount of work

Second, we check whether project shelf composition responds endogenously to the wage shock. In principal it is fixed at the start of the fiscal year (March 2007), but if officials had scope to reclassify or re-order projects they might have prioritized wage projects. In fact the fraction of projects that are daily wage *fell* from 74% before 1 May to 72% afterwards. More formally, Table 3 reports regressions of $FwdWageFrac$ on an indicator for the shock along with time controls. The point estimates are insignificant and correspond to a 0.02 standard deviation change in project 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 relatively observable form of cheating.[25]

In unreported results we also examined whether project shelf composition is correlated with key political variables like reservations for women and minorities at the sarpanch and samiti representative level; with the number of locally active NGOs; with village elders' perceptions of the relative wealth and relative political activism of the village; and with indicators for visits from block and district officials. In general we found no significant correlations; the one exception we uncovered was the corelation with the share of the population belonging to scheduled castes, and since very few scheduled castes live in our study area this explains very little variation in the shelf. We have also included these characteristics directly as controls in our regressions and they do not change our findings (available on request).

Finally, we check whether pre-shock rent extraction from daily wage and piece rate projects are similar, as predicated by Proposition 4. Dividing 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. 236 per actual day worked in daily wage projects as opposed to Rs. 221 in piece rate projects.[26]

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. Panels (a) and (b) of Figure 3 show the evolution of over-reporting over time – i.e. the difference between the number of days of work reported by officials and by households. Note that the sharp downward spikes generally occur on major holidays, suggesting that officials perceive over-reporting on holidays as particularly risky. The

done by an even greater proportion. See Niehaus and Sukhtankar (2010) for more detail.

[25]Note that we also ran estimations that used the pre-shock project shelf as an instrument for the post-shock project shelf; results are broadly consistent with the OLS results, although the first stage f-stats in the piece-rate regressions are weak and hence we do not report these results here (available on request).

[26]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.

superimposed fitted models summarize an exploratory regression-discontinuity analysis: we fit polynomials in day-of-year to the aggregate time series and allowed the coefficients to vary before and after the wage change took effect on 1 May. The fitted models suggest that there was a slight increase in daily-wage over-reporting following the shock, though this pattern is not evident to the naked eye and not always significant. This may seem surprising given the obvious effect of the wage hike on incentives for over-reporting, but as Proposition 2 suggests there may also be a countervailing dynamic effect.

Columns I-III in Panel A of Table 4 present a disaggregated analysis based on Equation 4.1. Column I presents estimates of the basic specification (Equation 4.1) with a linear time trend and no location effects; Column II adds district fixed effects, while Column III adds a linear trend interacted with the shock term. Consistently across these specifications we find that official reports are significantly higher when more actual work was done and, *conditional* on actual work done, significantly lower on major holidays (not reported). The estimated impact of the wage shock, on the other hand, is positive but not significant in each specification. To examine whether this is due to an offsetting dynamic effect, Columns IV-VI of Panel A separate panchayats that ran solely daily-wage projects from those that also ran piece rate projects (Equation 4.3). We find a differential reduction in over-reporting in the daily-wage only panchayats, significant at the 10% level; indeed, summing the point estimates implies a small *reduction* in over-reporting in these locations. In contrast, the estimated effect of the wage change in panchayats that ran at least some piece rate projects is larger and significant in Column IV. This suggests the presence of a substitution effect that is muting the overall impact of the wage change.

To further isolate the portion of this differential effect that is attributable to having future daily-wage projects, Columns I-III of Panel B report estimates of the interaction between the wage shock and categories of our constructed $FwdWageFrac$ measure (Equation 4.4). The estimated direct effect of the wage hike increases again and is significant at the 5% level; the interpretation is that this is the price effect that would obtain in a panchayat with *no* future daily wage projects planned. The differential effect in panchayats with solely wage projects upcoming is negative and significant at the 10% level, while the differential effect in panchayats with a mix of upcoming projects is negative but insignificant.

To better understand what drives these patterns of substitution, Columns IV-VI of Panel B present specifications that allow for both the future and the past to predict responsiveness to the shock (Equation 4.5). The direct effect of the shock remains positive and is significant. The differential change in corruption in panchayats with only daily-wage projects upcoming is negative, larger, and highly significant, confirming a strong substitution pattern. The analogous differential change for panchayats that had only run daily-wage projects in the past is positive and insignificant, which is inconsistent with time-symmetric interpretations of our forward-looking estimates. We do estimate a significant negative differential effect in panchayats that had implemented a mix of projects in the past, however. In contrast to the forward-looking results, this result is not robust to replacing categories of the $FwdWageFrac$ variable with the variable itself in our empirical model (not reported). This, and the fact that we do not find differential drops in panchayats with only wage projects in the past, lead us to treat it with some caution.

In the specifications discussed thus far we have included linear trends and allowed these to differ before and after the shock. We have also examined the sensitivity of the results to allowing for squared trend controls; an analogous set of tables in Appendix C reports these estimates (results for even higher-order trend controls available on request). Higher-order polynomials have little effect on any of our estimates.

5.3 Theft in Piece Rate Projects

We turn next to theft from piece-rate projects. This margin of corruption provides an attractive test for golden goose effects because it was not directly affected by the wage change, so that only dynamic effects should apply (Proposition 3). Panels (c) and (d) of Figure 3 show the evolution of the gap between official and actual payments on piece-rate projects over the sample period, again with fitted regression-discontinuity specifications superimposed. Theft was unusually low in May following the wage shock; indeed, officially reported payments fell while actual payments rose. The fitted models reflect this, consistently estimating a significant discrete drop on 1 May. Note also that theft rebounded in June; while various factors could be at play, this is also broadly consistent with a dynamic model since NREGS projects largely cease operation during the monsoons starting in late June in Orissa. This implies that future rent expectations were falling steadily through May and June.

Turning to a disaggregated analysis, Table 5 mirrors Table 4 but with the total reported payments on piece rate projects as the dependent variable and total actual payments on piece-rate projects as a predictor. In Column I of Panel A the main effect of the wage shock is negative and significant at the 5% level; the magnitude of the coefficient – about Rs. 78 per day – is also economically meaningful compared to the average theft per panchayat-day observation prior to the shock was Rs. 102. Columns II-III show that while the coefficient does not change much, standard errors are slightly larger and the result is hence significant at the 10% level. Columns IV-VI again separate those panchayats that ran only piece rate projects from those that ran both types of projects; as expected the coefficient on the interaction terms is positive, though insignificant. The estimated change in panchayats with both kinds of projects is larger and more precisely estimated. Note that the sum of the coefficient on the shock and the interaction term is not statistically significantly different from zero, suggesting that the shock itself had no effect on panchayats that only ran piece rate projects.

As before, Panel B adds interactions between the shock and the forward and backward fraction of daily wage projects. As with daily wage over-reporting we find a negative differential effect of the shock in panchayats with all projects in the future being daily wage, and a positive coefficient on the interaction between the shock and past high daily wage fractions. None of these estimates are statistically significant, however. In general our power to estimate piece rate effects is limited by the relative scarcity of piece-rate projects in Orissa. (For example, even the indicator for holidays, which is consistently statistically significant in daily wage models, is imprecisely estimated in piece-rate models.) Overall the estimated differential effects provide only suggestive evidence.

To obtain a more powerful test and address concerns about time-varying confounds we next use Andhra Pradesh as a control. Table 6 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. Subject to the caveats described above, these estimates support the golden goose hypothesis. Table C.3 reports similar estimates controlling for higher-order polynomials in day-of-year.

5.4 Robustness Checks

For our preferred estimators we use the fraction of daily wage project-days in the upcoming two months as the key interaction variable. A two-month window is sensible on several grounds. First, longer forecasts of project shelf composition would not likely be relevant given that (a) the tenure of bureaucrats in the relevant postings is quite short (approximately a year), and (b) very little NREGS activity takes place once the monsoon season starts in earnest. Second, as per program guidelines official reports are aggregated bi-weekly, so that it is plausible for an official to be detected and punished within a two-month window. As discussed above, when punishment does arrive it can arrive swiftly.

Nevertheless, the choice of an exact time window over which to calculate shelf composition is inherently somewhat arbitrary. Columns I and II (VI and VII) of Table 7 examine the sensitivity of the daily wage (piece rate) results to using one-month and three-month windows. Results using a one-month window are similar and if anything stronger than our baseline estimates. Results using a three-month window are somewhat smaller and not statistically significant but are qualitatively similar, as one would expect if the three-month window absorbs large periods of very little NREGS activity during the monsoon season.

Another potential concern is that the wage shock did have differential effects but that these were driven by other variables correlated with project shelf composition. The leading concern in this context would be a relationship with the reservation of key political posts for women or disadvantaged minorities. We checked earlier that shelf composition was not significantly correlated with reservations, and these are also included as controls in all our specifications. We can further include interactions between reservation categories and the wage change directly as controls in our regressions. Columns III and VIII include indicators for each type of reservation (women, Scheduled Castes, and Scheduled Tribes) and their interactions with the wage shock. This makes the daily wage results even stronger: both the positive main effect and the negative differential effect are significant at the 5% level. The piece rate results, on the other hand, are largely unchanged.[27]

A third issue has to do with the exact timing of the effects we are attributing to the May 1st policy change. Equation 4.4 implicitly assumes that the dynamic effects of the wage change take effect at the same point in time as the static ones. If, however, officials learned about the wage change before it took place then dynamic effects might begin earlier than the direct, static ones. The 1 May wage change we study was the culmination of a process that began on 10 January with the publication of a proposal to change wages, and it is possible that officials acquired information over time about whether or not the proposal would be implemented. To explore whether our causal interpretation of the coefficients on the post-May indicator is correct we re-ran our main specifications using more flexible functions of time. Columns IV and IX of Table 7 report results using indicators for each month (we ran similar specifications using bi-weekly dummies and reached similar conclusions). In general the estimates are imprecise. There is some evidence – significant for piece rate theft – that the differential effect of $FwdWageFrac$ (though not the direct effect of the shock) begins earlier in April. This is consistent with the view that at least some officials learned about the wage change before it took place and began adjusting accordingly.

Finally, we examine the effects of using the difference $\hat{y}_{pt} - y_{pt}$ between official and actual quantities as the dependent variable. Recall that this is equivalent to our approach if the true relationship between those quantities is linear with slope 1, but otherwise is more restrictive. Nevertheless, imposing that restriction makes little difference for the results (Columns V and X).

5.5 Is Monitoring Affected?

Another potential concern is that the intensity with which officials were monitored by their supervisors changed around the same time as the daily wage change. If panchayats

[27] The estimated main effect switches from an insignificant negative effect to an insignificant positive one. Note, however, that this is the estimate for panchayats without any reservations, which make up only 3% of our sample.

with more wage projects upcoming experienced the largest increases in scrutiny this could explain the role of $FwdWageFrac$ in predicting responses to the wage shock. Of course, if this were true then again one would expect $BkWageFrac$ to play a similar role. Moreover, there is no a priori reason to expect monitoring intensity to 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 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, one would like direct evidence on this point.

To test for changes in monitoring we use 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 NREGS implementation at the panchayat level. In our Orissa sample, 62% of panchayats had a BDO visit and 24% had a Collector visit since the beginning of the NREGS in 2005. For these panchayats, 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 with symmetric information. Call $p(\tau|\theta, d)$ be the probability that a panchayat in district d receives a visit at time τ. Assume that p has the logit form

$$p(t|\theta, d) = \frac{\exp\{\delta_d + \gamma 1(t \geq t^*) + f(t)\}}{1 + \exp\{\delta_d + \gamma 1(t \geq t^*) + f(t)\}} \tag{5.1}$$

If we had data on all official visits then we could estimate $p(\cdot|\theta, d)$ directly. Because we only observe the date of the most recent visit, we focus instead on the probability that the panchayat's last visit was at time t:

$$f(t|\theta, d) = p(t|\theta, d) \cdot \Pi_{\tau=t+1}^{T}(1 - p(\tau|\theta, d)) \tag{5.2}$$

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

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

where $\underline{t}$ is the NREGS 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 $\gamma = 0$. Table 8 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 significantly negative when controlling for a quadratic in time. We conclude that there is no evidence of an increase in monitoring intensity associated with the change in the daily wage.[28]

5.6 Interpreting Magnitudes

The coefficients above give some sense of the economic importance of golden goose effects. A more systematic way to describe effect sizes is to compare the estimated increase in theft due to the shock to a counterfactual estimate of the increase that would have been generated by a *temporary* wage hike, which would not generate any golden goose effects.

We estimate the actual increase in theft attributable to the shock as the sum of three components. First, there is a mechanical component equal to the predicted quantity of daily wage over-reporting *absent* the shock multiplied by the change in the average daily wage. Second, there is a behavioral response in daily-wage over-reporting that varies depending on panchayat shelf composition; we estimate this using the coefficients from Column II, Panel B of Table 4. Third, there is a negative behavioral response in piece-rate theft, which we estimate using the coefficient in Column II, Panel A of Table 5 (a conservative assumption given that the difference-in-difference estimates of the latter effect are larger). We sum these effects to obtain an estimate Δ_{actual} of the total effect of the shock on rent extraction. To construct a counterfactual estimate of the increase $\Delta_{counter}$ resulting from a temporary wage hike 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. Our estimates imply that the dampening effect $\frac{\Delta_{counter}-\Delta_{actual}}{\Delta_{counter}}$ was approximately 64%, or in other words that the increase in the daily wage raised theft by 64% 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 infeasible without rich data on *all* the sources of rent which a corrupt official would lose if suspended or fired, and the value of their outside options, which we lack. We can, however, provide some sense of whether NREGS rents are an important enough source of income for our effects to be plausible. We can estimate total NREGS rents per panchayat (or block) per month by calculating the difference between actual and reported payments in our sample and then scaling up by the inverse of the sampling percentage. Since we do not observe outside options we can only compare these rents to official compensation rates. Even so, the contrasts are stark. The estimated rate of rent extraction per panchayat is roughly 150 times the rate at which sarpanchs are compensated, and the rate per block is 1,100 times the rate at which Block Development Officers are compensated. Evidently the NREGS dominates official compensation as a source of income.

[28] 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 visit fell in the given year. Our results are insensitive to omitting these observations.

Last, we note that large golden goose effects are likely precisely where rent extraction is high due to weak monitoring. When the chance of detection is low, future rents not only become large but become very sensitive to changes in the likelihood of detection. Intuitively, weak monitoring lengthens the effective time horizon, increasing the sensitivity of rents to a daily wage change and thus magnifying golden goose effects.[29]

6 Conclusion

Dismissal, suspension, and transfer are standard tools for disciplining corrupt agents. We show that these incentives generate a "golden goose" effect: as steady-state opportunities to extract rent increase the value of continuing in office increases and this induces agents to act more cautiously. This dynamic mechanism tends to dampen, and may reverse, the predictions of static models.

We test for golden goose effects using panel data on corruption in India's National Rural Employment Guarantee Scheme, exploiting an exogenous increase in program wages to construct tests. We find two forms of evidence consistent with our theory: higher daily wages lead to lower theft from piece rate projects, and differentially lower theft in areas with a higher proportion of daily wage projects upcoming. Rough calculations based on the point estimates imply that these effects reduced the increase in corruption generated by the wage change by approximately 64%.

[29]As an 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

$$\frac{\partial \overline{V}}{\partial \overline{w}} = \frac{\hat{n} - n}{\pi(\hat{n}, n)} \tag{5.4}$$

becomes very large as the probability of detection π falls.

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A Proofs

A.1 Proof of Proposition 2

The official's problem during daily wage periods is

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

The posited attributes of π ensure that this problem has an interior solution satisfying $\overline{w} = \beta\pi_{\hat{n}}(\hat{n}, n^t)\overline{V}(\overline{w}, \phi)$. Differentiating with respect to $\overline{w}$ yields

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

Substitution in the first-order condition yields

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

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

A.2 Proof of Proposition 3

The official's problem during piece rate periods is

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

The posited attributes of μ ensure that this problem has an interior solution satisfying the Kuhn-Tucker condition $\overline{r} = \beta\mu_{\hat{q}}(\hat{q}, q^t)\overline{V}(\overline{w}, \phi)$. 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

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

Since $\mu_{\hat{q}\hat{q}} > 0$ it is sufficient to show $\frac{\partial \overline{V}}{\partial \overline{w}} > 0$. By the envelope theorem

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

A.3 Proof of Proposition 4

Let $\theta = (\phi, \overline{w}, \overline{r})$ represent the full set of parameters, and Θ the parameter space, which is closed and bounded by assumption. After some algebra,

$$\frac{\partial}{\partial \phi}\left[\frac{\partial \hat{n}}{\partial \overline{w}}\right] = A(\theta) + B(\theta)z(\theta)$$

with

$$\begin{aligned}
A(\theta) &= \frac{-\overline{w}\hat{n}}{(\beta\pi_{\hat{n}\hat{n}}\overline{V})(\phi y_o(1) + (1-\phi)y_o(0))} \\
B(\theta) &= \frac{\overline{w}\phi\hat{n}}{(\beta\pi_{\hat{n}\hat{n}}\overline{V})(\phi y_o(1) + (1-\phi)y_o(0))^2} + \frac{(1 - \frac{\overline{w}}{\overline{V}}\frac{\partial \overline{V}}{\partial \overline{w}})(\beta\pi_{\hat{n}\hat{n}\hat{n}}\frac{\pi_{\hat{n}}}{\overline{V}\pi_{\hat{n}\hat{n}}} + \beta\pi_{\hat{n}\hat{n}})}{(\beta\pi_{\hat{n}\hat{n}}\overline{V})^2(1 - \beta[\phi(1-\pi(\hat{n}, n^t)) + (1-\phi)(1-\mu(\hat{q}, q^t))])} \\
z(\theta) &= y_o(1) - y_o(0)
\end{aligned}$$

All these functions are assumed smoothly continuous. Fix $\epsilon > 0$, define $\Theta(\epsilon) \equiv \{\theta \in \Theta : |z(\theta)| < \epsilon\}$, and

$$U(\epsilon) \equiv \sup_{\theta \in \Theta(\epsilon)} A(\theta) + \sup_{\theta \in \Theta(\epsilon)} B(\theta) \cdot \epsilon$$

Then $|z(\theta)| < \epsilon$ implies $\frac{\partial}{\partial \phi}\left[\frac{\partial \hat{n}}{\partial \overline{w}}\right] \leq U(\epsilon)$. Since Θ is closed and bounded and $A(\theta) < 0$ for any fixed, finite θ we must have $\sup_{\theta \in \Theta} A(\theta) < 0$, and so $\lim_{\epsilon \to 0} \sup_{\theta \in \Theta(\epsilon)} A(\theta) < 0$. Meanwhile since $\Theta(\epsilon)$ shrinks with ϵ we must have $\lim_{\epsilon \to 0} \sup_{\theta \in \Theta(\epsilon)} B(\theta) \cdot \epsilon = 0$. Hence for ϵ sufficiently small $\frac{\partial}{\partial \phi}\left[\frac{\partial \hat{n}}{\partial \overline{w}}\right] \leq U(\epsilon) < 0$. The same argument holds for $\frac{\partial}{\partial \phi}\left[\frac{\partial \hat{q}}{\partial \overline{w}}\right]$ with

$$\begin{aligned}
A(\theta) &= \frac{-\mu_{\hat{q}}\hat{n}}{\mu_{\hat{q}\hat{q}}} \\
B(\theta) &= \frac{-\mu_{\hat{q}}\phi\hat{n}}{\mu_{\hat{q}\hat{q}}(\phi y_o(1) + (1-\phi)y_o(0))^2} - \frac{-\mu_{\hat{q}}(\mu_{\hat{q}\hat{q}}^2 - \mu_{\hat{q}}\mu_{\hat{q}\hat{q}\hat{q}})}{\mu_{\hat{q}\hat{q}}^3\overline{V}^2(1 - \beta[\phi(1-\pi(\hat{n}, n^t)) + (1-\phi)(1-\mu(\hat{q}, q^t))]))} \\
z(\theta) &= y_o(1) - y_o(0)
\end{aligned}$$

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

B Survey Results and Sample Description

We interviewed households during January and February 2008. 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.[30] We could not perfectly predict trouble spots in advance, hence out of the original sample of 1,938 households, we were unable to even attempt to reach 439. The main obstacles were an incident which caused tensions between a mining company and locals in Rayagada and a polite request by Maoist rebels ("Naxals") not to enter certain areas of Koraput. As Table 1 shows, the differences between the initial sample and the analysis sample generated by this attrition are reassuringly small and generally insignificant. Particularly important, there is no difference in the rate at which we reached households that worked before or after the wage change. The one significant difference is the fraction of spells performed by members of a Scheduled Caste or Scheduled Tribe, which is higher in the initial sample because the factors related to violence were concentrated in tribal areas. Values for the frame and initial sample are essentially identical by design.

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 an individual/household that was included in the official records was actually non-existent or dead or no longer lived in the village, we asked surveyors to confirm the status with 3 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 exclude from the analysis 91 households whose status we could not verify, who were temporarily away, or who declined to participate.

Of the 1328 households in which we completed interviews, only 821 confirmed having a household member who worked on an NREGS project during the period we asked about.[31] Those households that actually worked on NREGS are very similar to those that did not. In general, the sample is poor, uneducated, and uninformed, even when compared to averages across India or Orissa. Seventy-seven percent of households possess

[30] A number of people have been threatened, beaten, and even murdered for investigating NREGS 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 NREGS 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`.

[31] Since we had exact descriptions of the projects – e.g. "farm pond construction near main road X in village Y and Panchayat Z" – we are confident that respondents could distinguish between NREGS projects and other projects.

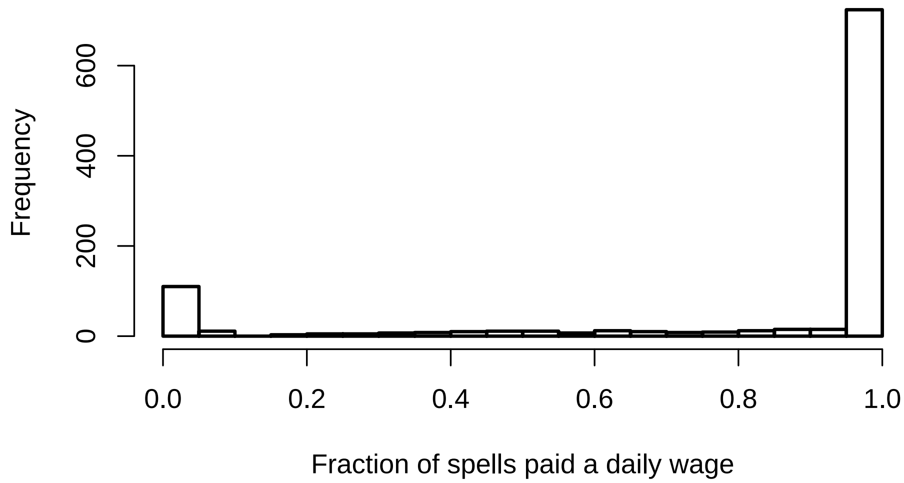
Table B.1: Sample Description

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	806	0.84	0.37	476	0.89	0.31
Number of Amenities Aware Of	810	0.96	0.85	494	0.78	0.82
HH Head has Heard of RTI Act	821	0.02	0.13	501	0.01	0.09

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 Act, a freedom of information act passed by the Indian government in 2005.

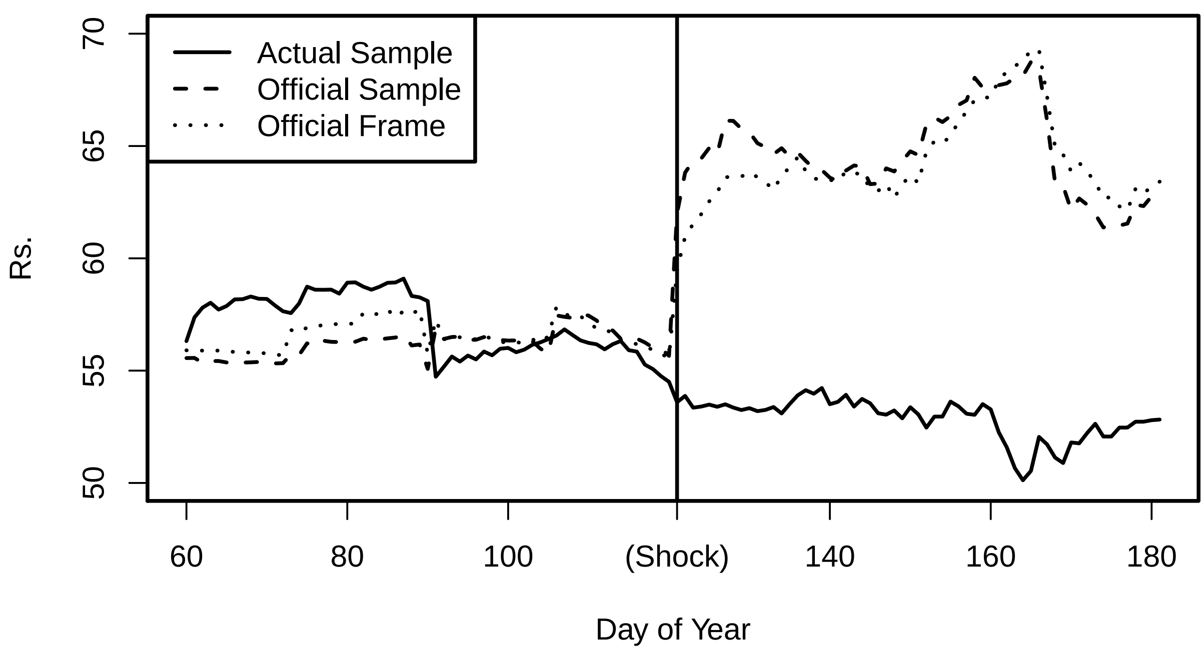
Below Poverty Line cards, only 27% of household heads are "literate" (able to write their names), and almost no one has heard of the Right to Information Act (which entitles citizens to request copies of most government records).

Figure 1: Distribution of Project Types



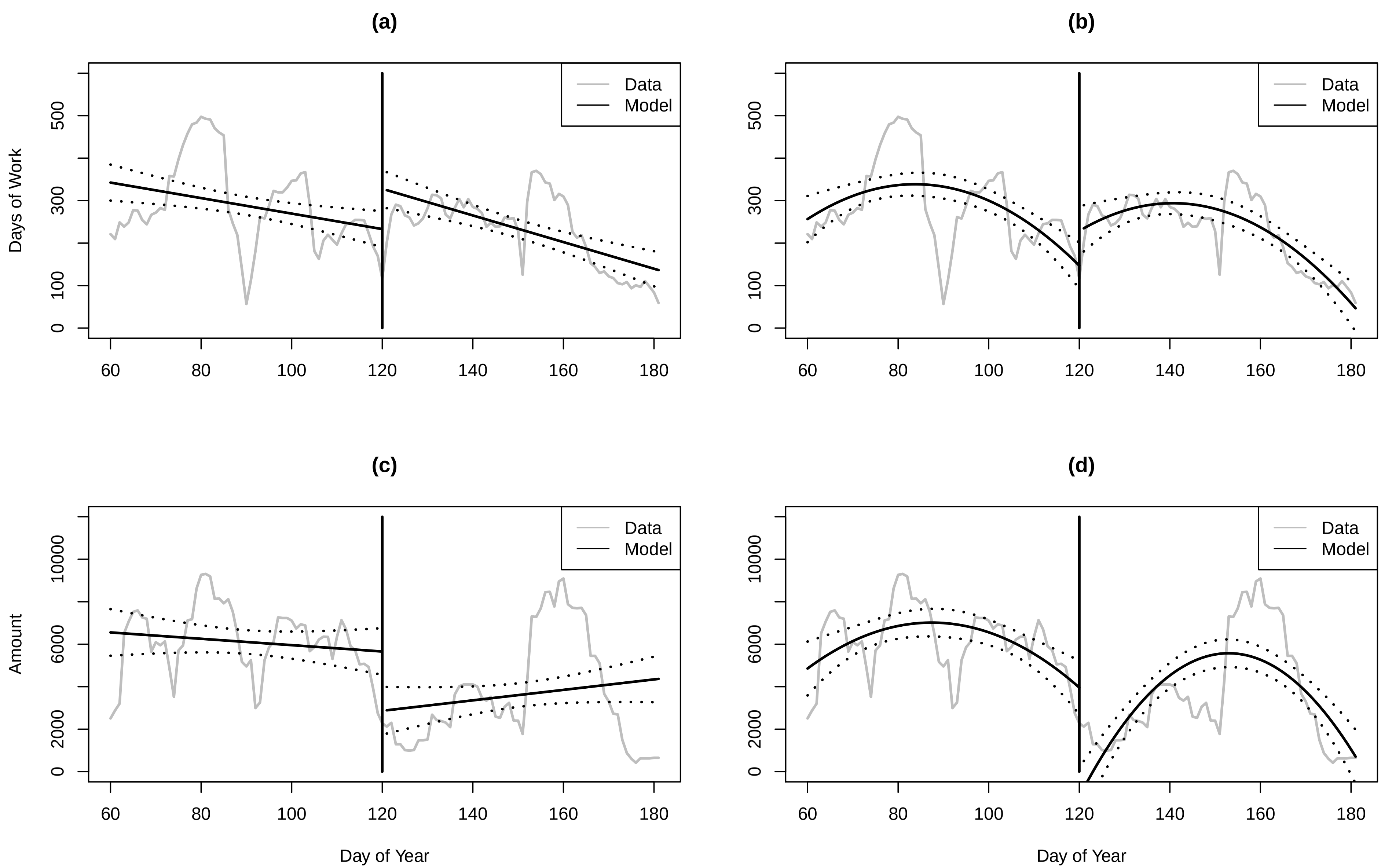
Plots distribution of projects in study panchayats by the fraction of spells of (reported) work done that were daily wage spells. Work spells are coded as daily wage spells if the payment per day is one of the statutory daily wages. (Orissa implements four different daily wages for varying skill levels.)

Figure 2: Daily Wage Rates Paid



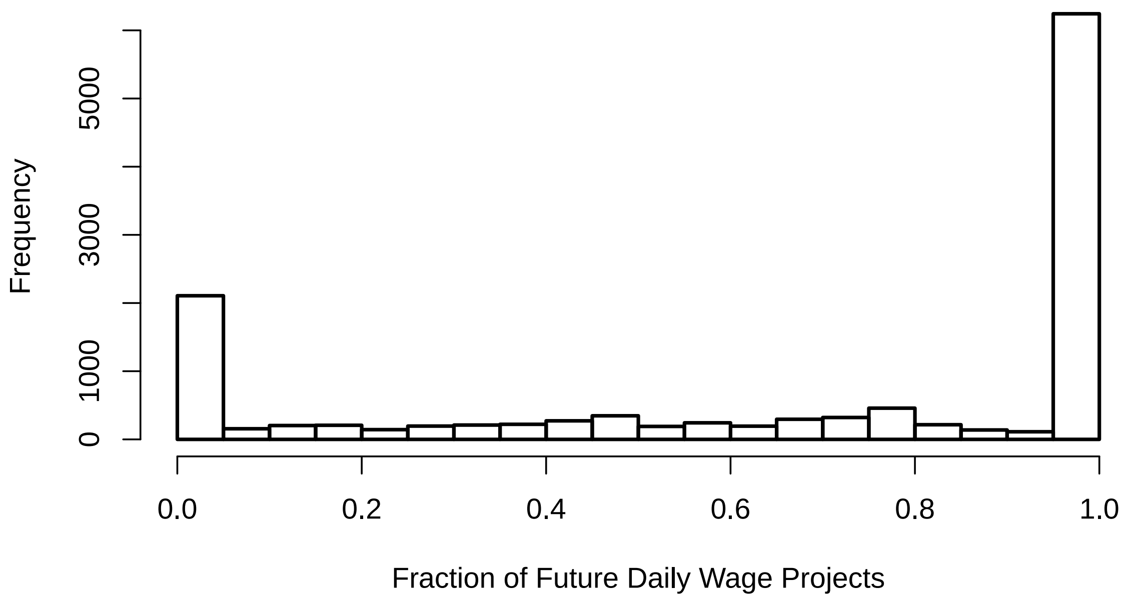
Plots a daily series of the average wage rate paid in daily wage projects in Orissa over the study period, according to official records and survey data. Day 60 corresponds to March 1st, 2007, the start of the study period; day 121 to May 1st, 2007, the date of the wage shock; and day 181 to June 30,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

Figure 3: Corruption Measures with Discontinuous Polynomial Fits



Plots daily series of the total amount of over-reporting of work days on daily wage projects (Panels (a) and (b)) and of earnings on piece-rate projects (Panels (c) and (d)) in Orissa. 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. Discrepancies were calculated by subtracting the quantities reported by survey respondents from those reported in official records. Superimposed solid lines represent fitted regression discontinuity models with linear (Panels (a) and (c)) and quadratic (Panels (b) and (d)) terms in day-of-year; dotted lines represent 95% confidence intervals.

Figure 4: Distribution of Future Daily Wage Project Fraction



Plots distribution of projects in study panchayats by the fraction of projects in the subsequent 2 months that were daily wage projects.

Table 1: Characteristics of Spells in Sample frame, Initial Sample, and Reached Sample

	All Spells			**Sampled Spells**			**Reached Spells**			
Variable	N	Mean	SD	N	Mean	SD	N	Mean	SD	p-value
Age	111109	37.6	14.93	7123	37.37	13.6	4791	37.55	13.28	0.33
Male	111057	0.54	0.5	7123	0.54	0.5	4791	0.54	0.5	0.67
SC/ST	111109	0.78	0.41	7123	0.79	0.41	4791	0.77	0.42	0.05
Post	111172	0.4	0.49	7126	0.43	0.49	4794	0.42	0.49	0.57
Spell Length	111172	11.13	2.92	7126	11.14	3.01	4794	11.09	3.14	0.33
Wage Spell	111172	0.83	0.37	7126	0.83	0.38	4794	0.84	0.36	0.2
Daily Rate	111172	63.48	17.24	7126	64.37	20.34	4794	63.9	18.92	0.3

Reports summary statistics at the work-spell level using official records and for (a) the universe of spells sampled from, (b) the initial sample of work spells we drew, and (c) the work spells done by households we were ultimately able to interview. The last column reports the p-value from a regression of the variable in question on an indicator for whether or not the observation is in our analysis sample (conditional on being in our initial sample), with standard errors clustered at the panchayat level.

Table 2: Summary Statistics of Main Regression Variables

	N	Mean	SD
Official DW Days	13054	3.31	6.30
Actual DW Days	13054	0.88	1.55
Official PR Payments	7320	94.08	259.70
Actual PR Payments	7320	12.96	43.43
FwdWageFrac	13908	0.67	0.40

This table provides summary descriptions of the aggregated variables used in the main result tables 4 and 5. The sample for each kind of project includes panchayats that had at least one of that kind of project active during the study period (March 1 through June 30 2007). "Official DW Days" is the days worked by panchayat-day on daily wage projects as reported officially. "Actual DW Days" is the days worked by panchayat-day on daily wage projects as reported by survey respondents. "Official PR Rate" is the total payments by panchayat-day on piece rate projects as reported officially, while "Actual PR Rate" corresponds to the same figure as reported by survey respondents. "FwdWageFrac" is the proportion of project-days in the next two months in a panchayat that are daily wage.

Table 3: Wage Shock Effects on Project Composition

Regressor	I	II	III
Shock	0.014	0.007	0.008
	(0.021)	(0.019)	(0.018)
Day	0.001	0.001	-0.003
	(0.001)	(0.001)	(0.002)
Day^2			0.002
			(0.001)
District FEs	N	Y	Y
N	12103	12103	12103
R^2	0.046	0.097	0.098

Each observation is a panchayat-day. The dependent variable in all regressions is "FwdWageFrac", the proportion of daily wage project-days in the panchayat in the next two months. "Shock" is an indicator equal to 1 on and after May 1, 2007. "Day" is a linear time trend; Day^2 has been re-scaled by the mean of Day. All columns include a third-order polynomial in the day of the month and 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 Daily Wage Reports

Regressor	I	II	III	IV	V	VI
Panel A: Wage Shock Effects						
Shock	0.95	0.94	0.89	1.30*	1.29	1.24
	(0.78)	(0.78)	(0.78)	(0.79)	(0.79)	(0.80)
Shock * AlwaysDW				-1.75*	-1.74*	-1.75*
				(1.00)	(0.98)	(0.99)
AlwaysDW				2.12**	2.27***	2.28***
				(0.83)	(0.86)	(0.86)
N	12810	12810	12810	12810	12810	12810
R^2	0.08	0.09	0.09	0.09	0.10	0.10
Panel B: Wage Shock Dynamic Effects						
Shock	2.39**	2.31**	2.25**	3.05**	3.00**	3.00**
	(0.95)	(0.96)	(0.95)	(1.22)	(1.23)	(1.23)
Shock * FdwAll	-1.94*	-1.84*	-1.80*	-4.03***	-3.78***	-3.78***
	(1.07)	(1.07)	(1.07)	(1.38)	(1.36)	(1.37)
Shock * FdwSome	-1.15	-1.12	-1.08	-0.21	-0.17	-0.17
	(1.03)	(1.03)	(1.02)	(0.94)	(0.94)	(0.94)
Shock * BdwAll				2.27	2.13	2.12
				(1.50)	(1.46)	(1.47)
Shock * BdwSome				-1.99**	-2.03**	-2.03**
				(0.94)	(0.97)	(0.97)
N	11386	11386	11386	10651	10651	10651
R^2	0.09	0.09	0.09	0.13	0.14	0.14
Time Controls	Day	Day	Shock*Day	Day	Day	Shock*Day
District FEs	N	Y	Y	N	Y	Y

Each observation is a panchayat-day. The dependent variable in all regressions is the number of days of daily-wage work officially reported. "Shock" is an indicator equal to 1 on and after May 1, 2007; in columns III and VI, it is the intercept difference at the time the shock occurs. "AlwaysDW" is a panchayat that had a daily wage project active throughout the study period. "FdwAll" is equal to 1 if the proportion of daily wage project-days in the panchayat in the next two months is equal to 1, and "BdwAll" is the analogous variable for the preceding two months. "FdwSome" is equal to 1 if the proportion of daily wage project-days in the next two months is greater than 0 but less than 1, and "BdwSome" is the analogous variable for the preceding two months. All regressions include controls for the number of days of daily-wage work reported by participants, an indicator for major holidays, a third-order polynomial in the day of the month, indicators for major agricultural seasons, and indicators for the panchayat chief seat being reserved for a minority group. 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: Wage Shock Effects on Piece Rate Reports

Regressor	I	II	III	IV	V	VI
Panel A: Wage Shock Effects						
Shock	-78.31**	-78.43*	-75.9*	-81.76**	-82.18**	-79.87**
	(39.91)	(40.29)	(40.08)	(40.26)	(40.66)	(40.58)
Shock * AlwaysPR				15.44	16.64	17.58
				(50.43)	(49.80)	(49.36)
AlwaysPR				-35.29	-33.19	-33.58
				(33.87)	(34.83)	(34.73)
N	7076	7076	7076	7076	7076	7076
R^2	0.04	0.05	0.05	0.04	0.05	0.05
Panel B: Wage Shock Dynamic Effects						
Shock	-38.58	-40.47	-38.18	-63.69	-62.16	-60.53
	(67.50)	(66.52)	(67.18)	(73.19)	(72.35)	(72.34)
Shock * FdwAll	-24.88	-20.36	-23.75	-44.14	-31.83	-39.19
	(69.39)	(67.39)	(68.79)	(93.40)	(90.06)	(93.11)
Shock * FdwSome	-74.61	-73.94	-72.84	-74.85	-73.83	-73.46
	(72.18)	(69.87)	(69.81)	(95.70)	(94.34)	(94.20)
Shock * BdwAll				109.23	105.72	113.68
				(81.61)	(81.84)	(84.81)
Shock * BdwSome				11.94	5.17	8.55
				(89.23)	(89.35)	(90.37)
N	6543	6543	6543	6209	6209	6209
R^2	0.08	0.08	0.08	0.11	0.11	0.12
Time Controls	Day	Day	Shock*Day	Day	Day	Shock*Day
District FEs	N	Y	Y	N	Y	Y

Each observation is a panchayat-day. The dependent variable in all regressions is the total amount paid on piece-rate projects officially reported. "Shock" is an indicator equal to 1 on and after May 1, 2007; in columns III and VI, it is the intercept difference at the time the shock occurs. "AlwaysPR" is a panchayat that had a piece rate project active throughout the study period. "FdwAll" is equal to 1 if the proportion of daily wage project-days in the panchayat in the next two months is equal to 1, and "BdwAll" is the analogous variable for the preceding two months. "FdwSome" is equal to 1 if the proportion of daily wage project-days in the next two months is greater than 0 but less than 1, and "BdwSome" is the analogous variable for the preceding two months. All regressions include controls for the number of days of daily-wage work reported by participants, an indicator for major holidays, a third-order polynomial in the day of the month, indicators for major agricultural seasons, and indicators for the panchayat chief seat being reserved for a minority group. 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: Effects on Piece Rate Reports using Andhra Pradesh as a Control

Regressor	I	II	III
OR Shock * OR	-87.86**	-87.90**	-87.54**
	(38.81)	(38.77)	(38.86)
AP Shock 1 * AP	-21.29	-21.45	-21.03
	(30.09)	(29.99)	(30.14)
AP Shock 2 * AP	117.84***	117.95***	119.38***
	(33.87)	(33.83)	(34.05)
OR Shock	31.15	31.40	53.64
	(32.51)	(32.38)	(32.88)
AP Shock 1	61.08**	60.69**	23.38
	(27.42)	(27.50)	(25.78)
AP Shock 2	-24.34	-24.71	-63.81**
	(25.89)	(25.85)	(26.00)
Actual PR Payments	0.19**	0.19**	0.19**
	(0.08)	(0.08)	(0.08)
Time Controls	Day	Day	Shock*Day
FEs	State	District	District
N	16470	16470	16470
R^2	0.06	0.06	0.06

This table uses data from both Orissa (OR) and Andhra Pradesh (AP). Each observation is a panchayat-day. The dependent variable in all regressions is the total amount paid out on piece-rate projects as officially reported. "OR Shock" is an indicator equal to 1 on and after May 1, 2007; in column III, it is the intercept difference at the time the shock occurs. "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. All columns include a third-order polynomial in the day of the month, an indicator for major holidays, and 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: Robustness Checks

	Daily Wage					Piece Rate				
Regressor	I	II	III	IV	V	VI	VII	VIII	IX	X
Shock	2.03**	1.69**	10.01***		2.44**	-69.45	-26.96	263.69		-37.90
	(0.89)	(0.84)	(3.84)		(0.97)	(65.13)	(75.21)	(219.33)		(67.03)
Shock * FdwAll	-1.75*	-1.35	-2.39**		-1.94*	1.98	-44.15	-44.32		-22.86
	(0.99)	(1.04)	(1.00)		(1.09)	(63.27)	(77.92)	(74.11)		(68.91)
Shock * FdwSome	-1.28	-0.33	-1.40		-1.19	-51.87	-96.82	-80.35		-81.61
	(0.92)	(0.98)	(0.90)		(1.04)	(64.72)	(80.02)	(76.74)		(72.31)
April				0.29					103.47	
				(1.25)					(67.50)	
May				2.76					42.39	
				(1.77)					(76.83)	
June				3.37					205.76	
				(2.24)					(154.60)	
April * FdwAll				-0.35					-125.72**	
				(1.41)					(57.67)	
May * FdwAll				-1.75					-39.77	
				(1.71)					(50.69)	
June * FdwAll				-2.60					-167.24	
				(1.92)					(119.43)	
Time Window (months)	1	3	2	2	2	1	3	2	2	2
Reservations	N	N	Y	N	N	N	N	Y	N	N
N	10740	11740	11386	11386	11386	6250	6653	6543	6543	6543
R^2	0.11	0.09	0.12	0.09	0.04	0.09	0.08	0.10	0.09	0.08

Each observation is a panchayat-day. The dependent variable in Columns I-IV is the number of days of daily-wage work officially reported; in Column V, the difference between this quantity and the number of days of daily-wage work reported by participants; in Columns VI-IX, the total amount paid out on piece-rate projects as officially reported; and in Column X, the difference between this quantity and the total amount paid out on piece-rate projects as reported by participants. “Shock” is an indicator equal to 1 on and after May 1, 2007. “FdwAll” is equal to 1 if the proportion of daily wage project-days in the panchayat in the next two months is equal to 1, and “BdwAll” is the analogous variable for the preceding two months. “FdwSome” is equal to 1 if the proportion of daily wage project-days in the next two months is greater than 0 but less than 1, and “BdwSome” is the analogous variable for the preceding two months. “Actual DW Days” is the number of days of daily-wage work reported by participants, and “Actual PR Payments” is the total amount paid out on piece-rate projects as reported by participants. All columns include a linear time trend, an indicator for major holidays, a third-order polynomial in the day of the month, indicators for major agricultural seasons, , and indicators for the panchayat chief seat being reserved for a minority group *except* Columns IV and IX which omit the polynomial in day-of-month. 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 8: 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)***
Day	0.082	0.082	0.048	0.147
	(0.017)***	(0.018)***	(0.024)*	(0.038)***
Day^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: $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table C.1: Wage Shock Effects on Daily Wage Reports, Squared Time Trends

Regressor	I	II	III	IV	V	VI
Panel A: Wage Shock Effects						
Shock	0.88	0.88	1.04	1.23	1.23	1.40
	(0.78)	(0.79)	(0.98)	(0.79)	(0.80)	(0.94)
Shock * AlwaysDW				-1.75*	-1.75*	-1.73*
				(1.01)	(0.99)	(0.99)
AlwaysDW				2.14**	2.28***	2.27***
				(0.84)	(0.86)	(0.86)
N	12810	12810	12810	12810	12810	12810
R^2	0.08	0.09	0.09	0.09	0.10	0.10
Panel B: Wage Shock Dynamic Effects						
Shock	2.28**	2.22**	2.28*	3.01**	2.97**	2.97*
	(0.94)	(0.95)	(1.26)	(1.24)	(1.24)	(1.53)
Shock * FdwAll	-1.83*	-1.76*	-1.78*	-3.90***	-3.71***	-3.70***
	(1.07)	(1.06)	(1.06)	(1.40)	(1.38)	(1.35)
Shock * FdwSome	-1.07	-1.05	-1.03	-0.17	-0.15	-0.11
	(1.02)	(1.02)	(1.02)	(0.94)	(0.94)	(0.94)
Shock * BdwAll				2.17	2.07	2.10
				(1.51)	(1.47)	(1.44)
Shock * BdwSome				-2.01**	-2.04**	-2.01**
				(0.95)	(0.98)	(0.94)
N	11386	11386	11386	10651	10651	10651
R^2	0.09	0.10	0.10	0.13	0.14	0.14
Time Controls	Day2	Day2	Shock*Day2	Day2	Day2	Shock*Day2
District FEs	N	Y	Y	N	Y	Y

Each observation is a panchayat-day. The dependent variable in all regressions is the number of days of daily-wage work officially reported. "Shock" is an indicator equal to 1 on and after May 1, 2007; in columns III and VI, it is the intercept difference at the time the shock occurs. "AlwaysDW" is a panchayat that had a daily wage project active throughout the study period. "FdwAll" is equal to 1 if the proportion of daily wage project-days in the panchayat in the next two months is equal to 1, and "BdwAll" is the analogous variable for the preceding two months. "FdwSome" is equal to 1 if the proportion of daily wage project-days in the next two months is greater than 0 but less than 1, and "BdwSome" is the analogous variable for the preceding two months. All regressions include controls for the number of days of daily-wage work reported by participants, an indicator for major holidays, a third-order polynomial in the day of the month, indicators for major agricultural seasons, and indicators for the panchayat chief seat being reserved for a minority group. 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 C.2: Wage Shock Effects on Piece Rate Reports, Squared Time Trends

Regressor	I	II	III	IV	V	VI
Panel A: Wage Shock Effects						
Shock	-78.02*	-77.69*	-107.05*	-81.48**	-81.52**	-111.41**
	(40.02)	(40.25)	(59.55)	(40.38)	(40.67)	(56.18)
Shock * AlwaysPR				15.56	16.98	18.41
				(50.35)	(49.60)	(48.96)
AlwaysPR				-35.38	-33.32	-34.25
				(33.82)	(34.78)	(34.62)
N	7076	7076	7076	7076	7076	7076
R^2	0.04	0.05	0.05	0.04	0.05	0.06
Panel B: Wage Shock Dynamic Effects						
Shock	-37.46	-39.62	-83.01	-63.16	-61.93	-100.15
	(67.85)	(66.82)	(73.64)	(73.25)	(72.47)	(84.32)
Shock * FdwAll	-27.71	-22.54	-20.67	-50.13	-37.42	-36.27
	(70.79)	(68.47)	(67.62)	(96.55)	(92.82)	(91.82)
Shock * FdwSome	-74.57	-73.74	-69.65	-75.65	-74.54	-69.08
	(72.20)	(69.9)	(69.30)	(96.15)	(94.64)	(93.13)
Shock * BdwAll				114.07	111.48	115.15
				(84.33)	(84.62)	(84.54)
Shock * BdwSome				14.69	8.19	4.83
				(90.81)	(90.69)	(89.31)
N	6543	6543	6543	6209	6209	6209
R^2	0.08	0.08	0.09	0.11	0.12	0.12
Time Controls	Day2	Day2	Shock*Day2	Day2	Day2	Shock*Day2
District FEs	N	Y	Y	N	Y	Y

Each observation is a panchayat-day. The dependent variable in all regressions is the total amount paid on piece-rate projects officially reported. “Shock” is an indicator equal to 1 on and after May 1, 2007; in columns III and VI, it is the intercept difference at the time the shock occurs. “AlwaysPR” is a panchayat that had a piece rate project active throughout the study period. “FdwAll” is equal to 1 if the proportion of daily wage project-days in the panchayat in the next two months is equal to 1, and “BdwAll” is the analogous variable for the preceding two months. “FdwSome” is equal to 1 if the proportion of daily wage project-days in the next two months is greater than 0 but less than 1, and “BdwSome” is the analogous variable for the preceding two months. All regressions include controls for the number of days of daily-wage work reported by participants, an indicator for major holidays, a third-order polynomial in the day of the month, indicators for major agricultural seasons, and indicators for the panchayat chief seat being reserved for a minority group. 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 C.3: Effects on Piece Rate Reports using Andhra Pradesh as a Control, Squared Time Trends

Regressor	I	II	III
OR Shock * OR	-87.39**	-87.31**	-86.87**
	(38.94)	(38.92)	(38.93)
AP Shock 1 * AP	-21.10	-21.24	-23.30
	(30.29)	(30.18)	(30.18)
AP Shock 2 * AP	119.97***	120.03***	119.74***
	(34.13)	(34.10)	(34.08)
OR Shock	52.21	52.51	-35.07
	(32.00)	(31.94)	(43.97)
AP Shock 1	-3.47	-3.57	18.89
	(26.52)	(26.43)	(22.00)
AP Shock 2	-63.85***	-63.91***	-44.79
	(24.27)	(24.22)	(27.81)
Actual PR Payments	0.20**	0.20**	0.20**
	(0.08)	(0.08)	(0.08)
Time Controls	Day2	Day2	Shock*Day2
FEs	State	District	District
N	16470	16470	16470
R^2	0.06	0.06	0.07

This table uses data from both Orissa (OR) and Andhra Pradesh (AP). Each observation is a panchayat-day. The dependent variable in all regressions is the total amount paid out on piece-rate projects as officially reported. "OR Shock" is an indicator equal to 1 on and after May 1, 2007; in column III, it is the intercept difference at the time the shock occurs. "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. All columns include a third-order polynomial in the day of the month, an indicator for major holidays, and indicators for major agricultural seasons. Robust standard errors multi-way clustered by panchayat and day are presented in parenthesis. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$