

## Corruption Dynamics: The Golden Goose Effect<sup>†</sup>

By PAUL NIEHAUS AND SANDIP SUKHTANKAR\*

*Theoretical work on disciplining corrupt agents has emphasized the role of expected future rents—for example, efficiency wages. Yet taken seriously this approach implies that illicit future rents should also deter corruption. We study this “golden goose” effect in the context of a statutory wage increase in India’s employment guarantee scheme, comparing official microrecords 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 percent. In short, rent expectations matter. (JEL D73, D82, H83, J41, K42, O17, O21)*

**D**isciplining corrupt officials is a key governance challenge in developing countries.<sup>1</sup> In an influential early analysis, Becker and Stigler (1974) argued that if there is some chance of detecting and dismissing corrupt agents then the principal can mitigate the problem by paying an efficiency wage. Intuitively, agents have an incentive to cheat less today in order to improve their chances of earning a wage premium (or pension) tomorrow. Subsequent work has maintained this emphasis on contracts designed to offer future rents.<sup>2</sup>

In contrast, the literature has put less emphasis on the role played by expectations of *illicit* future rents. This paper focuses explicitly on the dynamic trade-off between extracting rents today and improving one’s chances of surviving to extract rents tomorrow. We call this latter motive the “golden goose” effect to reflect the idea that

\*Niehaus: Department of Economics, University of California at San Diego, 9500 Gillman Drive #0508, San Diego, CA 92093 (e-mail: [pniehaus@ucsd.edu](mailto:pniehaus@ucsd.edu)); Sukhtankar: Department of Economics, Dartmouth College, 326 Rockefeller Hall, Hanover, NH 03755 (e-mail: [sandip.sukhtankar@dartmouth.edu](mailto:sandip.sukhtankar@dartmouth.edu)). We thank Nageeb Ali, Eric Edmonds, Edward Glaeser, Roger Gordon, Claudia Goldin, Gordon Hanson, Larry Katz, Asim Khwaja, Michael Kremer, Sendhil Mullainathan, Ben Olken, Rohini Pande, Andrei Shleifer, Jonathan Zinman, and seminar participants at Harvard, Yale, BREAD, Stanford, the World Bank, CGD, UNH, Indian Statistical Institute-Delhi, NEUDC-Boston University, Dartmouth, and UCSD for helpful comments. Thanks also to Manoj Ahuja, Arti Ahuja, and Kartikian Pandian for generous hospitality and insight into the way NREGS operates in practice, and to Sanchit Kumar for adept research assistance. We acknowledge funding from the National Science Foundation (grant SES-0752929), a Harvard Warburg Grant, a Harvard CID Grant, and a Harvard SAI Tata Summer Travel Grant. Niehaus acknowledges support from a National Science Foundation Graduate Student Research Fellowship; Sukhtankar acknowledges support from a Harvard University Multidisciplinary Program in Inequality & Social Policy Fellowship.

<sup>†</sup>Go to <http://dx.doi.org/10.1257/pol.5.4.230> to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

<sup>1</sup>Recent work has shown how corruption constrains redistribution (Reinikka and Svensson 2004; Olken 2006), creates new market distortions (Sequeira and Djankov 2010), and hinders efforts to remedy existing ones (Bertrand et al. 2007).

<sup>2</sup>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.

agents want to preserve “the goose that lays the golden eggs” (unlike the deplorably myopic farmer in the fable).<sup>3</sup> We show that incorporating the golden goose effect into standard models tends to weaken or even overturn the usual comparative statics because of a generic tendency for static and dynamic effects to offset each other.<sup>4</sup>

To assess the relevance of this mechanism we gathered 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, on-demand employment per year; it is also of intrinsic interest given that it covers roughly 11 percent of the world’s population. 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 to document the amounts of work actually done and payments actually received. The gap between official and actual payments—which includes both overreporting of days and underpayment of wages—is the primary form of corruption we study.<sup>5</sup>

Testing for golden goose effects requires an exogenous source of variation in anticipated rent-extraction opportunities. We exploit a policy change: a May 1, 2007 increase in the statutory wage due to program participants in the state of Orissa. A higher statutory wage means more lucrative corruption opportunities for officials, since they receive more money for every fictitious day of work reported. **Importantly, the wage reform was enacted by policymakers 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.

Interestingly, the effects of a wage change on daily-wage overreporting turn out to be theoretically ambiguous. There is an obvious static price effect: officials receive more money for every day of wage work they report, strengthening their incentives to overreport. If the wage increase were temporary this would be the only effect. **Following a permanent change, however, there is also a dynamic golden goose effect: officials anticipate a more lucrative future, weakening their incentives to overreport.**

To separate out golden goose effects we exploit the fact that compensation on roughly 30 percent of the NREGS projects in our sample was based on piece rates rather than a daily wage. This heterogeneity reflects the fact that piece rates could not be implemented on some projects where output was hard to measure. Because the schedule of projects had been fixed in advance of the May 1, 2007 wage change, and because piece-rate schedules were not revised along with the daily wage, the wage change should not have directly affected piece-rate projects. Officials who were managing piece-rate projects at the time of the wage change often had wage projects planned in the near future, however, and thus experienced a shift in their future rent expectations. This effect should also have been stronger in proportion to

<sup>3</sup> Our usage differs from McMillan (2001), who uses “golden goose” to describe ex ante investments by individuals that a government may hold up ex post. Commitment will not be an issue in our setting.

<sup>4</sup> Note that the framework here is one of observed types, as opposed to the career concerns framework in which the agent wishes to influence future perceptions of his ability (or honesty) (Holmström 1999).

<sup>5</sup> On the importance of measuring corruption directly, rather than using perceptions, see Olken (2009).

the share of upcoming projects that were daily wage. Theory thus predicts that the wage increase should (i) reduce theft from piece-rate projects, and (ii) 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 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 that 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 suggest large golden goose effects. Rough calculations imply that theft increased by 64 percent less than it would have had the wage increase been temporary.** This point estimate need not be externally valid for other settings, of course; we merely emphasize that dynamics appear to play a large role even in a setting where tenure is typically quite short.

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 rule out confounding changes in monitoring intensity.

Our analysis has four main implications for anticorruption policy. First, it provides evidence in support of the broad hypothesis that future rents matter, which is at the heart of the efficiency wage concept. As Olken and Pande (2012) discuss, government wages have received a great deal of attention, yet the empirical evidence has been limited to cross-country regressions and to the indirect test in Di Tella and Schargrodsky (2003) who study the differential effects of an audit crackdown. We simply exploit variation in expectations of illicit as opposed to licit rents to test the same underlying mechanism.<sup>6</sup>

Second, our data suggest that optimal contracts should take illicit as well as licit rents into account. Comparing what we know about the compensation of officials we study to our estimates of corruption implies that their illicit earnings are orders of magnitude greater than their licit *wage* (150 to 1,100 times wages), let alone their wage premium. Calculations that leave out these illicit rents are unlikely to hit the mark.

<sup>6</sup> As some NREGS officials are elected the results can also be read as supporting 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 2011).

Third, our data suggest that concerns about the “displacement” effects of anti-corruption work should be taken seriously. As Yang (2008) discusses, the possibility that cracking down on one kind of corruption may lead to increases in other kinds has been widely discussed but rarely tested. Our data support this hypothesis. Indeed, the golden goose mechanism generates displacement generically: any use of the “stick” that reduces future rent expectations also makes the “carrot” of job security less motivating. The analysis thus complements Yang’s model based on nonconvexities in lawbreaking.

Finally, our results suggest that policy pilots should be interpreted carefully in weakly institutionalized settings. Simply put, a pilot generates different dynamic incentives than permanent implementation. For example, distributing welfare benefits once does not generate future rent expectations, while distributing them repeatedly does; a pilot may therefore appear to perform artificially poorly. Auditing once does not reduce future rent expectations, while a regular program of audits does; a pilot may therefore appear to perform artificially well. Generally speaking, expectations matter for interpreting results on monitoring (Di Tella and Schargrodsky 2003; Nagin et al. 2002; Olken 2007) and on transparency more generally (Reinikka and Svensson 2005; Ferraz and Finan 2008).

The rest of the paper is structured as follows: Section I describes the NREGS context, Section II lays out the theoretical framework, Section III describes data collection and estimation equations, Section IV presents results, and Section V concludes.

## I. Contextual Background on the NREGS

India’s National Rural Employment Guarantee Scheme (now called the “Mahatma Gandhi National Rural Employment Guarantee Act”) is a landmark effort to redistribute income to the rural poor. The program was launched in February 2006 in the poorest 200 districts in India and as of April 2008 covers the entire country (604 rural districts). The total proposed budget allocation for the April 2010–March 2011 fiscal year is Rs 401 billion (US\$ 8.9 billion), which is 0.73 percent of 2008 GDP.<sup>7</sup> 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.

### A. 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

<sup>7</sup> Costs: <http://indiabudget.nic.in/ub2010-11/bh/bhi.pdf>. GDP: [http://mospi.nic.in/4\\_gdpind\\_cur.pdf](http://mospi.nic.in/4_gdpind_cur.pdf). The central government must by law contribute at most 90 percent of total expenditure, the rest of the funding coming from the states.

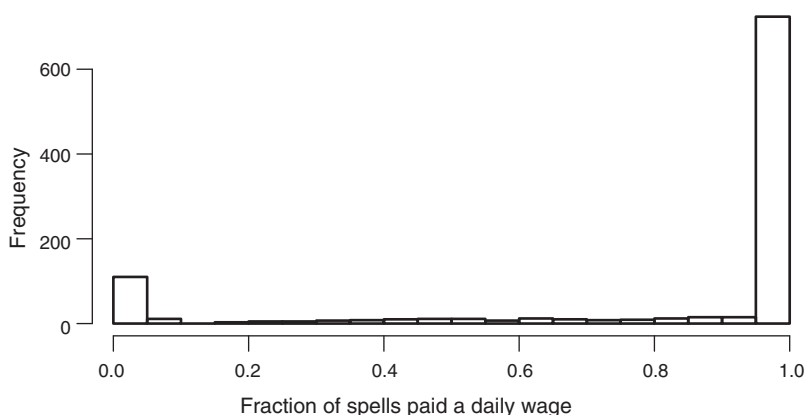


FIGURE 1. DISTRIBUTION OF PROJECT TYPES

*Notes:* 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).

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. There are broad categories of projects that are paid on piece rate as opposed to daily wage; for example all irrigation/water conservation projects which involve digging ditches are piece rate, while all road construction/paving projects are daily wage. 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 eligibility requirements.<sup>8</sup> To obtain work on a project, interested households must first apply for a *jobcard*.<sup>9</sup> 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.

<sup>8</sup> Officials thus do not have an opportunity cost of allocating work to workers, as in Banerjee (1997).

<sup>9</sup> 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 2006).

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 microdata on payments have been made publicly available through a web portal maintained by the central Ministry of Rural Development (<http://nrega.nic.in>).

### *B. 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"). district level program officials, including the District Collector, oversee block officials' work. While in principal 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 inquiry, which makes it difficult to fire someone outright in practice. 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 for elected Sarpanchs is five years. Tenure for appointed bureaucrats is typically shorter, 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 at least as short. In Gujarat, Block Development Officers keep that post for an average of sixteen months (*de Zwart* 1994, 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.

### *C. 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 percent of total spending, and in fact we find that theft in this category is so extensive that even if all of the 40 percent allocated to materials were stolen, the labor budget would still be the larger source of illegal rents.<sup>10</sup>

<sup>10</sup> We also found that bribes paid to obtain jobcards are uncommon (17 percent report paying positive amounts) and small (averaging Rs 10 conditional on being positive).



Theft from the labor budget comes in two conceptually distinct forms. First, officials can underpay workers for the work they have done (theft from beneficiaries). Second, officials can overreport the amount of work done when they send their reports up the hierarchy (theft from taxpayers). For example, a worker who worked for ten 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 ten. His total rents would then equal  $55 \cdot 20 - 45 \cdot 10 = 650$  rupees, the sum of the two sorts of theft.

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. On the other hand, workers have less incentive to monitor overreporting: because the program's budget is not fixed, a rupee stolen through overreporting does not mean a rupee less for the workers. In principle the NREGS Operational Guidelines address this issue by calling both for bottom-up monitoring via Gram Sabhas (village meetings), local Vigilance and Monitoring Committees, and biannual "social audits," and also top-down monitoring via inspection of works by superior officials (100 percent of works checked by block officials, 10 percent by district officials, and 2 percent by state officials). The guidelines do not provide incentives for auditing or link audit results to budget allocations, however. In practice there was no systematic audit process in Orissa during the period we study (in contrast with, for example, the setting in Olken 2007). What top-down monitoring did exist consisted primarily of informal tracking and worksite visits by officials. For example, some block and district officials we interviewed use the NREGS's management information system to track aggregate quantities of work done on various projects and compare these to technical estimates or their own best guesses of the resources required.

Officials caught cheating face a positive but small 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. 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.<sup>11</sup> A more reliable source may be the records of OREGS-Watch, a loose online coalition of nongovernmental 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.<sup>12</sup>

<sup>11</sup> <http://www.orissadiary.com/Shownews.asp?id=6201>.

<sup>12</sup> <http://www.dailypioneer.com/59458/Action-taken-after-study-finds-fake-muster-roll-in-Boudh.html>.

### D. Wage Setting

Our estimation strategy 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 percent of the unskilled labor budget, and 75 percent of the materials budget (defined to include the cost of skilled labor) (Ministry of Law and Justice, 2005). The states, however, set wages and piece rates. This provision 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 for unskilled workers in Orissa from Rs 55 to Rs 70. This change was announced on April 28, 2007 and went into effect on May 1, 2007. Importantly, this policy change did not directly affect payments on piece-rate projects, and it was specific to Orissa (did not affect neighboring Andhra Pradesh).<sup>13</sup> Note that wages for three categories of higher-skilled labor were also raised on May 1 from Rs 65/75/85 to Rs 80/90/100. Because skilled wages are rarely reported in our data (6.5 percent of work spells) and their use varies primarily within-project (65 percent of the variation), we focus our theoretical discussion around a single-wage rate.

## II. Dynamic Rent Extraction

Following Becker and Stigler (1974), a large theoretical literature has studied the use of dismissal threats to motivate corruptible agents. Dismissal typically matters in these models because agents who are not dismissed expect to receive compensation greater than their outside option—a wage premium or a pension, for example. In a dynamic setting, however, an agent's expected future rents include both an exogenous *licit* component provided by the contract and also an endogenous *illicit* component determined by their own future corrupt behavior. For example, an official thinking about whether to take a bribe today will rationally take into account the bribe revenue he expects to earn tomorrow. In this section, we develop a dynamic model to examine the role that such expectations play in decision making. We specialize the framework to our context by modeling the kinds of corruption that we see in our data but also comment on broader implications.

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

$$(1) \quad u_i(t) = \sum_{\tau=t}^{\infty} \beta^{\tau-t} y_i(\tau),$$

<sup>13</sup> The NREGS implementation guidelines state that the states should “devise productivity norms for all the tasks listed under piece-rate works for the different local conditions of soil, slope, and geology types in such a way that normal work for the prescribed duration of work results in earnings at least equal to the wage rate.” In practice, however, this occurs haphazardly and with long and variable lags. In Orissa wages were revised on May 1, 2007, but the piece-rate schedule was not amended until August 16, 2007, a month and a half after our study period ends, and at that time some rates were lowered rather than raised.



where  $y_i(\tau)$  are the earnings of agent  $i$  in period  $\tau$ . 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

$$(2) \quad \phi \equiv \mathbf{P}(\omega^t = 1 | \omega^{t-1}, \omega^{t-2}, \dots).$$

We suppose that all agents know  $\phi$  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  $\phi$ ) plays a key role. We treat  $\phi$  as exogenous here since *de jure* it should be predetermined, but will also test below whether it responds to the wage change.

Each worker inelastically supplies one indivisible unit of labor in each period. We 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  $w_i^t$  ( $r_i^t$ ) be the wage (piece rate) that participating worker  $i$  receives. This need not equal the statutory wage  $\bar{w}$  (the statutory piece rate  $\bar{r}$ ).

NREGS wages and employment levels emerge from bargaining between the official and the workers. In principal workers have two sources of bargaining power: they can threaten to complain if the official pays them less than the statutory rate  $\bar{w}$  ( $\bar{r}$ ), or can simply leave for the private sector and earn  $\underline{w}^t$  ( $\underline{r}^t$ ). Which of these threat points matters in practice is of course an empirical question. In a companion paper we study this issue in some detail; we find that the wages workers’ receive bear little relationship to the statutory wage but closely track variation in local market wages (Niehaus and Sukhtankar 2013). Motivated by these data, we model equilibrium wages and participation choices as tracking market wages ( $w_i^t = \underline{w}^t$  and  $n^t = n^t(\underline{w}^t)$ ). 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

$$y_o^t(\omega^t = 1) = \underbrace{(\bar{w} - w)}_{\text{Underpayment}} n + \underbrace{(\hat{n}^t - n)}_{\text{Overreporting}} \bar{w},$$

and analogously if it is a piece-rate project,

$$y_o'(\omega^t = 0) = \underbrace{(\bar{r} - r)}_{\text{Underpayment}} q + \underbrace{(\hat{q}^t - q)}_{\text{Overreporting}} \bar{r}.$$

The official can report up to  $\bar{n} > n$  work-days, where  $\bar{n}$  is the number of registered workers in his village. Overreporting 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 study the case where  $\pi(n, n) = 0$  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  $\pi$  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 overreporting or on the average rate of overreporting. The probability of detection on piece-rate projects is  $\mu(\hat{q}^t, q)$  for  $q \leq \hat{q} \leq \bar{q}$  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.<sup>14,15</sup>

The recursive formulation of the official's objective function is

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

where  $V(\bar{w}, 1)$  is the official's expected continuation payoff in a period with a daily-wage project,  $V(\bar{w}, 0)$  is his expected continuation payoff in a period with a piece-rate project, and  $\bar{V}(\bar{w})$  is his expected continuation payoff unconditional on project type.

As a benchmark, consider first the effects of a hypothetical, *temporary* increase in the statutory daily wage. Because the official's continuation value  $\bar{V}(\bar{w}, \phi)$  is unaffected by this change it strictly increases overreporting on daily-wage projects ( $\hat{n}' - n$ ). Intuitively, the wage change acts like a pure price shock for officials managing daily-wage projects: the value of overreporting a day of work goes up, while the cost is unaffected. Consequently overreporting increases. Theft on piece-rate projects ( $\hat{q}'\bar{r} - qr$ ) does not change, on the other hand, since neither the costs nor the benefits of stealing change.

<sup>14</sup> 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, we do not observe variation in tenure and so for simplicity we omit it from the model.

<sup>15</sup> We model  $\pi$  as independent of the daily wage and other program parameters since incentives for monitoring are not linked to other program parameters in our context. In Section IVE, we directly test for effects of  $\bar{w}$  on monitoring and do not find any evidence of a relationship.

Now consider the effects of a permanent increase in the statutory daily wage. Besides a static price effect, this also has a dynamic effect on the official's continuation value  $\bar{V}(\bar{w}, \phi)$ . Interestingly, this effect can potentially reverse the model's predictions for daily-wage overreporting. Whether it does hinges on the elasticity of future rents with respect to  $\bar{w}$ .

**PROPOSITION 1:** *Overreporting  $\hat{n}^t - n$  on daily-wage projects is increasing in  $\bar{w}$  if  $\frac{\bar{w}}{\bar{V}} \frac{\partial \bar{V}}{\partial \bar{w}} < 1$  and decreasing otherwise.*

**PROOF:**

Proofs are deferred to Appendix A.

Intuitively, a higher wage raises the value of future overreporting, which in turn increases the importance of keeping one's job. This effect dominates the price effect unless the elasticity of future benefits with respect to the wage is sufficiently small.<sup>16</sup>

While not easily refutable, Proposition 1 suggests two tests. First, we can 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 on piece-rate projects, but does increase expected future rents and thus discourages theft:

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

This result is particularly interesting since many mechanisms—in which different kinds of corruption complement each other—could generate the opposite effect. 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.

A second test exploits variation in the relative intensity of price and golden goose effects. Since the wage change only affects rents on future wage projects, we expect to see stronger effects in places with more future wage projects upcoming (higher  $\phi$ ). This turns out to be true if piece-rate and daily-wage projects are similarly lucrative:

**PROPOSITION 3:** *Restrict attention to any closed, bounded set of parameters  $(\phi, \bar{w}, \bar{r}, \underline{w}, r)$ . Then for  $|y_o(1) - y_o(0)|$  sufficiently small,*

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

The condition  $y_o(1) \simeq y_o(0)$  matters because without it changes in  $\phi$  generate “wealth effects” that can be additional sources of treatment heterogeneity. In our

<sup>16</sup> One can in fact go further and construct examples (available on request) in which the *total* amount stolen per period decreases.

empirical work we first verify that equilibrium rents from daily-wage and piece-rate projects are similar, and then then test Proposition 3, using our data to estimate categories of  $\phi$ .

### A. Effects of Wages and Monitoring

The results above characterize the effects of a wage reform to guide our empirical work. Earlier work, on the other hand, has emphasized the probability of audit and the official's wage as key exogenous parameters. To relate our model to this literature we next characterize their effects.

To formalize the probability of an audit let  $\pi(\hat{n}, n) = \gamma \tilde{\pi}(\hat{n}, n)$ , where  $\gamma$  is the probability a daily-wage project is audited and  $\tilde{\pi}$  the conditional probability of conviction. Then one can show that a one-period increase in  $\gamma$  decreases overreporting on daily-wage projects and has no effect on theft from piece-rate projects. A permanent increase in  $\gamma$ , on the other hand, generates a smaller decrease—or even an increase—in daily-wage overreporting, and increases theft from piece-rate projects. The contrast between these results yields a simple lesson for empirical work: the right interpretation of empirical evidence on the effects of a crackdown depends on whether officials perceived it to be temporary or permanent. In particular, temporary crackdowns generate larger reductions in corruption than permanent ones, and should thus be interpreted cautiously as guides to policymaking.

Efficiency wages, on the other hand, work here just as they would in a one-shot game. To see this let  $\bar{V}(\bar{w}, \phi) = \phi V(\bar{w}, 1, \phi) + (1 - \phi)V(\bar{w}, 0, \phi) + W$  where  $W \geq 0$  is a wage premium paid to the official in each period until he is not dismissed. It is straightforward to show that all forms of corruption are decreasing in  $W$ . Intuitively, the efficiency wage has no price effects and only deterrent effects. As this example illustrates the theory's novel predictions hinge not on dynamics per se but on the dynamic implications of future corrupt rents.

### B. Alternative Substitution Mechanisms

Some of our framework's implications could also be generated by alternative substitution mechanisms. We conclude our theoretical discussion with a brief overview of these mechanisms and highlight a key distinction between them and the golden goose effect: the latter predicts that only future rent expectations, and not past rent realizations, affect current behavior. We will exploit this asymmetry below to examine which story best fits our data.

One possible substitution mechanism involves the “production function” for corruption. Anecdotal evidence suggests 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 reallocate 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. In an extreme case

of income effects officials might even “target” a particular income level. **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.

One difference between the golden goose effect and these mechanisms is that the former is purely forward-looking while the latter are time-symmetric, in that they predict that increases in both past and future corruption opportunities should reduce corruption today. Consider the “input” model: suppose that the official can extract rents  $R_t$  today and  $R_{t+1}$  tomorrow only if  $f(R_t, R_{t+1}) \leq 0$  for some increasing function  $f$ . Clearly any factor that increases  $R_{t+1}$  must therefore decrease  $R_t$ , generating what might look like a golden goose effect. Similarly, however, any factor that increases  $R_t$  must decrease  $R_{t+1}$ , so that shocks to lagged rent extraction also negatively effect rent extraction today. An analogous argument applies to the monitoring story (for example, let the probability of an investigation be  $f(R_t + R_{t+1})$ ). Finally, consider a simple model with income effects in which officials maximize  $U(R_t + R_{t+1}) - D(R_t, R_{t+1})$  where  $U$  is an increasing, concave function and  $D$  is the expected nonmonetary disutility of punishments. (The income-targeting story is a limited case of this example.) Provided  $D_{12}$  is not too negative, changes in  $D$  that lower the cost of  $R_{t+1}$  will induce substitution away from  $R_t$  due to diminishing marginal utility ( $U'' < 0$ ). This also implies the converse, however.

### III. Empirical Approach

#### A. Official Data

To test the theoretical predictions in Section II we adopt an audit approach, comparing official microdata 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 straightforward to infer whether a project paid daily wages or piece rates because there are only a few allowed daily-wage rates.<sup>17</sup> (Figure 1).

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 essentially unchanged, but that the number of work

<sup>17</sup> We designate a project as daily wage if more than 95 percent of the wages paid are these amounts.

spells recorded for Andhra Pradesh increased by roughly 10 percent. 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 representativeness of our AP sample frame. We will emphasize the Orissa data and use AP as a control only in Table 5.

We sampled from the list of officially recorded NREGS work spells during the period March 1, 2007 to June 30, 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 percent 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 1,938 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.

### B. Survey Coverage

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. 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, we designed the survey instrument and trained enumerators to jog respondents' memories: for example, using major holidays as reference points. The results were encouraging: we obtained information on at least the month in which work was done for 93 percent 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 percent—2.2 percent 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. The one place where recall does matter is that recipients do 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 ( $\hat{q}^t \bar{r} - q^t r^t$  in terms of the model).

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 overreporting. 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 underreport 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 (2013) confirms that the wage shock had no effect on the self-reported variables we use in our analysis.

### C. 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. 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. Table C1 gives a numerical example of the construction of our dependent variables. 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. Table 1 presents summary statistics of the main outcome variables; the discrepancy between official and survey amounts is stark, but at leakage rates of around 75 percent within the range of corruption estimates across developing nations, other programs in India, as well as other estimates of corruption in NREGS in Orissa.<sup>18</sup>

<sup>18</sup> For example, Reinikka and Svensson (2004) find rates of 87 percent in a schooling program in Uganda, while Ferraz, Finan, and Moreira (2012) find leakage of up to 55 percent in a schooling program in Brazil. In the Indian context, Khera (2011) finds leakage rates of almost 90 percent in the flagship food subsidy program (TPDS) in Bihar, while a study done by an NGO (Center for Science and Environment) on corruption in the NREGS in Orissa found almost precisely the same numbers (75 percent).

TABLE 1—SUMMARY STATISTICS OF MAIN REGRESSION VARIABLES

	Observations	Mean	SD
Official DW days	13,054	3.31	6.30
Actual DW days	13,054	0.88	1.55
Official PR payments	7,320	94.08	259.70
Actual PR payments	7,320	12.96	43.43
<i>FwdWageFrac</i>	13,908	0.67	0.40

Notes: This table provides summary descriptions of the aggregated variables used in the main result Tables 3 and 4. 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.

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 May 1 period, and a number of time-varying controls summarized by  $\mathbf{T}_t$  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. **Certain specifications also include regression-discontinuity type controls where the *Shock* indicator is interacted with time trends.** Finally, we include indicators for political reservations  $\mathbf{R}_p$  and in **some specifications district fixed effects  $\delta_{d(p)}$**  to capture variation in program implementation across locations.<sup>19</sup>

(3) 
$$\hat{y}_{pt} = \beta_0 + \beta_1 y_{pt} + \beta_2 Shock_t + \mathbf{T}_t' \gamma + \mathbf{R}_p' \zeta + \delta_{d(p)} + \epsilon_{pt}.$$

**Standard errors are clustered at the panchayat as well as the day level using multi-way clustering.** 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 6 and the discussion in Section IVD).

**Identification in (3) rests on the assumption that unobserved factors affecting  $\hat{y}_{pt}$  are orthogonal to *Shock*, 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 25, 2007. Because of its proximity to the daily-wage change in Orissa this shock limits

<sup>19</sup> Key political positions in some villages are reserved by law for women and/or ethnic minorities.

the value of Andhra Pradesh as a control for high-frequency confounds, although it is still useful for low-frequency ones. Keeping these limitations in mind, we estimate

$$(4) \quad \hat{y}_{pt} = \beta_0 + \beta_1 y_{pt} + \beta_2 ORshock_t \times OR_p + \beta_3 APShock1_t \times AP_p \\ + \beta_4 APShock2_t \times AP_p + \beta_5 ORshock_t + \beta_6 APShock1_t \\ + \beta_7 APShock2_t + OR_p + \mathbf{T}'_t \gamma + \mathbf{R}'_p \zeta + \delta_{d(p)} + \epsilon_{pt},$$

where  $OR_p$  ( $AP_p$ ) indicates panchayats in Orissa (Andhra Pradesh). The coefficient of interest in this specification is  $\beta_2$ , 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 3 we need an empirical analogue to  $\phi$ , 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, we partition the set of panchayats into those that do and do not ever run piece-rate projects and estimate

$$(5) \quad \hat{y}_{pt} = \beta_0 + \beta_1 y_{pt} + \beta_2 Shock_t + \beta_3 Shock_t \times AlwaysDW_{pt} \\ + \beta_4 AlwaysDW_{pt} + \mathbf{T}'_t \gamma + \mathbf{R}'_p \zeta + \delta_{d(p)} + \epsilon_{pt}$$

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*. In this case we expect  $\beta_2 < 0$  while  $\beta_3 > 0$ .

While specification (5) has a simple differences-in-differences interpretation, we can obtain a more stringent test of the theory by isolating the differential response attributable only 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. Figure 2 plots the distribution of *FwdWageFrac* in our sample. Given the existence of clear mass points at zero and one 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*).<sup>20</sup> We then allow the effects of the wage change to vary across these categories:

$$(6) \quad \hat{y}_{pt} = \beta_0 + \beta_1 y_{pt} + \beta_2 Shock_t + \beta_3 Shock_t \times FdwAll_{pt} + \beta_4 FdwAll_{pt} \\ + \beta_5 Shock_t \times FdwSome_{pt} + \beta_6 FdwSome_{pt} + \mathbf{T}'_t \gamma + \mathbf{R}'_p \zeta + \delta_{d(p)} + \epsilon_{pt}.$$

<sup>20</sup> We have also estimated more restrictive models in which *FwdWageFrac* enters linearly and obtained qualitatively similar results (available on request).

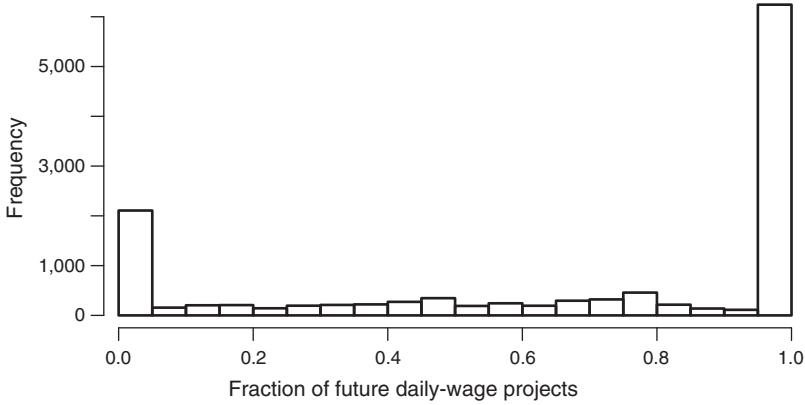


FIGURE 2. DISTRIBUTION OF FUTURE DAILY-WAGE PROJECT FRACTION

Note: Plots distribution of projects in study panchayats by the fraction of projects in the subsequent two months that were daily-wage projects.

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

To provide more insight into 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:<sup>21</sup>

$$\begin{aligned}
 (7) \quad \hat{y}_{pt} = & \beta_0 + \beta_1 y_{pt} + \beta_2 Shock_t + \beta_3 Shock_t \times FdwAll_{pt} + \beta_4 Shock_t \times BdwAll_{pt} \\
 & + \beta_5 Shock_t \times FdwSome_{pt} + \beta_6 Shock_t \times BdwSome_{pt} \\
 & + \beta_7 FdwAll_{pt} + \beta_8 BdwAll_{pt} + \beta_9 FdwSome_{pt} + \beta_{10} BdwSome_{pt} \\
 & + \mathbf{T}'_t \gamma + \mathbf{R}'_p \zeta + \delta_{d(p)} + \epsilon_{pt}.
 \end{aligned}$$

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

Table 1 presents summary statistics of the main variables used in our regressions.

<sup>21</sup> The correlation between *FwdWageFrac* and *BkWageFrac* is 0.75 within district, 0.6 within blocks, and 0.11 within panchayats; between *FwdWageFrac* and the *current* daily-wage fractions, the correlations are 0.85, 0.76, and 0.41, respectively. The results must be interpreted with these correlations in mind.

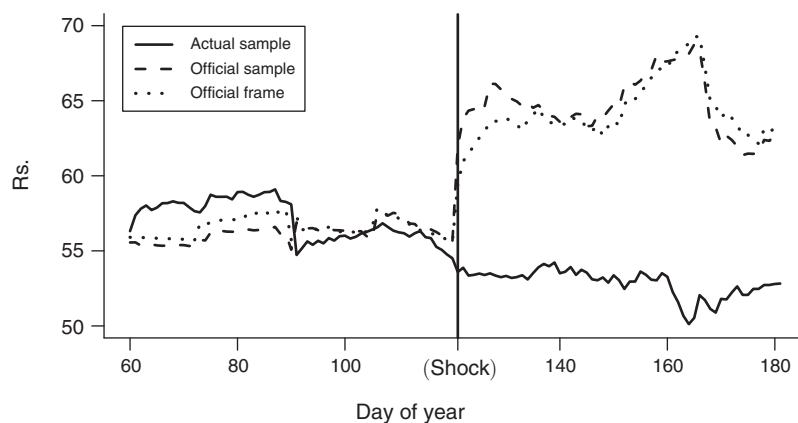


FIGURE 3. DAILY-WAGE RATES PAID

*Notes:* 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 1, 2007, the start of the study period; day 121 to May 1, 2007, the date of the wage shock; and day 181 to June 30, 2007, the end of the study period.

#### IV. Results: The Golden Goose Effect

##### *A. Preliminaries: Wages, Projects, and Rents*

We begin with tests of the main identifying assumptions. Figure 3 shows that the policy change was actually implemented: the average wage rate officially claimed on daily-wage projects hovers near Rs 55 until May 1 and then jumps up sharply thereafter. Interestingly, it does not immediately or permanently reach the new statutory wage of Rs 70. This is because not all panchayats implemented the change—some continued to claim the old rates after May 1, likely because they were not immediately informed about the change.<sup>22,23</sup> We also examined changes in the use of the “skilled” wage categories after May 1 and found a small decrease in the proportion of wage spells for which skilled wages were claimed, from 7.5 percent prior to May 1 to 6.3 percent after May 1. **While we cannot reliably assess the “true” skill level of any given spell, this decrease is consistent with the hypothesis that there is some skill inflation going on and that golden goose effects led officials to do less of it after the wage change.**

Figure 3 also reveals that the wage rate actually received by workers was unaffected by the shock; if anything it trends slightly downwards, though this effect is largely compositional and disappears once we control for district effects. In a companion paper we examine the determination of actual wages in some detail (Niehaus and Sukhtankar 2013). We find, *inter alia*, that while 72 percent of respondents were

<sup>22</sup> In Niehaus and Sukhtankar (2013), we show that panchayats that are closer to block and district offices are more likely to implement the wage change.

<sup>23</sup> 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 nonimplementing panchayats for us to precisely estimate the difference.

aware that the wage had changed and 81 percent of these correctly identified the new wage, these “aware” workers did not earn higher wages after May 1 relative to their less-aware peers. Similarly, literate workers were no more likely to see their wages increase. For further analysis and interpretation of these facts we refer the reader to Niehaus and Sukhtankar (2013); our analysis here will focus on testing our theoretical predictions about overreporting, taking the observed wage dynamics as given.<sup>24,25</sup>

Second, we check whether preshock rent extraction from daily-wage and piece-rate projects are similar, as predicated by Proposition 3. 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 preshock; Rs 236 per actual day worked in daily-wage projects as opposed to Rs 221 in piece-rate projects.<sup>26</sup> This is important both because it allows us to test Proposition 3 and also because it implies that officials would have had little incentive to distort project types prior to the wage change.

Finally, 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 percent before May 1 to 72 percent afterwards. More formally, Table 2 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 predetermined. They are also natural given that changing the designation of project is a relatively observable form of cheating.

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 correlation 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).

<sup>24</sup> Another intriguing feature of Figure 3 is that during the first month of our study period workers were on average *overpaid*. This pattern is driven by observations from Gajapati district where prevailing market wages were higher than the statutory program wage. If officials do not pay this prevailing market wage, workers will not participate in the program. If workers do not participate, officials cannot extract rents. Hence, according to local NGOs, officials in such areas overpay workers for participation, even though they report the correct statutory program wage on official reports, making up the difference by overreporting days worked.

<sup>25</sup> Cross-sectional variation in wages suggests another potential test of the golden goose effect: we would expect to see officials taking more risk in locations where the market wage  $\underline{w}$  is larger relative to the statutory wage  $\bar{w}$ . In results available on request we find that rent extraction is indeed (insignificantly) higher in panchayats with lower market wages, as predicted by their endowments of land and labor.

<sup>26</sup> 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 percent of spells are reported correctly, whereas 15 percent of piece-rate spells are reported as daily-wage spells.



TABLE 2—WAGE SHOCK EFFECTS ON PROJECT COMPOSITION

Regressor	(1)	(2)	(3)
<i>Shock</i>	0.014 (0.021)	0.007 (0.019)	0.008 (0.018)
<i>Day</i>	0.001 (0.001)	0.001 (0.001)	−0.003 (0.002)
<i>Day</i> <sup>2</sup>			0.002 (0.001)
District FEs	N	Y	Y
Observations	12,103	12,103	12,103
<i>R</i> <sup>2</sup>	0.046	0.097	0.098

Notes: 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*<sup>2</sup> has been rescaled 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 parentheses.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

### B. Overreporting of Days Worked in Daily-Wage Projects

We begin our core analysis by examining the reported number of days worked on daily-wage projects. Figure 4 shows the evolution of overreporting 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 overreporting on holidays as particularly risky. The 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 May 1. The fitted models suggest that there was a slight increase in daily-wage overreporting following the shock. This may seem surprising given the obvious effect of the wage hike on incentives for overreporting, but as Proposition 1 suggests there may also be a countervailing dynamic effect.

Columns 1–3 in panel A of Table 3 present a disaggregated analysis based on equation (3). Column 1 presents estimates of the basic specification (equation (3)) with a linear time trend and no location effects; column 2 adds district fixed effects, while column 3 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 4–6 of panel A separate panchayats that ran solely daily-wage projects from those that also ran piece-rate projects (equation (5)). We find a differential reduction in overreporting in the daily-wage only panchayats, significant at the 10 percent level; summing the point estimates implies a small *reduction* in overreporting in these locations. In contrast, the estimated effect of the wage change in panchayats that ran at least some piece-rate

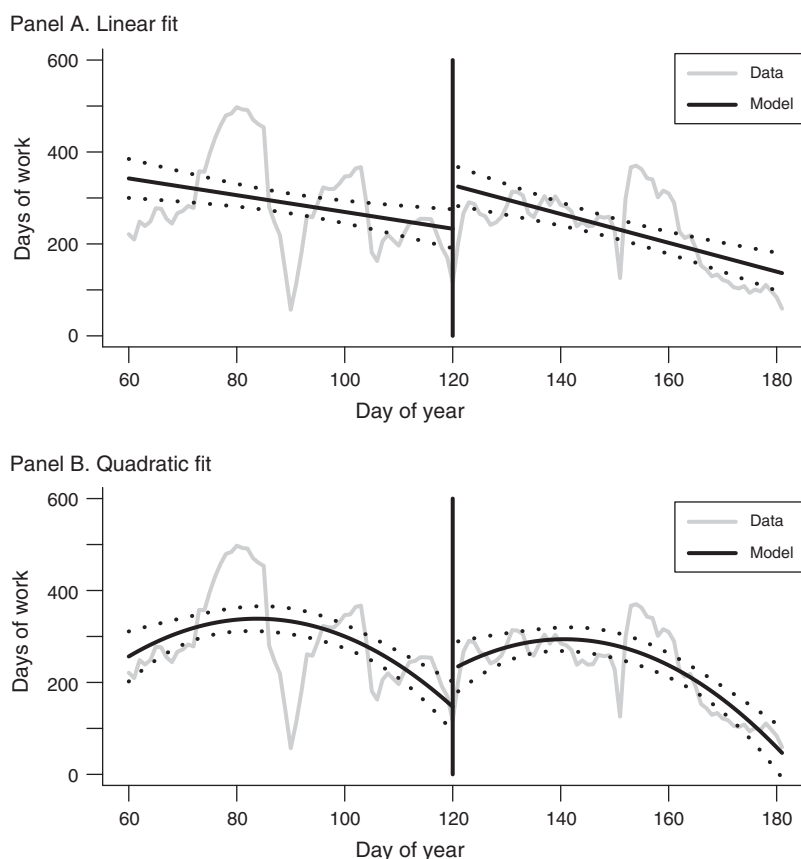


FIGURE 4. DAILY WAGE CORRUPTION MEASURES WITH DISCONTINUOUS POLYNOMIAL FITS

*Notes:* Plots daily series of the total amount of overreporting of work days on daily wage projects. Day 60 corresponds to March 1, 2007, the start of the study period; day 121 to May 1, 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 (panel A) and quadratic (panel B) terms in day-of-year; dotted lines represent 95 percent confidence intervals.

projects is larger and significant in column 4. 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, and in order to test Proposition 3, columns 1–3 of panel B report estimates of the interaction between the wage shock and categories of our constructed *FwdWageFrac* measure (equation (6)). The estimated direct effect of the wage hike increases again and is significant at the 5 percent level; the interpretation is that this is the price effect that would obtain in a panchayat with *no* future daily-wage projects planned. Note that this result also rules out alternative explanation based on strong diminishing marginal returns to income, such as income “targeting.” The differential effect in panchayats with solely wage projects upcoming is negative and significant at the 10 percent level, while the differential effect in

TABLE 3—WAGE SHOCK EFFECTS ON DAILY-WAGE REPORTS

Regressor	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Wage shock effects</i>						
<i>Shock</i>	0.95 (0.78)	0.94 (0.78)	0.89 (0.78)	1.30* (0.79)	1.29 (0.79)	1.24 (0.80)
<i>Shock</i> × <i>AlwaysDW</i>				−1.75* (1.00)	−1.74* (0.98)	−1.75* (0.99)
<i>AlwaysDW</i>				2.12** (0.83)	2.27*** (0.86)	2.28*** (0.86)
Observations	12,810	12,810	12,810	12,810	12,810	12,810
<i>R</i> <sup>2</sup>	0.08	0.09	0.09	0.09	0.10	0.10
<i>Panel B. Wage shock dynamic effects</i>						
<i>Shock</i>	2.39** (0.95)	2.31** (0.96)	2.25** (0.95)	3.05** (1.22)	3.00** (1.23)	3.00** (1.23)
<i>Shock</i> × <i>FdwAll</i>	−1.94* (1.07)	−1.84* (1.07)	−1.80* (1.07)	−4.03*** (1.38)	−3.78*** (1.36)	−3.78*** (1.37)
<i>Shock</i> × <i>FdwSome</i>	−1.15 (1.03)	−1.12 (1.03)	−1.08 (1.02)	−0.21 (0.94)	−0.17 (0.94)	−0.17 (0.94)
<i>Shock</i> × <i>BdwAll</i>				2.27 (1.50)	2.13 (1.46)	2.12 (1.47)
<i>Shock</i> × <i>BdwSome</i>				−1.99** (0.94)	−2.03** (0.97)	−2.03** (0.97)
Observations	11,386	11,386	11,386	10,651	10,651	10,651
<i>R</i> <sup>2</sup>	0.09	0.09	0.09	0.13	0.14	0.14
Time controls	<i>Day</i>	<i>Day</i>	<i>Shock</i> × <i>Day</i>	<i>Day</i>	<i>Day</i>	<i>Shock</i> × <i>Day</i>
District FEs	<i>N</i>	<i>Y</i>	<i>Y</i>	<i>N</i>	<i>Y</i>	<i>Y</i>

Notes: 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 one on and after May 1, 2007; in columns 3 and 6, 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 one 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 parentheses.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

panchayats with a mix of upcoming projects is negative but insignificant, providing support for Proposition 3.<sup>27</sup>

To better understand what drives these patterns of substitution, columns 4–6 of panel B present specifications that allow for both the future and the past to predict responsiveness to the shock (equation (7)). 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,

<sup>27</sup> One potential concern about these results is that intertemporal substitution occurs mechanically because of the 100 day limit on participation per household-year. In practice, however, we found that this limit was rarely reached. During fiscal year 2006–2007 only 4 percent of jobcards in our study area in Orissa reached 100 days, and all panchayats in our sample had a significant number of jobcards with less than 100 days—95 percent of the cards on average and at a minimum 22 percent.

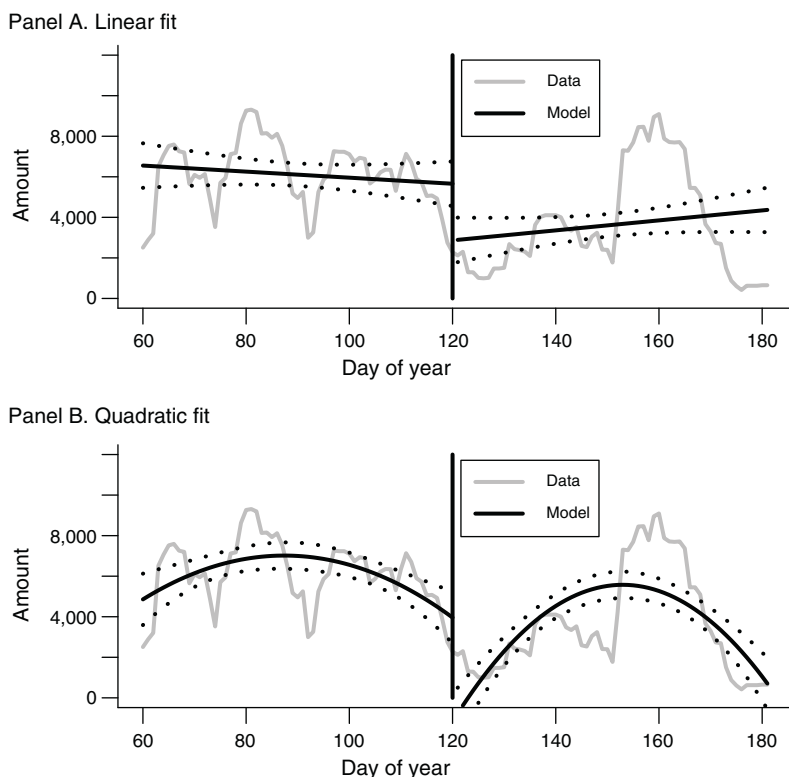


FIGURE 5. PIECE RATE CORRUPTION MEASURES WITH DISCONTINUOUS POLYNOMIAL FITS

*Notes:* Plots daily series of the total amount of theft on piece rate projects. Day 60 corresponds to March 1, 2007, the start of the study period; day 121 to May 1, 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 (panel A) and quadratic (panel B) terms in day-of-year; dotted lines represent 95 percent confidence intervals.

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.

### C. 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 2). Figure 5 shows the evolution of the gap between official and actual payments on

TABLE 4—WAGE SHOCK EFFECTS ON PIECE-RATE REPORTS

Regressor	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Wage shock effects</i>						
<i>Shock</i>	−78.31** (39.91)	−78.43* (40.29)	−75.9* (40.08)	−81.76** (40.26)	−82.18** (40.66)	−79.87** (40.58)
<i>Shock × AlwaysPR</i>				15.44 (50.43)	16.64 (49.80)	17.58 (49.36)
<i>AlwaysPR</i>				−35.29 (33.87)	−33.19 (34.83)	−33.58 (34.73)
Observations	7,076	7,076	7,076	7,076	7,076	7,076
<i>R</i> <sup>2</sup>	0.04	0.05	0.05	0.04	0.05	0.05
<i>Panel B. Wage shock dynamic effects</i>						
<i>Shock</i>	−38.58 (67.50)	−40.47 (66.52)	−38.18 (67.18)	−63.69 (73.19)	−62.16 (72.35)	−60.53 (72.34)
<i>Shock × FdwAll</i>	−24.88 (69.39)	−20.36 (67.39)	−23.75 (68.79)	−44.14 (93.40)	−31.83 (90.06)	−39.19 (93.11)
<i>Shock × FdwSome</i>	−74.61 (72.18)	−73.94 (69.87)	−72.84 (69.81)	−74.85 (95.70)	−73.83 (94.34)	−73.46 (94.20)
<i>Shock × BdwAll</i>				109.23 (81.61)	105.72 (81.84)	113.68 (84.81)
<i>Shock × BdwSome</i>				11.94 (89.23)	5.17 (89.35)	8.55 (90.37)
Observations	6,543	6,543	6,543	6,209	6,209	6,209
<i>R</i> <sup>2</sup>	0.08	0.08	0.08	0.11	0.11	0.12
Time controls	<i>Day</i>	<i>Day</i>	<i>Shock × Day</i>	<i>Day</i>	<i>Day</i>	<i>Shock × Day</i>
District FEs	N	Y	Y	N	Y	Y

*Notes:* 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 3 and 6, 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 one 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 parentheses.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

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 May 1. 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 4 mirrors Table 3 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 1 of panel A, the main effect of the wage shock is negative and significant at the 5 percent level, providing strong

support for Proposition 2. 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 of Rs 102. Columns 2–3 show that while the coefficient does not change much, standard errors are slightly larger and the result is hence significant at the 10 percent level. Columns 4–6 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 overreporting 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 for Proposition 2 and address concerns about time-varying confounds we next use Andhra Pradesh as a control. Table 5 reports estimates of equation (4), the difference-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.

#### *D. 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 (i) the tenure of bureaucrats in the relevant postings is quite short (approximately a year), and (ii) 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. Nevertheless, columns 1 and 2 (6 and 7) of Table 6 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 issue has to do with the exact timing of the effects we are attributing to the May 1 policy change. Equation (6) implicitly assumes that the dynamic effects



TABLE 5—EFFECTS ON PIECE-RATE REPORTS USING ANDHRA PRADESH AS A CONTROL

Regressor	(1)	(2)	(3)
<i>OR Shock</i> × <i>OR</i>	−87.86** (38.81)	−87.90** (38.77)	−87.54** (38.86)
<i>AP Shock 1</i> × <i>AP</i>	−21.29 (30.09)	−21.45 (29.99)	−21.03 (30.14)
<i>AP Shock 2</i> × <i>AP</i>	117.84*** (33.87)	117.95*** (33.83)	119.38*** (34.05)
<i>OR Shock</i>	31.15 (32.51)	31.40 (32.38)	53.64 (32.88)
<i>AP Shock 1</i>	61.08** (27.42)	60.69** (27.50)	23.38 (25.78)
<i>AP Shock 2</i>	−24.34 (25.89)	−24.71 (25.85)	−63.81** (26.00)
Actual PR payments	0.19** (0.08)	0.19** (0.08)	0.19** (0.08)
Time controls	Day	Day	Shock × Day
FEs	State	District	District
Observations	16,470	16,470	16,470
<i>R</i> <sup>2</sup>	0.06	0.06	0.06

*Notes:* 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 3, it is the intercept difference at the time the shock occurs. “*AP Shock 1*” is an indicator equal to one on and after March 5, 2007, while “*AP Shock 2*” equals one 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 parentheses.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

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 May 1 wage change we study was the culmination of a process that began on January 10 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 reran our main specifications using more flexible functions of time. Columns 4 and 9 of Table 6 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* categories (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.

We have also examined the sensitivity of the results to allowing for quadratic 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 results. Another alternative interpretation is that the

TABLE 6—ROBUSTNESS CHECKS

Regressor	Daily wage				Piece rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Shock</i>	2.03** (0.89)	1.69** (0.84)		2.44** (0.97)	−69.45 (65.13)	−26.96 (75.21)	−37.90 (67.03)	
<i>Shock</i> × <i>FdwAll</i>	−1.75* (0.99)	−1.35 (1.04)		−1.94* (1.09)	1.98 (63.27)	−44.15 (77.92)	−22.86 (68.91)	
<i>Shock</i> × <i>FdwSome</i>	−1.28 (0.92)	−0.33 (0.98)		−1.19 (1.04)	−51.87 (64.72)	−96.82 (80.02)	−81.61 (72.31)	
April			0.29 (1.25)				103.47 (67.50)	
May			2.76 (1.77)				42.39 (76.83)	
June			3.37 (2.24)				205.76 (154.60)	
April × <i>FdwAll</i>			−0.35 (1.41)				−125.72** (57.67)	
May × <i>FdwAll</i>			−1.75 (1.71)				−39.77 (50.69)	
June × <i>FdwAll</i>			−2.60 (1.92)				−167.24 (119.43)	
Time window (months)	1	3	2	2	1	3	2	2
Observations	10,740	11,740	11,386	11,386	6,250	6,653	6,543	6,543
$R^2$	0.11	0.09	0.09	0.04	0.09	0.08	0.09	0.08

Notes: Each observation is a panchayat-day. The dependent variable in columns 1–3 is the number of days of daily-wage work officially reported; in column 4, the difference between this quantity and the number of days of daily-wage work reported by participants; in columns 5–7, the total amount paid out on piece-rate projects as officially reported; and in column 8, 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 one on and after May 1, 2007. “*FdwAll*” is equal to one if the proportion of daily wage project-days in the panchayat in the next two months is equal to 1. “*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. All columns include controls for the actual quantities of work done/amounts received, 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 3 and 7 which omit the polynomial in day-of-month. Robust standard errors multi-way clustered by panchayat and day are presented in parentheses.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

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: this makes the daily-wage results stronger, while leaving piece-rate results unchanged. (Results available on request).

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 one, but otherwise is more restrictive. In practice, imposing that restriction makes little

TABLE 7—ADDITIONAL OUTCOME VARIABLES

Regressor	DW+PR	Daily wage				Piece rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Shock</i>	105.40 (82.45)	138.55 (97.14)	174.64** (77.29)	1.21 (1.17)	0.79 (0.84)	−25.30 (39.99)	−34.97 (48.18)
<i>Shock</i> × <i>FdwAll</i>	−73.79 (94.20)	−69.49* (38.02)	−192.76*** (36.61)	−1.78** (0.74)	−0.88*** (0.33)	7.89 (80.37)	−5.96 (76.89)
<i>Shock</i> × <i>FdwSome</i>	−95.45 (95.29)	−39.40 (36.79)	22.35 (69.19)	0.08 (0.21)	−0.26*** (0.03)	3.76 (53.23)	−16.41 (36.14)
Time controls	Day	Day	Shock×Day	Day	Day	Day	Day
Fixed effects	District	District	District	District	District	District	District
Observations	12,103	11,386	10,651	9,885	10,433	5,614	5,828
<i>R</i> <sup>2</sup>	0.04	0.04	0.08	0.05	0.03	0.08	0.06

Notes: Each observation is a panchayat-day. The dependent variable in column 1 is total extraction from daily-wage and piece-rate projects. In columns 2–3 it is the total value extracted from daily-wage projects. In columns 4–7 it is the number of daily-wage work done or piece-rate amounts for “fictitious” households as officially reported. “*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 “*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. All columns include controls for the actual quantities of work done/amounts received, 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. Robust standard errors multi-way clustered by panchayat and day are presented in parentheses.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

difference for the results (columns 4 and 8). We have also used the difference in total amounts extracted as the dependent variables, and as Table 7 shows again the results are very similar. This table also shows various other outcome variables: the total rents combined from piece-rate and daily-wage projects, as well as official reports for only “fictitious” households. The daily-wage results for the fictitious households are strongly statistically significant.

### E. 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 with more wage projects upcoming experienced the largest increases in scrutiny this could explain the role of *FwdWageFrac* categories in predicting responses to the wage shock. Of course, if this were true then again one would expect *BkWageFrac* categories 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 at 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. Of course these visits could have been for planning as well as monitoring purposes. In our Orissa sample, 62 percent of panchayats had a BDO visit and 24 percent 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.<sup>28</sup> 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. Let  $p(\tau|\theta, d)$  be the probability that a panchayat in district  $d$  receives a visit at time  $\tau$ . Assume that  $p$  has the logit form

$$(8) \quad p(t|\theta, d) = \frac{\exp\{\delta_d + \gamma 1(t \geq t^*) + f(t)\}}{1 + \exp\{\delta_d + \gamma 1(t \geq t^*) + f(t)\}}.$$

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  $t$ :

$$(9) \quad f(t|\theta, d) = p(t|\theta, d) \cdot \prod_{\tau=t+1}^T (1 - p(\tau|\theta, d)).$$

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

$$(10) \quad \prod_{\tau=\underline{t}}^T (1 - p(\tau|\theta, d)),$$

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  $\gamma = 0$ . Table 8 reports the results. The estimate of  $\gamma$  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. In short, we find no evidence of an increase in monitoring intensity associated with the change in the daily wage.<sup>29</sup>

<sup>28</sup>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.

<sup>29</sup>One natural question is how closely officials' expectations of changes in monitoring intensity corresponded to actual changes. While we cannot directly measure their beliefs, we can examine changes in monitoring surrounding earlier wages changes, which arguably shed light on what officials might reasonably have expected following the May 1, 2007 reform. We estimated models analogous to those in Table 8 for two earlier reforms, a February 2006 daily-wage increase and an April 2006 piece-rate increase. We find that the estimated impact of these reforms (not reported) on visit probabilities is negative in all but one specification. We read these results as suggestive that officials should, if anything, have expected a small reduction in monitoring.

TABLE 8—ML ESTIMATES OF CHANGING AUDIT PROBABILITIES OVER TIME

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

Notes: 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*<sup>2</sup>” are time trends. Koraput, Rayagada, and Gajapati are indicators for the three study districts in Orissa.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

### F. Interpreting Magnitudes

Given the confidence intervals around some of our coefficients, their magnitudes should be interpreted cautiously. With that caveat in mind we provide two calculations as benchmarks. First, we compare the actual increase in theft due to the shock to the counterfactual effect of a *temporary* wage hike without golden goose effects. We estimate that the permanent increase in daily wages that we study raised theft by 64 percent less than a temporary increase of the same magnitude would have, indicating that golden goose effects had a substantial “dampening” effect.<sup>30</sup> However, the 90 percent confidence interval around that estimate is 8–120 percent, suggesting that this number must be interpreted with caution. Second, we compare the magnitude of golden goose effects to the effects of other anti-corruption interventions studied in the literature. We estimate that the dynamic effects of the wage change lowered daily-wage overreporting and piece-rate theft by 49 percent and 77 percent, respectively.<sup>31</sup> These are meaningful effect sizes in comparison with other estimates from the literature. For example, Olken (2007) estimates that increasing the

<sup>30</sup> We estimate the actual increase in theft due to the shock as the sum of three components: (i) a mechanical component equal to the predicted quantity of daily-wage overreporting *absent* the shock multiplied by the change in the average daily wage, (ii) a behavioral response in daily-wage overreporting, which we estimate using the coefficients from column 2, panel B of Table 3, and (iii) a negative behavioral response in piece-rate theft, estimated using the coefficient in column 2, panel A of Table 4 (a conservative assumption given that the difference-in-differences estimates of the latter effect are larger). We sum these effects to obtain an estimate  $\Delta_{actual}$  of the total effect of the shock on rent extraction. To construct a counterfactual estimate of the effect  $\Delta_{counter}$  of 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. Putting these pieces together, we estimate  $\frac{\Delta_{counter} - \Delta_{actual}}{\Delta_{counter}} = 64$  percent.

<sup>31</sup> We estimate golden goose effects on daily-wage overreporting as the interaction coefficient from column 2, panel B of Table 3 multiplied by the average fraction of future projects that are daily wage, divided by mean daily-wage overreporting prior to the shock. Similarly, we estimate golden goose effects on piece-rate theft as the coefficient in column 2, panel A of Table 4 divided by mean piece-rate theft prior to the shock.

probability of audit from 4 percent to 100 percent reduced corruption on Indonesian road projects by 30 percent; Ferraz and Finan (2011) estimate that Brazilian mayors who are eligible for reelection misappropriate 27 percent fewer resources than those who are not.

For policy purposes it would be informative to conduct a complete calibration of our model. Unfortunately this is infeasible without richer data on *all* the sources of rent which a corrupt official would lose if suspended or fired, and the value of their outside options. We can, however, provide some sense of whether NREGS rents are a significant source of income relative to licit compensation. We estimate total NREGS rents per panchayat (or block) per month by calculating the difference between actual and reported payments in our sample multiplied by the inverse of the sampling probability, and compare these to sarpanch honorariums and BDO salaries as per the Government of Orissa's payscales (based on sixth Central Pay Commission). 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. These figures clearly suggest that optimal contracts should take the influence of illicit rents into account.

## V. 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 percent. Future work might focus on the longer-term implications of this effect for rent extraction.

## APPENDIX

### A. Proofs

#### PROOF OF PROPOSITION 1:

The official's problem during daily-wage periods is

$$\max_{\hat{n}} [(\bar{w} - w')n' + (\hat{n} - n')\bar{w} + \beta(1 - \pi(\hat{n}, n'))\bar{V}(\bar{w}, \phi)].$$

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



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

Substitution in the first-order condition yields

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

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

#### PROOF OF PROPOSITION 2:

The official's problem during piece-rate periods is

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

The posited attributes of  $\mu$  ensure that this problem has an interior solution satisfying the Kuhn-Tucker condition  $\bar{r} = \beta \mu_{\hat{q}}(\hat{q}, q^t) \bar{V}(\bar{w}, \phi)$ . Since  $(\bar{r}, r^t, q^t)$  are fixed we know that  $\hat{q}^t \bar{r} - q^t r^t$  moves with  $\hat{q}^t$ . Differentiating with respect to  $\bar{w}$  yields

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

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

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

#### PROOF OF PROPOSITION 3:

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

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

with

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

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

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

As before,  $(\bar{r}, r^t, q^t)$  fixed imply that  $\hat{q}^t\bar{r} - q^t r^t$  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

TABLE B1—CHARACTERISTICS OF SPELLS IN SAMPLE FRAME, INITIAL SAMPLE, AND REACHED SAMPLE

Variable	All spells		Sampled spells		Reached spells		<i>p</i> -value
	Mean	SD	Mean	SD	Mean	SD	
Age	37.60	14.93	37.37	13.60	37.55	13.28	0.33
Male	0.54	0.50	0.54	0.50	0.54	0.50	0.67
SC/ST	0.78	0.41	0.79	0.41	0.77	0.42	0.05
Post	0.40	0.49	0.43	0.49	0.42	0.49	0.57
Spell length	11.13	2.92	11.14	3.01	11.09	3.14	0.33
Wage spell	0.83	0.37	0.83	0.38	0.84	0.36	0.20
Daily rate	63.48	17.24	64.37	20.34	63.90	18.92	0.30

*Notes:* Reports summary statistics at the work-spell level using official records and for (i) the universe of spells sampled from ( $N = 111,172$ ), (ii) the initial sample of work spells we drew ( $N = 7,126$ ), and (iii) the work spells done by households we were ultimately able to interview ( $N = 4,794$ ). 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.

in any way.<sup>32</sup> 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 B1 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 1,499 households we did attempt to reach, we managed to reach or confirm the nonexistence/permanent migration/death of 1,408 households. In order to determine whether an individual/household that was included in the official records was actually nonexistent or dead or no longer lived in the village, we asked surveyors to confirm the status with three 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 1,328 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.<sup>33</sup> Those households that actually worked on NREGS are

<sup>32</sup> 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](http://news.bbc.co.uk/2/hi/south_asia/7486252.stm).

<sup>33</sup> 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 B2—SAMPLE DESCRIPTION

Variable	NREGA participants			Nonparticipants		
	Observations	Mean	SD	Observations	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

*Notes:* 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 1 and June 30, 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.

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 Below Poverty Line cards, only 27 percent 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).

### C. Additional Appendix Tables

TABLE C1—NUMERICAL EXAMPLE OF DEPENDENT VARIABLE CONSTRUCTION

Worker	Report	Attributed work by day					
		May 1	May 2	May 3	May 4	May 5	May 6
A	3 days between May 1–6	0.5	0.5	0.5	0.5	0.5	0.5
B	4 days from May 3–6	0	0	1	1	1	1
Totals:		0.5	0.5	1.5	1.5	1.5	1.5

*Notes:* This table presents numerical examples of how our dependent variables were aggregated up to the panchayat-day level from official and survey reports of work done. The rows show two typical reports of work done within a panchayat; the columns show how we attributed the number of days reported as worked across the period during which they were worked, and summed them up for each panchayat-day record.

TABLE C2—WAGE SHOCK EFFECTS ON DAILY-WAGE REPORTS, QUADRATIC TIME TRENDS

Regressor	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Wage shock effects</i>						
<i>Shock</i>	0.88 (0.78)	0.88 (0.79)	1.04 (0.98)	1.23 (0.79)	1.23 (0.80)	1.40 (0.94)
<i>Shock</i> × <i>AlwaysDW</i>				−1.75* (1.01)	−1.75* (0.99)	−1.73* (0.99)
<i>AlwaysDW</i>				2.14** (0.84)	2.28*** (0.86)	2.27*** (0.86)
Observations	12,810	12,810	12,810	12,810	12,810	12,810
<i>R</i> <sup>2</sup>	0.08	0.09	0.09	0.09	0.10	0.10
<i>Panel B. Wage shock dynamic effects</i>						
<i>Shock</i>	2.28** (0.94)	2.22** (0.95)	2.28* (1.26)	3.01** (1.24)	2.97** (1.24)	2.97* (1.53)
<i>Shock</i> × <i>FdwAll</i>	−1.83* (1.07)	−1.76* (1.06)	−1.78* (1.06)	−3.90*** (1.40)	−3.71*** (1.38)	−3.70*** (1.35)
<i>Shock</i> × <i>FdwSome</i>	−1.07 (1.02)	−1.05 (1.02)	−1.03 (1.02)	−0.17 (0.94)	−0.15 (0.94)	−0.11 (0.94)
<i>Shock</i> × <i>BdwAll</i>				2.17 (1.51)	2.07 (1.47)	2.10 (1.44)
<i>Shock</i> × <i>BdwSome</i>				−2.01** (0.95)	−2.04** (0.98)	−2.01** (0.94)
Observations	11,386	11,386	11,386	10,651	10,651	10,651
<i>R</i> <sup>2</sup>	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

*Notes:* 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 3 and 6, 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 parentheses.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

TABLE C3—WAGE SHOCK EFFECTS ON PIECE-RATE REPORTS, QUADRATIC TIME TRENDS

Regressor	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Wage shock effects</i>						
<i>Shock</i>	−78.02* (40.02)	−77.69* (40.25)	−107.05* (59.55)	−81.48** (40.38)	−81.52** (40.67)	−111.41** (56.18)
<i>Shock × AlwaysPR</i>				15.56 (50.35)	16.98 (49.60)	18.41 (48.96)
<i>AlwaysPR</i>				−35.38 (33.82)	−33.32 (34.78)	−34.25 (34.62)
Observations	7,076	7,076	7,076	7,076	7,076	7,076
<i>R</i> <sup>2</sup>	0.04	0.05	0.05	0.04	0.05	0.06
<i>Panel B. Wage shock dynamic effects</i>						
<i>Shock</i>	−37.46 (67.85)	−39.62 (66.82)	−83.01 (73.64)	−63.16 (73.25)	−61.93 (72.47)	−100.15 (84.32)
<i>Shock × FdwAll</i>	−27.71 (70.79)	−22.54 (68.47)	−20.67 (67.62)	−50.13 (96.55)	−37.42 (92.82)	−36.27 (91.82)
<i>Shock × FdwSome</i>	−74.57 (72.20)	−73.74 (69.9)	−69.65 (69.30)	−75.65 (96.15)	−74.54 (94.64)	−69.08 (93.13)
<i>Shock × BdwAll</i>				114.07 (84.33)	111.48 (84.62)	115.15 (84.54)
<i>Shock × BdwSome</i>				14.69 (90.81)	8.19 (90.69)	4.83 (89.31)
Observations	6,543	6,543	6,543	6,209	6,209	6,209
<i>R</i> <sup>2</sup>	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

*Notes:* 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 3 and 6, 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 parentheses.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.



TABLE C4—EFFECTS ON PIECE-RATE REPORTS  
USING ANDHRA PRADESH AS A CONTROL, QUADRATIC TIME TRENDS

Regressor	(1)	(2)	(3)
<i>OR Shock</i> × <i>OR</i>	−87.39** (38.94)	−87.31** (38.92)	−86.87** (38.93)
<i>AP Shock 1</i> × <i>AP</i>	−21.10 (30.29)	−21.24 (30.18)	−23.30 (30.18)
<i>AP Shock 2</i> × <i>AP</i>	119.97*** (34.13)	120.03*** (34.10)	119.74*** (34.08)
<i>OR Shock</i>	52.21 (32.00)	52.51 (31.94)	−35.07 (43.97)
<i>AP Shock 1</i>	−3.47 (26.52)	−3.57 (26.43)	18.89 (22.00)
<i>AP Shock 2</i>	−63.85*** (24.27)	−63.91*** (24.22)	−44.79 (27.81)
Actual PR payments	0.20** (0.08)	0.20** (0.08)	0.20** (0.08)
Time controls	Day2	Day2	Shock × Day2
FEs	State	District	District
Observations	16,470	16,470	16,470
<i>R</i> <sup>2</sup>	0.06	0.06	0.07

Notes: 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 3, 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 parentheses.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

## REFERENCES

- Acemoglu, Daron, and Thierry Verdier. 2000. “The Choice between Market Failures and Corruption.” *American Economic Review* 90 (1): 194–211.
- Ahlin, Christian. 2005. “Effects and (In)tractability of Decentralized Corruption.” <http://www.msu.edu/~ahlin/research/corruptnew.pdf>.
- Andvig, Jens Chr., and Karl Ove Moene. 1990. “How corruption may corrupt.” *Journal of Economic Behavior & Organization* 13 (1): 63–76.
- Banerjee, Abhijit V. 1997. “A Theory of Misgovernance.” *Quarterly Journal of Economics* 112 (4): 1289–1332.
- Barro, Robert. 1973. “The Control of Politicians: An Economic Model.” *Public Choice* 14 (1): 19–42.
- Becker, Gary S., and George J. Stigler. 1974. “Law Enforcement, Malfeasance, and Compensation of Enforcers.” *Journal of Legal Studies* 3 (1): 1–18.
- Bertrand, Marianne, Simeon Djankov, Rema Hanna, and Sendhil Mullainathan. 2007. “Obtaining a Driver’s License in India: An Experimental Approach to Studying Corruption.” *Quarterly Journal of Economics* 122 (4): 1639–76.
- Besley, Timothy, and John McLaren. 1993. “Taxes and Bribery: The Role of Wage Incentives.” *Economic Journal* 103 (416): 119–41.
- Cadot, Olivier. 1987. “Corruption as a gamble.” *Journal of Public Economics* 33 (2): 223–44.
- Campante, Felipe R., Davin Chor, and Quoc-Anh Do. 2009. “Instability and the Incentives for Corruption.” *Economics and Politics* 21 (1): 42–92.
- Chattopadhyay, Raghavendra, and Esther Duflo. 2004. “Women as Policy Makers: Evidence from a Randomized Policy Experiment in India.” *Econometrica* 72 (5): 1409–43.

- Das, S. K. 2001. *Public Office, Private Interest: Bureaucracy and Corruption in India*. New Delhi: Oxford University Press.
- de Zwart, Frank. 1994. *The Bureaucratic Merry-go-round*. Amsterdam: Amsterdam University Press.
- Di Tella, Rafael, and Ernesto Schargrodsky. 2003. "The Role of Wages and Auditing during a Crack-down on Corruption in the City of Buenos Aires." *Journal of Law & Economics* 46 (1): 269–92.
- Ferejohn, John. 1986. "Incumbent performance and electoral control." *Public Choice* 50 (1–3): 5–25.
- Ferraz, Claudio, and Frederico Finan. 2008. "Exposing Corrupt Politicians: The Effects of Brazil's Publicly Released Audits on Electoral Outcomes." *Quarterly Journal of Economics* 123 (2): 703–45.
- Ferraz, Claudio, and Frederico Finan. 2011. "Electoral Accountability and Corruption: Evidence from the Audits of Local Governments." *American Economic Review* 101 (4): 1274–1311.
- Ferraz, Claudio, Frederico Finan, and Daniela B. Moreira. 2012. "Corrupting Learning: Evidence from Missing Federal Education Funds in Brazil." National Bureau of Economic Research (NBER) Working Paper 18150.
- Holmström, Bengt. 1999. "Managerial Incentive Problems: A Dynamic Perspective." *Review of Economic Studies* 66 (1): 169–82.
- Iyer, Lakshmi, and Anandi Mani. 2009. "Traveling Agents: Political Change and Bureaucratic Turn-over in India." Harvard Business School Working Paper 09-006.
- Khera, Reetika. 2011. "Trends in Diversion of PDS Grain." Centre for Development Economics (CDE) Delhi School of Economics Working Paper 198.
- McMillan, Margaret. 2001. "Why Kill the Golden Goose? A Political-Economy Model of Export Taxation." *Review of Economics and Statistics* 83 (1): 170–84.
- Ministry of Law and Justice. 2005. "The National Rural Employment Guarantee Act (NREGA), 2005." *Gazette of India*. <http://nrega.nic.in/rajaswa.pdf> (accessed December 1, 2012).
- Ministry of Rural Development. 2006. *The National Rural Employment Guarantee Act 2005: Operation Guidelines 2006*. 2nd ed. New Delhi.
- Mookherjee, Dilip, and I. P. L. Png. 1995. "Corruptible Law Enforcers: How Should They Be Compensated?" *Economic Journal* 105 (428): 145–59.
- Nagin, Daniel S., James B. Rebitzer, Seth Sanders, and Lowell J. Taylor. 2002. "Monitoring, Motivation, and Management: The Determinants of Opportunistic Behavior in a Field Experiment." *American Economic Review* 92 (4): 850–73.
- Niehaus, Paul, and Sandip Sukhtankar. 2013. "The Marginal Rate of Corruption in Public Programs: Evidence from India." *Journal of Public Economics* 104 (C): 52–64.
- Niehaus, Paul, and Sandip Sukhtankar. 2013. "Corruption Dynamics: The Golden Goose Effect: Dataset." *American Economic Journal: Economic Policy*. <http://dx.doi.org/10.1257/pol.5.4.230>.
- Olken, Benjamin A. 2006. "Corruption and the costs of redistribution: Micro evidence from Indonesia." *Journal of Public Economics* 90 (4–5): 853–70.
- Olken, Benjamin A. 2007. "Monitoring Corruption: Evidence from a Field Experiment in Indonesia." *Journal of Political Economy* 115 (2): 200–49.
- Olken, Benjamin A. 2009. "Corruption perceptions vs. corruption reality." *Journal of Public Economics* 93 (7–8): 950–64.
- Olken, Benjamin A., and Rohini Pande. 2012. "Corruption in Developing Countries." *Annual Review of Economics* 4 (1): 479–509.
- Olson, Mancur. 2000. *Power and Prosperity: Outgrowing Communist and Capitalist Dictatorships*. New York: Basic Books.
- Persson, Torsten, Gérard Roland, and Guido Tabellini. 1997. "Separation of Powers and Political Accountability." *Quarterly Journal of Economics* 112 (4): 1163–1202.
- Reinikka, Ritva, and Jakob Svensson. 2004. "Local Capture: Evidence from a Central Government Transfer Program in Uganda." *Quarterly Journal of Economics* 119 (2): 679–705.
- Reinikka, Ritva, and Jakob Svensson. 2005. "Fighting Corruption to Improve Schooling: Evidence from a Newspaper Campaign in Uganda." *Journal of the European Economic Association* 3 (2–3): 259–67.
- Sequeira, Sandra, and Simeon Djankov. 2010. "An Empirical Study of Corruption in Ports." [http://econ.as.nyu.edu/docs/IO/14817/Sequeira\\_20100406.pdf](http://econ.as.nyu.edu/docs/IO/14817/Sequeira_20100406.pdf).
- Yang, Dean. 2008. "Can Enforcement Backfire? Crime Displacement in the Context of Customs Reform in the Philippines." *Review of Economics and Statistics* 90 (1): 1–14.

**This article has been cited by:**

1. Rema Hanna, Shing-Yi Wang. 2017. Dishonesty and Selection into Public Service: Evidence from India. *American Economic Journal: Economic Policy* 9:3, 262-290. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
2. Karthik Muralidharan, Paul Niehaus, Sandip Sukhtankar. 2016. Building State Capacity: Evidence from Biometric Smartcards in India. *American Economic Review* 106:10, 2895-2929. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
3. Gustavo J. Bobonis, Luis R. Cámara Fuertes, Rainer Schwabe. 2016. Monitoring Corruptible Politicians. *American Economic Review* 106:8, 2371-2405. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
4. Clément Imbert, John Papp. 2015. Labor Market Effects of Social Programs: Evidence from India's Employment Guarantee. *American Economic Journal: Applied Economics* 7:2, 233-263. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]