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# Reciprocity in Labor Relations: Evidence from a Field Experiment with Long-Term Relationships

Matthieu Chemin
Joost DeLaat
André Kurmann

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Chemin: McGill University and CIRPÉE
matthieu.chemin@mcgill.ca
DeLaat: World Bank
jdelaat@worldbank.org
Kurmann: UQÀM and CIRPÉE
kurmann.andre@gmail.com

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**Abstract:**
We followed field workers administering a household survey over a 12-week period and examined how their reciprocal behavior towards the employer responded to a sequence of exogenous wage increases and wage cuts. To disentangle the effects of reciprocal behavior from other explicit incentives that occur naturally in long-term employment relationships, we devised a novel measure of effort that not only captures the notion of work morale but that field workers perceived as unmonitored. While wage increases had no significant effect, wage cuts led to a strong and significant decline in unmonitored effort. This finding provides clear evidence of a highly asymmetric reciprocity response to wage changes. Our estimates further imply that field workers quickly adapted to higher wages and revised their reference point accordingly when deciding on reciprocity. Finally, we consider a second measure of effort that was explicitly monitored and found no significant effect to any of the wage changes. This lack of impact illustrates that explicit incentives can easily outweigh the effects of reciprocity and highlights the importance of having a measure of effort that workers perceive as unmonitored when testing for reciprocity in long-term relationships.

**Keywords:** Reciprocity, Gift exchange, Efficiency wages, Field experiment

**JEL Classification:** C93, J30

# 1 Introduction

Reciprocity in labor relations implies that workers derive a psychological benefit from returning a generous treatment by their firm with better work morale. Accordingly, even in the absence of explicit incentives, workers provide higher (lower) effort if the firm's wage offer is higher (lower) than some reference wage perceived as fair. Introduced into modern economics under the name of 'partial gift-exchange' and 'fair wage hypothesis' by Solow (1979) and Akerlof (1982), the theory provides an explanation for many labor market phenomena, ranging from unemployment to wage rigidity (e.g. Akerlof and Yellen, 1990).

Numerous studies have found empirical support for reciprocity in labor relations.[1] Yet, the exact consequences of reciprocity for actual labor markets remain largely unresolved. One of the main reasons is, perhaps, that the available evidence is predominantly based on short-term experiments whereas in actual labor markets workers and firms typically engage in long-term relationships. This raises a number of important questions. As workers get used to a given wage increase, does their perception of what constitutes a fair wage change and does this affect their reciprocal behavior? Do workers care more about wage cuts than they care about wage increases? Do explicit incentives that occur naturally in long-term relationships whenever there is monitoring crowd out the propensity to reciprocate?

The ideal experiment to test for reciprocity in long-term relationships consists of measuring the effects of exogenous wage changes on a dimension of effort that captures reciprocal behavior but is truly unmonitored in the eyes of the worker. Otherwise, it is impossible to disentangle the effects of reciprocal behavior from other explicit incentives such as firing threats or career motives. Empirically, observing an unmonitored dimension of effort is difficult because the very act of measuring effort (e.g. a piece rate) makes it likely that the worker perceives it as being monitored. Furthermore, if a firm can monitor effort, it is typically interested in using it as an explicit incentive device.

In this paper, we solve these empirical problems by conducting a field experiment in which we consider the effects of a sequence of exogenous wage changes on a measure of work

[1] See Kahneman, Knetsch and Thaler (1986); Blinder and Choi (1990); Agell and Lundborg (1995, 1999); Campbell and Kamlani (1997); Bewley (1999) and the surveys by Bewley (2002) and Rotemberg (2006) for interview evidence. Examples of laboratory experiments simulating worker-firm interactions are Fehr, Kirchsteiger et Riedl (1993); Fehr and Falk (1999); Hannan, Kagel and Moser (2002); Charness, Frechette and Kagel (2004) or Charness and Kuhn (2007). Fehr and Gaechter (2000a) provide an extensive survey of some of this evidence. There is also a more recent but growing body of field experiments testing for reciprocal behavior in labor relations. Among them are Gneezy and List (2006); Bellemare and Shearer (2009); Cohn, Fehr and Goette (2009); Kim and Slonin (2010); Kube, Marechal and Puppe (2010); and Cohn, Fehr, Hermann and Schneider (2011). We discuss the relation of our paper to some of these studies below.

effort that was computed only long after the employment relationship had ended. Since no indication of this ex-post control was given during the experiment, workers perceived this effort measure as unmonitored.

The experiment took place in rural Kenya where, over a 12-week period, local field workers were employed to administer a household survey of more than 900 questions to approximately 3,000 community members. Answers to different questions of the survey could contradict each other and field workers were expected to spot and resolve these inconsistencies. However, at no point during the employment relationship did the work supervisors attempt to check or punish in any way for inconsistencies, nor did anyone know that we would compute such a measure ex-post. In fact, the inconsistency statistics were computed via an algorithm only more than a year later after the survey answers had been manually entered into an electronic database. For all means and purposes of this experiment, inconsistencies therefore constitute a (inverse) measure of effort that field workers perceived as unmonitored. In addition, to assess the impact of explicit incentives, we consider a second measure of work effort, blanks and mistakes, on which field workers were monitored daily, with the clear understanding that insufficient performance in this dimension would lead to dismissal.

Field workers were paid per survey and the experiment consisted of the following wage changes. After six weeks of work at a constant wage that was several times higher than the going market wage, the wage was increased by 45%. Three weeks later, the wage was reduced back to the original level for one week. Finally, the wage was cut by 27% relative to the original wage for the last two weeks. The field workers did not know in advance about any of the wage changes, nor did they know that they were taking part in an experiment.

Local discontinuity tests and panel estimates reveal that the 45% increase in the wage did not have a significant effect on inconsistencies (our measure of unmonitored effort). By contrast, the decrease in the wage after the 3-week period of higher wages led to a large and significant increase in the rate of inconsistencies of about 35% relative to the rate before the wage increase *even though* the wage after this decrease was again exactly the same as before the wage increase. The wage cut of 27% below the initial wage rate during the last two weeks resulted in an additional significant increase in inconsistencies. Blanks and mistakes (our measure of monitored effort), on the other hand, did not respond significantly to any of the wage changes.

To interpret these results, we present an efficiency wage model of worker effort that allows for both explicit incentives from monitoring as in Shapiro and Stiglitz (1984) and reciprocity concerns as proposed by Rabin (1993). The model shows that if workers have no reciprocity concerns, unmonitored effort does not react to either positive or negative wage changes. The observed increase in inconsistencies (i.e. the drop in unmonitored effort) in response to the wage cuts therefore provides clear evidence of negative reciprocity. The finding that inconsistencies increase even when the wage returns to its initial level implies that workers use past wages as an important reference point in their assessment of what constitutes a fair wage. Our experiment thus fully confirms Bewley's (2002) conclusion from interviews with managers and labor leaders that *"...employees usually have little notion of a fair or market value for their services and quickly come to believe that they are entitled to their existing wage, no matter how high it may be..." (page 7).* Furthermore, the absence of a significant drop in inconsistencies after the wage increase is consistent with findings in laboratory experiments that the propensity to punish negative actions is stronger than the propensity to reward positive actions (e.g. Charness and Rabin, 2002). In our model, this asymmetry in reciprocal behavior obtains naturally either if workers have loss aversion or if the marginal productivity of the firm with respect to effort is decreasing.

The lack of any significant reaction of blanks and mistakes (our measure of monitored effort) illustrates the importance of testing for reciprocal behavior in long-term experiments with a dimension of effort that workers perceive as truly unmonitored. According to our model, this result obtains because the no-shirking constraint from monitoring binds across all wage changes, thus outweighing the workers' negative reciprocity concerns. At the same time, our finding of negative reciprocity for inconsistencies implies that the presence of explicit incentives does not necessarily crowd out reciprocal behavior, as suggested by some laboratory experiments (e.g. Fehr and Gächter, 2000b). Otherwise, workers would have provided minimal effort on inconsistencies throughout the entire experiment.

A possible concern about our results is that inconsistencies increased because of some idiosyncratic shocks that coincided with the exogenous wage cuts. The absence of a significant reaction of blanks and mistakes to any of the wage changes makes this a highly unlikely possibility. Nevertheless, a seemingly superior approach would be to control for unobserved shocks with a random control group of workers for which wages remain constant throughout the experiment. The problem with such a randomization for our experiment is that, as in most labor market situations, field workers all knew each other, making it impossible to prevent information spillovers. These spillovers could have led to potentially strong social comparison effects in the treatment group (e.g. Akerlof and Yellen, 1990), thus contaminating the estimated reciprocity effect of wage changes. In addition, the control group might have reacted to not receiving the treatment, with the sign of the resulting bias depending on whether the control group wished to emulate or oppose the treatment group.[2] Instead,

[2]Another way to prevent information spillover would be to set up an experimental environment in which

our strategy consists of following field workers through time and simulatenously subjecting all of them to the exogenous wage changes. Hence, the control group for a given field worker is the same field worker immediately before the wage changes (which were implemented in the middle of the week on otherwise uneventful days). The advantage of this strategy, which is close in spirit to the one adopted in another context by Bandiera et al. (2005), is that the estimates do not suffer from contamination biases and that we can control for all time-invariant sources of heterogeneity with worker fixed-effects, thus increasing statistical power. Moreover, to address the issue of potential time-varying unobservables, our panel estimations allow for flexible interactions with time effects.

Our paper contributes to a growing body of field experiments on reciprocity in labor relations (see footnote 1 for references). Together with Kube, Maréchal and Puppe (2010) and Cohn, Fehr, Hermann and Schneider (2011), we are the first to examine the effects of wage cuts on reciprocal behavior in an actual labor market situation. In Kube, Maréchal and Puppe (2010), workers performed a one-time task and received either a higher or lower compensation than the advertised wage. Workers with higher than expected compensation showed little evidence of increased productivity whereas workers with lower than expected compensation showed a strong negative reaction. In Cohn, Fehr, Hermann and Schneider (2011), workers were assigned to teams of two to perform an identical task at the same wage during one weekend. The following weekend, the wage was randomly lowered for either one or both workers of some teams. Wage cuts generally led to a significant decline in productivity but this decline was more than twice as large for workers whose team member's wage was not cut. By contrast, workers whose wages remained the same but witnessed their team member's wage being cut did not show a significant reaction in productivity. These results indicate that the worker's reference of what constitutes a fair wage is influenced importantly by expectations and social comparisons, and that the effect of deviations from this reference is asymmetric.

The novelty of our paper relative to these two studies – and, to our knowledge, all other field experiments on reciprocity in labor relations – is that we devise a measure of effort that is unmonitored in the eyes of the workers. This allows us to test for reciprocal behavior and in particular the presence of wage entitlement by following the same field workers over an extended period of time and estimating their effort response to actual wage changes. If we had instead adopted the usual approach in the literature and measured effort with a directly observable productivity variable, we would have had to limit our study to an experiment

the workplace of the treatment group is completely separated from the one of the control group. This is unlikely to solve the identification issue, however, since the two groups would then be subject to differing workplace conditions.

of very short duration so as to disentangle reciprocal behavior from explicit incentives that occur naturally in repeated employment interactions.

We also believe that our inconsistency measure captures in many ways the notion of work morale that the literature typically associates with reciprocal behavior; i.e. a cooperative attitude *"...whereby gaps are filled, initiative is taken, and judgement is exercised "* (Williamson, 1985) and a willingness to make voluntary sacrifices for the company (Bewley, 2002). Indeed, detecting and resolving inconsistencies implied that field workers needed to pay extra attention when administering the survey and ask the respondent to clarify his/her answers when an inconsistency was spotted. This was an onerous and time-consuming process, especially because respondents were often household heads who commanded substantial respect in their community. Since field workers did not receive any direct or indirect reward for this additional effort, inconsistencies are likely to reflect how much workers identified with the survey collection and how willing they were to 'go the extra mile' for the employer.

The remainder of the paper proceeds as follows. Section 2 provides context for our experiment by developing an efficiency wage model that combines explicit incentives from monitoring with implicit incentives due to reciprocity concerns. Section 3 describes the environment and the experimental design. Sections 4 and 5 present the different econometric results as well as a variety of robustness checks. Section 6 concludes.

# 2 A simple model of efficiency wages

To provide context for our wage experiment, we build a simple model of efficiency wages that combines explicit incentives due to monitoring with implicit incentives due to reciprocity concerns. The monitoring part is a discrete-time application of the shirking model of Shapiro and Stiglitz (1984). The fair wage part is close in spirit to Rabin's (1993) two-player game with reciprocity, as adapted to the labor market by Danthine and Kurmann (2008, 2010).

## 2.1 Model

There are $T$ time periods during which a worker may be employed by the firm. If employed, the firm offers wage rate $w$ per unit of work and the worker, after observing the wage offer, decides to provide effort level $e$ per unit of work. If not employed, the worker is engaged in an alternative activity that pays $b < w$.

Individuals do not discount time and have preferences over consumption, effort and reciprocity. Per-period utility is

$$U = u(c) - v(e) + \lambda R(\cdot), \tag{1}$$

where $u(c)$ denotes the standard utility from consumption $c$ with $u' > 0$, $u'' < 0$; and $v(e)$ denotes the disutility from providing effort $e$ on the job, with $v' > 0$ and $v'' > 0$ if $e$ exceeds some basic level of effort for which the disutility of effort is minimized and $v' < 0$ and $v'' > 0$ otherwise. Without loss of generality, we restrict this basic level of effort to $e = 0$ and thus $v'(0) = 0$. The term $R(\cdot)$, finally, denotes the psychological benefit from reciprocity. If the worker has no reciprocity concerns, then $\lambda = 0$. Otherwise, $\lambda > 0$.[3]

Following Rabin (1993), we define $R(\cdot)$ as the product of the gift $g(w, \cdot)$ a firm's wage $w$ represents to the worker and the gift $r(e, \cdot)$ the worker provides to the firm when reciprocating with effort $e$

$$R(\cdot) = g(w, \cdot) \times r(e, \cdot). \tag{2}$$

When workers perceive a wage offer as generous, i.e. $g(w, \cdot) > 0$, their utility increases if they reciprocate with higher effort as long as $r_e(e, \cdot) > 0$. Vice versa, if the gift of the firm is perceived as negative, workers can make themselves better off by reciprocating negatively.

To make (2) specific, we follow Rabin (1993) one more step and assume that $g(w, \cdot)$ and $r(e, \cdot)$ are measured as the difference in payoffs implied by the other player's action (i.e. the wage paid by the firm, respectively, the effort provided by the worker) and some reference or norm level. For the firm, the payoff implied by worker's effort $e$ is naturally given by the profit function $\pi(e, \cdot) = f(e, \cdot) - tc(\cdot)$, where $f(e, \cdot)$ denotes the firm's production and $tc(\cdot)$ denotes total cost. Both $f(e, \cdot)$ and $tc(\cdot)$ depend on potentially many arguments but only production depends on the worker's effort. Given our assumptions about $v(\cdot)$ above, the norm effort level for the worker is naturally $e = 0$. The worker's gift to the firm from reciprocating with effort level $e$ therefore becomes

$$r(e, \cdot) = f(e, \cdot) - f(0, \cdot). \tag{3}$$

Under the standard assumption that $f(e, \cdot)$ is strictly concave in a particular worker's effort (or at least perceived as such by the worker), $r(e, \cdot)$ is strictly concave in $e$. For the worker, the payoff function is naturally given by consumption utility $u(c)$. Under the assumption of no savings, $u(w)$ is the worker's payoff from an observed wage $w$ and $u(w^*)$ is the payoff from

[3] As opposed to our field experiment where workers provide different kinds of effort, the model considers only one effort dimension so as to keep the analysis more tractable. None of the implications is affected if the worker supplied effort along different independent dimensions.

reference wage level $w^*$ that the worker considers as fair.[4] Hence, the firm's gift towards the worker becomes

$$g(w, \cdot) = u(w) - u(w^*). \tag{4}$$

Given the strict concavity of $u(\cdot)$, $g(w, \cdot)$ is strictly concave in $w$. Furthermore, $g(w, \cdot)$ is decreasing in the fair wage reference $w^*$. This fair wage reference $w^*$ depends potentially on a number of different arguments, among them the workers' outside option (e.g. Akerlof, 1982); wages of peer workers (e.g. Akerlof and Yellen, 1990); the firm's ability to pay (e.g. Kahneman et al., 1986); and the worker's own past wages (e.g. Bewley, 1999). Since the focus of our wage experiment is on the effect of past wages on $w^*$, we do not need to take a stand on the relative importance of other arguments in $w^*$. At the same time, this discussion makes clear that in order to study the effects of past wages on reciprocity, it is crucial that other arguments in $w^*$ remain constant throughout the wage experiment.[5]

To introduce explicit incentives for the provision of effort, we assume as in Shapiro and Stiglitz (1984) that firms stipulate some no-shirking level of effort $e^{NS} > 0$ and monitor workers with constant probability $d$. If a monitored worker is found shirking (i.e. if $e < e^{NS}$), the worker is fired in which case he obtains the outside option $b < w$ for the time periods thereafter (i.e. there is no rehiring). Otherwise, the worker gets to keep the job. Any non-monitored worker gets to keep the job independently of the effort level.[6]

Given these assumptions, consider a worker who is employed at the beginning of time period $t$ and receives wage offer $w_t$. The value of employment is

$$V_t^E = \max_{e_t} \left\{ \begin{array}{c} 1(e_t \geq e^{NS})[u(w_t) - v(e_t) + \lambda R(e_t, w_t) + V_{t+1}^E] \\ +1(e_t < e^{NS})[u(w_t) - v(e_t) + \lambda R(e_t, w_t) + (1-d)V_{t+1}^E + dV_{t+1}^U] \end{array} \right\} \tag{5}$$

[4] All results go through if we allow for savings as long as consumption is positively related to the wage.

[5] Several comments about our formulation of reciprocity relative to the literature are in order. First, compared to Rabin (1993) who formulates reciprocity as part of a two-player simultaneous move game, our environment has a clear sequential order where one player (i.e. the firm) is the first mover and the other player (i.e. the worker) is the follower. Furthermore, we only consider the problem of the follower. This considerably simplifies the analysis because the players's beliefs of the other player's actions and beliefs collapse to the first mover's action as observed by the follower. Second, Rabin's specification of $r(e, \cdot)$ and $g(w, \cdot)$ is somewhat more complicated because he specifies the gifts as the observed difference in payoffs relative to some maximum possible difference in payoffs. This difference is not important as long as concavity of $r(e, \cdot)$ and $g(w, \cdot)$ is guaranteed. Third the literature emphasizes that a crucial determinant of reciprocal behavior is the intention that a certain action conveys (e.g. Falk and Fischbacher, 2006). The maintained assumption in our environment is that the firm's wage offer appropriately conveys intentions.

[6] Alternatively, we can assume that there is no clearly stipulated no-shirking level of effort $e^{NS}$ but that the worker has beliefs about the probability $d$ of getting fired as a function of the provided effort level; i.e. $d = d(e)$ with $d' < 0$. It is possible to show that the results derived below are robust to such an extension of the basic model.

where

$$V_{t+1}^{U} = \sum_{s=t+1}^{T} u(b) \qquad (6)$$

is the value of being detected shirking and getting fired at the end of $t$; $V_{t+1}^{E}$ is the value of continuing employment given some expected path of wages $\{w_s\}_{s=t+1}^{T}$; while $1(e_t < e^{NS})$ and $1(e_t \geq e^{NS})$ are indicator functions with value 1 if $e_t < e^{NS}$ and $e_t \geq e^{NS}$, respectively. To solve for optimal effort, we focus first on reciprocity concerns and temporarily abstain from monitoring (i.e. we set $d = 0$). Under relatively weak additional conditions needed for existence, we obtain the following result.

**Proposition 1** *There is a unique reciprocity effort level $e_t^R$ that solves $v'(e_t^R) = \lambda r_e(e_t^R, \cdot) g(w_t, \cdot)$ and is strictly concave in $w_t$.*

Proof: Appendix.

The optimality condition that defines $e_t^R$ comes directly from maximizing utility with respect to $e_t$ and states that the marginal disutility from providing effort equals the marginal psychological benefit from reciprocating wage offer $w_t$.[7] The strict concavity of $e_t^R$ in $w_t$ is a direct implication of strict convexity assumption of the disutility of effort $v(\cdot)$ and the strict concavity assumption of $u(\cdot)$ and $f(e, \cdot)$ (the strictness part of the assumption could be relaxed for two of the three functions).

With $e_t^R$ uniquely determined, we return to the optimal effort problem in (5)-(6).

**Proposition 2** *Given wage offer $w_t$ and an expected path of wages $\{w_s\}_{s=t+1}^{T}$, there is a unique optimal level of effort $e_t^*$ defined as:*

1. $e_t^* = e_t^R$ *if* $e_t^R < e^{NS}$ *and* $\left[v(e^{NS}) - v(e_t^R)\right] - \lambda \left[R(e^{NS}, w_t) - R(e_t^R, w_t)\right] > d\left[V_{t+1}^E - V_{t+1}^U\right]$;
2. $e_t^* = e^{NS}$ *if* $e_t^R < e^{NS}$ *and* $\left[v(e^{NS}) - v(e_t^R)\right] - \lambda \left[R(e^{NS}, w_t) - R(e_t^R, w_t)\right] \leq d\left[V_{t+1}^E - V_{t+1}^U\right]$;
3. $e_t^* = e_t^R$ *if* $e_t^R > e^{NS}$.

Proof: Appendix.

The intuition behind the three cases is straightforward. Workers faces two different constraints implied by a given wage offer: the implicit constraint from reciprocity; and

[7] Note that this optimal reciprocity condition assumes that $g_e(w, \cdot) = 0$; i.e. in the eyes of the worker, the firm's output is not affected by a particular worker's effort. Hence, the firm's ability to pay (which may be an argument of the reference wage $w^*$ and therefore influence the firm's gift) is considered exogenous.

the explicit constraint from monitoring. The 'reciprocity constraint' is described by the condition $v'(e_t^R) = \lambda r_e(e_t^R, \cdot)g(w_t, \cdot)$ in Proposition 1. The 'no-shirking' constraint from monitoring is described by the inequality constraint in Proposition 2. The left-hand side of the constraint describes the utility loss of providing effort $e^{NS}$ instead of $e_t^R$. This loss is necessarily positive by the fact that, absent monitoring, $e_t^R$ maximizes utility. The right-hand side of the constraint is the expected loss in future utility from getting caught shirking and being fired. The two constraints are depicted in Figure 1 and together form what we call the 'effort function'.

Depending on the level of the wage, either the reciprocity constraint or the no-shirking constraint binds. In particular, if $w_t < w^{NS}$, where $w^{NS}$ is the wage for which the no-shirking constraint holds with equality, the utility loss from providing $e^{NS}$ outweighs the expected cost from getting caught shirking and the worker provides effort $e_t^R < e^{NS}$ according to his reciprocity concerns (solution 1). Vice versa, if $w_t > w^{NS}$ as drawn in the figure, the no-shirking constraint outweighs the reciprocity constraint and the worker provides effort $e^{NS} > e_t^R$ (solution 2). Finally, for a sufficiently high wage, reciprocity concerns imply an effort level $e_t^R > e^{NS}$ in which case the no-shirking constraint becomes moot since monitored workers are never found shirking (solution 3).

Notice that depending on functional form assumptions, we may not observe all three of the solutions. For example, if $e_t^R$ exceeds $e^{NS}$ at $w^{NS}$, solution 2 never occurs. In turn, if $e_t^R < e^{NS}$ for any wage level, solution 3 never occurs. Also, a special but as it turns out relevant shape of the reciprocity constraint obtains if workers perceive the firm's payoff $f(e, \cdot)$ as increasing in effort up to some effort level $e = \tilde{e}$ and constant thereafter (i.e. $f'(e) = 0$ for $e > \tilde{e}$). For this particular functional form, there still exists a unique reciprocity constraint that is increasing in the wage up to $e_t^R = \tilde{e}$ and is flat thereafter.[8]

## 2.2 Implications

The reciprocity constraint and the no-shirking constraint depend on the wage and the time left in the employment relationship. We now consider the implications on effort of varying these two determinants, conditional on different assumptions about monitoring and reciprocity.

[8] To see this, note that the assumption of $f'(e) = 0$ for $e > \tilde{e}$ introduces a non-differentiability in $r(e, \cdot)$ at $e = \tilde{e}$. Hence, $\lim_{e \to \tilde{e}_-} \lambda r_e(e, \cdot)g(w_t) > v'(e)$ and optimal effort from reciprocity solves $v'(e^R) = \lambda r_e(e^R, \cdot)g(w, \cdot)$ for $e_t^R < \tilde{e}$ and $e_t^R = \tilde{e}$ thereafter. The resulting reciprocity constraint is close to the reduced-form effort function $e = \min(w/w^*, 1)$ postulated in Akerlof and Yellen (1990).

### 2.2.1 No monitoring

Consider first a situation in which workers believe that effort is not monitored (i.e. $d = 0$). Under the standard assumption that workers do not have reciprocity concerns (i.e. $\lambda = 0$), we obtain the following unambiguous prediction.

**Result 1** *For $d = 0$ and $\lambda = 0$, workers always supply effort equal to the norm level $e = 0$, independent of wage changes or the time left in the employment relationship.*

The intuition for this result is straightforward. If workers do not have reciprocity, $e_t^R = 0$ by assumption that the disutility of effort $v(e)$ is at its minimum at $e = 0$. Since $v(\cdot)$ does not depend on either the wage or the time left to $T$, $e_t = 0$ for all $w_t$ and $t$.

If we assume instead that workers have reciprocity concerns (i.e. $\lambda > 0$), the predictions of the model are radically different.

**Result 2** *For $d = 0$ and $\lambda > 0$:*

1. *An increase (decrease) in wages leads to an increase (decrease) in effort. In addition, the increase in effort in response to a given wage increase is strictly smaller (in absolute terms) than the decrease of effort in response to a wage decrease of the same magnitude.*

2. *Effort depends negatively on past wages as long as the reference wage level $w^*$ is increasing in past wages.*

3. *Effort does not change as $t \to T$.*

Result 2.1 follows directly from the concavity of the reciprocity constraint and the fact that for $d = 0$, the no-shirking constraint never binds. The asymmetric response of effort to positive and negative wage changes has been discussed in several empirical studies (see references in introduction) but, to our knowledge, as not been formally explored to date. Also note that this asymmetry can be extreme for the special case discussed above where the reciprocity constraint becomes flat above a certain wage for which $e_t^R = \tilde{e}$. In this case, an increase in the wage does not increase effort whereas a decrease in the wage may lead to lower effort (provided that the wage cut is sufficiently large to imply $e_t^R < \tilde{e}$).

Results 2.2 and 2.3 are also direct implications of the optimal effort condition in Proposition 1. Together, the two results generate what Bewley (2002) calls 'wage entitlement'; i.e. that workers adapt over time to a given wage treatment, no matter how high it may be, and come to use it to assess the fairness of the firm.[9]

[9] Without Result 2.3, we would not be able to disentangle the effect of a reduction in time left in the employment relationship from the effect of changes in wages relative to past wages.

### 2.2.2 Monitoring

Now consider a situation in which workers believe that effort is monitored (i.e. $d > 0$). Under the standard assumption that workers do not have reciprocity (i.e. $\lambda = 0$), the model predicts the following.

**Result 3** *For $d > 0$ and $\lambda = 0$:*

1. *An increase in the path of wages $\{w_s\}_{s=t}^{T}$ leads to an increase in effort from $e_t = 0$ to $e_t = e^{NS}$ if $-v(0) + v(e^{NS}) > d\left[V_{t+1}^{E} - V_{t+1}^{U}\right]$ before the change and the resulting increase in $V_{t+1}^{E} - V_{t+1}^{U}$ is sufficiently large so as to revert the inequality. The exact opposite inequality conditions have to be met for a decrease in the path of wages to lead to a decline in effort from $e_t = e^{NS}$ to $e_t = 0$.*

2. *As $t \to T$, effort decreases from $e_t = e^{NS}$ to $e_t = 0$ for a given wage path if $-v(0) + v(e^{NS}) \leq d\left[V_{t+1}^{E} - V_{t+1}^{U}\right]$ for some $t < t_0 < T$ and $V_{t+1}^{E} - V_{t+1}^{U}$ becomes sufficiently small for some $t_0 < t < T$ such that the inequality changes sign*

Both of these results are a direct application of Proposition 2 for the special case where the worker has no reciprocity concerns (in which case optimal effort is 0 if the wage does not satisfy the no-shirking constraint).

If workers also have reciprocity concerns ($\lambda > 0$), the general solution from Proposition 2 obtains and the model makes the following predictions.

**Result 4** *For $d > 0$ and $\lambda > 0$:*

1. *An increase in the path of wages $\{w_s\}_{s=t}^{T}$ leads to an increase in optimal effort if the reciprocity constraint is binding (i.e. solution 1 or solution 3 in Proposition 2); or if the no-shirking constraint is binding (i.e. solution 2), the resulting increase in $V_{t+1}^{E} - V_{t+1}^{U}$ is sufficiently large so as to make the reciprocity constraint binding. The exact opposite conditions have to be met for a decrease in the path of wages to lead to a decrease in optimal effort.*

2. *As $t \to T$, effort decreases from $e^{NS}$ to $e_t^{R} < e^{NS}$ for a given wage path if the no-shirking constraint is binding for some $t < t_0 < T$ and $V_{t+1}^{E} - V_{t+1}^{U}$ becomes sufficiently small for some $t_0 < t < T$ such as to make the reciprocity constraint binding (i.e. solution 1 of Proposition 2).*

While these two results seem may complicated, they are a simple extension of Results 3 and can be easily understood by reconsidering Figure 1 for different wage levels.

Three key lessons come out of this analysis. First, absent explicit incentives (either through monitoring or other performance controls), effort varies with wage changes only if workers have reciprocity concerns. Second, wage entitlement in reciprocal behavior implies that a temporary increase in the wage has a negative overall effect on effort. Third, the presence of explicit incentives (e.g. through a monitoring-induced firing threat) may outweigh reciprocity concerns, thus highlighting the importance of having a measure of effort that workers perceive as unmonitored when testing for reciprocity in long-term relationships.

# 3 Environment and experimental design

We first provide an overview of the environment in which the field experiment was conducted. Then we discuss the details of the exogenous wage changes and the measures of monitored and unmonitored effort.

## 3.1 Environment

The experiment was conducted in the context of a household survey that took place in a rural part of Kenya in 2007. The primary purpose of the survey was not the wage experiment, but to collect socioeconomic information on participants in different community-based development projects and consisted of an average of about 900 questions per survey (depending on the size and activities of the household). The number of households to be surveyed was initially targeted at 2500 and was later extended to more than 3000, as discussed below.

To administer the surveys, the principal investigators (PIs) hired 12 members of the local community, which were selected based on a competitive interview process. The hired field workers were aged between 19 and 37, 7 women and 5 men, with a median age of 24. All were economically average residents, all spoke English but none had university education, and previous work experience was limited to occasional low paid employment and/or home production (e.g. farming).

Prior to the start of the survey collection, the field workers were invited to an extensive 4-day training camp that was organized by one of the PIs, assisted by a Kenyan student with previous survey experience and a foreign student. The two students were responsible for the supervision of the survey collection afterwards. The camp was held at a secluded lodge to ensure full focus on the training and to foster a sense of team spirit. The workers also received a specially designed survey T-shirt and they were informed that upon successful completion of the survey collection, they would be invited to a weekend retreat to another community in Kenya. Furthermore, the PIs promised to organize a CV workshop and to provide a letter of recommendation. All of these perks were offered in an effort to generate a friendly and cooperative work environment that should dampen any reaction to wage changes.

After the 4-day training camp and a final performance assessment, the field workers started administering the surveys. During the first two weeks of work, one of the PIs was present to help the two students in supervising and fine-tuning the survey collection. Thereafter, regular work (i.e. without direct presence of the PIs) started. In the beginning, field workers typically administered between two and three surveys per day, six days a week. Later on, as the survey collection became more efficiently organized, field workers increased their workload to four surveys per day but were explicitly discouraged from doing more.[10]

## 3.2 Experimental design

Field workers were paid per survey. Under the initial compensation scheme, the first three surveys per day were paid 150 Ksh each and all subsequent surveys of the same day were paid 100 Ksh each.[11] Since field workers administered on average between three and four surveys per day, this implied a daily salary of about 500 Ksh – three to four times more than what a field worker could hope to earn elsewhere.[12]

During the first six weeks of regular employment, field workers were paid the just described compensation scheme, called the '150/100 treatment' from hereon. In the beginning of work week 7, the wage rate was raised to 200 Ksh per survey (including for the fourth survey of the day and beyond). This new '200/200 treatment' represented an average increase in daily compensation of about 45% and was communicated to the field workers through a video announcement by the PIs. The announcement came without specific information on whether the raise was permanent or not. In return, the field workers were asked to continue administering the surveys with diligence and were reminded that they should not exceed four surveys per day.[13] The new '200/200 treatment' continued for three weeks. In the

[10] For three weeks of the total employment period, field workers administered surveys for only 5 days. Also, some field workers occasionnally exceeded and one field worker consistently exceeded the limit of 4 surveys per day. All of the results reported below are robust to whether we consider only the first four surveys per field worker per day; and to whether we exclude the field worker who consistently exceeded the limit of 4 surveys per day.

[11] Whenever possible, field workers tried to administer four surveys per day, confirming that even 100 Ksh per survey was well above their marginal outside option.

[12] At the time of the surveys, 500 Ksh were worth about US$7.4.

[13] The exact wording of all announcements is available in the appendix.

beginning of week 10, the two student supervisors played a second video announcement to the field workers in which the PIs informed them that compensation reverted back to the initial 150/100 treatment (i.e. 150 Ksh for each of the first three daily surveys and 100 Ksh for any additional survey). The justification given for this wage cut was budget limitations that made the wage of 200 Ksh per survey unsustainable. A week later, in the beginning of week 10, a third video was played to the field workers in which they were informed that employment would continue for an additional three weeks so as to expand the survey beyond the initially planned 2500 households. For this extension of employment to be feasible, the workers were explained that the wage would need to be cut to 100 Ksh for each survey per day. This '100/100 Ksh treatment' represented an average wage cut of about 27% but it also implied that employment continued for approximately three weeks longer than initially anticipated.[14] Finally, so as to avoid possible end-of-employment effects, a final video in the beginning of week 13 (i.e one week before the planned end of employment) informed the workers that since the target number of households had been reached, survey collection would halt immediately.[15]

Figure 2 summarizes the different wage treatments over the 12 weeks of regular employment. Since work weeks started on Wednesdays, all video announcements about wage changes were made on Wednesday mornings before work started and took effect immediately. Hence, the different work weeks in Figure 1 effectively lasted from Wednesday to Tuesday. None of the videos were preanounced and, to the best of our knowledge, did not coincide with any other exceptional events. Also, at no point were the workers informed that they were taking part in an experiment.

To measure work effort, we consider two different types of errors for each survey. The first type of error we consider is 'inconsistencies' across different answers of a survey. An inconsistency occurred if, for example, a respondent answered in the occupation section of the survey that he/she was not farming but indicated in the time-use section that he/she spent time farming. In total, there were 93 possible inconsistencies per survey (see the appendix for the full list). Field workers were made aware of the possibility of inconsistencies during training (without knowing about the 93 possibilities) and were instructed to pause the interview if they spotted an inconsistency and probe the respondent in order to resolve the problem. However, the supervisors never monitored or punished in any way for incon-

[14] The announcement also reassured the field workers that the planned post-survey weekend retreat and CV workshop was still on regardless of participation in the extra surveys. All field workers continued employment to the end even though they were free to quit at any time; and all of them joined the promised post-survey retreat and participated in the CV workshop.

[15] Field workers continued to be paid 400 Ksh per day for the last week without work so as to honor the promised employment contract.

sistencies, nor did anyone know that we would compute such a measure ex-post. In fact, we drew up the list of 93 possible inconsistencies and computed the rate of inconsistencies per survey via a computer algorithm only more than a year later after the different survey answers had been manually entered into a database. For all means and purposes of this experiment, inconsistencies therefore constitute a measure of effort that field workes perceived as unmonitored.

The second type of errors we consider is 'blanks and mistakes' and occurred if a survey field was either left blank (e.g. the field worker forgot to ask/pencil in the question or the respondent refused to answer) or the field contained a clear mistake (e.g. reporting zero households in the visited homestead). In contrast to inconsistencies, field workers were explicitly trained to avoid these blanks and mistakes, possibly insisting with the respondents on an answer, and the two students supervisors checked incoming surveys randomly each day for these errors (between 40% and 100% of the surveys were checked each day, depending on the time available). We therefore label blanks and mistakes as 'monitored errors'. If a survey contained too many blanks and mistakes, the field worker was given a warning and, in case of repeated subpar performance, risked dismissal. This threat of dismissal was real. In fact, during the first two weeks of employment, one field worker consistently made numerous avoidable mistakes. Despite further extensive training, performance did not improve, and the field worker was subsequently laid off.

### 3.3 Discussion

As emphasized in the introduction and formalized by the model in the previous section, the long-term nature of the wage experiment implies that field workers had an explicit incentives to perform well on monitored dimensions of effort so as not to loose their job. The availability of an effort dimension that was truly unmonitored in the eyes of the worker is therefore crucial to test for reciprocity. Our inconsistency measure fits this criteria. Hence, under the standard assumption that workers do not have reciprocity concerns, inconsistencies should be distributed randomly and unrelated to wage changes. If we find instead that inconsistencies change systematically with wage changes, then this represents prima facie evidence in favor of reciprocal behavior.

## 4 Basic results

Table 1 reports descriptive statistics for inconsistencies – our (inverse) measure of unmonitored effort – and blanks and mistakes – our (inverse) measure of monitored effort. For the total of 2864 administered surveys during the 12 weeks of regular employment, there was an average 4.65 percent of inconsistencies per survey (out of an average of 93.8 possible inconsistencies per survey). This is considerably higher than the average rate of blanks and mistakes of 1.31 percent per survey (out of an average of 911.6 possible blanks and mistakes per survey).

As the standard deviations and extreme values in Table 1 indicate, there is considerable variation in the two effort measures. Closer inspection reveals that a substantial part of this variation is idiosyncratic and not systematically associated with particular field workers or time in the employment relationship. To show the general evolution of inconsistencies and blanks and mistakes, we therefore use local linear regressions to smoothen out this idiosyncratic variation. In addition, to foreshadow our results below, we impose a discontinuity at the days when the changes in wage treatment occurred (i.e. in the beginning of work weeks 7, 10 and 11).[16] Figures 3 and 4 display the result. Three basic observations stand out:

1. There is a clear secular downward trend in the rate of inconsistencies. By contrast, the rate of blanks and mistakes is trending upwards (abstracting from the first two weeks). This suggests that throughout the employment, field workers accumulated experience in detecting and resolving inconsistencies whereas for blanks and mistakes, this learning-by-doing effect was present only in the beginning or was outweighed later on by other effects, as discussed below.

2. Inconsistencies jump up substantially in the beginning of weeks 10 and 11 when the two wage cuts took place. Interestingly, there is also a small *positive* jump in inconsistencies at the beginning of week 7 when the wage increase was administered.

3. Blanks and mistakes also display jumps around the wage change days. But these jumps are generally smaller and always negative.

While instructive, this visual inspection does not tell us whether any of the jumps are significant, nor does it indicate (by construction) whether there are important jumps for weeks when no wage changes took place. To assess these issues formally, we proceed by testing econometrically for jumps at the beginning of each workweek (i.e. each Wednesday).

[16] The discontinuities are imposed by estimating the local linear regressions separately on each side of the days when a wage change occured. The idea to smoothen noisy data with local linear regressions around discrete cut offs is taken from the literature on regression discontinuity designs (see Imbens and Lemieux, 2007 for a survey). The local linear regressions are computed in STATA using an Epanechnikov kernel. Somewhat more variable plots but with exactly the same qualitative features would have obtained with other kernels or if we had applied a simple moving average to the data.

First, we reduce some of the idiosyncratic variation by purging the two effort measures of survey-specific effects as described and estimated in the panel regressions of the next section.[17] Then we compute the difference between the 3-day average of the resulting residual effort measures immediately preceding the beginning of the workweek and the corresponding 3-day average starting with the beginning of the workweek. Finally, to conduct inference, we compute the bootstrapped 90% confidence interval of the differences.

Table 2 displays the results. As column (1) shows, the rate of inconsistencies increases significantly by 0.53 percentage points and 0.35 percentage points, respectively, in the beginning of week 10 and week 11 when the wage cuts were administered. Relative to the average rate of inconsistencies of 4.65 percent per survey, this represents an increase of 11.3 percent and 7.5 percent, respectively. By contrast, there is no significant change for any of the other weeks. In particular, in the beginning of week 7 when the wage was increased, inconsistencies do not react significantly. Column (2) shows the corresponding results for the rate of blanks and mistakes. While there are several significant changes during the first 6 weeks, there are no significant changes thereafter. Importantly, in the beginning of weeks 7, 10 and 11, the rate of blanks of mistakes essentially remains flat.

Four key implications come out of these results. First, the significant increase in the rate of inconsistencies in the beginning of weeks 10 and 11 when wages are cut provides clear evidence of negative reciprocity. As implied by Results 1 and 2 of our model, unmonitored effort reacts systematically to wage changes only if workers have reciprocity concerns. Specifically, a wage cut signals a smaller gift by the firm to which workers react with reduced effort. By contrast, there are no positive reciprocity effects in response to the wage increase in the beginning of week 7. As discussed towards the end of Section 2, such an extreme asymmetry is consistent with the model if the reciprocity constraint becomes flat above a certain wage level. This can occur if the initial wage-effort equilibrium is already so high that, in the workers' minds, additional effort in response to an even higher wage does not lead to a further increase in the psychological benefits from reciprocating. Given that the initial 150/100 treatment amounted to a daily compensation that was three to four times higher than the going market compensation, this is a distinct possibility. By the same token, the generous initial treatment makes the decrease of unmonitored effort in response to the wage cuts all the more striking – especially since the PIs went to great lengths to foster a cooperative work environment and the wage cuts were framed as necessary to respect budget limitations.

[17] These survey-specific effects are a field worker fixed effects; a gender of the respondent control; a sublocation control (where the survey took place); and a relationship control (i.e. relationship between the the interview respondent and the household head).

Second, the absence of a significant drop in inconsistencies in the beginning of week 7 *together* with the presence of a significant increase in inconsistencies in week 10 when the wage was lowered back to the original 150/100 treatment suggests that workers adapted quickly to the higher 200/200 treatment from week 7 to week 10 and came to believe that this was the new reference against which a given wage offer should be judged. As implied by Results 2.2 and 2.3 of our model, this wage entitlement effect is a potentially important source of asymmetric reciprocity behavior. At the same time, the local discontinuity tests that we perform in Table 2 do not allow us to separate the effects of wage entitlement from secular time trends in inconsistencies as observed in Figure 3. The panel estimation that we perform in the next section allows us disentangle these two temporal phenomena from each other.

Third, the absence of any significant response of blanks and mistakes to the different wage changes in the beginning of week 7, 10 and 11 suggests that monitoring imposed an important additional constraint that outweighed field workers' negative reciprocity behavior. Specifically, recall from Result 4 of the model that a decrease in wages only leads to a decrease in monitored effort (i.e. an increase in blanks and mistakes) if either the no-shirking constraint is not binding before the wage decrease or the wage decrease is sufficiently large for the reciprocity constraint to replace the no-shirking constraint as the binding constraint. The absence of a significant reaction in blanks and mistakes to the two wage cuts therefore implies that the no-shirking constraint was binding not only at the initial 150/100 treatment but also at the lower 100/100 treatment, which seems plausible given the limited outside options of the workers.

Fourth, the finding of negative reciprocity for inconsistencies implies that the presence of explicit incentives does not necessarily crowd out reciprocal behavior, as suggested by certain laboratory experiments (e.g. Fehr and Gächter, 2000b).[18] Otherwise, workers would have provided minimal effort on resolving inconsistencies throughout the entire experiment.

A possible concern about the results in Table 2 is that reciprocal behavior is irrelevant and that inconsistencies increased instead because of some idiosyncratic shocks that coincided with the wage cuts in the beginning of weeks 10 and 11. Several reasons speak against this possibility. First and most importantly, if inconsistencies had increased because of some large idiosyncratic shock (e.g. inclement weather, uncooperative survey respondents), one would expect to see the same shock to also increase the rate of blanks and mistakes. This is clearly not the case.[19] Second, as described above, the estimates in Table 2 control for

[18] More specifically, this crowding out argument means that reciprocity concerns simply disappear (i.e. $\lambda = 0$ in our model) if firms impose explicit incentives.

[19] As noted earlier, blanks and mistakes did change signficantly during the first 6 weeks even though wages

different survey-specific effects. Third, the wage changes were implemented on Wednesdays, in the middle of the regular week, on otherwise uneventful days. Closer inspection of the data reveals, moreover, that the field workers' behavior did not change noticeably in other dimensions (e.g. the time used per survey or the average number of surveys administered per day). Fourth, as Figure 3 shows, the significant increases in inconsistencies in the beginning of weeks 10 and 11 were not the result of a strong positive secular time trend due, for example, to fatigue effects. In fact, exactly the opposite is true: throughout the entire employment relationship, the rate of inconsistencies exhibited a marked downward trend, interrupted only by the jumps in response to the wage cuts. For all these reasons, it seems highly unlikely that our results are driven by events other than the exogenous wage changes.

# 5 Panel estimates

To assess the effect of wage changes further, we perform panel estimations on the full dataset. The use of the entire dataset instead of just data around particular days increases power and allows us to directly control for secular time trends.

## 5.1 Econometric specification

The panel regressions take the form

$$e_{ijt} = \alpha_j + \boldsymbol{\beta}\mathbf{D}_{wage} + \boldsymbol{\delta}\mathbf{X}_{ijt} + \gamma_1 t + \gamma_2 t^2 + u_{ijt}, \tag{7}$$

where $i$ identifies the survey; $j$ the field worker; and $t$ the survey day. The dependent variable $e_{ijt}$ is alternatively the rate of inconsistencies or the rate of blanks and mistakes for a given survey. The coefficient $\alpha_j$ captures fixed worker effects; $\mathbf{D}_{wage}$ is a vector of dummy variables for each of the wage regimes (described in detail below); and $\mathbf{X}_{ijt}$ represents a set of observable non-wage controls that may change systematically across surveys, field workers and time.[20] The term $\gamma_1 t + \gamma_2 t^2$ captures secular trends due for example to learning-by-doing as observed for inconsistencies in Figure 3. We specify this trend in quadratic form so as to provide the estimation with flexibility to accommodate effects that are either slowly dying out over time or manifest themselves only over time. As shown below, the results are robust to other forms of the time trend. Note also that this time trend is identified separately from

remained constant. By contrast, inconsistencies did not change significantly during these first six weeks. This suggests that blanks and mistakes are more sensitive to idiosyncratic shocks than inconsistencies.

[20] Specifically, $\mathbf{X}_{ijt}$ includes indicators for the area in which the interview took place; the gender of the interview respondent; and the relationship of the interview respondent to the household head.

the wage dummies in $\mathbf{D}_{wage}$ because we make it a function of survey day $t$. Finally, $u_{ijt}$ denotes an uncorrelated error term.

The key coefficients of interest are contained in the vector $\boldsymbol{\beta}$ and measure the effect that the different wage dummies in $\mathbf{D}_{wage}$ have on the error rate in question. In defining these dummies, we face a choice of time interval per dummy. We choose to define one separate dummy per week. This is a natural benchmark because all wage changes occurred on Wednesdays and because it provides a good trade-off between sample size and sufficiently small time intervals to capture the discontinuity around the wage changes.[21] To identify the effect of each dummy on $e_{ijt}$, we define week 6 as the reference, which is the last week of the initial 150/100 treatment before the increase to the 200/200 treatment. Vector $\mathbf{D}_{wage}$ therefore contains eleven dummies taking on the value of 1 for the respective week and 0 otherwise; and the different coefficients in $\boldsymbol{\beta} = [\beta_1, ...\beta_5, \beta_7, ...\beta_{12}]$ capture the impact of each week relative to the omitted reference week 6. Remembering the timing of the wage changes described in Figure 2, $\beta_7$ captures the impact on $e_{ijt}$ of the 200/200 treatment in week 7, as opposed to the 150/100 treatment during reference week 6; $\beta_{10}$ captures the impact of returning to the 150/100 treatment in week 10 relative to the initial 150/100 treatment during the reference period in week 6; and so forth.

## 5.2 Estimates

Column (1) of Table 3 displays the results for inconsistencies. Robust standard errors are reported in parentheses below each estimate. There is no significant difference in inconsistencies between the reference week and the first five weeks, where compensation is at the initial 150/100 treatment (for space reasons, we do not report these coefficients). The first three coefficients ($\beta_7$ to $\beta_9$) capture the effect on inconsistencies of the increase in compensation to the 200/200 treatment in weeks 7 to 9. None of these effects are significant. By contrast, the last three coefficients ($\beta_{10}$ to $\beta_{12}$) show that relative to the initial 150/100 treatment during the reference period in week 6, the rate of inconsistencies jumps significantly as the wage first returns to the original 150/100 treatment in week 10 and then jumps further as compensation is lowered to the 100/100 treatment in weeks 11 and 12. In addition, as the positive and significant difference in coefficients $\beta_{10} - \beta_9$ and $\beta_{11} - \beta_{10}$ indicates, the increase in inconsistencies is significant not only with respect to the reference period in week 6 but also with respect to the weeks directly preceding the wage cuts.

These estimates fully confirm the basic results of Table 2. Specifically, the estimate for

[21] Results are robust to using smaller 3-day regimes and are available from the authors upon request.

$\beta_7$ is insignificant whereas the differences $\beta_{10} - \beta_9$ and $\beta_{11} - \beta_{10}$ are positive and significant. These two differences are somewhat larger than the estimates for weeks 10 and 11 in Table 2, representing an increase of 18.5 percent and 13.1 percent in the rate of inconsistencies relative to the average rate of 4.65 percent. The larger magnitude of these estimates is due to the fact that the panel estimation considers weekly intervals rather than 3-day intervals and includes a time trend. In line with the overall evolution of inconsistencies displayed in Figure 2, this time trend is estimated to be negative except for the very beginning of the employment relationship (i.e. the negative quadratic term quickly overpowers the positive linear term), presumably capturing a learning-by-doing effect.

The inclusion of a time trend allows us to separately identify the effects of wage entitlement and is captured by the large and significant point estimate $\beta_{10}$ of 1.573 percentage points. Relative to the average rate of inconsistencies of 4.65 percent, this estimate represents a jump of 34 percent and measures the impact of returning to the 150/100 treatment in week 10 relative to the same 150/100 treatment in reference week 6. Workers therefore appear to have quickly adapted to the 200/200 treatment in weeks 7 to 9 and considered this as the new norm even though the daily compensation implied by this treatment was several times higher than any available outside options. This finding offers direct support for Bewley's (2002) conclusion from interviews with managers and labor leaders that *"...employees usually have little notion of a fair or market value for their services and quickly come to believe that they are entitled to their existing wage, no matter how high it may be..."* (page 7).

Column (2) of Table 3 shows that none of the wage changes has a significant effect on the rate of blanks and mistakes, confirming again the basic results of Table 2. Furthermore, most of the significant jumps in weeks 1-4 now disappear (not shown for space reasons). The absence of significant results for blanks and mistake in weeks 10 and 11 indicates one more time that the large and significant response of inconsistencies to wage cuts are not driven by some random coincident events. Instead, the results in Column (2) suggest that the presence of explicit incentives through monitoring outweighed the field workers' negative reciprocity behavior.

## 5.3 Robustness

To provide additional support for our results, we perform several robustness checks. Table 4 reports results from exploiting the particular wage structure during the 150/100 treatment and the 100/100 treatment. The first row tests whether, during weeks 1 to 6 when the initial 150/100 treatment was in place, there were possible effects on monitored and unmonitored effort of wage changes *within* each day. As the estimates show there is no significant difference in either effort measure between the third survey (paid 150 Ksh) and the fourth survey and beyond (paid 100 Ksh). Hence, the negative reciprocity effects found for wage changes *across time* in Table 3 do not apply to wage changes *within each day.* This suggests that workers' reciprocity depends on changes in the wage contract as opposed to the details of a given contract, lending further support to Bewley's (2002) conclusion that employees have little notion of a fair or market value in absolute terms.

Rows 2 to 6 of Table 4 checks the robustness of our main results in Table 3 by using only the first three surveys for each day. All results are confirmed: (i) there is no significant reaction of inconsistencies in week 7 when the wage per survey is increased to the 200/200 treatment; (ii) inconsistencies increase significantly as the wage returns to the baseline 150/100 treatment in week 10; (iii) inconsistencies increase even further as the wage drops to the 100/100 treatment in week 11; and (iv) there is no significant reaction in blanks and mistakes for any of the wage changes.

The last row of Table 4, finally, shows that there is also a strong and significant increase in inconsistencies for the first three surveys per day in week 11, paid 100 Ksh each, relative to the fourth survey per day in weeks 1 to 6 even though this fourth survey was paid the same 100 Ksh and was administered at the end of the day. This test provides further confirmation of the wage entitlement effect discussed above.

Table 5 goes through a battery of additional robustness checks for the panel regressions on inconsistencies in Table 3. Column (1) repeats the baseline estimation of Column (1) in Table 3. Columns (2) and (3) show that none of the results change when (i) the reference week is changed to week 5; and (ii) the two training weeks prior to the regular work relationship are included (the weeks when one of the PIs was present). Columns (4) to (6) show that the results are also robust to (i) omission of respondent controls; (ii) omission of sublocation fixed effects; and (iii) replacement of the quadratic time trend with a linear time trend (now estimated to be negative, consistent with Figure 2).[22] Lastly, as discussed above, since wage changes were enacted on a weekly basis, a week is the natural choice of time interval per dummy. A more refined but less powerful analysis at a 3 day level, where Wednesday-Thursday-Friday would form a first block, and Saturday-Monday-Tuesday another (workers took Sunday off), leads to essentially similar results.[23]

[22]Likewise, the results are robust to the inclusion of a cubic time trend.

[23]Results available upon request.

# 6 Conclusion

This paper tests for reciprocity in labor relations using a field experiment in an actual labor market. The novelty of our paper relative to existing field experiments in this literature is that we devised a measure of effort that workers perceived as truly unmonitored. This allowed us to follow the same workers over an extended period of time and estimate how their reciprocal behavior responded to a sequence of wage raises and wage cuts. The two main results coming out of our experiment is that (i) workers exhibited negative reciprocity with respect to wage cuts but no positive reciprocity with respect to wage raises; and that (ii) workers quickly adapted to a new higher reference wage when deciding on the reciprocity response to a given wage offer. Our analysis also reveals that explicit incentives on monitored dimensions of effort can easily outweigh the effects of reciprocity; but that the presence of explicit incentives in itself does not necessarily crowd out the workers propensity to reciprocate.

These results may help understand a number of important labor market phenomena. In particular, as Collard and De la Croix (2000) and Danthine and Kurmann (2004, 2010) show in a dynamic general equilibrium context, the assumption of wage entitlement is crucial for reciprocity-based efficiency wage models to generate endogenous wage rigidity and for relatively small shocks to imply large and persistent business cycle fluctuations. Furthermore, the asymmetric response of unmonitored effort to wage cuts relative to wage increases provides an explanation for the lack of wage cuts (i.e. downward wage rigidity) observed in many micro wage data sets of industrialized countries (e.g. Dickens et al., 2007). As Bewley (1999) argues: "*...resistance to pay reduction comes primarily from employers, not from workers or their representatives, though it is anticipation of negative employee reactions that makes employers oppose pay cutting. The claim that wage rigidity gives rise to unexploited gains from trade is invalid, because a firm would lose more money from the adverse effects of cutting pay than it would gain from lower wages and salaries.*" *(page 430-31).* Viewed in this way, this field experiment represents a counterfactual of what a firm should *not* do, with the negative reaction of workers to the wage cuts confirming Bewley's point.

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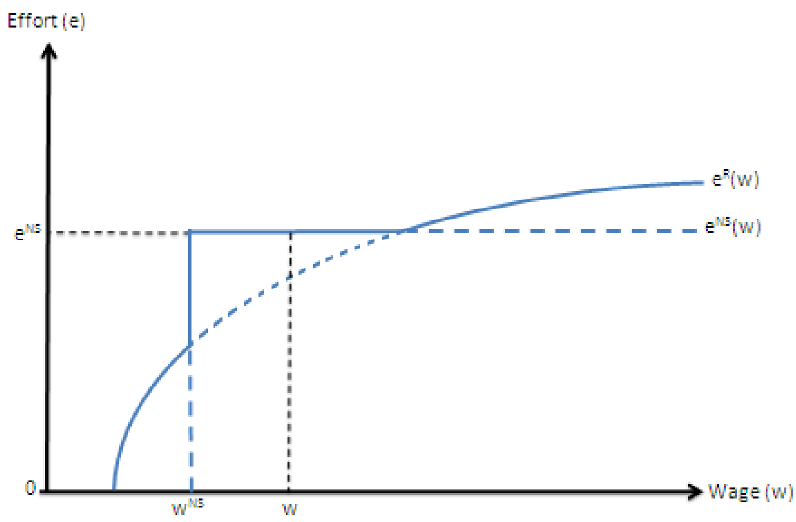


Figure 1: The effort function

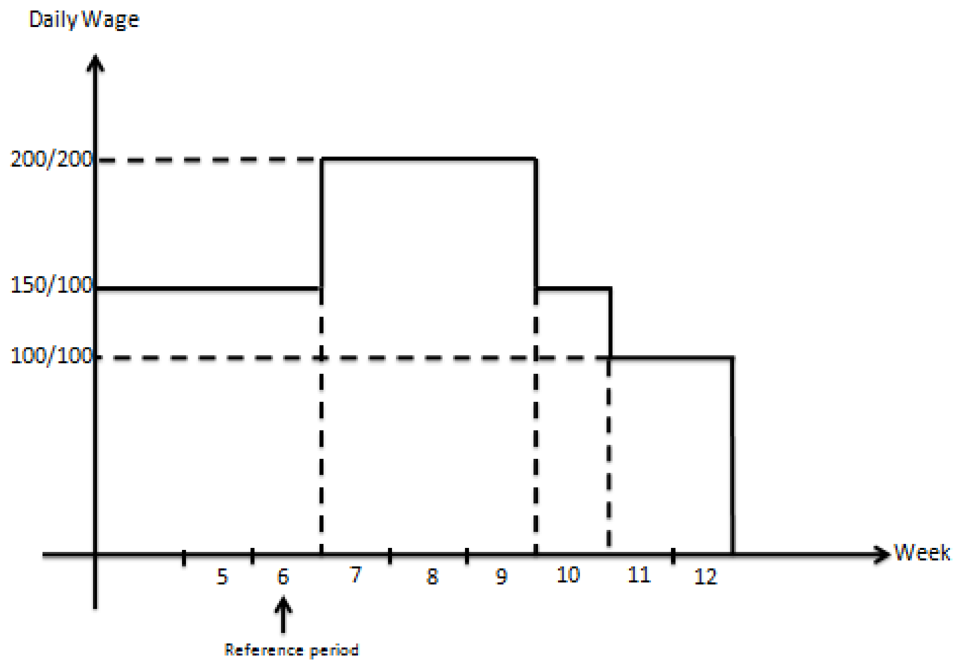


Figure 2: Timing of changes in wage treatments.

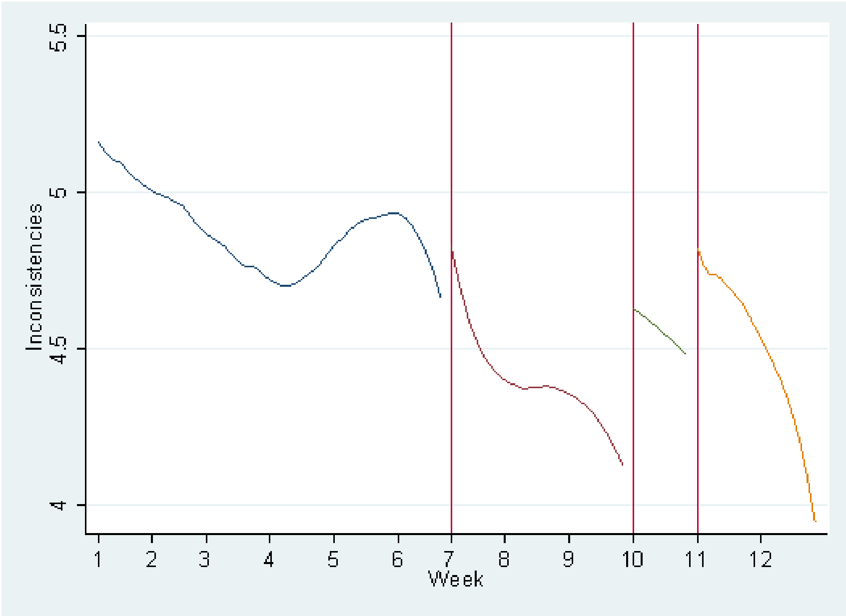


Figure 3: Average rate of inconsistencies (smoothed by local linear regression with Epanechnikov kernel)

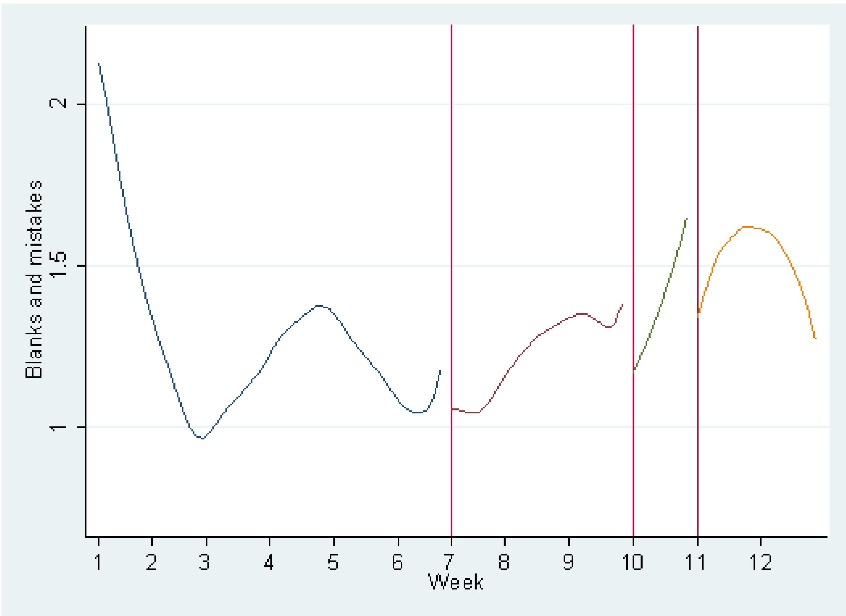


Figure 4: Average rate of blanks and mistakes (smoothed by local linear regression with Epanechnikov kernel)

**Table 1: descriptive statistics**

| | (1) Inconsistencies | (2) Blanks and Mistakes |
|---|---|---|
| Average possible number per survey | 93.8 | 911.6 |
| Average rate across surveys | 4.65% | 1.31% |
| Standard deviation | 2.50 | 2.03 |
| Maximum rate | 22.83% | 33.56% |
| Minimum rate | 0.00% | 0.09% |

**Table 2: Impact of wage changes on rate of errors**
(Difference in averages 3 days after, to 3 days before,
90% confidence interval in brackets)

| | (2) | (4) |
|---|---|---|
| | Inconsistencies | Blanks and Mistakes |
| Week 2 | -0.06 | -0.82 |
| | [-0.65,0.44] | [-1.48,-0.21]** |
| Week 3 | -0.03 | -0.31 |
| | [-0.51,0.46] | [-0.63,-0.05]* |
| Week 4 | 0.09 | 0.22 |
| | [-0.20,0.41] | [-0.06,0.54] |
| Week 5 | -0.0004 | 0.42 |
| | [-0.36,0.35] | [0.14,0.73]** |
| Week 6 | 0.04 | -0.37 |
| | [-0.38,0.44] | [-0.62,-0.14]* |
| Week 7 (Wage=150-Wage=200) | 0.16 | -0.09 |
| | [-0.23,0.40] | [-0.34,0.13] |
| Week 8 | 0.09 | -0.11 |
| | [-0.14,0.38] | [-0.27,0.20] |
| Week 9 | 0.06 | -0.41 |
| | [-0.20,0.34] | [-0.72,0.06] |
| Week 10 (Wage=200-Wage=150) | 0.53 | -0.14 |
| | [0.21,0.84]** | [-0.40,0.14] |
| Week 11 (Wage=150-Wage=100) | 0.35 | -0.14 |
| | [0.003,0.72]* | [-0.43,0.15] |
| Week 12 | -0.39 | 0.1 |
| | [-0.79,0.01] | [-0.21,0.44] |

Difference between the average of the dependent variable 3 days after the start of a week, and 3 days before. The confidence interval at 90% is obtained through bootstrapping 400 times the sample with replacement. * significant at 10%; ** significant at 5%; *** significant at 1%. In column (1), the dependent variable is the residual from the regression of the rate of inconsistencies on respondent controls, sublocation fixed effects, fieldworker fixed effects. Column (2) replicates the analysis with blanks and mistakes.

**Table 3: Impact of wages on rate of errors**
(reference period: Week 6; 150/100 treatment)

| | (1) | (2) |
|---|---|---|
| | Inconsistencies | Blanks and Mistakes |
| Fixed effects for previous weeks | Yes | Yes |
| Week 7; 200/200 treatment ($\beta_7$) | -0.016 | -0.033 |
| | (0.277) | (0.208) |
| Week 8; 200/200 treatment ($\beta_8$) | 0.292 | 0.071 |
| | (0.368) | (0.294) |
| Week 9; 200/200 treatment ($\beta_9$) | 0.711 | -0.057 |
| | (0.496) | (0.396) |
| Week 10; 150/100 treatment ($\beta_{10}$) | 1.573 | -0.066 |
| | (0.678)** | (0.541) |
| Week 11; 100/100 treatment ($\beta_{11}$) | 2.185 | -0.127 |
| | (0.884)** | (0.706) |
| Week 12; 100/100 treatment ($\beta_{12}$) | 2.691 | -0.167 |
| | (1.146)** | (0.930) |
| $\beta_{10} - \beta_9$ | 0.86 | -0.01 |
| (P-value) | (0.003)*** | (0.968) |
| $\beta_{11} - \beta_{10}$ | 0.61 | -0.06 |
| (P-value) | (0.057)* | (0.815) |
| Time trend | 0.017 | -0.002 |
| | (0.065) | (0.051) |
| Time trend squared | -0.001 | 0.0003 |
| | (0.0005) | (0.0002) |
| Fieldworker fixed effects | Yes | Yes |
| Sublocation of interview fixed effects | Yes | Yes |
| Respondent controls | Yes | Yes |
| Observations | 2864 | 2864 |
| R-squared | 0.19 | 0.08 |

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable in column (1) is the rate of inconsistencies (number of inconsistencies in a survey divided by the total number of potential inconsistencies, multiplied by 100). Fixed effects for previous weeks are included. The reference category is the 6th week where the wage was set at 150. Beta10-beta9 is simply the difference between the two coefficients. The P-value associated is the P-value of the t-test comparing this difference to zero. A time trend, and a time trend squared, are always included to take into account learning effects. Fieldworker fixed effects are included. Respondents' controls (sublocation fixed effects, gender, and relationship to household head) are included. In column (2), the dependent variable is the rate of blanks per survey (number of blanks in a survey divided by the number of cells to be filled in a survey, multiplied by 100), plus the rate of mistakes per survey (number of mistakes divided by the total number of potential mistakes, multiplied by 100).

**Table 4: Impact of wages on rate of errors by number of survey**
(reference survey: survey 1, week 6; 150/100 treatment)
(regression coefficients not shown, only t-tests of equality)

| T-tests: | Inconsistencies | Blanks and Mistakes |
|---|---|---|
| Survey 3 in weeks 1-6/survey 4 in weeks 1-6 | -0.24 | -0.23 |
| | (0.502) | (0.267) |
| Survey 1,2,3 in week 7/survey 1,2,3 in week 6 | 0.22 | -0.11 |
| | (0.447) | (0.629) |
| Survey 1,2,3 in week 10/survey 1,2,3 in week 6 | 1.92 | -0.02 |
| | (0.005)*** | (0.966) |
| Survey 1,2,3 in week 11/survey 1,2,3 in week 6 | 2.58 | -0.06 |
| | (0.004)*** | (0.934) |
| Survey 1,2,3 in week 10/survey 1,2,3 in week 9 | 0.96 | 0.02 |
| | (0.004)*** | (0.941) |
| Survey 1,2,3 in week 11/survey 1,2,3 in week 10 | 0.65 | -0.04 |
| | (0.073)* | (0.901) |
| Survey 1,2,3 in week 11/survey 4 in week 1-6 | 2.47 | -0.35 |
| | (0.008)*** | (0.735) |

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variables are the rate of inconsistencies in column (1), and blanks and mistakes in column (2). The reference category is the first survey in the 6th week. The wage dummies are interacted with dichotomous variable for the number of the survey in the day. The regression coefficients are not reported. Only the difference of the coefficients is reported, with a t-test of equality. For example, in the first row "Survey 3 in weeks 1-6/survey 4 in weeks 1-6", the average of the six coefficients for the third survey in weeks 1 to 6 is compared to the average of the six coefficients for the fourth survey in weeks 1 to 6. A time trend, and a time trend squared, are always included to take into account learning effects. Fieldworker fixed effects are included. Respondents' controls (sublocation fixed effects, gender, and relationship to household head) are included.

**Table 5: Robustness checks**
(reference period: Week 6; 150/100 treatment)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Inconsistencies | | | | | |
| | Baseline | Other reference category | With first two weeks | No respondent controls | No sublocation and respondent controls | No time trend squared |
| Fixed effects for previous weeks | Yes | Yes | Yes | Yes | Yes | Yes |
| Week 6; 150/100 treatment ($\beta_6$) | | 0.184 | | | | |
| | | (0.315) | | | | |
| Week 7; 200/200 treatment ($\beta_7$) | -0.016 | 0.168 | 0.024 | -0.081 | -0.089 | -0.056 |
| | (0.277) | (0.418) | (0.277) | (0.284) | (0.248) | (0.246) |
| Week 8; 200/200 treatment ($\beta_8$) | 0.292 | 0.475 | 0.370 | 0.137 | 0.100 | 0.128 |
| | (0.368) | (0.509) | (0.363) | (0.370) | (0.342) | (0.342) |
| Week 9; 200/200 treatment ($\beta_9$) | 0.711 | 0.894 | 0.770 | 0.540 | 0.453 | 0.424 |
| | (0.496) | (0.643) | (0.476) | (0.500) | (0.481) | (0.479) |
| Week 10; 150/100 treatment ($\beta_{10}$) | 1.573 | 1.757 | 1.636 | 1.300 | 1.154 | 1.019 |
| | (0.678)** | (0.814)** | (0.645)** | (0.682)* | (0.669)* | (0.648)* |
| Week 11; 100/100 treatment ($\beta_{11}$) | 2.185 | 2.369 | 2.227 | 1.904 | 1.815 | 1.520 |
| | (0.884)** | (1.000)** | (0.828)*** | (0.886)** | (0.876)** | (0.807)* |
| Week 12; 100/100 treatment ($\beta_{12}$) | 2.691 | 2.875 | 2.726 | 2.290 | 2.012 | 1.500 |
| | (1.146)** | (1.239)** | (1.069)** | (1.154)** | (1.137)* | (0.974) |
| $\beta_{10} - \beta_9$ | 0.86 | 0.86 | 0.87 | 0.76 | 0.70 | 0.60 |
| (P-value) | (0.003)*** | (0.003)*** | (0.003)*** | (0.008)*** | (0.011)** | (0.016)** |
| $\beta_{11} - \beta_{10}$ | 0.61 | 0.61 | 0.59 | 0.60 | 0.66 | 0.50 |
| (P-value) | (0.057)* | (0.057)* | (0.048)** | (0.059)* | (0.033)** | (0.051)* |
| Time trend | 0.017 | 0.017 | 0.017 | 0.023 | -0.003 | -0.058 |
| | (0.065) | (0.065) | (0.065) | (0.066) | (0.063) | (0.027)** |
| Time trend squared | -0.001 | -0.001 | -0.001 | -0.001 | -0.001 | |
| | (0.0005) | (0.0005) | (0.0005) | (0.0005) | (0.0004) | |
| Fieldworker fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Sublocation of interview fixed effects | Yes | Yes | Yes | Yes | | |
| Respondent controls | Yes | Yes | Yes | | | |
| Observations | 2864 | 2864 | 3012 | 2873 | 2873 | 2873 |
| R-squared | 0.19 | 0.19 | 0.19 | 0.17 | 0.16 | 0.16 |

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable in all columns is the rate of inconsistencies (number of inconsistencies in a survey divided by the total number of potential inconsistencies, multiplied by 100). Fixed effects for previous weeks are included. In column (1), the reference category is the 6th week where the wage was set at 150. In column (2), the reference category is the 5th week where the wage was set at 150. In column (3), the initial two weeks of training are included. In column (4), the respondents' controls (gender, and relationship to household head) are excluded. In column (5), the sublocation fixed effects and respondents' controls are excluded. In column (6) the time trend squared is excluded.

# Appendix

## A Proofs of Proposition 1 and 2

### A.1 Proposition 1

In the absence of monitoring (i.e. $d = 0$), the worker's problem reduces to

$$\max_e u(w) - v(e) + \lambda r(e, \cdot) g(w, \cdot).$$

with

$$r(e, \cdot) = f(e, \cdot) - f(0, \cdot).$$

The first-order condition is

$$v'(e) = \lambda f_e(e, \cdot) g(w, \cdot)$$

Consider first the case in which $g(w, \cdot) > 0$. Then, the convexity of $v\,(e)$ and the concavity of $f(e, \cdot)$ imply that there exists a unique solution for $e$ under the condition that $\lim_{e \to 0} v'(e) < \lim_{e \to 0} \lambda f_e(e, \cdot) g(w, \cdot)$. This last condition simply imposes that around $e = 0$, the marginal psychological benefit of reciprocating is higher than the marginal disutility of providing effort. Second, consider the case in which $g(w, \cdot) < 0$. Then, there exists a unique solution for $e$ as long $\lim_{e \to 0} v'(e) < \lim_{e \to 0} \lambda f_e(e, \cdot) g(w, \cdot)$; and $v'(e) > -f_e(e, \cdot)$ for $e$ below some $\underline{e} < 0$. The first of the two condtions is as before. The second condition imposes that the marginal harm that the worker can inflict on the firm by exerting negative effort (or more generally: less than norm effort) is at some point exceeded by the marginal disutility of doing so.

To prove concavity of optimal effort in $w$, rewrite the first-order condition as an implicit function

$$\Gamma(e, w) = -v'(e) + \lambda f_e(e, \cdot) g(w, \cdot) = 0.$$

Applying the implicit function theorem, we obtain

$$\frac{de}{dw} = -\frac{\partial \Gamma(e, w)/\partial w}{\partial \Gamma(e, w)/\partial e} = \frac{-\lambda f_e(e, \cdot) g_w(w, \cdot)}{-v''(e) + \lambda f_{ee}(e, \cdot) g(w, \cdot)} > 0$$

by the concavity of $f(e, \cdot)$ and $g(w, \cdot)$ and the convexity of $v(e)$. Applying a second derivative with respect to $w$ yields

$$\frac{d^2 e}{dw^2} = \frac{-\lambda f_e(e, \cdot) g_{ww}(w, \cdot)[-v''(e) + \lambda f_{ee}(e, \cdot) g(w, \cdot)]}{[-v''(e) + \lambda f_{ee}(e, \cdot) g(w, \cdot)]^2} + \frac{\lambda f_e(e, \cdot) g_w(w, \cdot) \times \lambda f_{ee}(e, \cdot) g_w(w, \cdot)]}{[-v''(e) + \lambda f_{ee}(e, \cdot) g(w, \cdot)]^2} < 0,$$

again by the concavity of $f(e, \cdot)$ and $g(w, \cdot)$ and the convexity of $v(e)$. This proves Proposition 1.

## A.2 Proposition 2

Consider the first solution in which shirking is assumed to result in a higher value of employment than not shirking; i.e.

$$-v(e_t^R) + \lambda R(e_t^R, w_t) + (1-d)V_{t+1}^E + dV_{t+1}^U > -v(e^{NS}) + \lambda R(e^{NS}, w_t) + V_{t+1}^E.$$

Rearranging this equation yields the condition in Solution 1 of Proposition 2

$$[v(e^{NS}) - v(e_t^R)] - \lambda[R(e^{NS}, w_t) - R(e_t^R, w_t)] > d[V_{t+1}^E - V_{t+1}^U]$$

Since by definition, $e_t^R$ maximizes the total utility from reciprocating, the left-hand side is positive and represents the loss that would be incurred by not shirking. The right-hand side represents the loss of being caught shirking. Since this right-hand side is assumed smaller in this first solution, it is optimal for the worker to supply $e^* = e^R < e^{NS}$. The other two solutions follow naturally. This proves Proposition 2.

# B Announcements

The first of the three announcements was read to the field workers by one of the student supervisors. The second and third announcement were made by video to the field workers. The PIs were not present for any the announcements. Hence, the scope for transmission of additional information was very limited.

## B.1 Wage change from 150/100 treatment to 200/200 treatment

*I have to make an announcement on behalf of [the PIs]. It is unacceptable to do 5 surveys per day. We only pay for 4 surveys per day. But we want you to do a really good job on the four surveys. For that reason, we raise your salary to 200/survey for 4 surveys per day. Please apply care and diligence when filling the surveys.*[24]

## B.2 Wage change from 200/200 treatment back to 150/100 treatment

*Hi guys,*

*I hope everything is going fine in Kenya. Because we cannot be here in Kenya, we asked [the supervisor] to play this movie for you so that you get the news directly from us.*

*We're happy with your work up to now and we decided to do even more surveys. This is very important for the research in order to have a better picture of the whole community.*

*Unfortunately, our budget is fixed. For this reason, we'll have to return to the regular salary: 150 per survey for the first 3 surveys and 100 for the 4th one. As usual, you can only do a max of 4 surveys per day.*

*Thanks again for all your work and I hope to see you soon.*

## B.3 Wage change from 150/100 treatment to 100/100 treatment

*Hi guys,*

*I hope everything is going fine in Kenya since last week. As [the supervisor] probably told you, we have some more information about the rest of the data collection.*

*As [one of the PIs] discussed with you during the training, we planned to interview about 2500 households. We now reached this goal, and so the original data collection officially comes to an end: we want to thank you for the work that you've done on the project.*

*Now, it is important for us to obtain more data, so we decided to do three more weeks of interviews. The last day of these three weeks is therefore Tuesday the 14th of August.*

*In order to reach our target of three more weeks of interviews, we have to offer a lower pay of 100Ksh per survey for each of the first three surveys instead of 150Ksh. This includes lunch allowance.*

[24] A possible concern about this announcement is that field workers interpreted the emphasis on the maximum number of 4 surveys per day as a reduction in the firm's gift. This is unlikely for two reasons. First, only one field worker consistently exceeded 4 surveys per day and all results are robust to excluding this field worker from the estimation. Second, the supervisors never enforced the maximum number of 4 surveys per day and instead paid field workers for all surveys they handed in per day.

*As before, you can do only 4 surveys max per day, with the 4th survey still being paid 100Ksh. So you can earn 400Ksh per day.*

*We realise that this is lower than before but with our budget, this is the only way for us to do three more weeks.*

*Also we want you to know that the trip to Masailand is still on after these three weeks.*

*Thanks again for all your work and I hope to see you soon.*

**Table Appendix 1: inconsistencies**

(NB: in some cases, there may be multiple inconsistency possibilities for a given reason (e.g. if applied to multiple household members or multiple farm plots))

| Inconsistencies | |
|---|---|
| Section | Description |
| Cover | Survey end time before survey start time |
| | Survey number blank but not a neighbour (a non-neighbour should have a survey number) |
| | Information on friend, but the respondent is a neighbour (there should be no information on friend if the repsondent is a neighbour) |
| | The respondent is a neighbour, but no survey number of the corresponding NGO member |
| Household | No other households but somebody in the homestead |
| | Married and living with spouse but no spouse |
| | Not married, married but spouse not in homestead, divorced, widowed; but with a spouse |
| | Someone is polygamous but there is no cowife |
| | Reports having only 1 household in the homestead, yet reports at least 1 member in the homestead roster |
| | No other households in homestead but received a gifts from other household in homestead |
| | No other households in homestead but provided gifts to another household in homestead |
| | Did not rent room to others but received room rental income |
| | Total homework time inferior to sum of homework time at some moments of the day |
| | People's combined time use yesterday across tasks is greater than 24 hours |
| | No tv but use a tv |
| | No tv in the homestead but no travel time to use it |
| | There is a tv in the home but it takes travel time to use it |
| | No household tv but use a household tv (IN TIME USE SECTION) |
| | No homestead tv but use a homestead tv (IN TIME USE SECTION) |
| | Homework - someone is in school, but reports having spent less homework in the entire day than just after sunset |
| | Numbers of years of schooling is at least 3 years greater than age-7 |
| | Reports being in school but also reports reasons for not being in school |
| Farming | Did not rent land to others but received land rental income |
| | Household claims not to farm, but has >0 plots on which it farms |
| | Household member worked on own farm according to roster, but when asked whether members of this hh farm, the answer is no |
| | No farming, but time spent farming on plots (for hhead and spouse) |
| | Got milk from milking but has no cattle |
| | Sold more milk than milked from cattle |
| | Owns the land, but doesn't select this option in the rental arrangement of the land |
| | Acres cultivated more than total acre of the same plot |
| | Acres irrigated more than total acre |
| | Acres irrigated more than cultivated |
| | No acres irrigated but method of irrigation |
| Business | No Head or spouse is reported working in own business (roster) yet hh reports having a business run by head or spouse (business section) |
| | Report having business that runs on electricity and is located inside homestead, yet homestead does not have electricity |
| Credit | Amount of debt repaid of the principal is more than the principal |
| | Credit - report that did not try to borrow, yet provided reasons for being turned down |
| Energy | Used electric applicance but did not have access to any sources of electricity |
| | Other households in homestead used electric applicance used but did not have access to any sources of electricity |
| | Uses electric saving lights but no form of electricity |
| | Use normal lights but no form of electricity |
| | No access to electricity type that can power an electric stove, yet powers an electric stove |
| | No access to electricity type that can power an electric pump, yet powers an electric pump |
| | No electricity whatsoever in household, yet use a household tv (IN TIME USE SECTION) |
| | No electricity whatsoever in homestead, yet use a homestead tv (IN TIME USE SECTION) |
| | Reports no cell phone but uses its electricity to charge their cell phone |
| Social attitudes and activities | No spouse but religion for spouse |
| | Spouse but no religion for spouse (or put as not applicable) |
| | No spouse, but spouse reported to attend church/mosque, or to be a member of up to 8 social groups |
| | Spouse, but spouse not reported to attend church/mosque, or to be a member of up to 8 social groups (or put as not applicable) |
| | People watch tv from this homestead, but no other households in the homestead: |
| | Do not own a tv (in hh or homestead), but people come to watch |
| | No radio listening but yes to some specific radio shows |
| | No tv watching but yes to some specific tv shows |
| | We need larger income diferences but the government should take money from the wealthy and give to poor: |
| | We need smaller income diferences but the government should not take money from the wealthy and give to poor |
| | We need larger income diferences but incomes should be made more equal: |
| | We need smaller income differences but incomes should not be made more equal |
| | Would be willing to take out loan for NGO shareholder, but do not trust NGO with your money |
| | Would very much like NGO to supply electricity to rural area but do not trust at all to manage money to build more projects (and vice versa) |

**Table Appendix 2: blanks and mistakes**

| Section | Name of variable blank | Condition to be not blank | Tests |
|---|---|---|---|
| All | entryname lastnamehead firstnameshead longo lato respondentfirstname respondentrelationship fieldworkername dateofinterview starttime endtime editorfirstname officecheckdate dataentryfirstname sa0901 sa0902 sa0903 sa0904 sa0905 en041st1 a02 a05p1 a06p1 a06p2 a07p1 a07p2 a08p1 a08p2 a09p1 cs0109 location sublocation neighbour greenpowermember longitude long2 long3 latitude lat2 lat3 interviewcomplete retinterviewcomplete hc01 hc02 hc03 hc04 hc051 hc061 hc07 hc0801 hc0802 hc0803 hc0804 hc0805 hc0902 hc0903 hc0904 hc0905 hc0906 hc1001 hc1002 hc1003 hc1004 hc1101 hc1102 hc1103 hc1104 hc1201 hc1201ohh hc1202 hc1202ohh hc1203 hc1203ohh hc1204ohh hc1205 hc1205ohh hc1206 hc1206ohh hc1207 hc1207ohh hc1208 hc1208ohh hc13 hc14 hc15 hc16 hc1701 hc1702 hc18 hc19 hc20 hc21 hc22 hc23 hc24 hc25 hc26 hc27b hc27c hc27e hc27f hc27g hc27h hc27i hc282 hc283 hc29 en0101 en0101ohh en0102 en0102ohh en0103 en0103ohh en0104 en0104ohh en0105 en0105ohh en0106 en0106ohh en02a en02aohh en02b en02bohh en02c en02cohh en02d en02dohh en02e en02eohh en02f en02fohh en02g en02gohh en02h en02hohh en02i en02iohh en02j en02johh en03a en03aohh en03b en03bohh en03c en03cohh en03d en03dohh en041st2 en0501 en0502 en0601 en0602 en0603 en0604 en0605 en0606 en0607 en0608 en0609 a01 a03p1 a03p2 a04p1 a11p1 a11p2 a1201p1 a1201p2 a1202p1 a1202p2 a1203p1 a1203p2 a1204p1 a1204p2 a1205p1 a1205p2 a1206p1 a1206p2 a1207p1 a1207p2 a1301p1 a1301p2 a1302p1 a1302p2 a1303p1 a1303p2 a1304p1 a1304p2 a1305p1 a1305p2 a1306p1 a1306p2 a1307p1 a1307p2 cs0101 cs0102 cs0103 cs0104 cs0105 cs0106 cs0107 cs0108 cs0110 cs0111 cs0112 cs0213 cs0214 cs0315 cs0316 cs0317 cs0318 cs0319 cs0320 s0101 s0201 s0102 s0202 s0103 s0203 s0104 s0204 s0105 s0205 s0106 s0206 s0107 s0207 s0108 s0208 s0109 s0209 s0110 s0210 sa01 sa02 sa02sp sa0301 sa0301sp sa0302 sa0302sp sa0303 sa0303sp sa0304 sa0304sp sa0305 sa0305sp sa0306 sa0306sp sa0307 sa0307sp sa0308 sa0308sp s0401 sa0401sp sa0402 sa0402sp sa05 sa05sp sa0701 sa0701conf sa0702 sa0702conf sa0703 sa0703conf sa0704 sa0704conf sa0901member sa0902member sa0903member sa0904member sa0905member sa1001 sa1002 sa1003 sa1101 sa1102 sa1103 sa1201 sa1202 sa1203 sa13gp sa13nongp sa13child sa13sp at01 at02 at03 at04 at05 at06 at07 at08 at0901 at0902 at0903 at0904 at0905 at0906 at1001 at1002 at1003 at1004 at1005 at1006 at1007 at1008 at1009 at1010 at1011 at1012 at1101 at1102 at1103 at1104 at1105 at1106 at1107 at1108 at1109 at1110 at1111 at1112 at1113 at1114 at1115 at1116 at1117 at1118 gp01 gp02 gp03 gp04 gp05 gp06 gp07 gp08 gp09 | None | 336 |
| Household | hr02‘i’ hr03‘i’ hr04‘i’ hr05‘i’ hr06‘i’ hr07‘i’ hr09‘i’ hr10‘i’ hr11‘i’ hr12‘i’ hr13‘i’ hr14‘i’ hr15‘i’ hr16‘i’ hr17‘i’ hr18‘i’ hr24‘i’ hr25‘i’ | if name present (hr01‘i’ ="") | 216 |
| | question 8 (smoke) | >=12 years | 12 |
| | questions38-58 | >=5 years | 252 |
| | questions 19-22 | if hr18‘i’=1 | 48 |
| | questions 28, 33 | >=12 years | 24 |
| | questions 30, 31, 32 | >=12 years and hr28‘i’==1 | 36 |
| | questions 35, 36, 37 | >=12 years and hr33‘i’==1 | 36 |
| Homestead | hm02‘i’ hm03‘i’ hm04‘i’ hm05‘i’ hm06‘i’ hm07‘i’ hm08‘i’ hm09‘i’ hm10‘i’ hm11‘i’ hm12‘i’ | if name present (hm01‘i’ ="") | 132 |
| Agricultural | Unit price of a product blank | if quantity sold | 62 |
| | Quantity sold blank | if unit price nonblank | 62 |
| Business | Reason for not doing business blank | No business | 1 |
| | bu03‘i’ bu04‘i’ bu05‘i’ bu06‘i’ bu07‘i’ bu08‘i’ bu09m‘i’ bu09y‘i’ bu10‘i’ bu11‘i’ bu12‘i’ bu13‘i’ bu14‘i’ bu15‘i’ bu16‘i’ bu17no‘i’ bu17kshs‘i’ bu18‘i’ bu19no‘i’ bu19kshs‘i’ bu20no‘i’ bu20kshs‘i’ bu21‘i’ | Business | 23 |
| Credit | cr010‘i’ cr020‘i’ cr110‘i’ | | 27 |
| | cr030‘i’ | if cr020‘i’==2 | 9 |
| | cr060‘i’ cr070‘i’ cr0100‘i’ | if cr020‘i’==1 | 27 |
| | cr08a0‘i’ cr08b0‘i’ | if cr020‘i’==1 | 18 |
| | cr120‘i’ cr130‘i’ cr140‘i’ | if cr110‘i’==1 | 27 |
| Total | | | 1348 |
| Mistakes | | | |
| Section | Description | | |
| Household | Zero household in the homestead | | 1 |
| | Smoke but less than 12 (question not asked to under 12): | | 12 |
| | Not in school, but homeworks. | | 12 |
| Total | | | 25 |