Do Workers Feel Entitled to High Wages? Evidence from a Long-Term Field Experiment*

Matthieu Chemin
McGill University

André Kurmann
Federal Reserve Board

June 5, 2012

Abstract

We followed fieldworkers 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 fieldworkers perceived as unmonitored. While wage increases had no significant effect, wage cuts back to the initial wage and then below the initial wage led to a strong and persistent decline in unmonitored effort, even though compensation throughout the experiment remained several times higher than the going market wage. The estimates provide clear evidence of a highly asymmetric reciprocity response to wage changes and imply that fieldworkers quickly came to feel entitled to higher wages when deciding on reciprocity. Together, these findings explain why firms rarely cut wages, an empirical phenomenon known as Downward Wage Rigidity.

Keywords: Reciprocity; Field experiment; Downward Wage Rigidity
JEL Classification: C93, J30

*We thank Ernst Fehr, Luke Taylor as well as seminar participants at The Wharton School and McGill University for valuable comments. Chemin thanks the Social Sciences and Humanities Research Council of Canada (SSHRC) and the Fonds de Recherche sur la Société et la Culture (FQRSC) for financial support. Kurmann gratefully acknowledges the hospitality of The Wharton School where part of this project was completed. All views are those of the authors and do not necessarily reflect those of the Federal Reserve Board; or any other institution with which the authors are affiliated. Contact information: matthieu.chemin@mcgill.ca; andre.kurmann@frb.gov.

"...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..." (Bewley, 2002, page 7).

1 Introduction

One of the most enduring puzzles in economics is why firms rarely cut wages, even during severe recessions.[1] Interview studies suggest that an important part of the answer for this phenomenon, commonly called Downward Wage Rigidity (DWR), is reciprocity in labor relations.[2] In this paper, we conduct a long-term field experiment that allows us to formally test the conditions necessary for reciprocity in labor relations to explain DWR.

Introduced into modern economics under the name of "partial gift exchange" and "fair wage hypothesis" by Solow (1979) and Akerlof (1982), reciprocity in labor relations posits that even in the absence of explicit incentives, workers derive a psychological benefit from providing high (low) effort in return for a wage above (below) some reference wage perceived as fair. For this theory to explain why firms find it optimal to rarely cut wages, three important conditions have to be met. First and somewhat obviously, workers need to react more negatively to wage offers below the fair wage reference than they react positively to wage offers above the reference.[3] Second, the negative effects of wage cuts have to be sufficiently long-lasting for them to affect the firm's wage setting decision. Third, workers need to pay little attention to market wages when assessing the fairness of a given wage offer and instead take their own existing wage as the main reference point.

This third condition – which is the message of Bewley's (2002) quote above – may appear less obvious but is essential for the fair wage hypothesis to deliver DWR during recessions and, more generally, wage rigidity over the business cycle. Intuitively, as job opportunities worsen during a recession, the market wage a worker can expect to make outside the current job falls. If this were to substantially lower the worker's fair wage reference, firms would cut wages without incurring negative effects on work effort. This would lower expected market wages even further, making wage cuts even more acceptable. By contrast, if workers primarily care about their own past wage, firms refrain from cutting wages even during recessions.[4]

[1] More generally, wage data for many countries display a remarkable absence of wage cuts for workers who do not switch jobs. See for example Dickens et al. (2007) on summary evidence from the International Wage Flexibility Project. Also see Fallick et al. (2011) and Daly et al. (2012) for evidence that downward wage rigidity in the U.S. remained remarkably stable during the recent Great Recession.

[2] See Kahneman et al. (1986); Blinder and Choi (1990); Levine (1993); Agell and Lundborg (1995, 1999); Campbell and Kamlani (1997); Bewley (1999) and the surveys by Bewley (2002) and Rotemberg (2006).

[3] See Elsby (2009) for a formal demonstration of the crucial role of asymmetry in the effort function for reciprocity theory to generate DWR and more generally the type of skewed cross-sectional wage change distribution observed in the data.

[4] The importance of this point has been shown formally in a dynamic general equilibrium context by Danthine and Donaldson (1990); Collard and De la Croix (2000); and Danthine and Kurmann (2004, 2010).

While numerous laboratory experiments have found general support for reciprocal behavior, it remains an open question whether the conditions for DWR are satisfied in actual labor markets.[5] Testing for these conditions is difficult for two related reasons. First, in order to determine whether the effects of reciprocal behavior are long-lived and whether workers adapt their wage reference to their own existing wage, it is crucial to observe the effort response of workers in ongoing employment relationships to a *sequence* of exogenous positive and negative wage changes. Second, the long-term nature of this test implies that the observed measure of effort needs to be considered as truly unmonitored by workers. Otherwise, it is impossible to disentangle the effects of reciprocal behavior from other explicit incentives such as firing threats or career motives that occur naturally in long-term relationships. In actual labor markets, both of these requirements are usually violated. Firms rarely change wages for truly exogenous reasons, let alone institute wage cuts; and the very act of measuring a given dimension of effort (e.g., a piece rate) makes it likely that workers perceive it as being monitored. Furthermore, if a firm can monitor effort, it is typically interested in using it as an explicit incentive.

In this paper, we present a field experiment that explicitly addresses these two difficulties, therefore allowing us to test whether the conditions for DWR hold. The experiment took place in rural Kenya where, over a 12-week period, local fieldworkers were employed to administer a household survey to approximately 3,000 community members. Fieldworkers were paid per survey at a rate that was several times higher than the going market wage. After six weeks of work at a constant 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 fieldworkers did not know in advance about any of the wage changes, nor did they know that they were taking part in an experiment.

The survey was initially designed to evaluate a development project and contained more than 900 questions. Answers to different questions of the survey could contradict each other, thus generating "inconsistencies". Surveys were never checked for inconsistencies in any way during the employment relationship for the simple reason that we as principal investigators had not established a list of possible inconsistencies at the time of data collection. Only more than a year later, after the survey answers had been manually entered into an electronic database, did we compile such a list and compute the number of inconsistencies for each

Also note that if the worker's wage reference were to mainly depend on peer workers' wages within the firm, a similar absence of wage rigidity would obtain because firms could then simply cut wages for all workers without suffering much of a negative reciprocity effect.

[5] Laboratory experiments providing support for reciprocity in labor relations 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). Also see Fehr and Gaechter (2000a) for an extensive survey. Levitt and List (2007) and Al-Ubaydli and List (2012) discuss the lack of generalizability of these experimental finding to real-world situations. More recently, field experiments have attempted to test for reciprocity in real-world labor markets. We discuss the relation of our paper to some of these studies below.

survey via a computer algorithm. Fieldworkers were expected to turn in surveys of "good quality" but had no reason to expect inconsistencies to be a performance target, nor were they aware that such a measure would be computed ex-post. The number of inconsistencies per survey therefore constitutes an (inverse) measure of effort that fieldworkers perceived as unmonitored.

We use panel regressions to analyze the total of 2,864 observations collected by 12 fieldworkers over the 12-week period. To take into account the correlation of the residuals within fieldworkers, we block-bootstrap and cluster the standard errors at the field worker level. The 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 wage cut back to baseline after the 3-week period of higher wages led to a large and significant increase in the rate of inconsistencies of about 28% (relative to the rate before the wage increase), *even though* the wage after this decrease was exactly the same as before the wage increase. The wage cut of 27% below the initial wage rate during the last two weeks of data collection resulted in an additional significant increase in inconsistencies.

To interpret our results, we present a reciprocity-based model of efficiency wages similar to the one 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 asymmetric reciprocal behavior, 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 occurs naturally either if workers have loss aversion or if the marginal productivity of the firm with respect to effort is decreasing. The finding that inconsistencies increase even when the wage returns to its initial level implies that workers adapt to their own existing wage and use it as an important reference point when assessing what constitutes a fair wage. The experiment thus fully confirms Bewley's (2002) conclusion that workers quickly come to feel entitled to their wage, no matter how high it may be. Furthermore, the persistent drop in effort lasting for several weeks suggests that the effects of negative reciprocity are sufficiently long-lived for the firm to avoid wage cuts. The evidence thus supports all three conditions necessary for reciprocity in labor relations to explain DWR.

Our strategy of following the same fieldworkers through time and simultaneously administering the same exogenous wage changes to all of them is close in spirit to the empirical approach adopted in other contexts by Bandiera et al. (2005, 2007, 2009). Two fundamental considerations motivate this strategy. First and as emphasized above, following the same fieldworkers through time is essential to test for the conditions behind DWR. It also has the advantage that all time-invariant sources of unobservable heterogeneity can be eliminated with worker fixed-effects, thus increasing statistical power. Second, simultaneously administering the same wage changes to all fieldworkers ensures that the experiment is not contaminated by social comparison effects. As Bandiera et al. (2011) stress, this is a first-order issue in labor market experiments where it is difficult to prevent information spillover. This was certainly the case for our experiment because fieldworkers interacted with each other on a daily basis. Hence, had we administered the wage changes just to a treatment group while leaving wages unchanged for a control group, effort of both the treatment and the control group would have likely been affected by this wage differential.[6]

A potential concern remains that inconsistencies could have increased because of adverse shocks that coincided with the exogenous wage cuts. This is unlikely for three reasons. First, we base all of the estimates on week-long averages rather than day-to-day changes so as to minimize the effects of random noise.[7] Second, we control for longer-lasting adverse shocks with a second (inverse) measure of effort for each fieldworker and survey, called "blanks and mistakes". Blanks and mistakes occurred if a survey field was left empty or contained an obvious error (e.g., zero household members). The crucial difference to inconsistencies is that blanks and mistakes were explicitly monitored, with the clear understanding that insufficient performance in this dimension would lead to dismissal. Standard shirking theory (e.g., Shapiro and Stiglitz, 1984) predicts that, given a sufficiently high wage relative to the outside market wage, monitoring should outweigh reciprocity concerns, making blanks and mistakes unresponsive to wage changes. To the extent that longer-lasting adverse shocks influence blanks and mistakes in the same way as inconsistencies, blanks and mistakes can therefore be used as a control group. Panel estimates indicate that blanks and mistakes are indeed sensitive to adverse shocks but that they did not change during the wage cuts. Moreover, a formal difference-in-differences estimation of inconsistencies on blanks and mistakes confirms all of our results. Third, we consider various hypothetical adverse shocks that would have increased inconsistencies but not blanks and mistakes at the time of the wage cuts (e.g. increased frequency of inconsistent answers towards the end of the experiment). We can, however, rule out all of these hypotheticals because they contradict a basic property of our data, which is that inconsistencies followed a secular downward (not upward) trend over the entire course of the experiment, interspersed by positive jumps at the dates of the wage cuts.

Our paper contributes to a growing body of field experiments on reciprocity in labor relations. Most closely related are three contemporaneous and independent studies that test for the effects of both positive and negative wage changes.[8] Kube et al. (2011) employ workers for a 6-hour data entry task, paying them either a higher or lower hourly wage than advertised prior to employment. Workers with higher than expected wage show no evidence of higher productivity whereas workers with lower than expected wage display a

[6] Alternatively, to prevent social comparisons effects, a control group could have been obtained from running a parallel survey without wage changes in another location. This is unlikely to solve the identification issue, however, since the two groups would then potentially be subject to different workplace conditions and different events.

[7] The results are robust to using 3-day averages instead of week-long averages.

[8] Other field experiments that consider the effects of positive wage changes include Gneezy and List (2006); Bellemare and Shearer (2009); Cohn, Fehr and Goette (2009); Kim and Slonin (2010).

strong negative reaction. Cohn et al. (2011) hire teams of two workers to sell promotional cards for two consecutive weekends. The second weekend, hourly wages are randomly lowered for either one or both workers of some teams. The wage cuts lead to a significant decline in the number of cards sold, which is more than twice as large for workers whose team member's wage is not cut. This clearly illustrates the importance of the aforementioned social comparison effects. Al-Ubaydli et al. (2008) hire workers for a two-day packaging job and measure the effects on both a monitored and an unmonitored dimension of effort of paying, among other schemes, a wage that is higher or lower than the advertised wage. They also find asymmetric effects of wage cuts, although their results are generally less conclusive. Our paper advances beyond these three studies in several ways. First, while the strong asymmetric reaction of inconsistencies to wage cuts confirms the general finding about the importance of negative reciprocity, our paper adds further that these negative reciprocity effects exist even if the initial wage is several times higher than the going market wage. Second and more importantly, the long-term nature of our employment relationship with first a wage increase and then a wage cut back to the initial wage allows us to test two of the essential conditions for DWR; i.e., whether a wage increase leads workers to adapt their wage reference upwards independent of outside market conditions and whether the effects of negative reciprocity are persistent. Neither of these questions can be addressed with a short-duration experiment in which the wage is changed only once. Third and related, the long-term nature of our experiment required us to measure a dimension of effort that workers perceived as unmonitored. Otherwise, it would have been impossible to disentangle explicit incentive effects from reciprocal behavior. Our inconsistency measure satisfies this requirement and, in our opinion, also 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).

The remainder of the paper proceeds as follows. Section 2 analyzes the effects of wage changes in an efficiency wage model with reciprocity concerns and explicit incentives from monitoring. Section 3 describes the wage experiment. Sections 4 reports basic results. Section 5 describes the econometric methodology and presents the main results. Section 6 discusses potential alternative explanations. Section 7 concludes.

2 A simple model of efficiency wages

To provide context for our wage experiment, we present 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).

For tractability, we assume that workers supply only one dimension of effort. The main results are robust, however, to a more general formulation in which workers supply different dimensions of (monitored and unmonitored) effort.[9]

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

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

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 reference level of effort for which the disutility of effort is minimized and $v' < 0$ and $v'' > 0$ otherwise. Without loss of generality, we normalize this reference level to $e = 0$ and thus $v'(0) = 0$. The term $R(e, w)$, finally, denotes the psychological benefit from reciprocating wage offer w with effort e. If the worker has no reciprocity concerns, then $\lambda = 0$. Otherwise, $\lambda > 0$.

Following Rabin (1993), we define $R(e, w)$ 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

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

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. Following Rabin (1993) one more step, we measure $g(w, \cdot)$ and $r(e, \cdot)$ 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 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

[9] Specifically, the analysis of the basic model remains exactly the same if the costs and benefits of the different effort dimensions are independent of each other. This is confirmed for our empirical analysis since we find no significant correlation between inconsistencies and blanks and mistakes before the wage changes (i.e., in the first 6 weeks). Even if the different effort dimensions are complements or substitutes, an equilibrium exists under relatively general conditions and the different interpretations of our empirical results hold. Details are available upon request.

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 the 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 $u(w^{ref})$ is the payoff from reference wage level w^{ref} that the worker considers as fair.[10] Hence, the firm's gift towards the worker becomes

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

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^{ref}. This fair wage reference w^{ref} potentially depends 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 existing wage (e.g., Bewley, 1999). Since the focus of our wage experiment is on the importance of the worker's own wage as a reference point, we do not need to take a stand on the relative importance of the other arguments in w^{ref}. At the same time, in order to study the effects of changes in the worker's own wage on reciprocal behavior, it is crucial that the other arguments in w^{ref} remain constant throughout the experiment. We return to this issue below in the description of the experiment.[11]

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. For $d = 0$, effort is not monitored, making explicit incentives irrelevant. For $d > 0$, 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.[12]

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

[11] 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.

[12] 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.

2.2 Implications for unmonitored effort

If effort is unmonitored; i.e., $d = 0$; then the worker faces the static problem of choosing e to maximize (1) subject to (2) – (4). Under relatively weak additional conditions needed for existence, we obtain:

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

Proof: Appendix.

The optimality condition states that the marginal disutility from providing effort e^* equals the marginal psychological benefit from reciprocating wage offer w.[13] The strict concavity is a direct implication of strict convexity assumption for the disutility of effort $v(\cdot)$ and the strict concavity assumptions for $u(\cdot)$ and $f(e, \cdot)$ (the strictness part of the assumption could be relaxed for two of the three functions).

Proposition 1 has a number of important implications. First, under the assumption that workers do not have reciprocity concerns (i.e., $\lambda = 0$), we obtain:

Result 1 *For $\lambda = 0$, workers always supply effort equal to the reference level $e = 0$.*

The intuition for this result is clear: if workers do not have reciprocity, $e^* = 0$ by assumption that the disutility of effort $v(e)$ is at its minimum at (the normalized level) $e = 0$.

Second, under the alternative assumption that workers have reciprocity concerns (i.e., $\lambda > 0$), we obtain:

Result 2 *For $\lambda > 0$:*

1. *The increase in effort to a given increase in the wage is strictly smaller (in absolute terms) than the decrease in effort in response to a wage cut of the same magnitude.*
2. *As long as the reference wage level w^{ref} is increasing in the worker's own existing wage, effort depends negatively on the worker's own existing wage.*
3. *Effort is independent of the time left in the employment relationship.*

$d = d(e)$ with $d' < 0$. It is possible to show that the results derived below are robust to such an extension of the basic model.

[13]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 one of the arguments of the reference wage w^{ref} and therefore influence the firm's gift) is considered exogenous.

Result 2.1 follows directly from the concavity of e^* in w established in Proposition 1. The asymmetric response of reciprocal behavior to positive and negative wage changes has been discussed in the literature (see references in the introduction) but, to our knowledge, has not been formally derived. Also, an extreme but as it turns out relevant case of asymmetry obtains if workers perceive the firm's payoff $f(e,\cdot)$ as increasing in e up to some effort level $e = \tilde{e}$ and constant thereafter (i.e., $f'(e) = 0$ for $e > \tilde{e}$).[14] In this case, an increase in the wage above the level for which $e^* = \tilde{e}$ does not increase effort whereas a wage cut may lead to a decrease in effort (provided that the wage cut is sufficiently large to imply $e^* < \tilde{e}$).

Results 2.2 and 2.3 follow from the fact that w^{ref} enters the firm's gift $g(w,\cdot)$ negatively, respectively that the worker's reciprocity problem is static. Together, the two results generate what Bewley (2002) calls "wage entitlement"; i.e., workers adapt to a given wage treatment, no matter how high it may be, and use it to assess the fairness of the firm.[15] Result 2.2 also makes clear that in order to test for the presence of wage entitlement, it is crucial to observe a worker's effort response to a sequence of exogenous positive and negative wage changes. Otherwise, it is impossible to determine whether the wage reference is primarily a function of the worker's own existing wage or, alternatively, a function of other factors such as the worker's outside option, wages of peers, or the firm's perceived ability to pay.

2.3 Implications for monitored effort

If effort is monitored; i.e., $d > 0$; the worker's problem is more complicated because there is both the static problem with respect to reciprocity and the dynamic problem with respect to the no-shirking level of effort imposed by the firm. Formally, consider a worker who is employed at the beginning of time period t and receives wage offer w_t. The value of employment is

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

where V_{t+1}^E is the value of continuing employment next period given some expected path of wages $\{w_s\}_{s=t+1}^T$;

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

is the value of being detected shirking, getting fired at the end of t and receiving outside option b thereafter; and $1(e_t < e^{NS})$ and $1(e_t \geq e^{NS})$ are indicator functions with value 1 if

[14] 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 $v'(e^*) = \lambda r_e(e^*,\cdot)g(w,\cdot)$ for $e^* < \tilde{e}$ and $e^* = \tilde{e}$ thereafter. The resulting reciprocity constraint is close to the reduced-form effort function $e^* = \min(w/w^{ref}, 1)$ postulated in Akerlof and Yellen (1990).

[15] 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.

$e_t < e^{NS}$ and $e_t \geq e^{NS}$, respectively.

To solve for optimal effort, denote by e_t^R the reciprocity effort level from Proposition 1 that would obtain if there was no monitoring. The solution to (5)-(6) can then be described as follows:

Proposition 2 *Given wage offer w_t and an expected path of wages $\{w_s\}_{s=t+1}^T$, there is a unique optimal effort level 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. The worker faces two different constraints implied by a given wage w_t: the implicit "reciprocity constraint" defined by $v'(e_t^R) = \lambda r_e(e_t^R, \cdot)g(w_t, \cdot)$ from Proposition 1; and the "no-shirking" constraint from the monitoring-induced firing threat described by the inequality in Proposition 2. The left-hand side of this 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. 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 $e_t^R < e^{NS}$. 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).[16]

Proposition 2 has a number of important implications. First, under the assumption that the worker does not have reciprocity (i.e., $\lambda = 0$), we obtain:

Result 3 *For $\lambda = 0$:*

1. *An increase in the expected 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*

[16] 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.

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 (i.e., $e_t^R = 0$).

Second, if the worker also have reciprocity concerns ($\lambda > 0$), the general solution from Proposition 2 applies and we obtain:

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

1. *An increase in the expected 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 the two results may seem complicated, they are a simple extension of Results 3 and can be easily understood by reconsidering Figure 1 for different wage levels.

The results have two important implications for the purpose of our wage experiment. First, suppose wages in the beginning of the employment relationship are sufficiently high relative to the outside option so that the no-shirking constraint outweighs reciprocity concerns (i.e., solution 2 applies). Then monitored effort will be unresponsive to wage increases and cuts back to the initial wage. This highlights the importance of having a measure of effort that workers perceive as unmonitored when testing for reciprocity in long-term field experiments.

Second, suppose an adverse shock negatively affects the worker's disutility of effort (e.g., heavy rainfall that makes survey collection more onerous). If sufficiently important, this shock makes it optimal for workers to reduce monitored effort below the no-shirking effort level e^{NS} imposed by the firm. Monitored effort therefore represents a measure to control for potential adverse shocks that coincide with the wage cuts in our experiment. This control is incomplete because an adverse shock may not be large enough to change the inequality constraint of the no-shirking condition; or because adverse shocks may affect reciprocal behavior but not explicit incentives due to monitoring. Notice, however, that by the forward-looking nature of the no-shirking condition, monitored effort is likely to be sensitive even to relatively small adverse shocks as long as workers expect them to be sufficiently persistent.

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 a community-based development project 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, with an estimated duration of 8 to 10 weeks.

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 fieldworkers 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 fieldworkers 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 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 cooperative work environment that should dampen any negative reaction to wage cuts.

After the 4-day training camp and a final performance assessment, the fieldworkers 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 without direct presence of the PIs started. In the beginning, fieldworkers typically administered between two and three surveys per day, six days a week. As the survey collection became more efficiently organized, fieldworkers increased their workload but were explicitly discouraged from doing more than 4 surveys per day. This target was generally well respected throughout the entire experiment, with the average number of surveys per field worker per day equalling 3.8 from week 4 onward.[17]

3.2 Experimental design

Under the initial compensation scheme, the first three surveys per day were paid 150 Ksh each and the fourth survey was paid 100Ksh. The fourth survey was paid 100Ksh to reduce disappointment for days when only 3 surveys were possible. Since the daily number of surveys was approximately constant from week 4 onward, this essentially implied a daily salary of 550 Ksh – three to four times more than what a field worker could hope to earn elsewhere.[18]

3.2.1 Wage changes

Figure 2 summarizes the different wage treatments over the 12 weeks of regular employment. Work weeks started on Wednesdays; hence, the weeks in Figure 2 represent intervals from Wednesday through the following Tuesday. During the first six weeks of regular employment, fieldworkers 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. This new "200/200 treatment" represented an average increase in daily compensation of about 45%. The announcement came without specific information on whether the raise was permanent or not. The new "200/200 treatment" continued for three weeks. In the beginning of week 10, 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). A week later, in the beginning of week 11, the wage rate was cut to 100 Ksh/survey for all surveys. This "100/100 Ksh treatment', which represented a cut of about 27% in daily compensation, remained in effect for the last two weeks of the experiment.

Figure 2 also compares our wage experiment with other field experiments, discussed in the introduction, that consider one single wage cut (or one single wage increase) in employment relationships of very short duration. By design, these experiments can only provide insights into the asymmetry of reciprocal behavior but not into whether the negative effects of wage

[17] Some field workers occasionally 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. Also, our regressions always control for the number of surveys done per day, and for fieldworkers fixed effects.

[18] At the time of the surveys, 550 Ksh were worth about US$7.4.

cuts are persistent; and whether the fair wage reference adapts to the worker's own existing wage. To see this, consider a one-shot experiment in which a wage cut leads to a drop in work effort. According to reciprocity theory, this drop in effort arises because the difference between the paid wage and the reference wage became smaller or turned negative. But nothing in the experiment says what influences the reference wage: it could be the going market wage, the wage of peers; the perceived ability of the firm to pay; or the worker's own existing wage.

In contrast, the sequence of wage changes in our experiment allows us to separate out the influence of the worker's own existing wage on the reference wage and therefore work effort. If the reference wage essentially depends on a combination of the going market wage, wages of peers and the firm's ability to pay, then the wage cut back to the initial 150/100 treatment in week 10 should not affect effort relative to the level of effort during the first 6 weeks with the 150/100 treatment. If the reference wage depends importantly on the worker's own existing wage, however, then the wage cut back to the 150/100 treatment in week 10 may affect effort because the worker adapted to the higher 200/200 treatment in weeks 7-9.

Of course, our experiment does not allow us to say anything about the relative importance of the other factors that may influence the reference wage. Our focus on the role of the worker's own existing wage in the reference wage seems of prime interest, however, considering the emphasis interview studies put on wage entitlement and the importance of wage entitlement for reciprocity theory to explain DWR.

3.2.2 Communication of wage changes

All wage changes were communicated through the supervisors, either by reading an email (for the wage raise) or by playing a pre-recorded video from the PIs (for the two wage cuts).[19] The complete scripts of the wage announcements are available in the Appendix. None of the wage changes were preannounced and did not come with any information about the length of the new wage treatment.

In theory, to measure reciprocity effects, no justification should be given for either a wage increase or a wage cut. In real-world labor market relationships such as ours, however, employees typically expect a justification for wage cuts. Absent a justification, employees may believe that the project is mismanaged, which may affect performance not because of reciprocal behavior but because workers' beliefs about the quality of the operation are altered.

To address this issue, we chose to provide minimal justifications for the wage cuts that were intended to dampen any negative reciprocity effects. In particular, the justification for the first wage cut in the beginning of week 10 back to the original wage was that our budget

[19] The wage cuts were communicated by video so as to avoid any suspicion on part of the workers that the supervisors embezzled the money destined to pay for wages.

was limited. At the same time, we reassured the workers that the project was not in sudden financial difficulty. This information conveyed a reduction in the firm's ability to pay which, if the workers' fair wage reference was affected by rent-sharing motives (i.e., Kahneman et al., 1986), should have a positive impact on reciprocal behavior on its own.

The announcement of the second wage cut was more involved as by the end week 10, we had reached the planned objective of 2500 surveys and therefore the end of the originally agreed upon employment contract. In the beginning of week 11, we reminded fieldworkers of this agreement and at the same time offered them a new employment opportunity to collect additional surveys for three more weeks at 100Ksh per survey. We justified this lower wage treatment by budget limitations. At the same time, 100Ksh per survey was still well above the best available outside option and thus, the 3-week extension can be considered, if anything, as an unanticipated bonus opportunity to earn more money. Indeed, all fieldworkers decided to stay on for the 3 additional weeks of work even though they were free not to participate (without losing out on any of the promised perks after the end of survey collection).

Finally, so as to avoid possible end-of-employment effects, we informed workers at the beginning of week 13 (i.e., one week before the planned end of the bonus employment) that since the target number of households had been reached, survey collection would halt immediately. Fieldworkers continued to be paid 400 Ksh per day for the last week without work so as to honor the promised employment contract.

3.2.3 Ethics

While workers did not know at any point in or after the employment relationship that they were taking part in an experiment, the different wage changes and related justifications respected the ethical principles of no breach of promise and beneficence (see Bandiera et al., 2011).[20] First, as employers, we respected all agreed upon contracts. In week 7, we increased the wage with no information about duration. Reverting back to the initial wage in week 10 therefore did not represent a breach of promise. In week 11, after the end of the initial data collection, we offered a new employment relationship. Even though this new relationship came with a lower wage, it therefore did not represent a breach of promise either. Moreover, the justification given for the wage cuts (a limited budget) was true: the original budget allowed collection of exactly 2500 surveys at the initial 150/100 treatment. Only the extra financial assistance from one of the PIs for the explicit purpose of the wage experiment made it possible to increase wages in weeks 7-9 and to extend employment for 3 weeks.

Second, the experiment did not cause any decrease in total compensation. To the contrary. The experiment allowed fieldworkers to make more money, first because surveys in weeks 7-9 were paid at a higher rate and second because there was an extension in employ-

[20] All ethical approvals from the relevant authorities were obtained and are available upon request.

ment for 3 weeks. Fieldworkers were free to terminate employment at any time but everyone chose to keep working through the entire experiment.

3.3 Unmonitored effort

The first (inverse) measure of effort we consider is inconsistencies. An inconsistency occurs when two or more answers to different questions in the survey contradict each other. For example, one 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. We argue that the number of inconsistencies per survey is a good measure of reciprocal behavior because fieldworkers perceived resolving inconsistencies as (i) unmonitored; (ii) beneficial for the employer; and (iii) costly to achieve.

First, the supervisors never monitored or punished in any way inconsistencies for the simple reason that we as PIs had not established a list of possible inconsistencies at the time of data collection. Nor did anyone (including we as the PIs) know at the time of the survey collection that such a measure would be computed ex-post. Only more than a year later, after the survey answers had been manually entered into an electronic database, did we determine 93 possible inconsistencies (see the appendix) and compile the number of inconsistencies for each survey via a computer algorithm. For all means and purposes of this experiment, inconsistencies therefore constitute a measure of effort that fieldworkers perceived as unmonitored.

Second, while we as PIs did not explicitly talk about inconsistencies during neither training nor actual survey collection, we repeatedly emphasized that we needed “good data” to rigorously evaluate the community-based development project for which we used the survey. Inconsistencies represent an obvious example of what “bad data” is and thus, fieldworkers likely perceived resolving inconsistencies as beneficial for the employer.

Third, detecting and resolving inconsistencies was costly in terms of effort because it implied that fieldworkers needed to pay extra attention when administering the survey; remember potentially conflicting answers; flip back and forth through the 20 pages of the survey; ask the respondent to clarify his/her answers; and resolve the inconsistency on the survey. This was an onerous and time-consuming process, especially because respondents were often household heads who commanded substantial respect in their community. Since fieldworkers 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. Inconsistencies therefore capture 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).

3.4 Monitored effort

The second (inverse) measure of effort we consider is "blanks and mistakes". A blank or mistake occurred, respectively, if a survey field was left empty (i.e., the field worker omitted to ask the question / pencil in the answer) or the field contained a clear error (i.e., reporting zero members in a household).

In contrast to inconsistencies, fieldworkers were explicitly trained to avoid blanks and mistakes. In addition, the supervisors randomly checked between 40% and 100% of all incoming surveys for these errors each day, depending on the time available. We therefore label blanks and mistakes as "monitored errors". If a survey with too many blanks and mistakes was detected, the fieldworker 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 fieldworker consistently made numerous avoidable mistakes. Despite further extensive training, performance did not improve, and the fieldworker was subsequently laid off.

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 also 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 fieldworkers 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).[21] Figures 3 and 4 display the result. Two basic observations stand out:

1. Inconsistencies jump up substantially in the beginning of weeks 10 and 11 when the two

[21] 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 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.

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.

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

To explore the statistical significance of these jumps, we compute the difference between the 3-day average of the effort measures immediately preceding the beginning of the workweek and the corresponding 3-day average starting with the beginning of the workweek, and block-bootstrap at the fieldworker level.[22] Confirming the visual inspection, we find that inconsistencies jump significantly in the beginning of weeks 10 and 11 but do not change significantly in the beginning of week 7 (nor in any of the other weeks when no wage change occurred). Furthermore, blanks and mistake do not change significantly for any of the weeks when there were wage changes. These basic results point towards absence of positive reciprocity in response to the wage increase but the presence of negative reciprocity in response to wage cuts.

An additional basic observation from Figures 3 is that inconsistencies display a secular downward trend over the entire course of the experiment, interspersed by positive jumps at the time of the wage cuts. For blanks and mistakes, by contrast, no such trend is discernible (apart from the first two weeks). This suggests that field workers may have accumulated experience in detecting and resolving inconsistencies as the work progressed (i.e. learning-by-doing). Our panel estimates in the next section control for this secular downward trend in inconsistencies.

5 Panel estimates

To increase power and to compare effort levels across the sequence of the different wage changes, we now turn to panel estimations on the full dataset.

5.1 Methodology

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 α_j captures fixed worker effects; $\mathbf{D}_{wage}$ is a vector of dummy

[22] We block-bootstrap at the fieldworker level to address the concern of positive autocorrelation in the rate of inconsistencies within fieldworkers. We thus resample entire fieldworkers instead of individual surveys (Bertrand, Duflo, Mullainathan, 2004).

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, fieldworkers and time.[23] 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 at the end of Section 6, all of the main results are robust to other forms of the time trend. Note also that this time trend is identified separately from the wage dummies in D_{wage} because we make it a function of survey day t. Our panel estimation is thus best viewed as a regression discontinuity design that detects jumps in inconsistencies over and above any secular trend. Finally, u_{ijt} denotes the disturbance term. Standard errors are clustered at the level of the field worker, to take into account issues of serial correlation within fieldworkers, the unit at which the wage changes are implemented (Moulton, 1986).

The key coefficients of interest are contained in the vector β and measure the effect that the different wage dummies in 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 averages out random noise while keeping time intervals sufficiently small to captures discontinuities from wage changes.[24] 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.[25] Vector 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, β_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; β_{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 Main results

Column (1) of Table 2 displays the estimates for inconsistencies, our (inverse) measure of unmonitored effort. Robust standard errors clustered at the level of fieldworkers are reported in parentheses below each estimate. The first five coefficients (β_1 to β_5) show that 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.

[23]Specifically, $\mathbf{X}_{ijt}$ includes indicators for the area in which the interview took place; the relationship of the interview respondent to the household head, and the number of surveys done per day by the fieldworker.

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

[25]As shown at the end of Section 6, all results are robust to choosing week 5 as the reference.

The next three coefficients (β_7 to β_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 either. By contrast, the last three coefficients (β_{10} to β_{12}) show that relative to the initial 150/100 treatment during reference week 6, the rate of inconsistencies jumps significantly by 1.38 percentage points as the wage first returns to the original 150/100 treatment in week 10, and jumps by 2.4 percentage points as compensation is lowered to the 100/100 treatment in weeks 11 and 12. Relative to week 6, this represents an increase in inconsistencies of 28 percent and 49 percent, respectively. 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. Specifically, the rate of inconsistencies increases significantly by 0.82 percentage points from week 9 to week 10; and by 0.62 percentage points from week 10 to week 11. Relative to the average rate of inconsistencies of 4.65 percent per survey, this represents an increase of 17.6 percent and 13.3 percent, respectively.

Section 6.3 below shows that all results are robust to using only the first three surveys per fieldworker per day in the regression; or when comparing the fourth survey per day paid at 100Ksh in the initial 150/100 treatment to the four surveys per day in the 100/100 treatment of the last two weeks.

Several key implications come out of these results. First, as discussed in Section 2, the absence of positive reciprocity in response to the wage increase in weeks 7-9 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. An alternative explanation for the absence of significant coefficients for weeks 7-9 is that positive reciprocity effects from the wage increase are very short-lived and therefore average out over the week interval. This would be consistent with Gneezy and List (2006) who find in their field experiment that positive reciprocity disappears already after a few hours of work. In any case, our estimates show that the 45% increase in daily compensation did not result in a persistent increase in monitored effort.

Second, the significant increase in inconsistencies in weeks 10 and 11 when wages are cut shows the presence of negative reciprocity. Together with the absence of positive effects in response to the wage increase, this provides clear evidence of asymmetric reciprocal behavior with respect to wage cuts. The result is all the more striking since compensation throughout the entire experiment remained several times higher than the going market wage; we as PIs went to great lengths to foster a cooperative work environment; and the wage cuts were justified as necessary to respect budget limitations (for the first wage cut) and as an additional bonus employment contract (for the second wage cut).

Third, the fact that there are more inconsistencies in week 10 than in week 6, although the wage treatment are exactly the same in both weeks, suggests that workers adapted quickly to the higher 200/200 treatment in weeks 7-9 and used it as the new reference against which to assess any new wage offer. Again, this is all the more striking because daily compensation in all these weeks was several times the going market wage, thus offering strong support for Bewley's (2002) conclusion from the beginning of the paper 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).

Fourth, the negative reciprocity effects from the wage cuts are persistent, lasting for at least two weeks in the case of the second wage cut.

Overall, our estimates thus provide empirical support for all three conditions necessary for reciprocity in labor relations to imply DWR.

5.3 Controlling for coincidental adverse shocks

A possible concern about the results in Table 2 is that reciprocal behavior is irrelevant and that inconsistencies increased instead because of adverse shocks that coincided with the wage cuts in weeks 10 through 12. Notice first that all of our estimates pertain to week-long averages rather than day-to-day comparisons of inconsistencies. This minimizes the influence of random noise. Moreover, extensive debriefing with the supervisors did not reveal any longer-lasting shocks such as an extended period of rain or festivities that would have adversely affected work performance. Still, this does not rule out other unobserved shocks that coincided with the wage cuts.

To test and control for such adverse shocks, we use our second (inverse) measure of effort "blanks and mistakes" committed by the same fieldworker on the same survey. As explained in Section 2, if wages are sufficiently high for the no-shirking constraint in Proposition 2 to bind in the beginning of the experiment, blanks and mistakes do not react to the wage increase in week 7 followed by the wage cut in week 10. Blanks and mistakes may, however, still react to adverse shocks that also affect inconsistencies, especially if these shocks are perceived as relatively persistent (e.g., a persistent shift in the disutility of effort) and thus gain in importance through the present-value nature of the no-shirking constraint.

Column (2) of Table 1 shows the panel estimates for blanks and mistakes. First, blanks and mistakes remain constant during weeks 1-4 but increase significantly in week 5. Blanks and mistakes are thus picking up some adverse shock for week 5 that, at the same time, did not significantly affect inconsistencies. This confirms that blank and mistakes are relatively sensitive to changes in the environment, making them a promising measure to control for coincident shocks. Second, blanks and mistakes do not change significantly in weeks 7-12 during which the different wage changes occurred. This suggests that wages were indeed high enough for the no-shirking constraint to always outweigh worker's reciprocity concerns with respect to blanks and mistakes, thus highlighting the importance of having an unmonitored measure of effort when testing for reciprocity in long-term employment relationships. At the same time, the absence of a significant reaction in blanks and mistakes during weeks 7-12 provides further evidence against the hypothesis that the increase in inconsistencies during the weeks of the wage cuts was driven by coincidental adverse shocks instead of reciprocal behavior.

To underline this point, we perform a difference-in-differences estimation of inconsistencies and blanks and mistakes on our set of wage dummies and control variables

$$Inconsistencies_{ijt} - BlanksAndMistakes_{ijt} = \alpha_j + \boldsymbol{\beta}\mathbf{D}_{wage} + \boldsymbol{\delta}\mathbf{X}_{ijt} + \gamma_1 t + \gamma_2 t^2 + u_{ijt}. \quad (8)$$

The coefficients in $\boldsymbol{\beta}$ now isolate the impact of the wage changes on the difference between inconsistencies and blanks and mistakes. Column (3) of Table 2 shows that none of our results about inconsistencies change, thus confirming the above conclusion. Of course, blanks and mistakes represent a good control group only to the extent that they are affected in the same way by adverse shocks as inconsistencies. Hence, our difference-in-differences results only rule out a subset of possible adverse events. We return to this issue in the next section. Nevertheless, the difference-in-differences results in Table 2 are encouraging for our interpretation of the results.

Another (more standard) approach to control for coincidental adverse events would have consisted of keeping wages constant for a randomized group of workers and using their time path of inconsistencies as a control group. As Bandiera et al. (2011) stress, however, this approach is highly problematic for labor market experiments in which it is difficult to prevent information spillover across workers. This was certainly the case for our experiment because fieldworkers interacted with each other on a daily basis. Treatment and control group would thus have immediately known of their differential wage treatment, which would have likely biased the outcome of our experiment due to social comparison effects. Results in Shi (2010) and Cohn et al. (2011) demonstrate that these social comparison effects can be very strong for both the control and the treatment group. We thus purposefully avoided to form a randomized control group of workers with constant wages, and instead simultaneously changed the wage of all workers in our experiment.

6 Alternative explanations and robustness checks

As discussed above, certain adverse shocks may affect inconsistencies without impacting blanks and mistakes. Here, we consider a set of such hypothetical shocks but argue that none of them are likely to drive our results. An important part of this argument is that any such adverse shock would imply a secular increase in inconsistencies during weeks 10-12, which is contradicted by the basic observation in Figure 3 that inconsistencies display positive jumps at the time of the wage cuts followed by a gradual decrease. The end of the section provides additional robustness checks in support of our main results.

6.1 Coincidental changes to frequency of inconsistent answers

One of the differences between inconsistencies and blanks and mistakes is that the latter depend only on the fieldworker's effort, while the former also depend on the respondents' ability to correctly answer questions. An alternative explanation is thus that inconsistencies but not blanks and mistakes increased in weeks 10-12 because of a coincidental shock to the quality of respondents that increased the frequency of inconsistent answers. First, one might be concerned that fieldworkers happened to interview first the households for which it was easy to meet the household head (the preferred individual to interview) and only later on interviewed the households for which the household head was not available and instead a next-of-kin provided the answers. As next-of-kins may not be aware of as many details about the household than household heads, this could have resulted in a higher frequency of inconsistent answers. To address this concern, we control in all regressions for 18 dichotomous variables coding the relationship of the respondent to the household head. Furthermore, Table 3 shows that the results are very similar independent of whether these respondent controls are included (column (1) which replicates column (3) of Table 2) or not (column (2) of Table 3).[26]

Alternatively, one might be concerned that respondents became systematically harder to interview over time because fieldworkers selected the "easiest" respondents at the beginning of the data collection, and kept the "hardest" ones for the end. Inconsistencies would thus naturally increase with time, but not blanks and mistakes. This is unlikely for a number of reasons. First, the list of households to interview throughout the experiment were organized according to sublocations, and within a sublocation, households were chosen randomly. This made it impossible to first select the easiest ones. Second, we made sure that the different wage changes did not coincide with a change in sublocation. Third, sublocations were chosen randomly and not kept for the end. Fourth, all of our regressions control for sublocation fixed effects. Column (3) of Table 3 confirms that the results are not dependent on the inclusion of these controls.

Aside from these controls, a coincidental increase in the frequency of inconsistent answers would have implied a secular increase in inconsistencies during weeks 10-12. As explained above, this is contradicted by the basic observation in Figure 3 that inconsistencies jumped up at the time of the wage cuts and gradually decreased thereafter.

Based on these considerations, we conclude that coincidental changes to the frequency of inconsistent answers are unlikely to drive our results.

[26] For space reasons, Table 3 does not show the coefficients for the initial weeks of the wage experiment. The coefficients are, however, included in all regressions.and do not change significantly.

6.2 Coincidental changes specific to correcting inconsistencies

Another potential alternative explanation is that there were adverse shocks that only affected worker's reciprocal incentives to correct inconsistencies but not blanks and mistakes, either because the shocks were too small to invalidate the no-shirking constraint or because the shocks did not affect explicit incentives from monitoring. We consider four such hypothetical cases.

6.2.1 Fatigue

The first hypothetical case is that fieldworkers became tired over the course of the data collection. This would explain an increase in inconsistencies towards the end of the data collection that have nothing to do with reciprocity effects. Blanks and mistakes, by contrast, could remain stable if fieldworkers' fear of dismissal if caught shirking remained sufficiently important.

As discussed above, however, a basic observation about Figure 3 is that there was no secular increase in inconsistencies but, to the contrary, a secular decrease that lasted until the very end of the experiment. If at all, workers therefore benefitted from learning-by-doing effects.

Based on these considerations, we conclude that fatigue is unlikely to be behind our results.

6.2.2 Coincidental changes in the number of surveys completed

A second hypothetical case is that after the wage cuts, fieldworkers increased the number of surveys per day to keep their total income constant. This increase in quantity could have led fieldworkers to commit more inconsistencies per survey, independent of any reciprocity effects. By contrast, since blanks and mistakes are easier to avoid, fieldworkers could have kept effort constant on that dimension so as to minimize the risk of being caught shirking.

As already noted in Section 3, however, the average daily number of surveys per fieldworker remained very stable throughout the experiment. We confirm this formally with a panel regression of the number of daily surveys per fieldworker on the different wage dummies and other control variables in (7) and find all coefficients to be far from significance. This lack of variation in the number of daily surveys should not come as a surprise given that we constantly insisted on a limit of 4 surveys per day.[27] Moreover, our panel estimates control

[27] For example, in the announcement of the first wage increase, we said "It is unacceptable to do 5 surveys per day. We only pay for 4 surveys per day." In the announcement of the first wage cut, we repeated "As usual, you can only do a max of 4 surveys per day." In the announcement of the second wage cut, we said "As before, you can do only 4 surveys max per day". This was well respected in practice, with an average of 3.6 surveys per day. 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.

for the number of daily surveys done per fieldworkers. Column (4) of Table 3 confirms that estimates without this control are very similar to the baseline results.

Based on all these results, we conclude that there are no coincidental changes in the number of surveys.

6.2.3 End-of-game effects

A third hypothetical case is that fieldworkers anticipated the end of the data collection and thus decreased their effort (i.e., corrected less inconsistencies) towards the end of the experiment. By contrast and parallel to the previous case, they could have kept effort on the blanks and mistakes dimension constant so as to minimize the risk of being caught shirking.

Several arguments speak against this case. First, reciprocal behavior should not be subject to end-of-game effects, exactly because reciprocity is based on psychological incentives of returning the favor of a given wage treatment and not explicit incentives. Proposition 1 of our model in Section 2 makes this point very clearly: the worker's optimal reciprocity decision for unmonitored effort is a static one and does not depend on expectations or the time left in the relationship. By contrast, the no-shirking constraint behind monitored effort is forward-looking and implies that as $t \to T$, the incentives to keep monitored effort at the no-shirking level imposed by the firm diminishes. So, on theoretical grounds, end-of-game effects should apply, if at all, to blanks and mistakes but not inconsistencies.

Second, the theoretical argument is confirmed in our data. As discussed before, Figure 3 shows no increase in inconsistencies towards the end of the experiment. To the contrary, there is a secular downward trend in inconsistencies.

Based on these arguments, we conclude that end-of-game effects are not responsible for the increase in inconsistencies.

6.2.4 Loss of confidence in employer

A fourth hypothetical case is that the sudden wage changes in different directions made fieldworkers lose confidence in the ability of their employers to manage the project. This could have consequently led fieldworkers to believe that correcting surveys for inconsistencies to assure "good quality" is not important. By contrast, fieldworkers could have kept blanks and mistakes low so as to conform to the no-shirking constraint.

Similar to the arguments made above, this case seems unlikely because inconsistencies do not display a secular increase over time but rather jumps at the time of the wage cuts followed by a recurring downward trend that is picked up by our time trend. Furthermore, as explained in section 3.2.2, we were careful to thoroughly justify the wage cuts so as to prevent fieldworkers from thinking that the project was mismanaged. Debriefing with some fieldworkers after the end of the survey collection confirmed that they did not think the project was mismanaged.

Based on these arguments, we conclude that loss of confidence is not responsible for the increase in inconsistencies either.

6.3 Other robustness checks

Columns (5) – (7) of Table 3 report a number of additional robustness checks for our panel regressions in Table 2. Column (5) shows that none of the results change when the reference week is changed to week 5. Additionally, this column shows no significant change in inconsistencies for week 6, indicating that fieldworkers did not anticipate the wage increase in week 7. Column (6) shows that none of the results change when the two training weeks prior to the regular work relationship are included (the weeks when one of the PIs was present). Column (7), finally, shows that the main conclusions of the paper are robust to the replacement of the quadratic time trend with a linear time trend. The estimates are, however, somewhat less strong since the linear time trend picks up less of the secular decrease in inconsistencies that follows the jump in response to the wage cuts (as displayed in Figure 3).[28] In other words, if one insists that the secular decrease in inconsistencies over the course of the experiment should be linear, then the persistence of the negative reciprocity effects to the wage cuts is reduced.

Table 4 reports further robustness 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 the difference between inconsistencies and blanks and mistakes of wage changes *within* each day. As the estimates show there is no significant difference 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 2 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 2 by using only the first three surveys for each day. All results are confirmed: (i) there is no significant reaction 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; and (iii) inconsistencies increase even further as the wage drops to the 100/100 treatment in week 11.

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

[28] By constrast, the significance of the results for weeks 10-12 is robust to the inclusion of a cubic term for the time trend.

100 Ksh and was administered at the end of the day. This test provides further confirmation of the wage entitlement effect discussed above.

7 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 follow workers over a 12-week employment relationship and estimate their reciprocal behavior to a sequence of wage increases and wage cuts. To disentangle the explicit incentives that naturally arise in such long-term relationships, we devised a measure of effort that workers perceived as truly unmonitored. The three main results coming out of our experiment are that (i) workers exhibited a pronounced asymmetric reciprocity response to wage cuts, even though wages throughout the entire experiment were several times higher than the going market wage; (ii) workers quickly adapted their wage reference to a new higher level of pay when deciding on the reciprocity response to a given wage offer; (iii) the negative reciprocity effects of wage cuts were persistent, lasting for a week or more.

As discussed in the introduction, the results provide the necessary conditions for why firms are typically reluctant to cut wages – a phenomenon known as DWR. 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, the field experiment represents a counterfactual of what a firm should *not* do, with the negative and persistent reaction of workers to the wage cuts confirming Bewley's point.

References

[1] Agell, J. and P. Lundborg, 1995. Theories of pay and unemployment: survey evidence from Swedish manufacturing firms. *Scandinavian Journal of Economics* 97, 295—307.

[2] Agell, J. and P. Lundborg, 1999. Survey evidence on wage rigidity: Sweden in the 1990s. FIEF Working Paper 154.

[3] Akerlof, G., and J. Yellen, 1990. The Fair-Wage Effort Hypothesis and Unemployment. *Quarterly Journal of Economics*, 105 (1990), 255-283.

[4] Akerlof, G. A., 1982. Labor Contracts as Partial Gift Exchange. *Quarterly Journal of Economics,* 97, 543- 569.

[5] Al-Ubaydli. O., Andersen, S., Gneezy, U., and J. List. (2008). For love or money? Comparing the effects of non-pecuniary and pecuniary incentive schemes in the workplace. George Mason University. mimeo.

[6] Al-Ubaydli, O. and J. List (2012). On the generalizability of experimental results in economics, in Frechette, G. and A. Schotter (eds) The Methods of Modern Experiments, Oxford University Press.

[7] Bandiera, O., Barankay, I., and I. Rasul, 2005. Social Preferences and the Response to Incentives: Evidence from Personnel Data. *Quarterly Journal of Economics*, 120(3): 917–62.

[8] Bandiera, O., Barankay, I., and I. Rasul, 2007. Incentives for Managers and Inequality Among Workers: Evidence From a Firm-Level Experiment. *The Quarterly Journal of Economics*, MIT Press, vol. 122(2), pages 729-773, 05.

[9] Bandiera, O., Barankay, I., and I. Rasul, 2009. Social Connections and Incentives in the Workplace: Evidence From Personnel Data. *Econometrica*, Econometric Society, vol. 77(4), pages 1047-1094, 07.

[10] Bandiera, O., Barankay, I., and I. Rasul, 2011. Field Experiments with Firms. *Journal of Economic Perspectives*, American Economic Association, vol. 25(3), pages 63-82, Summer.

[11] Bellemare, C., and B. Shearer, 2009. Gift Giving and Worker Productivity: Evidence from a Firm Level Experiment. *Games and Economic Behavior*, vol. 67, pp. 233-244

[12] Bertrand M., Duflo E., and S. Mullainathan, 2004. How Much Should We Trust Differences-in-Differences Estimates? *The Quarterly Journal of Economics*, vol. 119(1), pages 249-275, February.

[13] Bewley, T. F., 1999. Why Wages Don't Fall During a Recession. Cambridge: Harvard University Press.

[14] Bewley, T. F., 2002. Fairness, Reciprocity, andWage Rigidity. Cowles Foundation Discussion Paper No. 1383.

[15] Blinder, A. S., and D. H. Choi, 1990. A Shred of Evidence on Theories of Wage Stickiness. *The Quarterly Journal of Economics*, 105 (1990), 1003-1015.

[16] Campbell, C., Kamlani, K., 1997. The Reasons for Wage Rigidity: Evidence from Survey of Firms. *Quarterly Journal of Economics,* 112, 759-789.

[17] Charness, G., G. R. Frechette, and J. H. Kagel, 2004. How Robust Is Laboratory Gift Exchange? *Experimental Economics*, 7 (2004), 189-205.

[18] Charness, G., and P. Kuhn, 2007. Does Pay Inequality Affect Worker Effort? Experimental Evidence. *Journal of Labor Economics*, 25 (2007), 693-723.

[19] Charness, G., and M. Rabin, 2002. Understanding Social Preferences With Simple Tests. *The Quarterly Journal of Economics*, MIT Press, vol. 117(3), pages 817-869, August.

[20] Cohn, A., E. Fehr, and L. Goette, 2009. Fair Wages and Effort: Evidence from a Field Experiment. IEW Working Paper, University of Zurich.

[21] Cohn, A., E. Fehr, B. Herrmann, and F. Schneider, 2011. Social Comparison in the Workplace: Evidence from a Field Experiment. IZA Discussion Papers 5550, Institute for the Study of Labor (IZA)

[22] Collard, F., De la Croix, D., 2000. Gift Exchange and the Business Cycle: The Fair Wage Strikes Back. *Review of Economic Dynamics,* 3, 166-193.

[23] Daly, Mary, Bart Hobijn and Brian Lucking. 2012. Why Has Wage Growth Stayed Strong? *Federal Reserve Bank of San Francisco Economic Letter* 2012-10.

[24] Danthine, J-P. and J. Donaldson, 1990. Efficiency Wages and the Real Business Cycle. *European Economic Review* 34, 1275-1301.

[25] Danthine, J.-P. and A. Kurmann, 2010. The Business Cycle Implications of Reciprocity in Labor Relations. *Journal of Monetary Economics*, vol. 57(7), 837-850.

[26] Danthine, J.-P. and A. Kurmann, 2008. The Macroeconomic Consequences of Reciprocity in Labor Relations. *Scandinavian Journal of Economics,* vol. 109(4), 857-881.

[27] Danthine, J. P., Kurmann, A., 2004. Fair Wages in a New Keynesian Model of the Business Cycle. *Review of Economic Dynamics,* 7, 107-142.

[28] Dickens, W., Goette, L., Groshen, E., Holden, S., Messina, J., Schweitzer, M., Turunen, J., and M. Ward, 2007. How Wages Change: Micro Evidence from the International Wage Flexibility Project. *Journal of Economic Perspectives,* Volume 21, N.2, Spring 2007, Pages 195–214.

[29] Falk, Armin & Fischbacher, Urs, 2006. A theory of reciprocity. *Games and Economic Behavior*, Elsevier, vol. 54(2), pages 293-315, February.

[30] Fallick, Bruce, Michael Lettau and William Wascher. 2011. Downward Nominal Wage Rigidity in the United States during the Great Recession. Working paper.

[31] Fehr, Ernst, Georg Kirchsteiger, and Arno Riedl. 1993. Gift Exchange and Ultimatum in Experimental Markets. Vienna Economics Papers 9301, University of Vienna, Department of Economics

[32] Fehr, E. and A. Falk, 1999. Wage Rigidity in a Competitive Incomplete Contract Market. *Journal of Political Economy,* 107, 106-134.

[33] Fehr, E. and S. Gächter, 2000a. Fairness and Retaliation: The Economics of Reciprocity. *Journal of Economic Perspectives,* 14, 159-181.

[34] Gneezy, U., and J. List, 2006. Putting Behavioral Economics to Work: Field Evidence of Gift Exchange. *Econometrica*, 74 (2006), 1365-1384.

[35] Hannan, R. L., J. H. Kagel, and D. V. Moser, 2002. Partial Gift Exchange in an Experimental Labor Market: Impact of Subject Population Differences, Productivity Differences, and Effort Requests on Beha. *Journal of Labor Economics*, 20 (2002), 923-951.

[36] Kahneman, D., Knetsch, J. L., Thaler, R., 1986. Fairness as a Constraint of Profit Seeking: Entitlements in the Market. *American Economic Review,* 76, 728-241.

[37] Kim, M. and R. Slonin, 2010. The effect of the gift exchange with multidimensional effort: Evidence from a hybrid field-lab experiment.

[38] Kube, S., M. Maréchal, and C. Puppe, 2011. Do Wage Cuts Damage Work Morale? Evidence from a Natural Field Experiment. Forthcoming in *Journal of the European Economic Association.*

[39] Levine, D. I., 1993. Fairness, Markets, and Ability to Pay: Evidence from Compensation Executives. *American Economic Review* 93(5), 1241-59.

[40] Levitt, S.D., & List, J.A. (2007). What do laboratory experiments measuring social preferences reveal about the real world? The Journal of Economic Perspectives, 21(2), 153-174.

[41] Moulton, B., 1986. Random Group Effects and the Precision of Regression Estimates. *Journal of Econometrics* 32 (1986) 385-397.

[42] Rabin, M., 1993. Incorporating Fairness into Game Theory and Economics. *American Economic Review,* 83, 1281-1302.

[43] Rotemberg, J., 2006. Altruism, Reciprocity and Cooperation in the Workplace, in Serge Christophe Kolm and Jean Mercier Ythier, eds., *Handbook on the Economics of Giving, Reciprocity and Altruism,* vol. 2, North Holland, pp 1371-1407.

[44] Shapiro, C., Stiglitz, J. E., 1984. Equilibrium Unemployment as a Worker Discipline Device. *American Economic Review,* 74, 433-444.

[45] Shi, L., 2010. Incentive Effect of Piece-Rate Contracts: Evidence from Two Small Field Experiments. *The B.E. Journal of Economic Analysis & Policy*, Berkeley Electronic Press, vol. 10(1), pages 61.

[46] Solow, R. M., 1979. Another Possible Source of Wage Rigidity. *Journal of Macroeconomics,* 1, 79-82.

[47] Williamson, O., 1985. The Economic Institutions of Capitalism: Firms, Markets, Relational Contracting. New York, Free Press, 1985.

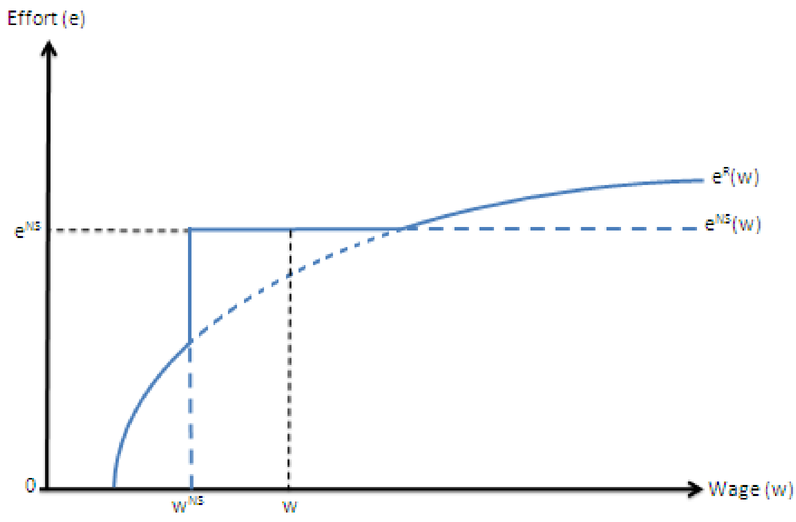

Figure 1: The effort function

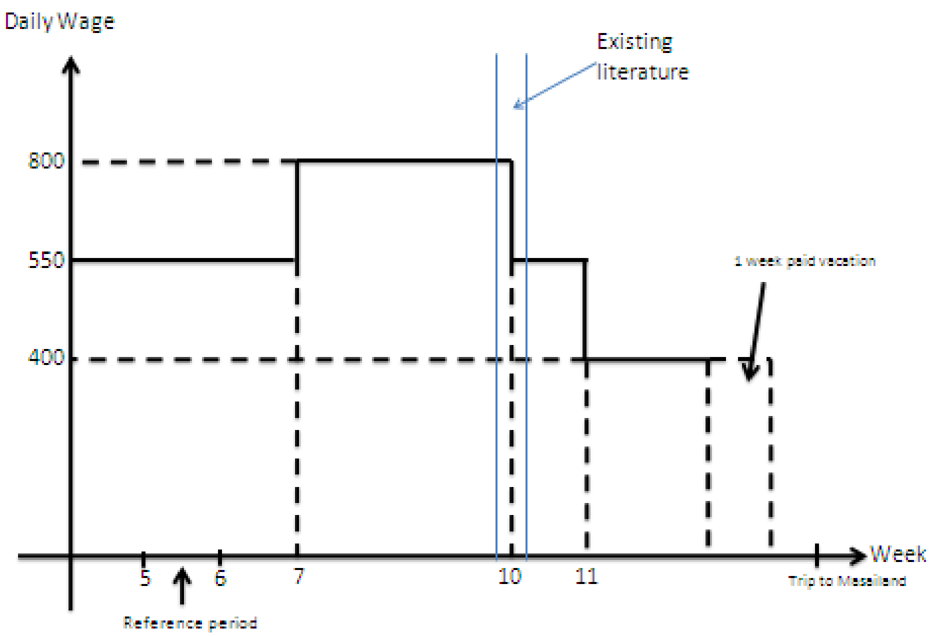

Figure 2: Timing of changes in wage treatments.

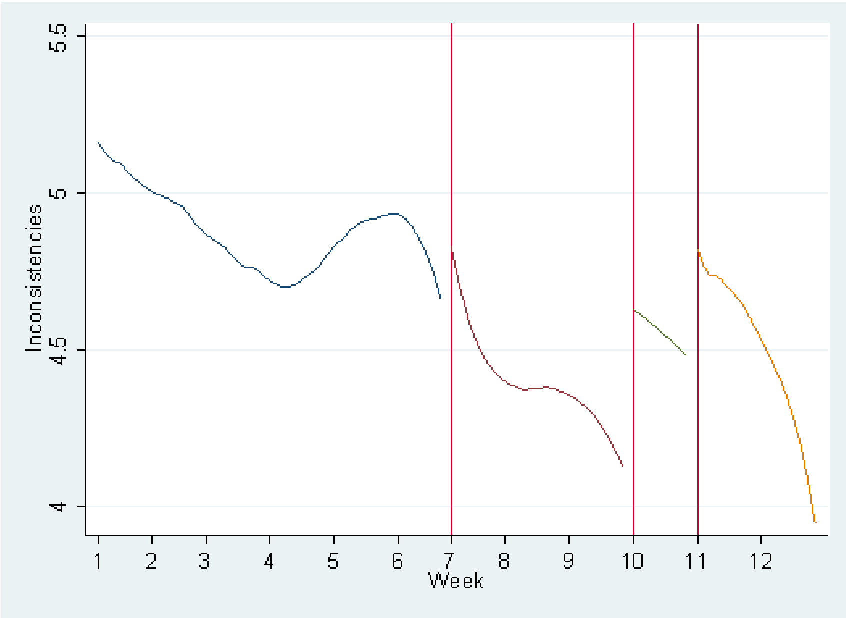

Figure 3: Average rate of inconsistencies (smoothed by local linear regression with Epanechnikov kernel)

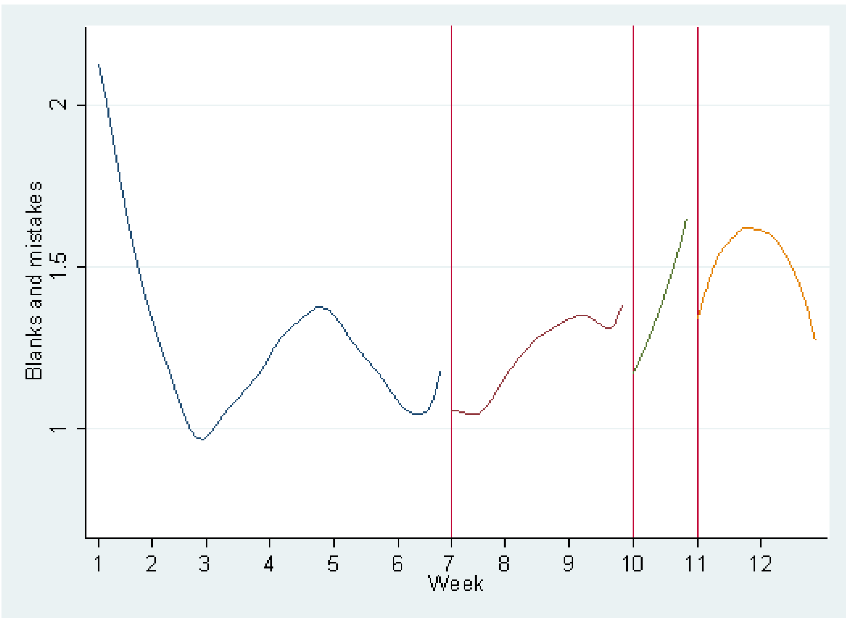

Figure 4: Average rate of blanks and mistakes (smoothed by local linear regression with Epanechnikov kernel)

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

	(1)	(2)	(3)
	Inconsistencies	Blanks and Mistakes	Difference
Week 1; 150/100 treatment (β_1)	-0.207	0.879	-1.086
	(1.122)	(0.713)	(1.529)
Week 2; 150/100 treatment (β_2)	-0.354	0.104	-0.458
	(0.949)	(0.473)	(1.245)
Week 3; 150/100 treatment (β_3)	-0.338	-0.019	-0.319
	(0.702)	(0.374)	(0.902)
Week 4; 150/100 treatment (β_4)	-0.311	0.143	-0.454
	(0.422)	(0.336)	(0.574)
Week 5; 150/100 treatment (β_5)	-0.088	0.377	-0.465
	(0.288)	(0.114)***	(0.341)
Week 7; 200/200 treatment (β_7)	-0.058	-0.049	-0.009
	(0.356)	(0.237)	(0.275)
Week 8; 200/200 treatment (β_8)	0.171	0.055	0.116
	(0.345)	(0.364)	(0.361)
Week 9; 200/200 treatment (β_9)	0.559	-0.085	0.645
	(0.392)	(0.361)	(0.457)
Week 10; 150/100 treatment (β_{10})	1.375	-0.096	1.471
	(0.543)**	(0.620)	(0.732)*
Week 11; 100/100 treatment (β_{11})	1.996	-0.159	2.154
	(0.630)***	(0.686)	(0.833)**
Week 12; 100/100 treatment (β_{12})	2.412	-0.203	2.615
	(0.982)**	(0.865)	(1.282)*
$\beta_{10} - \beta_9$	0.82	-0.01	0.83
(P-value)	(0.015)**	(0.973)	(0.056)*
$\beta_{11} - \beta_{10}$	0.62	-0.06	0.68
(P-value)	(0.006)***	(0.819)	(0.006)***
Fieldworker fixed effects	Yes	Yes	Yes
Sublocation of interview fixed effects	Yes	Yes	Yes
Respondent controls	Yes	Yes	Yes
Time trend, Time trend squared	Yes	Yes	Yes
Number of surveys per day	Yes	Yes	Yes
Observations	2864	2864	2864
R-squared	0.17	0.08	0.15

Robust standard errors in parentheses, clustered at the fieldworker level. * 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). 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, and relationship to household head) are included. The number of surveys per day collected by the fieldworker is always 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). In column (3), the dependent variable is the difference between the rate of inconsistencies and the rate of blanks and mistakes per survey.

Table 3: Robustness checks

(reference period: Week 6; 150/100 treatment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Difference between rate of inconsistencies and rate of blanks and mistakes						
	Baseline	No respondent controls	No sublocation and respondent controls	No number of surveys per day	Other reference category	With first two weeks	No time trend squared
Fixed effects for previous weeks	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week 6; 150/100 treatment (β_6)					0.465		
					(0.341)		
Week 7; 200/200 treatment (β_7)	-0.009	-0.058	-0.012	0.004	0.456	-0.210	0.054
	(0.275)	(0.283)	(0.161)	(0.246)	(0.436)	(0.270)	(0.279)
Week 8; 200/200 treatment (β_8)	0.116	0.067	0.009	0.129	0.581	-0.153	0.171
	(0.361)	(0.384)	(0.286)	(0.365)	(0.607)	(0.278)	(0.358)
Week 9; 200/200 treatment (β_9)	0.645	0.586	0.557	0.668	1.110	0.365	0.606
	(0.457)	(0.482)	(0.421)	(0.431)	(0.668)	(0.386)	(0.450)
Week 10; 150/100 treatment (β_{10})	1.471	1.373	1.378	1.496	1.937	1.321	1.242
	(0.732)*	(0.760)*	(0.760)*	(0.727)*	(0.861)**	(0.695)*	(0.684)*
Week 11; 100/100 treatment (β_{11})	2.154	2.056	2.025	2.180	2.620	2.176	1.653
	(0.833)**	(0.848)**	(0.879)**	(0.813)**	(0.942)**	(0.800)**	(0.751)**
Week 12; 100/100 treatment (β_{12})	2.615	2.473	2.525	2.645	3.081	2.955	1.719
	(1.282)*	(1.292)*	(1.283)*	(1.224)**	(1.354)**	(1.280)**	(1.033)
$\beta_{10} - \beta_9$	0.83	0.79	0.82	0.83	0.83	0.96	0.64
(P-value)	(0.057)*	(0.057)*	(0.03)**	(0.077)*	(0.059)*	(0.10)*	(0.09)*
$\beta_{11} - \beta_{10}$	0.68	0.68	0.65	0.68	0.68	0.86	0.41
(P-value)	(0.006)***	(0.006)***	(0.002)***	(0.004)***	(0.023)**	(0.09)*	(0.084)*
Fieldworker fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sublocation of interview fixed effects	Yes	Yes		Yes	Yes	Yes	Yes
Respondent controls	Yes		Yes	Yes	Yes	Yes	Yes
Time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time trend squared	Yes	Yes	Yes	Yes	Yes	Yes	
Number of surveys per day	Yes	Yes	Yes		Yes	Yes	Yes
Observations	2864	2873	2864	2864	2864	3012	2864
R-squared	0.15	0.14	0.14	0.15	0.15	0.14	0.15

Robust standard errors in parentheses, clustered at the level of fielworkers. * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable in all columns is the difference between the rate of inconsistencies, and the rate of blanks and mistakes. Fixed effects for previous weeks are included. Column (1) replicates column (3) of Table 2. In column (2), the respondents' controls (relationship to household head) are excluded. In column (3), the sublocation fixed effects are excluded. In column (4), the number of surveys per day is excluded. In column (5), the reference category is the 5th week where the wage was set at 150. In column (6), the initial two weeks of training are included. In column (7) the time trend squared is excluded.

Table 4: Impact of wages on difference between rate of inconsistencies, and rate of blanks and mistakes by number of survey

(reference survey: survey 1, week 6; 150/100 treatment)

(regression coefficients not shown, only p-value of F-test of equality)

T-tests:	Difference
Survey 3 in weeks 1-6/survey 4 in weeks 1-6	-0.08
	(0.80)
Survey 1,2,3 in week 7/survey 1,2,3 in week 6	0.27
	(0.37)
Survey 1,2,3 in week 10/survey 1,2,3 in week 6	1.74
	(0.06)**
Survey 1,2,3 in week 11/survey 1,2,3 in week 6	2.42
	(0.01)***
Survey 1,2,3 in week 10/survey 1,2,3 in week 9	0.94
	(0.10)*
Survey 1,2,3 in week 11/survey 1,2,3 in week 10	0.68
	(0.003)***
Survey 1,2,3 in week 11/survey 4 in week 1-6	2.44
	(0.06)**

P-value in parentheses, standard errors clustered at the level of fieldworkers. * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variables is the difference between the rate of inconsistencies and the rate of blanks and mistakes. 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 F-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, and relationship to household head), and the number of surveys per day are included.

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

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

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 fieldworkers by one of the student supervisors. The second and third announcement were made by video to the fieldworkers. 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.[29]

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.

Follow-up email: "When you see them tomorrow, please let them know that the end of survey trip to maasailand is still on of course. The project is not suddenly in financial difficulty. We just can't offer the bonus anymore and get the number of surveys done that we would like. We had the option of taking out the bonus or reducing the total number of surveys and we decided to take out the bonus. But the maasailand trip will happen regardless."

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.

[29] 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.

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.

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