# Principal-Agent Settings with Random Shocks

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September 26, 2012

**Abstract**

Using a gift exchange experiment, we show that the ability of reciprocity to overcome incentive problems inherent in principal-agent settings is greatly reduced when the agent's effort is distorted by random shocks and transmitted imperfectly to the principal. Specifically, we find that gift exchange contracts without shocks encourage effort and wages well above standard predictions. However, the introduction of random shocks reduces wages and effort, regardless of whether the shocks can be observed by the principal. Moreover, the introduction of shocks significantly reduces the probability of fulfilling the contract by the agent, the payoff of the principal, as well as total welfare.

*JEL Classifications*: C72, C91, D63, D81, H50
*Keywords*: gift exchange, principal-agent model, contract theory, reciprocity, effort, shocks, laboratory experiment

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* We thank Ron Harstad, Brice Corgnet, Roberto Hernán-González, Gary Charness, Charlie Plott, David Rojo-Arjona, and seminar participants at Chapman University for helpful comments. We also thank Economic Science Institute and Chapman University for providing facilities and Koch Foundation for financial support. Any remaining errors are ours.

## 1. Introduction

This paper addresses two related sources of inefficiency that can arise in principal-agent relationships. First, a large literature notes that if the agent's effort is signaled imperfectly to the principal and monitoring is expensive or impossible, then it may be impossible to write a first-best contract, since the observed outcomes are not perfectly correlated with the agent's actions.[1] Second, if contracts are not exogenously enforceable, endogenous enforcement through incentive compatibility requirements generally incentivize agents to provide suboptimal levels of effort.[2] These two problems are related because it is impossible to exogenously enforce (through legal or other institutions) a contract which specifies effort requirements when effort is unobservable.

The unobservable effort problem is a common one for large firms, as there are many types of tasks in which effort is positively correlated with observable outcomes, but these outcomes are also a function of random shocks (such as profits, number of sales, etc.). For example, the quantity of sales made by regional salespeople reflects both their effort and local demand fluctuations, where the latter are ostensibly random and difficult to observe. Hence, an employee can put in very little effort but perform well because of luck. Under these conditions, what is fair remuneration? Should the employee be punished for lack of effort or rewarded for a good performance which predominantly came from luck? On the other hand, another employee can put forth very high effort but perform poorly because of bad luck. In that case, should the employee be punished for a bad outcome or rewarded for a high effort?

There is large theoretical literature addressing contractual problems in the firm under imperfect information. The central question of this literature is how the principal should design the optimal contract in order to elicit the desirable effort level from the agent, given the problem of potential moral hazard and conflict of interest. According to the "informativeness principle" of Holmström (1979), when perfect information is not available, any observable measure of performance reveals information about the effort level chosen by the agent and should be used in the compensation contract. When effort is perfectly observable, however, the problem of optimal contract design is trivial: remuneration should be based on effort and not luck. This is sometimes referred to as the "accountability principle" (Konow, 2000, 2003), which states that remuneration

[1] See, for example, Harris and Raviv (1979), Holmström (1979), Shavell (1979), Holmström and Milgrom (1991), Baker (1992). Prendergast (1999) provides a more general overview of the contracts literature that emerged in the 1970s - 1990s.

[2] See, for example, Grossman and Hart (1983), Milgrom and Roberts (1992), and Laffont and Martimort (2002).

should be based on the relevant variables that an individual can influence (i.e., effort) but not those that he cannot influence (i.e., luck). Despite settled theoretical predictions, there is very little empirical research investigating how luck and effort play in remuneration in settings where effort is either observable or unobservable (Charness and Kuhn, 2011). This is understandable because it is difficult to measure empirically to what degree effort versus luck impact individual performance. It is even more difficult to evaluate how employers reward effort versus luck, because remuneration is usually based on final performance which is a function of effort, ability and luck (Ericsson and Charness, 1994).

The second problem firms can face when contracting with employees is contract enforceability. Even where legal institutions exist, writing a first-best, fully contingent contract is often impossible. This problem is especially stark when random shocks affect the mapping from effort to outcome. For example, if a contract offers a wage in return for the first-best effort, the agent has incentive to provide less than the first-best effort if there is a high enough probability that he will get lucky (due to a positive production shock). Since the principal cannot verify whether the outcome is due to effort or luck, the principal cannot enforce the contract.

Fehr et al. (1997) provide experimental evidence that the contract enforceability problem is partially mitigated by behavioral concerns for reciprocity. They build on an extensive literature which suggests that the reciprocity motivation can help explain a host of results that are contrary to standard economic theory.[3] One implication of this literature is that contracts based on reciprocity come closer to the first-best than standard contract theory dictates. Fehr et al. (1997) test this implication with a gift exchange experiment, where principals offer contracts that include wages and desired effort levels. Agents who accept the contracts receive the wage and choose an effort level (where higher effort improves the principal's payoff), but they do not have to abide by the desired effort level in the contract. The money-maximizing Nash equilibrium is for the agent to provide zero effort (since it is costly and they cannot be punished) and for the principal to thus offer the lowest possible wage. In the experiment, however, agents frequently show positive reciprocity; not only do they provide more effort than the Nash equilibrium

[3] There is a wealth of experimental evidence that both positive reciprocity and negative reciprocity have important effects on actions, with negative reciprocity being shown as more salient. In the context of the gift-exchange experiment employed in this paper, see Charness and Haruvy (2002), Charness (2004), Fehr and Schmidt (2007), and Houser at al. (2008). Rabin (1993) provides the canonical model introducing reciprocity into game theory, and Falk and Fischbacher (2006) provide a theory connecting the reciprocity motive to a host of standard experimental results. Fehr and Gächter (2000) provide a survey of the literature on reciprocity.

prediction, but their effort level is increasing in the wage offered by the principal. These results are exacerbated when principals are also allowed to exhibit reciprocity. In one treatment, Fehr et al. (1997) introduce a third stage in which principals can pay to punish or reward the agent after observing their effort. Although the addition of this stage does not alter the Nash equilibrium predictions of wage or effort, they find that allowing both sides to exhibit reciprocity significantly increases effort (and thus efficiency), and that both principals and agents are better off than they are when only agents are allowed to show reciprocity. Fehr et al. (2007) provide further evidence that this type of bonus contract vastly outperforms standard incentive-based contracts despite relying on unenforceable actions.

These papers contribute significantly to our knowledge of how behavioral incentives encourage contract enforcement in the absence of explicit incentives. Yet, Fehr et al. (1997) and Fehr et al. (2007) only consider how reciprocity improves contract efficiency under perfect information. In their experiments, principals can reward or punish agents based on perfectly observed effort – there are no random shocks affecting the mapping from effort to outcome. This is an important omission, because the types of contracts they are concerned with are often difficult to enforce in the real world *precisely because* outcomes are affected by shocks and thus optimal effort levels are impossible to induce in an exogenously enforced contract. Indeed, it is not clear ex-ante how the introduction of shocks interacts with the reciprocity motive. Do principals exhibit reciprocity when they are unsure that the outcome which they observe is the result of the agent's effort?

This paper addresses this problem. We conduct a gift exchange experiment similar to Fehr et al. (1997), except that the principal receives an imperfect signal of the agent's effort in some treatments. Our first treatment is similar to Fehr et al.'s (1997) bonus treatment. Principals and agents are randomly matched and the principal offers a wage and asks for a desired effort. The agent then receives the wage and can choose any effort (where the cost of effort is increasing in effort chosen). The principal can then reward or punish the agent, although either is costly. There are no shocks in this treatment, so we employ it as our baseline. The second treatment is exactly the same as the first, except that we add a random (uniformly distributed) number to the agent's effort. In this treatment, there is still perfect information; the principal observes both the effort level *and* the random number when making her decision of how much to punish or reward the agent. The final treatment is exactly like the second treatment, except that principals only observe the outcome (effort + random number), not the agent's effort. Relationships in all treatments are one-shot and anonymous, so reputational concerns are absent.

Consistent with previous literature on gift exchange (Fehr et al., 1997, 2007; Charness and Kuhn, 2011), we find that bonus contracts without shocks encourage effort and wages well above standard predictions. However, we also find evidence that this result is greatly qualified when random shocks are present. The mere introduction of shocks reduces wages and effort, *regardless of whether the shocks are observed by the principal*. If it is solely reciprocity encouraging high wages and effort, there should be no difference between the baseline treatment (without shocks) and the treatment where shocks are perfectly observed, since the reciprocity motive is based on the other's action, not the outcome emanating from the action.[4] Yet, this is not what we find. Wages and effort are significantly lower in treatments where shocks are perfectly observed relative to the baseline. Moreover, we observe no differences in behavior between treatments where shocks are observable and treatments where shocks are unobservable.

These results have important implications regarding the ability of behavioral responses to help mitigate principal-agent problems inherent in contract design. These results by no means deny that reciprocity can support greater cooperation than equilibrium predictions; indeed, even with the introduction of random shocks, effort and efficiency are well above standard predictions. Our results do indicate, however, that the effects of reciprocity are qualified by the information available to subjects and the manner in which intentions are related to observable outputs. In particular, these results suggest that the reciprocity motive is based on the outcome (i.e., which is a function of effort and shocks) of others' actions and not simply on their intentions (i.e., effort).

## 2. Experimental Design and Procedures

Our experimental design is built on a variation of a gift exchange game. The game consists of three stages. In stage 1, the principal offers contract ($w$,$\underline{e}$) to the agent. That is, the principal offers a wage $w$ (any integer number between 1 and 100) and the desired effort $\underline{e}$ (an

[4] However there are several recent experiments documenting that subjects often make their decisions based on both outcomes and intentions (actions) of others (Charness and Levine, 2007; Erkal et al., 2011; Cappelen et al., 2012; Rey-Biel et al., 2012; Gurdal et al., 2012).

integer number between 0 and 14) that she would like the agent to undertake.[5] In stage 2, the agent receives the wage $w$ and chooses an effort level $e$, which does not have to be equal to the desired effort $\underline{e}$ specified by the contract. The cost of effort $c(e)$ is an increasing and convex function of effort, where $c(e) = e^2/2$. In stage 3, the principal first observes the outcome $y = e + \varepsilon$, which is a function of effort $e$ and a uniformly distributed random component $\varepsilon$ (an integer number between -2 and +2).[6] As we will explain below, the primary difference between treatments is what the principal can observe ($\{y, e, \varepsilon\}$ or just $y$). After observing $y$, the principal chooses an adjustment level $a$ (an integer number between -5 and +5), which can be either in a form of a bonus ($a > 0$) or punishment ($a < 0$).[7] The payoff of the principal is $U^P = 10y - w - |a|$ and the payoff of the agent is $U^A = w - c(e) + 10a$.

We employ three treatments, which we name based on what the principal observes. In the baseline Effort-Only treatment there is no random component (i.e., $\varepsilon = 0$), and the principal directly observes effort $e$ (there is no difference between effort and outcome, since $y = e$). This treatment is similar to Fehr et al.'s (1997) "bonus" treatment and provides a baseline to which we compare our results. In the Effort-Shock treatment, there is a random shock component $\varepsilon$, which the principal observes. That is, the principal directly observes $y$, $e$, and $\varepsilon$. Finally, the Outcome-Only treatment is the same as the Effort-Shock treatment, but the principal only observes outcome $y$ and does not know the composition of $y$.

In all treatments, the individually optimal subgame perfect equilibrium is for the agent to choose an effort of zero (i.e., $e = 0$) and for the principal to make an adjustment of zero (i.e., $a = 0$). The socially optimal actions are for the agent to choose an effort of 10 (i.e., $e = 10$) and for the principal to provide an adjustment level of +5 (i.e., $a = 5$).

The experiment was conducted at Chapman University's ESI laboratory. A total of 216 subjects were recruited from a standard campus-wide subject pool. Subjects interacted with each

[5] We chose the range between 0 and 14 for effort and desired effort for two reasons. First, it ensures that the maximum cost of effort is less than the maximum possible wage (cost of effort of 14 is equal to 98). Second, we wanted the efficient effort (10) to be an internal point (between 0 and 14), so that agents were not anchored to the efficient effort artificially (which could happen if the effort range was between 0 and 10).

[6] We chose the range between -2 and 2 for the random number because we did not want the shock effect to be so large that it would dominate the effect that effort has on the principal's earnings. We believe this to be the most realistic and general situation. Likewise, we want the shock to potentially be large enough to have a salient effect on actions (which may not be the case if the range was, say between -0.2 and 0.2).

[7] We chose the range between -5 and 5 for the adjustment because we wanted the ability to punish or reward to be large enough that most subjects would choose an internal point (to reduce censoring biases). We felt that this range accomplishes both of these goals while not being so large that contracts are completely based on bonuses or punishments.

other anonymously over a local computer network. The experiment, which lasted an average of 45 minutes total, proceeded as follows. Upon arrival, subjects were randomly assigned to computer terminals and received instructions (see Appendix) corresponding to one of the three treatments. The experiment was computerized using z-Tree (Fischbacher, 2007). We ran 9 sessions (3 sessions per treatment) with 24 subjects in each session.

Within each session, subjects were split into 3 groups of 8.[8] Within each group of 8, 4 subjects were assigned to be principals and 4 were assigned to be agents. Subjects stayed in their role assignment throughout the entire experiment. In each session there were 10 periods of play. Each period subjects from opposite role assignments were randomly matched to form a principal-agent pair. After each period subjects were randomly re-matched with someone of the opposite role assignment within their 8-person group to form a new principal-agent pair. Each period proceeded in three stages. In the first stage, the principal chose a reward (an integer number between 0 and 100) and a desired effort (an integer number between 0 and 14) for the agent. After observing the reward and the desired effort, in the second stage, the agent chose an effort level (an integer number between 0 and 14). To determine the outcome, in the Outcome-Only and Effort-Shock treatments, the computer added to the effort a randomly selected number (an integer between -2 and +2). Then, depending on the treatment, the computer displayed to the principal either only the outcome (Outcome-Only), the outcome, effort, and the random number (Effort-Shock), or effort (Effort-Only). After observing the relevant information, in the third stage, the principal choose an adjustment level for the agent (an integer between -5 and +5).

At the end of each experiment, 1 out of 10 periods were randomly selected for payment. The earnings in this period were exchanged at rate of 10 francs = $1. All subjects also received a participation fee of $20 to cover potential losses. On average, subjects earned $26 each (maximum $42 and minimum $7), which was paid anonymously and in cash.

## 3. Results

When examining individual behavior, we mainly focus on contracts that satisfy individually rational and incentive compatibility requirements (IR/IC). These are the contracts, in which both the principal's and the agent's payoffs are non-negative, conditional on the contract

[8] We divided the subjects into three groups per session in order to have three independent observations at the session level. This allows for the use of non-parametric tests, which we employ in Section 3.

being fulfilled. Specifically, we focus on all $(w,\underline{e})$ contracts, such that $10\underline{e} - w \geq 0$ and $w - c(\underline{e}) \geq 0$.[9] Out of 2160 contracts observed in our experiment, 1338 (62%) can be classified as satisfying IR/IC.[10] Table 1 provides the summary statistics across all three treatments, using only IR/IC contracts (top panel) and all contracts (bottom panel).

When performing statistical tests, we mainly use non-parametric tests to examine treatment effects. Each treatment has a total of 9 independent observations (72 subjects per treatment, split into 9 separate groups of 8 subjects each). When appropriate, we also estimate panel models with individual subjects representing the random effects (to control for individual effects), standard errors clustered at the single re-matching group level of 8 subjects (to control for possible correlation within a matching group), and an inverse period trend (to control for learning and experience).

We consider the results starting with stage 3 first and work our way backwards to stage 1. This allows us to use backwards induction to shed light on the motivation underlying actions at each stage. If the reciprocity motive is absent (which is assumed in conventional Nash equilibrium analyses), then wage, effort, and adjustments should all equal zero. In stage 3, the principal chooses adjustment $a = 0$, since any other adjustment is costly. Knowing this, the agent choose effort $e = 0$ in stage 2, since effort is costly. Hence, the principal chooses a contract with wage $w = 0$ in stage 1.

However, previous gift exchange experiments have shown that reciprocity plays a salient role in guiding the actions of both principals and agents (Fehr et al., 1997, 2007; Charness and Haruvy, 2002; Charness, 2004; Fehr and Schmidt, 2007; Houser at al., 2008). We consider reciprocity in a context similar to Rabin (1993), where perceived kindness is rewarded and unkindness is punished. Importantly, the perception of kindness or unkindness is based on the *intention* of the player's actions, not the *outcome* of the actions; this is also known as the "accountability principle" (Konow, 2000, 2003). We assume that principals view agents who give effort at least (less than) desired effort to be acting kindly (unkindly), while agents view principals who give a positive wage to be acting kindly. These distinctions guide our discussion as we dissect the experimental results.

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[9] We chose not to put any restrictions on the principal's decisions, because some ex-ante "non-IR/IC" contracts $(w,\underline{e})$ may be IR/IC ex-post, given a certain level of an adjustment $a$. For an experiment where the principal can only offer contracts which satisfy IR/IC see Bartling et al. (2012).

[10] The number of contracts which satisfy IR/IC is very similar across the three treatments (i.e., 64% in the Effort-Only, 59% in the Outcome-Only and 62% in the Effort-Shock treatment).

### 3.1. Adjustment

In stage 3, principals choose an adjustment after seeing either the effort of the agent (in Effort-Only and Effort-Shock) or the outcome (in Outcome-Only and Effort-Shock) in stage 2. Figure 1 displays the average adjustment by treatment, while Figure 2 displays the distribution of adjustment by treatment. Both the distribution and the average adjustment levels are very similar in all three treatments. Based on the Wilcoxon rank-sum test there is no significant difference in the adjustment level between treatments: Effort-Only versus Outcome-Only (0.26 versus -0.62; p-value = 0.11, $n_1 = 9$, $n_2 = 9$), Effort-Only versus Effort-Shock (0.26 versus -0.51; p-value = 0.20, $n_1 = 9$, $n_2 = 9$), and Outcome-Only versus Effort-Shock (-0.62 versus -0.51; p-value = 0.96, $n_1 = 9$, $n_2 = 9$).[11]

**Result 1**: There is no difference in the *unconditional adjustment* level between treatments.

This suggests the possibility that the adjustment mechanism works relatively similar in all three treatments. However, these results may arise from the fact that we consider the *unconditional* adjustment in stage 3; if the first two stages are different in the two treatments and principals condition their adjustment on actions taken in the previous stages, then Result 1 obscures the determinants of the adjustment across treatments. Indeed, if the reciprocity motive is present in the principal's decision, we expect the adjustment to be a function of how "kindly" she was treated by the agent in stage 2. In other words, we expect the principal's adjustment to be a function of the difference between the effort (or outcome) she observes in stage 2 minus the desired effort proposed in stage 1. It is also possible that the principal expects the agent to show reciprocity in stage 2 if the principal gives a large wage in stage 1, so the adjustment may also be conditional on wage.

We first test whether principals condition their adjustments based on previous actions. Table 2 reports the estimation results of different panel models where individual subjects represent the random effects, and the standard errors are clustered at the single re-matching group level. The dependent variable in all specifications is the *adjustment* and the independent

[11] Based on the Wilcoxon matched-pairs signed-rank test, we also find that the absolute value of adjustment in all three treatments is significantly higher than the standard Nash equilibrium prediction of $a = 0$ (all p-values < 0.01).

variables in specifications (1)-(3) are an inverse of a *period* trend, *wage*, *effort-desired_effort* (in Effort-Only and Effort-Shock), and *outcome-desired_effort* (in Outcome-Only).[12] In specifications (1) and (2), *adjustment* is positively correlated with *effort-desired_effort*. In specification (3), *adjustment* is positively correlated with *outcome-desired_effort*. This finding supports the idea that principals show reciprocity, since they reward higher effort (outcome) relative to desired effort. Also note that there is a positive correlation between *adjustment* and *wage* (although not significant in specification 3). One interpretation is that principals use *adjustment* and *wage* as complements. Finally, in the Effort-Shock treatment, the principal chose the *adjustment* level after observing both the *effort* and *shock.* In specification (4), we also include the *shock* as an independent variable. We find that the *adjustment* and the *shock* variables are positively correlated, suggesting that principals punish/reward agents based on both *intentions* and *outcomes*. Nevertheless, the coefficients on *wage* and *effort-desired_effort* in specification (4) are significant and have very similar values as coefficients in specification (2).

It is possible that the relationship between "reciprocity" (adjustment) and "kindness" (effort gap or outcome gap) is not linear (Baumeister et al., 2001; Offerman, 2002; Andreoni et al., 2003; Charness, 2004; Bellemare et al., 2007). Bellemare et al. (2007), for example, suggest that reciprocity is a concave function of kindness (i.e. increasing in the degree of kindness increases reciprocity, but at a diminishing rate). Moreover, following the seminal paper by Baumeister et al. (2001), many studies have shown that "negative" reciprocity is stronger than "positive" reciprocity.[13] Hence, we control for both non-linearities and distinctions between positive and negative kindness. Table 3 reports the random effects estimation results. We included an (*effort-desired_effort*)$^2$ or (*outcome-desired_effort*)$^2$ term to account for non-linearities. Specification (1) reports results where all observations are included, specification (2) reports results where only observations in which agents showed "kindness" in the second stage are included (i.e., effort gap or outcome gap $\geq 0$), and specification (3) reports results where only observations in which agents showed "unkindness" are included (i.e., effort gap or outcome gap $< 0$).

Estimation results in Table 3 show that both positive and negative reciprocity increase in the degree of "kindness" (i.e., *effort-desired_effort* and *outcome-desired_effort* are positively

[12] Principals do not see effort in the Outcome-Only treatment, so we condition on outcome minus desired effort.
[13] For further discussion see Charness and Kuhn (2011).

correlated with *adjustment*). Moreover, positive reciprocity increases at a diminishing rate (i.e., $(effort\text{-}desired\_effort)^2$ and $(outcome\text{-}desired\_effort)^2$ are negative) and negative reciprocity decreases at a diminishing rate (i.e., $(effort\text{-}desired\_effort)^2$ and $(outcome\text{-}desired\_effort)^2$ are positive).[14] This can also be seen in Figure 3, which maps the predicted values of the regressions reported in specifications (2) and (3) in Table 3 across varying levels of effort/outcome gap.[15] This figure confirms the finding in the literature that negative reciprocity is stronger than positive reciprocity, and it suggests that negative reciprocity is diminishing in unkindness, but positive reciprocity is neither increasing nor diminishing in kindness.

Finally, we find that there is no difference in conditional adjustments between the three treatments. Table 4 reports three sets of results in which each permutation of treatment is compared. Controlling for the effort or outcome gap, we ran different panel regressions, as in Table 3, with the adjustment as a dependent variable and treatment dummy as an independent variable. All treatment dummy variables are not significantly different from zero.

**Result 2**: There is no difference in the *conditional adjustment* level between treatments.

### 3.2. Effort

We next consider the effort that the agent chooses in stage 2. Figure 4 displays the average effort by treatment, while Figure 5 displays the distribution of effort by treatment. Based on the Wilcoxon rank-sum test, we find that the average unconditional effort in the Effort-Only treatment is higher than in the Outcome-Only treatment (6.86 versus 5.08; p-value $< 0.01$, $n_1 = 9$, $n_2 = 9$) and the Effort-Shock treatment (6.86 versus 5.07; p-value $< 0.01$, $n_1 = 9$, $n_2 = 9$). On the other hand, the average effort is not different between the Outcome-Only and Effort-Shock treatments (5.08 versus 5.07; p-value $= 0.99$, $n_1 = 9$, $n_2 = 9$).

[14] When the effort/performance gap $\geq 0$ the coefficient on the squared term is negative, indicating that the curve is concave. When the effort/performance gap $< 0$ the coefficient on the squared term is positive, indicating that the curve is convex (or, its magnitude increases at a diminishing rate as one moves away from zero).
[15] This figure maps over the range [-7,3] for effort and performance gap because a vast majority of the observations fall in this range, as can be seen in Figure 6. The results are reported at wage = 50 and period = 2.

**Result 3:** There is a greater *unconditional effort* in the Effort-Only treatment than in the other two treatments, while there is no difference in *unconditional effort* between the Effort-Shock and Outcome-Only treatments.

There are two reasons why agents may choose effort greater than the Nash prediction of zero. First, they may exhibit positive reciprocity if the principal gives them a high wage in the first stage. That is, their effort is in part *conditional* on actions taken in stage 1. Second, they may believe that a higher effort will lead to a greater reward (or smaller punishment) in stage 3. We test the first possibility by conducting a panel analysis within each treatment. Table 5 reports the estimation results of different panel models, where the dependent variable in all specifications is the subject's *effort* and the independent variables are an inverse of a *period* trend, *wage* and *desired_effort*. In all specifications, there is a positive and significant relationship between *wage* and *effort,* suggesting a gift exchange story between the principal and the agent. Similarly, there is a positive correlation between *effort* and *desired_effort* (although it is not significant in specification 3).[16]

Next, we test how conditional effort differs across treatments. Table 6 reports three sets of results in which each permutation of treatment is compared. These results suggest that the conditional effort is also greater in the Effort-Only treatment than in the other two treatments, but there is no difference between effort in the Effort-Shock and Outcome-Only treatments. The magnitude is far from trivial: conditional on the contract offered in the first stage and the period, agents give about 0.8 additional units of effort in the Effort-Only treatment. This corresponds to an increase of 8 units to the principal's payoff, and a substantial welfare gain if effort is less than 10 (which it is on average, according to Table 1).

**Result 4:** There is a greater *conditional effort* in the Effort-Only treatment than in the other two treatments, while there is no difference in *conditional effort* between the Effort-Shock and Outcome-Only treatments.

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[16] One interpretation of why *desired_effort* is significant in the Effort-Only and Effort-Shock treatments and it not significant in the Outcome-Only treatment is that in the Outcome-Only treatment the principal cannot identify precisely whether the contract has been fulfilled or not, while in the other two treatments the principal can always check whether *effort* is equal to *desired_effort*.

### 3.3. Effort versus Desired Effort

The second reason noted above that agents choose positive effort in stage 2 is that principals may show reciprocity in stage 3 if agents choose a high enough effort in stage 2, meaning that agents will receive a greater reward (or smaller punishment). We test this motivation by analyzing agents' effort relative to the desired effort announced by the principal in stage 1.

We begin by analyzing the relationship between effort and desired effort within treatments. If the cheap talk of the desired effort announcement is truly cheap, then we should find no relationship between effort and desired effort. We do not find this, however. Figure 6 displays the distribution of the difference between effort and desired effort. The distributions in all three treatments are clearly not uniform. Moreover, the vast majority of observations in all three treatments fall within the interval [-2, +2], suggesting that agents do not perceive the desired effort simply as a cheap talk but rather as a concrete indication of the principal's expectations. When examining the number of instances when the agents exactly fulfill the contract (i.e., effort is equal to desired effort), we find that 41% of contracts are exactly fulfilled in the Effort-Only treatment, 23% of contracts in the Effort-Shock treatment, and 12% of contracts in the Outcome-Only treatment.

Tables 5 and 6 indicated that effort is increasing in wage, so it is possible that the probability of exactly fulfilling the contract is also affected by wage. Table 7 reports estimation results of different random effects probit models, where the dependent variable in all specifications is the probability of *effort=desired_effort* and the independent variables are an inverse of a *period* trend and *wage*. In all specifications, we find that there is no significant relationship between *wage* and the probability of exactly fulfilling the contract.

Next, we compare the relationship between effort and desired effort across treatments. Even if agents correctly predict how principals act in stage 3, it is not clear ex-ante how the introduction of shocks affect effort relative to desired effort. If agents are very risk averse, they may choose to give more effort than desired effort in the Outcome-Only treatment in order to avoid any chance of underperforming the desired effort. If they choose effort equal to the desired effort, they have a 0.4 probability of underperforming relatively to the desired effort in the Outcome-Only treatment (i.e., in case the shock realization is -1 or -2); they can only be assured of performing at least as great as the desired effort by choosing an effort two or greater than desired. On the other hand, for every effort level within 2 of the desired effort in the Outcome-Only treatment, an increase of one unit of effort only increases the probability that outcome is above the desired effort by 0.2. Since effort is increasingly costly at higher effort levels, agents thus may choose to provide lower effort levels in the Outcome-Only treatment, since the marginal benefit is lower.

This logic indicates that agents are more likely to choose effort greater than the desired effort in the Outcome-Only treatment if they believe that principals reward/punish based on *outcome* and not *intention*. Meanwhile, it also suggests that agents are likely to choose *at least* the desired effort more frequently in the Effort-Only treatment (and possibly the Effort-Shock treatment), if they believe that principals reward/punish based on *intention* and not *outcome*. The latter hypothesis stems from the fact that the marginal benefit (in terms of stage 3 reward) from choosing effort equal to desired effort is greater when the principal can perfectly observe effort (Effort-Only) than when he cannot (Outcome-Only).

When examining the probability of effort exceeding the desired effort we find, based on the Wilcoxon rank-sum test, that it is significantly lower in the Effort-Only treatment than in the Outcome-Only treatment (0.07 versus 0.19; p-value = 0.03, $n_1 = 9$, $n_2 = 9$). On the other hand, there is no significant difference between the Effort-Only and Effort-Shock treatments (0.07 versus 0.14; p-value = 0.28, $n_1 = 9$, $n_2 = 9$) or between the Outcome-Only and Effort-Shock treatments (0.19 versus 0.14; p-value = 0.59, $n_1 = 9$, $n_2 = 9$). These findings confirm the intuition noted above.

**Result 5:** There is a greater probability that *effort exceeds desired effort* in the Outcome-Only treatment than in the Effort-Only treatment.

Looking at the distribution in Figure 6, it is clear that agents choose efforts exactly specified by the contract in the Effort-Only treatment significantly more often than in the Outcome-Only treatment (0.41 versus 0.12; p-value < 0.01, $n_1 = 9$, $n_2 = 9$) and the Effort-Shock treatment (0.41 versus 0.23; p-value = 0.03, $n_1 = 9$, $n_2 = 9$). There is no difference between the Outcome-Only and Effort-Shock treatments (0.12 versus 0.23; p-value = 0.10, $n_1 = 9$, $n_2 = 9$), although the difference nears statistical significance.

**Result 6:** There is a higher probability of *exactly fulfilling the contract* in the Effort-Only treatment than in the other two treatments.

Results 5 and 6 indicate that agents are less likely to provide more than the desired effort in the Effort-Only treatment, but they are more likely to provide the exact amount of desired effort.[17] We combine these two results to test whether agents are likely to provide effort *at least* as great as the effort desired. When comparing the three treatments, we find that 46% of contracts are either exactly fulfilled or exceeded in the Effort-Only treatment, 38% of contracts in the Effort-Shock treatment, and 31% of contracts in the Outcome-Only treatment. Based on the Wilcoxon rank-sum test, the probability that agents provide effort greater than or equal to the desired effort is significantly higher in the Effort-Only treatment than in the Outcome-Only treatment (0.46 versus 0.31; p-value = 0.03, $n_1 = 9$, $n_2 = 9$). However, there is no significant difference between the Effort-Only and Effort-Shock treatments (0.46 versus 0.38; p-value = 0.40, $n_1 = 9$, $n_2 = 9$), and between the Outcome-Only and Effort-Shock treatments (0.31 versus 0.38; p-value = 0.51, $n_1 = 9$, $n_2 = 9$).

**Result 7:** There is a higher probability that *effort is greater than or equal to desired effort* in the Effort-Only treatment than in the Outcome-Only treatment.

### 3.4. Wage and Desired Effort

In terms of welfare, the most important result presented thus far is Result 3, which indicates that unconditional effort is greater in the Effort-Only treatment than in the other two treatments. Where does this extra effort come from? Result 5 indicates that it does not come from agents giving extra effort relative to desired effort. This leaves two, non-mutually exclusive, possibilities: (i) principals give higher wages and/or, (ii) ask for higher desired efforts in stage 1.

Figure 7 displays the average wage and desired effort by treatment, while Figure 8 displays the distribution of wage and desired effort by treatment. The average wage and desired effort are the highest in the Effort-Only treatment. Using the average within a single re-matching

[17] Both of these results hold when we estimate random effects probit models, conditioning probability of exactly fulfilling the contract or exceeding the contract on wage.

group over all periods as one independent observation, the Wilcoxon rank-sum test shows that the average wage in the Effort-Only treatment is significantly higher than in the Effort-Shock treatment (46.27 versus 38.87; p-value = 0.05, $n_1 = 9$, $n_2 = 9$) and Outcome-Only treatment (46.27 versus 37.36; p-value = 0.02, $n_1 = 9$, $n_2 = 9$). Similarly, we find that the average desired effort is higher in the Effort-Only treatment than in the Effort-Shock treatment (8.13 versus 6.91; p-value < 0.01, $n_1 = 9$, $n_2 = 9$) and the Outcome-Only treatment (8.13 versus 6.65; p-value < 0.01, $n_1 = 9$, $n_2 = 9$). On the other hand, wage and desired effort are not different between the Outcome-Only and Effort-Shock treatments (p-values = 0.89 and 0.63, respectively).

**Result 8:** There is a greater *wage* and *desired effort* in the Effort-Only treatment than in the other two treatments, while there is no difference in *wage* and *desired effort* between the Effort-Shock and Outcome-Only treatments.

Result 8 indicates that the higher effort level observed in the Effort-Only treatment in Result 3 results from both higher wages and higher desired effort levels in the Effort-Only treatment. Why do principals offer a higher wage and ask for greater desired effort in the Effort-Only treatment? Part of the answer follows from Table 5, which indicated that effort responds more strongly to desired effort in the Effort-Only treatment than in the other two treatments (0.61 versus 0.41 and 0.12, comparing the coefficients on *desired_effort* in all three specifications). Hence, principals have more to gain from asking for higher desired effort in the Effort-Only treatment than in the other two treatments. If higher wages are necessary to induce such effort, this would also explain why wage is greater in the Effort-Only treatment. In fact, we find that there is a strong correlation between wage and desired effort, $\rho = 0.85$, indicating that higher wages are associated with higher desired effort.

### 3.5. Payoffs and Welfare

As a consequence of higher wage and higher effort, the Effort-Only treatment generates significantly higher payoff to the principal than the Outcome-Only treatment (20.39 versus 9.17; p-value = 0.03, $n_1 = 9$, $n_2 = 9$) and the Effort-Shock treatment (20.39 versus 12.09; p-value = 0.05, $n_1 = 9$, $n_2 = 9$). Yet, the principal's payoff is not different between the Outcome-Only and Effort-Shock treatments (9.17 versus 12.09; p-value = 0.57, $n_1 = 9$, $n_2 = 9$).

**Result 9:** *Principals' payoffs* in the Effort-Only treatment are higher than in the other two treatments, while there is no difference between the Effort-Shock and Outcome-Only treatments.

When comparing payoffs of agents, we find no significant differences between the three treatments (all p-values > 0.45).

**Result 10:** There is no difference in the *agents' payoffs* between any of the treatments.

The fact that principals are better off in the Effort-Only treatment but agents are not suggests that the introduction of random shocks harms the principals but not the agents. Although principals offer higher wages in the Effort-Only treatment, this translates into higher effort levels which leave the agents equally well off but make principals better off. The principals are made better off by enough in the Effort-Only treatment that the *overall* welfare (principal's payoff + agent's payoff) is greater in the Effort-Only treatment than in the other two treatments: Effort-Only versus Outcome-Only (39.30 versus 31.58; p-value = 0.02, $n_1 = 9$, $n_2 = 9$) and Effort-Only versus Effort-Shock (39.30 versus 29.44; p-value < 0.01, $n_1 = 9$, $n_2 = 9$). On the other hand, there is no difference in the total welfare between the Effort-Shock and Outcome-Only treatments (29.44 versus 31.58; p-value = 0.57, $n_1 = 9$, $n_2 = 9$).

**Result 11:** The *total welfare* is greater in the Effort-Only treatment than in the other two treatments, while there is no difference between the Effort-Shock and Outcome-Only treatments.

### 3.6. Non-IR/IC Contracts

The analysis in Sections 3.1-3.5 is based on contracts that satisfy individual rationality and incentive compatibility constraints (IR/IC), i.e. contracts in which both the principal's and the agent's payoffs are non-negative, conditional on the contract being exactly fulfilled. Often researchers deliberately restrict subject's strategy space so that they can choose only contracts which satisfy IR/IC (i.e., Bartling et al., 2012). In our experiment, we chose not to put such restrictions on the principal's decisions, because some ex-ante non-IR/IC contracts ($w$,$\underline{e}$) may actually give positive payouts for both the principal and the agent ex-post, given a certain level of an adjustment $a$. Moreover, when receiving a non-IR/IC contract, the agent may still choose not to accept it.

Before we examine non-IR/IC contracts, we want to emphasize two facts. First, the majority of contracts observed in our experiment satisfy IR/IC (62%). Moreover, the number of contracts satisfying IR/IC is very similar across the three treatments (64% in Effort-Only, 59% in Outcome-Only and 62% in Effort-Shock). Second, practically all results (i.e., Results 1-11) reported in Sections 3.1-3.5 hold when we combine IR/IC and non-IR/IC contracts together.[18] This can be easily seen be by comparing the top and the bottom part of Table 1. The only reason we chose to focus mainly on IR/IC contracts is that these contracts are easy to interpret economically and they are also most likely to be employed in practice.

Examining the non-IR/IC contracts, we find that 36% of contracts in the Effort-Only, 41% in the Outcome-Only and 38% in the Effort-Shock treatment can be characterized as non-IR/IC contracts. The vast majority of the non-IR/IC contracts (91%, 744 out of 822) are the ones where, conditional on contract being exactly fulfilled, the agent is expected to receive a negative payoff (i.e., $w - c(\underline{e}) < 0$). We refer to these contracts as the "stingy" contracts, since they do not satisfy the incentive compatibility requirements for the agent. The rest of the non-IR/IC contracts (9%, 78 out of 822) are the ones where the principal is expected to receive a negative payoff (i.e., $10\underline{e} - w < 0$). We refer to these contacts as the "generous" contacts, since in such contracts the principal offers very generous wages relatively to the desired effort, although they are not individually rational. Table 8 provides the summary statistics across all three treatments, using "stingy" contracts (top part) and "generous" contracts (bottom part).

Examining first the 78 generous contracts where the principal is expected to make a negative payoff (the bottom panel of Table 8), we find that in all three treatments principals make negative payoffs. Due to the very small number of such contracts there are only 5 independent observations in the Effort-Only treatment, 4 in the Effort-Shock treatment, and 6 in

[18] There are three results which do not hold exactly but come very close to statistical significance. For Result 2, the conditional adjustment is marginally higher in Effort-Only versus Outcome-Only ($p = 0.08$). For Result 7, there is still a higher probability that effort is greater than or equal to desired effort in the Effort-Only treatment than in the Outcome-Only treatment, but with weaker statistical significance ($p = 0.14$). For Result 8, there is still a greater wage and desired effort in the Effort-Only treatment than in Outcome-Only, but with weaker statistical significance ($p = 0.15$).

the Outcome-Only treatment. Although the full statistical analysis is not appropriate with such a small (and very noisy) number of independent observations, it appears that when the principals offer "generous" contracts, the agents reciprocate. The effort is about 2 units higher than the desired effort, which is in sharp contrast to the IR/IC contacts in Table 1. However, this reciprocation by agents is not nearly enough to compensate for very generous wage offers by principals. As a consequence, principals make negative payoffs, while agents make positive and very high payoffs. It is important to emphasize that such behavior by principals is unlikely to be caused by mistakes. The 78 generous contracts are offered by 52 independent subjects. Moreover 34 of these contacts are offered in the last five periods of the experiment. A possible explanation for generous contracts is that principals use such contracts hoping that agents will show significant reciprocity. We do find some evidence that reciprocity is greater for generous contracts than for all other contracts, but not by enough to make these contracts optimal for the principal. We cannot rule out the possibility that generous contracts are caused by mistakes, although we can rule out the possibility that these contracts offer "efficiency wages" – aimed at keeping agents happy in the long-run – since relationships are one-shot.

When examining the 744 stingy contracts where the agent is expected to make a negative payoff (the top panel of Table 8), we find that in all three treatments agents make significantly positive payoffs (based on the Wilcoxon signed-rank test, separately for each treatment, all p-values are less than 0.01). This is mainly because the effort is about 5-6 units below the desired effort (which is in sharp contrast to the generous contracts in Table 8 and the IR/IC contacts in Table 1). Interestingly, on average, principals do not punish such behavior. One reason for this is that principals make very substantial payoffs even when agents do not fully fulfill the terms of the stingy contract. In fact, the principals offering stingy contracts on average receive very similar payoffs than the principals offering IR/IC contracts (based on the Wilcoxon signed-rank test, separately for each treatment, all p-values are greater than 0.20). Agents, on the other hand, receive significantly lower payoffs under the stingy contracts than under the IR/IC contracts (based on the Wilcoxon signed-rank test, separately for each treatment, all p-values are less than 0.02). These two findings can explain why the majority of contracts satisfy IR/IC, yet some principals still choose to offer stingy contracts. On the one hand, principals should be indifferent between offering the IR/IC and stingy contracts, since the expected payoff is not different between the two. So, we should observe both types of the contacts. On the other hand, IR/IC contracts provide significantly higher payoffs to the agents, and thus benevolent principals should choose the IR/IC contracts more often than the stingy contracts.

## 4. Discussion and Conclusion

We conduct a gift exchange experiment in which the agent's outcome depends on both effort and luck. In all treatments, the principal first offers a wage and asks for a desired effort, then the agent chooses any effort, and finally the principal decides whether to reward or punish the agent. In the first treatment (Effort-Only), the agent's outcome depends only on effort. In the second treatment (Effort-Shock), outcome is the sum of effort and a shock (a random number), and the principal observes both effort and the random number before deciding whether to punish or reward the agent. The third treatment (Outcome-Only) is exactly like the second treatment, except that principals only observe the outcome (effort + random number).

Consistent with the previous literature on gift exchange (Fehr et al., 1997, 2007; Charness and Kuhn, 2011), we find that bonus contracts without a shock component encourage effort and wages well above standard predictions. Also, we find that a significant number of agents do not shirk and exert efforts specified by the contract.

The novel and important result of our study is that the introduction of a shock in the principal-agent settings significantly reduces wages and effort, regardless of whether the shock can be observed by the principal. This finding contrasts with the "accountability principle" (Konow, 2000, 2003), which states that remuneration should be based on the relevant variables that an individual can influence (i.e., effort) but not those that he cannot influence (i.e., luck). Nevertheless, this result is consistent with a large literature on retrospective voting that finds voters reward/punish politicians based on outcomes over which politicians have no control (Healy et al., 2010; Gasper and Reeves, 2011). It is also consistent with a large literature in psychology on outcome bias (Baron and Hershey, 1988; Marshall and Mowen, 1993; Mazzocco et al., 2004). Finally, there are recent studies on risk taking, redistribution and charitable giving that show that some subjects condition their giving and reciprocity on both effort and luck of others (Charness and Levine, 2007; Erkal et al., 2011; Cappelen et al., 2012; Rey-Biel et al., 2012; Gurdal et al., 2012). Similarly, in our experiment, we find that subjects in the Effort-Shock treatment reward/punish based on both *intentions* (effort) and *outcomes* (effort + shock).

Not only does the introduction of shocks in the principal-agent settings significantly reduce wages and effort, but it also significantly reduces the probability of fulfilling the contract by the agent, the payoff of the principal, as well as the total welfare. The fact that shocks, even perfectly observable, have such a significant and perhaps unexpected effect in principal-agent settings has important implications for the design of optimal contracts. What is it about the addition of shocks – observed or unobserved – that encourages principals to offer contracts with lower wages and desired effort levels? Why does the addition of shocks make agents less responsive to desired effort? We provide several conjectures.

As wage and desired effort levels increase, the downside risk becomes greater due to the gift exchange nature of the game – the agent may not choose the desired effort level, and thus the higher wage is wasted. Likewise, when agents choose higher efforts, the downside risk that the principal will not reciprocate in the third stage is greater, since the wage chosen is more costly. As the costs increase, players must be compensated by either higher payouts or lower risk, if they are risk averse. The Effort-Only treatment offers the lowest uncertainty of the three, since agents know that principals will receive exactly the amount of effort that they give. This, in turn, allows higher levels of effort to be sustained in the Effort-Only treatment; the additional risk inherent in the Effort-Shock and Outcome-Only treatments makes high levels of effort too costly to be worth the risk.

Another possible explanation is that shocks interact with the reciprocity function in complex ways. That is, individuals may have an aversion to punishing behavior that should be rewarded (i.e., an agent chooses effort one greater than desired effort but the random number is -2) or an aversion to rewarding behavior that should be punished (i.e., an agent chooses effort one less than desired effort but the random number is 2). This makes higher levels of effort more difficult to sustain. As effort increases, so does the cost of effort. If principals are unlikely to reward higher effort in the Outcome-Only treatment (for fear of falsely rewarding underperformance), then agents have incentive to give effort lower than desired effort. Knowing this, principals will ask for lower desired effort – and give lower wages – in the first stage. This problem does not arise in the Effort-Only treatment, where shocks are absent.[19]

[19] This problem also arises in the Effort-Shock treatment if reciprocity is a function of *outcomes* as well as *effort*; we show that this is indeed the case.

Finally, it may also be the case that the factors affecting *expected* reciprocity (e.g., fairness) interact with shocks in unforeseen ways. That is, agents may be afraid that they will be treated unfairly if they receive a bad realization in the Outcome-Only or Effort-Shock treatments. If they believe that they will be unjustly punished if they choose effort equal to the desired effort but receive a negative random number, they may instead choose effort levels lower than desired effort. In fact, this may even be an optimal strategy. As noted above, when an agent chooses effort within two levels of desired effort, the marginal gain of an additional unit of effort is only a 20% increase in the chance that the desired effort level will be reached. Thus, agents have incentive at high effort levels to scale back their effort; this saves on rather large costs while minimally decreasing the probability of achieving at least the desired effort. This effect is *exacerbated* if agents are averse to what they view as "unjust" punishment, since the marginal benefit to choosing at least the desired effort is lower when shocks are present.

None of these possibilities are mutually exclusive. In fact, they call for further research on just how and why shocks affect contract choice. Ignoring the presence of shocks in gift exchange settings obscures one of the primary reasons why contracts are difficult to design; if the mapping from effort to outcome were shock-free, then there would be no need to set up a gift exchange in the first place – one could simply formulate a complete, first-best contract. While we know that this is often not possible when shocks are present, our results suggest that the reciprocity motive does not completely mitigate this problem. Reciprocity does allow for more efficient results than standard contract theory would have us believe, but its effect is reduced by the presence of shocks, whether or not the shocks are observed.

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**Figure 1: Average Adjustment.**

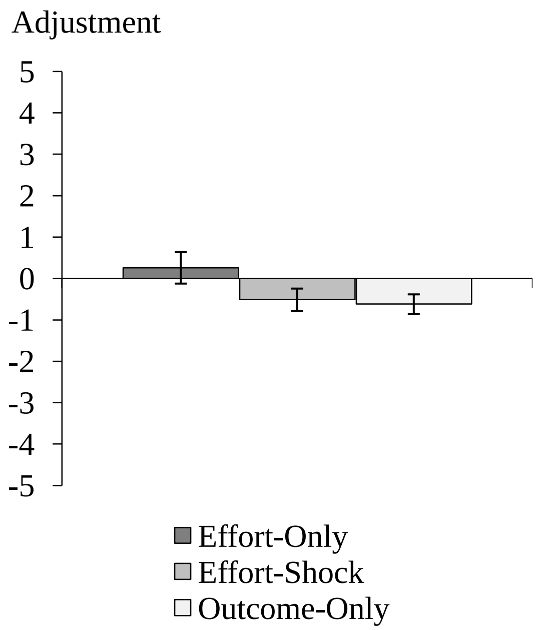


**Figure 2: Distribution of Adjustment.**

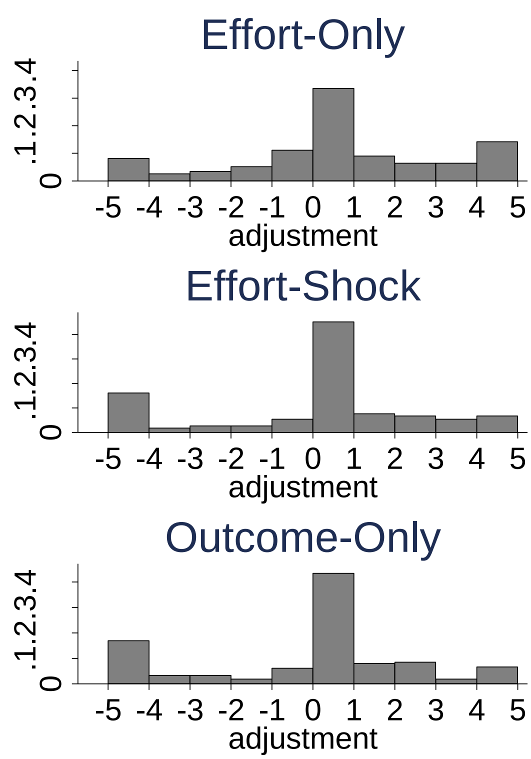

**Figure 3: Adjustment versus Effort/Outcome Gap: Predicted Values.**

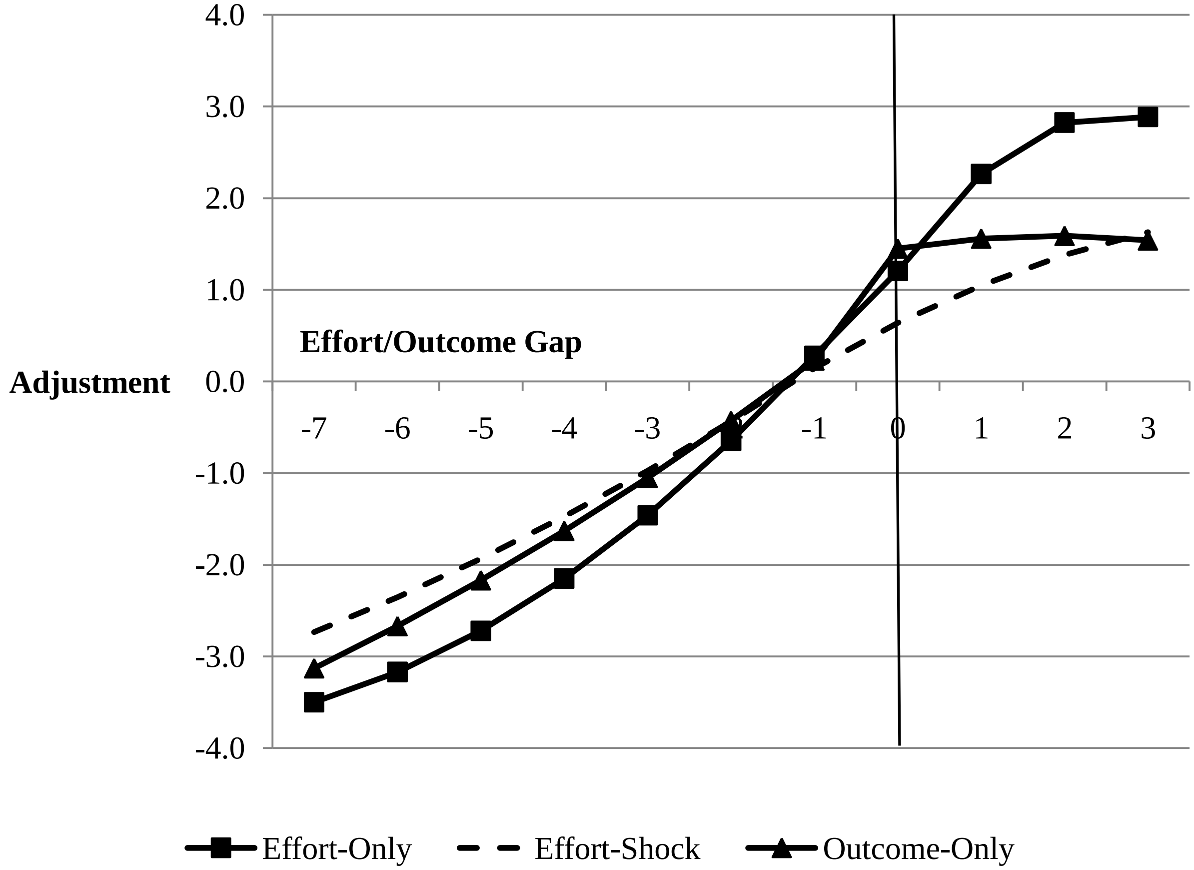

**Figure 4: Average Effort.**

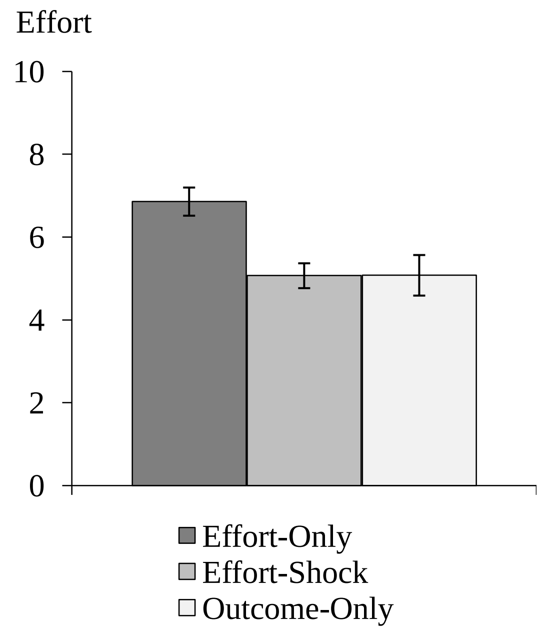


**Figure 5: Distribution of Effort.**

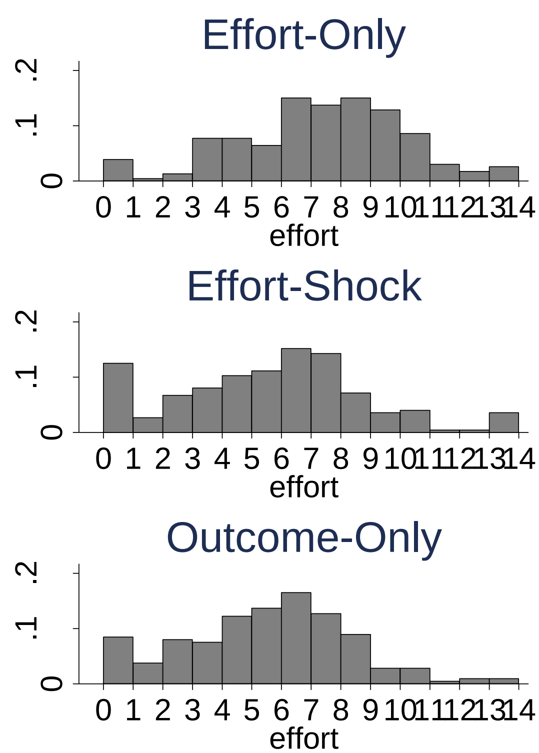

**Figure 6: Distribution of Effort – Desired Effort.**

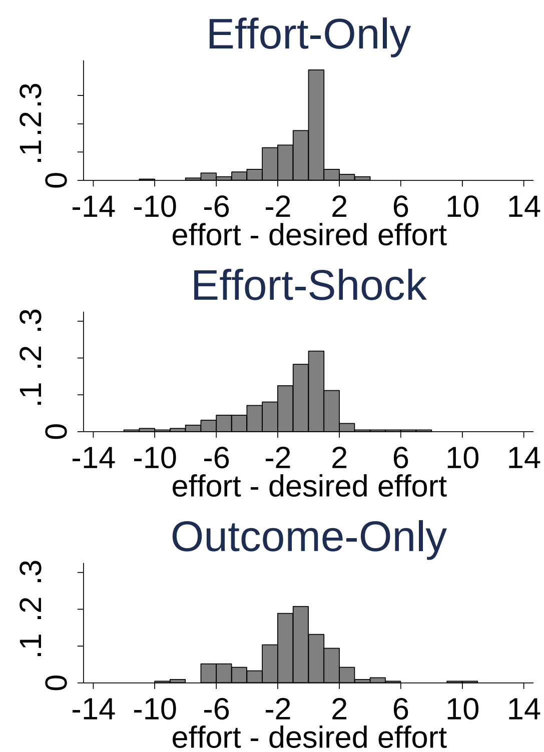

**Figure 7: Average Wage and Desired Effort.**

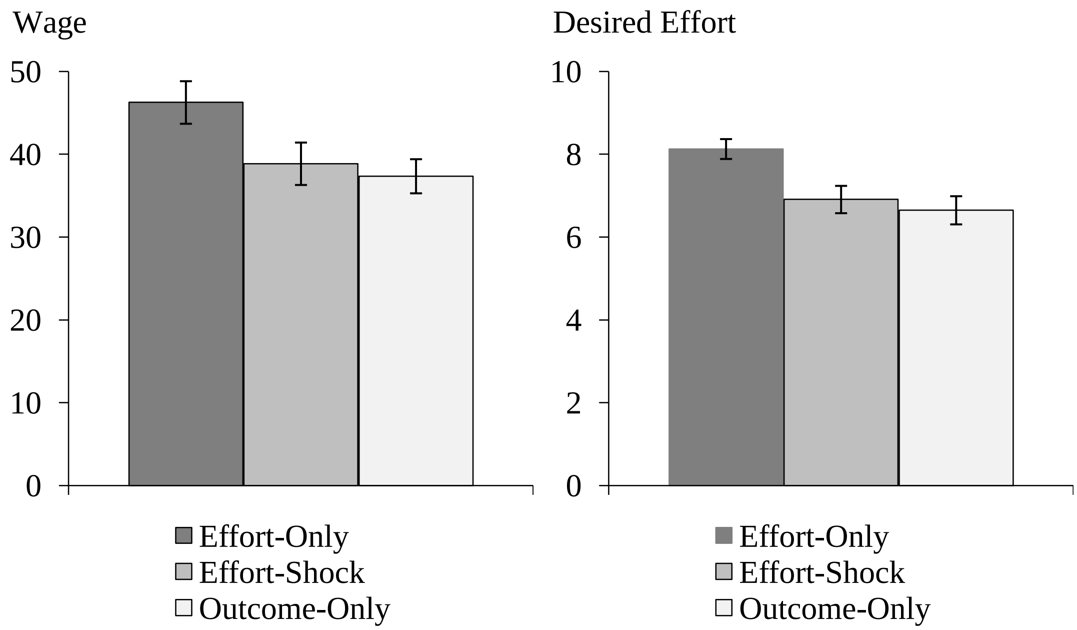


**Figure 8: Distribution of Wage and Desired Effort.**

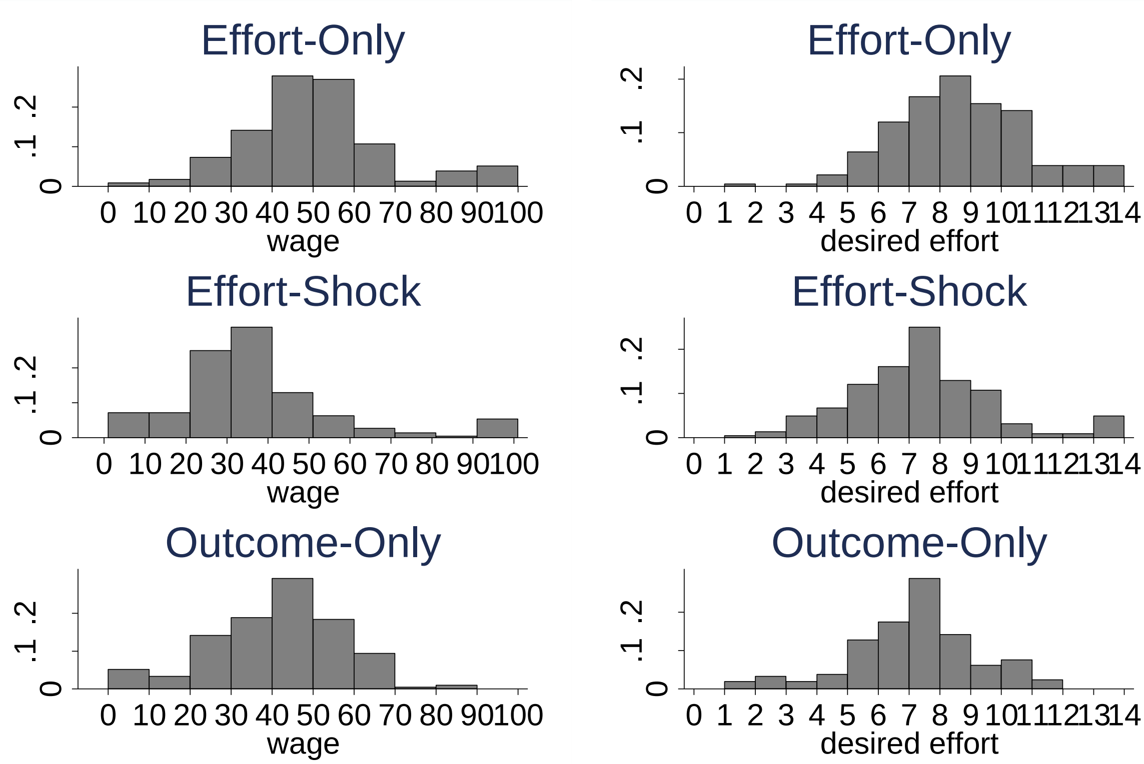

**Table 1: The Average Statistics: IR/IC Contracts versus All Contracts.**

| Treatment | Wage | Desired Effort | Effort | Outcome | Adjustment | Principal's Payoff | Agent's Payoff | Total Welfare |
|---|---|---|---|---|---|---|---|---|
| | | | | *Contracts Satisfying IR/IC* | | | | |
| *Effort-Only* | 46.27 | 8.13 | 6.86 | 6.86 | 0.26 | 20.39 | 18.91 | 39.30 |
| | (2.57) | (0.24) | (0.34) | (0.34) | (0.38) | (2.89) | (1.68) | (1.48) |
| *Effort-Shock* | 38.87 | 6.91 | 5.07 | 4.98 | -0.51 | 9.17 | 20.27 | 29.44 |
| | (2.56) | (0.33) | (0.30) | (0.33) | (0.27) | (3.75) | (2.06) | (2.19) |
| *Outcome-Only* | 37.36 | 6.65 | 5.08 | 5.12 | -0.62 | 12.09 | 19.50 | 31.58 |
| | (2.06) | (0.34) | (0.49) | (0.48) | (0.24) | (3.65) | (2.10) | (2.33) |
| | | | | *All Contracts* | | | | |
| *Effort-Only* | 41.14 | 8.95 | 6.40 | 6.40 | 0.14 | 20.91 | 15.71 | 36.62 |
| | (3.22) | (0.30) | (0.43) | (0.43) | (0.34) | (3.11) | (1.88) | (1.91) |
| *Effort-Shock* | 33.45 | 7.63 | 4.69 | 4.62 | -0.18 | 11.23 | 16.40 | 27.64 |
| | (2.98) | (0.33) | (0.34) | (0.32) | (0.22) | (3.46) | (2.49) | (1.90) |
| *Outcome-Only* | 33.85 | 7.63 | 4.69 | 4.75 | -0.50 | 12.04 | 17.63 | 29.67 |
| | (2.28) | (0.25) | (0.41) | (0.38) | (0.13) | (2.85) | (1.89) | (2.11) |

Standard errors in parenthesis (based on 9 independent observations).

**Table 2: Panel Models of Adjustments.**

| Specification | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Treatments | *Effort-Only* | *Effort-Shock* | *Outcome-Only* | *Effort-Shock* |
| Dependent variable | *adjustment* | *adjustment* | *adjustment* | *adjustment* |
| *wage* | 0.01* | 0.01*** | 0.02 | 0.01*** |
| [wage] | (0.01) | (0.00) | (0.02) | (0.00) |
| *effort-desired_effort* | 0.78*** | 0.45*** | | 0.46*** |
| [effort gap] | (0.12) | (0.07) | | (0.08) |
| *outcome-desired_effort* | | | 0.44*** | |
| [outcome gap] | | | (0.10) | |
| *shock* | | | | 0.35** |
| [random number] | | | | (0.14) |
| *period* | -1.03** | -0.56 | -0.14 | -0.63 |
| [inverse period] | (0.41) | (0.44) | (0.55) | (0.44) |
| *constant* | 1.02 | 0.11 | -0.54 | 0.15 |
| [constant term] | (0.67) | (0.30) | (0.82) | (0.28) |
| *Observations* | 233 | 224 | 212 | 224 |
| *Clusters* | 9 | 9 | 9 | 9 |
| *Overall R-squared* | 0.33 | 0.23 | 0.20 | 0.27 |

* significant at 10%, ** significant at 5%, *** significant at 1%. Standard errors in parenthesis are clustered at the group level.

**Table 3: Panel Models of Adjustments: Including Non-Linearities.**

| Specification | (1) | (2) | (3) |
|---|---|---|---|
| Restrictions | *all observations* | *effort/outcome gap ≥ 0* | *effort/outcome gap < 0* |
| Dependent variable | *adjustment* | *adjustment* | *adjustment* |
| | | *A: Effort-Only* | |
| *wage* | 0.01* | 0.02* | 0.00 |
| [wage] | (0.01) | (0.01) | (0.01) |
| *effort-desired_effort* | 0.83*** | 1.31 | 1.11*** |
| [effort gap] | (0.31) | (1.14) | (0.22) |
| (*effort-desired_effort*)$^2$ | 0.01 | -0.25 | 0.06** |
| [effort gap squared] | (0.04) | (0.34) | (0.02) |
| *Observations* | 233 | 108 | 125 |
| *Clusters* | 9 | 9 | 9 |
| *Overall R-squared* | 0.33 | 0.05 | 0.29 |
| | | *B: Effort-Shock* | |
| *wage* | 0.01*** | 0.01 | 0.01*** |
| [wage] | (0.00) | (0.01) | (0.00) |
| *effort-desired_effort* | 0.44*** | 0.45** | 0.64** |
| [effort gap] | (0.05) | (0.21) | (0.25) |
| (*effort-desired_effort*)$^2$ | 0.00 | -0.04 | 0.02 |
| [effort gap squared] | (0.01) | (0.03) | (0.03) |
| *Observations* | 224 | 84 | 140 |
| *Clusters* | 9 | 9 | 9 |
| *Overall R-squared* | 0.23 | 0.10 | 0.16 |
| | | *C: Outcome-Only* | |
| *wage* | 0.02 | 0.02 | 0.02 |
| [wage] | (0.01) | (0.02) | (0.01) |
| *outcome-desired_effort* | 0.34*** | 0.15 | 0.72*** |
| [outcome gap] | (0.09) | (0.13) | (0.22) |
| (*outcome-desired_effort*)$^2$ | -0.03** | -0.04* | 0.02 |
| [outcome gap squared] | (0.01) | (0.02) | (0.02) |
| *Observations* | 212 | 75 | 137 |
| *Clusters* | 9 | 9 | 9 |
| *Overall R-squared* | 0.23 | 0.09 | 0.14 |

* significant at 10%, ** significant at 5%, *** significant at 1%. Standard errors in parenthesis are clustered at the group level. All regressions include inverse period variable and constant term.

**Table 4: Panel Models of Adjustments: Comparing Treatments.**

| Specification | (1) | (2) | (3) |
|---|---|---|---|
| Restrictions | *all observations* | *effort/outcome gap* $\geq 0$ | *effort/outcome gap* $< 0$ |
| Dependent variable | *adjustment* | *adjustment* | *adjustment* |
| | *A: Effort-Only & Effort-Shock* | | |
| *Effort-Only* | 0.13 | 0.65 | -0.31 |
| [Effort-Only treatment dummy] | (0.43) | (0.62) | (0.43) |
| *Observations* | 457 | 192 | 265 |
| | *B: Effort-Only & Outcome-Only* | | |
| *Effort-Only* | 0.23 | 0.42 | -0.21 |
| [Effort-Only treatment dummy] | (0.51) | (0.66) | (0.50) |
| *Observations* | 445 | 183 | 262 |
| | *C: Effort-Shock & Outcome-Only* | | |
| *Outcome-Only* | -0.32 | -0.02 | -0.36 |
| [Outcome-Only treatment dummy] | (0.39) | (0.47) | (0.44) |
| *Observations* | 436 | 156 | 280 |
| *wage* | YES | YES | YES |
| *effort/outcome-desired_effort* | YES | YES | YES |
| (*effort/outcome-desired_effort*)$^2$ | YES | YES | YES |

* significant at 10%, ** significant at 5%, *** significant at 1%. Standard errors in parenthesis are clustered at the group level. All regressions include inverse period variable and constant term.

**Table 5: Panel Models of Effort.**

| Specification | (1) | (2) | (3) |
|---|---|---|---|
| Treatments | *Effort-Only* | *Effort-Shock* | *Outcome-Only* |
| Dependent variable | *effort* | *effort* | *effort* |
| *wage* | 0.04*** | 0.03* | 0.04* |
| [wage] | (0.01) | (0.02) | (0.02) |
| *desired_effort* | 0.61*** | 0.41** | 0.12 |
| [desired effort] | (0.11) | (0.16) | (0.17) |
| *period* | 0.01 | 0.82 | 0.03 |
| [inverse period] | (0.55) | (0.76) | (0.38) |
| *constant* | 0.01 | 0.73 | 2.92*** |
| [constant term] | (0.54) | (0.63) | (0.71) |
| *Observations* | 233 | 224 | 212 |
| *Clusters* | 9 | 9 | 9 |
| *Overall R-squared* | 0.49 | 0.27 | 0.16 |

* significant at 10%, ** significant at 5%, *** significant at 1%. Standard errors in parenthesis are clustered at the group level.

**Table 6: Panel Models of Effort: Comparing Treatments.**

| Specification | (1) | (2) | (3) |
|---|---|---|---|
| Treatments | *Effort-Only & Effort-Shock* | *Effort-Only & Outcome-Only* | *Effort-Shock & Outcome-Only* |
| Dependent variable | *effort* | *effort* | *effort* |
| *Effort-Only* | 0.84** | 0.78* | |
| [Effort-Only treatment dummy] | (0.34) | (0.46) | |
| *Effort-Shock* | | | -0.16 |
| [Effort-Shock treatment dummy] | | | (0.43) |
| *wage* | 0.04*** | 0.05*** | 0.04*** |
| [wage] | (0.01) | (0.01) | (0.01) |
| *desired_effort* | 0.49*** | 0.30*** | 0.25** |
| [desired effort] | (0.09) | (0.11) | (0.13) |
| *period* | 0.33 | -0.27 | 0.32 |
| [inverse period] | (0.45) | (0.36) | (0.41) |
| *constant* | 0.15 | 1.33** | 1.79*** |
| [constant term] | (0.44) | (0.67) | (0.68) |
| *Observations* | 457 | 445 | 436 |
| *Clusters* | 18 | 18 | 18 |
| *Overall R-squared* | 0.40 | 0.38 | 0.22 |

* significant at 10%, ** significant at 5%, *** significant at 1%. Standard errors in parenthesis are clustered at the group level.

**Table 7: Panel Probit Models of Probability of Fulfilling the Contract.**

| Specification | (1) | (2) | (3) |
|---|---|---|---|
| Treatments | *Effort-Only* | *Effort-Shock* | *Outcome-Only* |
| Dependent variable | *effort = desired_effort* | *effort = desired_effort* | *effort = desired_effort* |
| *wage* | 0.00 | 0.00 | 0.00 |
| [wage] | (0.01) | (0.01) | (0.01) |
| *period* | -0.09 | -0.47 | 0.14 |
| [inverse period] | (0.32) | (0.38) | (0.42) |
| *constant* | -0.35 | -0.50* | -1.39*** |
| [constant term] | (0.29) | (0.26) | (0.40) |
| *Observations* | 233 | 224 | 212 |

* significant at 10%, ** significant at 5%, *** significant at 1%. Standard errors in parenthesis.

**Table 8: The Average Statistics: Non-IR/IC Contracts.**

| Treatment | Wage | Desired Effort | Effort | Outcome | Adjustment | Principal's Payoff | Agent's Payoff | Total Welfare |
|---|---|---|---|---|---|---|---|---|
| | | | | *Stingy Contracts* | | | | |
| *Effort-Only* | 30.31 | 10.68 | 5.35 | 5.35 | -0.08 | 21.23 | 9.42 | 30.64 |
| | (3.94) | (0.51) | (0.75) | (0.75) | (0.41) | (4.96) | (2.37) | (3.45) |
| *Effort-Shock* | 23.00 | 9.12 | 3.83 | 3.66 | 0.22 | 12.44 | 9.49 | 21.93 |
| | (3.92) | (0.47) | (0.50) | (0.49) | (0.31) | (4.17) | (3.05) | (3.04) |
| *Outcome-Only* | 21.21 | 10.38 | 4.00 | 4.09 | -0.50 | 18.23 | 7.62 | 25.85 |
| | (4.03) | (0.51) | (0.56) | (0.53) | (0.30) | (3.95) | (3.02) | (3.24) |
| | | | | *Generous Contacts* | | | | |
| *Effort-Only* | 54.00 | 2.20 | 4.00 | 4.00 | -2.00 | -16.00 | 42.00 | 26.00 |
| | (8.27) | (0.80) | (0.95) | (0.95) | (1.22) | (11.53) | (9.82) | (4.16) |
| *Effort-Shock* | 44.88 | 1.88 | 3.44 | 4.19 | -0.94 | -4.06 | 36.50 | 32.44 |
| | (6.71) | (0.72) | (0.86) | (1.12) | (0.60) | (8.06) | (5.30) | (9.80) |
| *Outcome-Only* | 57.47 | 2.96 | 5.44 | 5.61 | 0.44 | -3.07 | 35.38 | 32.31 |
| | (6.46) | (0.33) | (0.88) | (1.02) | (0.60) | (13.62) | (11.62) | (3.83) |

Standard errors in parenthesis (based on 9 independent observations). In the bottom panel of the table there are only 5 independent observations in the Effort-Only treatment, 4 in the Effort-Shock treatment, and 6 in the Outcome-Only treatment.

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