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The effect of random shocks on reciprocal behavior in dynamic principal-agent settings*

Rudolf Kerschbamer[†] Regine Oexl[‡]

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Abstract

Previous work has shown that unobservable random shocks on output have a detrimental effect on effort provision in short-term ('static') employment relationships. Given the prevalence of long-term ('dynamic') relationships in firms, we investigate whether the impact of shocks is similarly pronounced in gift-exchange relationships where the same principal-agent pair interacts repeatedly. In dynamic relationships, shocks have a significantly less pronounced negative effect on the agent's effort provision than in static relationships. In an attempt to identify the drivers for our results we find that the combination of a repeated-game effect and a noise-canceling effect is required to avoid the detrimental effects of unobservable random shocks on effort provision.

Keywords: Gift exchange, principal agent model, incomplete contracts, random shocks, reciprocity, laboratory experiments, long-term contracts

JEL: C72, C91, D81

1 Introduction

A large literature studies how mutual gift-exchange based on reciprocity can improve the relationship between parties with conflicting interests, such as employers (henceforth principals) and workers (henceforth agents). Starting with the seminal study by Fehr et al. (1993), many experimental papers find a positive association between the wage the principal offers and the effort the agent exerts in response in a two-stage gift-exchange game (for reviews, see Gächter and Falk, 2002; Fehr et al., 2009; Charness and Kuhn, 2011). This link is even more pronounced when the principal additionally can reward or punish the agent for his effort in a three-stage gift-exchange game (Fehr et al., 1997; Fehr and Schmidt, 2007; Fehr et al., 2007). An important aspect of the gift-exchange mechanism in both cases is that it is effective even in short-term relationships where contract enforcement based on reputational concerns is not available.

In a thought-provoking paper, Rubin and Sheremeta (2016) show that mutual gift-exchange may lose its effectiveness when unobservable random shocks obscure the relation between the agent's effort and output. Specifically, the authors conduct a three-stage gift-exchange experiment similar to that in Fehr et al. (1997), except that the principal cannot directly observe the

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agent's action, but only a noisy signal.[1] Gift-exchange and welfare are significantly depressed in this setting compared to the same situation without random shocks. Davis et al. (2017) confirm this result in a replication study. This finding is concerning, since in many real-world relationships the outcome from exchange is subject to random shocks and cannot cleanly be attributed to kind or unkind acts. However, while short-term (static) interactions with random shocks are certainly relevant, many real world interactions last longer than one period. An interesting question therefore is whether the adverse effects of unobservable random shocks are contained in long-term (dynamic) gift-exchange interactions. The present paper addresses this question in a series of lab experiments.

In our experiments, we follow Rubin and Sheremeta (2016) and Davis et al. (2017) in employing a three-stage gift-exchange game, in which both sides of the market – the principal and the agent – can respond reciprocally to previous actions. We do so for several reasons. First, the three-stage nature of the game does not change the standard prediction (based on the assumption that it is common knowledge that all players are exclusively interested in their own material payoffs) for the two stages in the standard gift-exchange game: a purely self-interested principal would not implement a (costly) bonus or fine in the third stage; anticipating this, the self-interested agent would have no incentive to provide effort above the minimum in the second stage; and the principal would in turn have no incentive to offer a wage above the minimum in the first stage. Second, we think that the three-stage game better reflects real-world employment interactions: In some real world employment relationships based on reciprocity voluntary bonus payments are an important component of the overall compensation package of the employee; and in many others, there is implicitly a third stage, as for agents motivated by social concerns, a friendly (or unfriendly) word when leaving the contract may represent a 'bonus' (or 'fine'). Third and most importantly, it has been shown that reciprocal behavior is stronger in a three-stage game than in a two-stage game (Fehr et al., 1997; Fehr and Schmidt, 2004; Fehr et al., 2007). This makes the results of Rubin and Sheremeta (2016) and Davis et al. (2017) even more remarkable: Even in those static relationships where reciprocity has been shown to be a powerful means to improve efficiency, unobservable random shocks have a detrimental effect on efficiency. This makes the three-stage game particularly suitable to study the impact of extending the relationship length to see whether the adverse effects of unobservable random shocks are contained in long-term interactions.

Neither Rubin and Sheremeta (2016) nor Davis et al. (2017) directly address the question why gift-exchange is depressed – i.e., why wage and effort are lower – in the presence of unobservable random shocks. One possibility is that agents no longer trust in reciprocal acts by principals, when the latter cannot disentangle the agent's effort from good or bad luck. This might diminish the agent's effort motivation, which in turn might lead the (anticipating) principal to offer a less generous wage arrangement in the first place. If this explanation were true, then gift-exchange might be restored in a dynamic relationship, since part of the noise cancels out in the long run.[2] We call this the *noise-canceling effect*. The noise-canceling effect potentially has two components, a passive one and an active one. The passive component is present even when the agent keeps the effort constant over time: By observing several outputs the principal gets more information about the agent's behavior over time. We call this part of the noise-canceling effect

[1] This setting reflects the incomplete contracting environment – with its defining feature that the principal cannot enforce the amount of effort the agent exerts when performing the task – much better than the deterministic setting, in which the payment could, in principle, be made contingent on the outcome and thereby on the effort provided.

[2] Under the standard assumption of common knowledge that all players are rational and exclusively interested in their material payoffs, the theoretical prediction does not depend on the length of the relationship: The unique subgame-perfect Nash equilibrium (SPNE) of the one-shot game is for the principal to pay no bonus in stage three; for the agent to exert the minimum effort in stage two; and for the principal to pay only the minimum wage in stage one. Since the stage game has a unique SPNE, the unique SPNE of the finitely repeated game is the repetition of the stage game outcome.

the *learning component*. In addition to the learning component there might also be an active component of the noise-canceling effect: The agent might react to a negative (positive) shock in the previous period by exerting more (less) effort in the current period – thereby protecting (at least in part) the principal from the shock. We call this part of the noise-canceling effect the *insurance component*.[3] Gift exchange might also work better in a dynamic relationship because current misbehavior can be punished in future periods. In the limit (when the relationship lasts infinitely long) this allows for some kind of forcing contracts à la folk theorem, where both partners do not want to risk the benefits from future gift-exchange by committing adverse acts in the current period.[4] We call this the *repeated-game effect*. While the repeated-game effect is predicted to be present in dynamic employment relationships independently of whether they are plagued by unobservable random shocks or not, the noise-canceling effect is by definition present only in dynamic interactions plagued by unobservable random shocks.

To separate the noise-canceling effect from the repeated-game effect, our main experiments are based on a 2 x 2 design. In one dimension we vary whether unobservable random shocks are absent (in this case the agent's effort can be perfectly inferred from the output) or present (in this case the output is only a noisy signal of effort), in the other dimension we vary whether the interaction is static (each principal-agent pair plays the gift-exchange game only once) or dynamic (a principal-agent pair interacts over several periods). We isolate the repeated-game effect by comparing behavior and overall efficiency in the dynamic principal-agent relation without random shocks to its static counterpart. To receive aggregate information about the importance of the noise-canceling effect, we use a difference-in-difference approach: We compare the difference in behavior and efficiency between the dynamic principal-agent relation with random shocks and its static counterpart (where both the repeated-game effect and the noise-canceling effect might play a role) to the difference between the dynamic principal-agent relation without random shocks and its static counterpart (where arguably only the repeated-game effect is at work).

We also search for direct evidence for the insurance component of the noise-canceling effect and for the repeated-game effect in our data. The insurance component implies that current effort is negatively related to the size of the shock in the previous period – a part of the shock is in effect absorbed by the agent. By contrast, the repeated-game effect predicts that current effort is positively related to previous adjustment and that the current wage is positively related to previous output.[5]

Our results are as follows: While we find some direct evidence for the presence of a repeated-game effect in our treatments without random shocks, the effect seems to be insufficient to make the dynamic relationship more efficient than the static one. Indeed, in the absence of shocks, extending the relationship length does neither increase the average effort nor the average adjustment. As a consequence, overall efficiency is also not significantly different between the two treatments without random shocks. This result is probably due to the fact that the three-stage gift-exchange game already leaves sufficient possibilities to reward and punish behavior within a single round.

By contrast, in the presence of unobservable random shocks the dynamic interaction is signif-

[3] A consequence of the insurance component is that the agent absorbs part of the shock. If the principal is risk-neutral and the agent is risk-averse (as is typically assumed in the principal-agent literature) then this leads to inefficiencies not considered in the present paper.

[4] Indeed, a number of recent studies show that the breach of the implicit agreement in a reciprocal relationship can cause long-lasting adverse effects (see Heinz et al., 2020; Friebel et al., 2017, among others).

[5] The former relationship is predicted because in the repeated relationship the agent might wish to punish (reward) a low (high) adjustment in the present period by exerting low (high) effort in the next period. The latter relationship is predicted because in a dynamic relationship the principal has in fact two punishment mechanisms – current adjustment (which is costly) and future wage (a lower wage actually saves the principal money).

icantly more efficient than the static one. Comparing treatments without random shocks to those with shocks, we find that unobservable random shocks have a pronounced negative effect on efficiency in the static interaction (the results reported in Rubin and Sheremeta, 2016; Davis et al., 2017), but efficiency is roughly the same across the two dynamic interactions. These results together suggest that the noise-canceling effect is mainly responsible for the result that in dynamic relationships, shocks have a significantly less pronounced negative effect on the agent's effort provision than in static relationships.

To address the question whether noise-canceling alone is sufficient to eliminate the negative effect, we run two additional treatments – the 'no-repeated-game-effect treatments'. In both, the interaction is dynamic, and, again, we vary whether there are unobservable random shocks on effort or not. In contrast to the dynamic treatments, in these two additional treatments, a principal-agent pair interacts under the *same* contract over the whole duration of the relationship. In this setup, the two components of the noise-canceling effect – that is, the learning component and the insurance component – potentially are still active, while the repeated-game effect is turned off. While we find some direct evidence for the presence of the noise-canceling effect in the data of the no-repeated-game-effect treatment with random shocks, the effect seems to be insufficient to neutralize the negative impact of random shocks on efficiency. Indeed, in the no-repeated-game-effect condition, the presence of unobservable shocks has a similarly pronounced negative effect on efficiency as in the static interaction.

Taken together, our results indicate that neither the repeated-game effect alone nor the noise-canceling effect alone is sufficient to alleviate the detrimental effects of unobservable random shocks on effort provision. What is needed to create a setting where unobserved random shocks do not impact reciprocal behavior substantially is an environment in which both the repeated-game effect and the noise-canceling effect can be active.

Turning to related literature, our results may help to understand recent findings from field experiments: Gneezy and List (2006) run a field experiment aimed at increasing worker effort in two quite distinct tasks: data entry for a university library and door-to-door fundraising for a research center. In both settings the authors offer individuals either the wage as announced (no-gift condition) or a higher wage (gift condition). The authors find for both tasks that worker effort in the first few hours on the job is considerably higher in the gift condition than in the no-gift condition. However, the effect fades out after a few hours, and for later hours no difference in outcomes is observed. In line with this, de Ree et al. (2018) find in a sample of teachers in India that while an unconditional salary increase does improve teacher satisfaction and other measures in the short run, it does not impact student performance in the long run. While these papers do not explicitly discuss the presence of random shocks in their environments, some elements of randomness are clearly present.[6] Our results suggest that the fading out of the effect found in this literature might be due to the missing repeated-game effect: regularly adjusting the wage based on the observed performance might restore gift-exchange.

Turning to related laboratory experiments, the papers closest to ours are Rubin and Sheremeta (2016) and Davis et al. (2017). These papers study the impact of unobservable random shocks on behavior in static gift-exchange games but do not consider dynamic interactions. Dynamic gift-exchange games have been investigated by Falk et al. (1999), Gächter and Falk (2002), and Brown et al. (2004). In contrast to our experimental design these papers investigate only gift-exchange games without random shocks. Another difference is that the basic game implemented in those papers is a two-stage interaction where the principal offers a wage in the first stage and the agent decides about her effort in the second stage. By contrast our basic game has a third stage in which the principal can reward or punish the agent after seeing her effort choice. This

[6] For instance, for data-entry and fundraising, individuals can find easier or more difficult items to enter, and they can get assigned a richer or poorer neighborhood to collect donations.

latter difference might explain why we do not find a repeated-game effect in our treatments without random shocks while the mentioned papers find that extending the relationship length fosters reciprocal behavior and leads to more efficient outcomes. To the best of our knowledge there is no experimental literature on the effects of unobservable random shocks in dynamic gift-exchange games.

The rest of the paper is organized as follows. Section 2 describes the experimental design, the four main treatments and the procedures. Section 3 reports the results. In section 4, we introduce two additional treatments and investigate the impact of removing the repeated-game effect on gift-exchange relationships plagued by random shocks. Section 5 concludes.

2 Experimental design, treatments and procedures

The baseline game Our baseline game of a static interaction without shocks is identical to the baseline game in Davis et al. (2017): The game has three stages. In stage one, the principal (she) offers a contract (w, e^*), specifying a wage $w \in \{1, 2, \ldots, 100\}$ and an (unenforceable) desired effort $e^* \in \{0, 1, \ldots, 14\}$ that she would like the agent to undertake. In stage two, the agent (he) observes the contract chosen by the principal and decides about the effort level $e \in \{0, 1, \ldots, 14\}$. The cost of effort – $c_e(e) = e^2/2$, rounded to the next highest integer – is common knowledge among the players, as are all other details. In stage three, the principal observes the outcome y and chooses an adjustment level $a \in \{-50, -40, \ldots, 0, \ldots, 40, 50\}$, where positive values are bonuses to the agent and negative values are fines. Adjustments are costly for the principal, with an adjustment cost of $c_a(a) = \frac{|a|}{10}$.

Table 1: Overview treatments

	no-shock	shock
static	$\mathrm{S}_{\text{no-shock}}$	$\mathrm{S}_{\text{shock}}$
dynamic	$\mathrm{D}_{\text{no-shock}}$	$\mathrm{D}_{\text{shock}}$

Notes: In *static* interactions a principal-agent pair interacts only once (i.e., in a single period); in *dynamic* interactions a principal-agent pair remains intact for five periods; *no-shock* refers to an interaction where the effort translates directly into output; *shock* refers to an interaction where output is composed by the sum of effort and a shock.

The roles and the periods Individuals play over 10 periods; the player roles (agent or principal) are assigned before the first period and stay constant for the remaining periods.

The four treatments Our main design comprises four treatments. We vary two dimensions of the principal-agent relationship, as summarized in Table 1. The first dimension is whether a shock occurs ('shock') or not ('no-shock'); the second dimension is whether the relationship lasts for one period ('S', for static relationship), or for five periods ('D', for dynamic relationship). In the following we refer to a series of five periods as 'one block'.

The variation on the shock refers to how effort translates into outcome: In the 'no-shock' treatments, the outcome y corresponds to the effort e; in the 'shock' treatments, y is the sum of e and an uniformly distributed random integer component $\epsilon_i \in \{-2, -1, 0, 1, 2\}$. The variation on the duration of the relationship refers to the length of the interaction within the same principal-agent pair. In the 'S' treatments, subjects form groups of eight (four agents, four principals) and are randomly rematched within their group with a partner of the other role at *the end of each period.* In the 'D' treatments, subjects form groups of four (two agents, two principals) and are rematched within their group at *the end of a block*; that is, a principal-agent pair remains intact for five periods.

The payoffs In all treatments one randomly selected period is chosen for payment; in this period, the payoff function is $\pi^P = 10y - w - c_a(a)$ for the principal and $\pi^A = w - c_e(e) + a$ for the agent.

Information provided In all treatments, the agents receive information about the wage and the desired effort before making their effort decision and the principals receive information about the wage, the desired effort and the output (which corresponds to the effort in the no-shock treatments and to effort plus shock in the shock treatments) before making their adjustment decision. In addition, all participants receive the following information at the end of each period: the wage; the desired effort; the output; the adjustment; as well as the individual earnings for that period. After each period, participants have the opportunity to record this information in a personal recording sheet.

Procedures The experiment was programmed in z-Tree (Fischbacher, 2007) and participants were recruited via hroot (Bock et al., 2014). Sessions were run at the Innsbruck EconLab and lasted on average around 70 minutes. Average earnings per participant were €13.83. We ran three sessions à 24 subjects per treatment; with three matching groups of eight in the S sessions (respectively, six matching groups of four in the D sessions) this results in 9 (respectively 18) independent observations per treatment, when using the average within a single matching group over all periods as one independent observation.[7]

Variables of interest Our main variables of interest are the effort and the adjustment, since these two variables reveal reciprocity towards kind or unkind behavior in the previous stage. In addition, these are the productive actions, in the sense that they directly affect efficiency (total welfare), defined as the sum of the payoffs. Furthermore, we report the wage, since both effort as well as adjustment may be influenced by first stage behavior. Finally, we report the payoffs and the resulting welfare.

3 Results

3.1 The impact of extending the relationship length

We investigate the effect of extending the relationship length in an environment without shocks by comparing the $\text{S}_{\text{no-shock}}$ to the $\text{D}_{\text{no-shock}}$ data. Averages of the main variables of interest are summarized in Table 2 and Figure 1.

Average wage, av. effort and av. adjustment do not differ significantly between the two treatments. Mann-Whitney U-tests (MWU-test) yield $p = 0.88$ for the wage ($w_{S|\text{no-shock}} = 35.19$, $w_{D\,|\text{no-shock}} = 35.61$; $n_{S|\text{no-shock}} = 9$, $n_{D|\text{no-shock}} = 18$), $p = 0.54$ for the effort ($e_{S|\text{no-shock}} = 5.81$; $e_{D|\text{no-shock}} = 5.46$) and $p = 0.43$ for the adjustment ($a_{S|\text{no-shock}} = 6.96$, $a_{D|\text{no-shock}} = 4.72$).[8] We record this in Result 1:

[7] Given the difference in the mean effort between the 'Effort' and the 'Effort Shock' treatment in Rubin and Sheremeta (2016)'s data and given the respective standard deviations, with a sample size of nine observations in each condition and an α of 5%, we have a power of 88% (t-test).

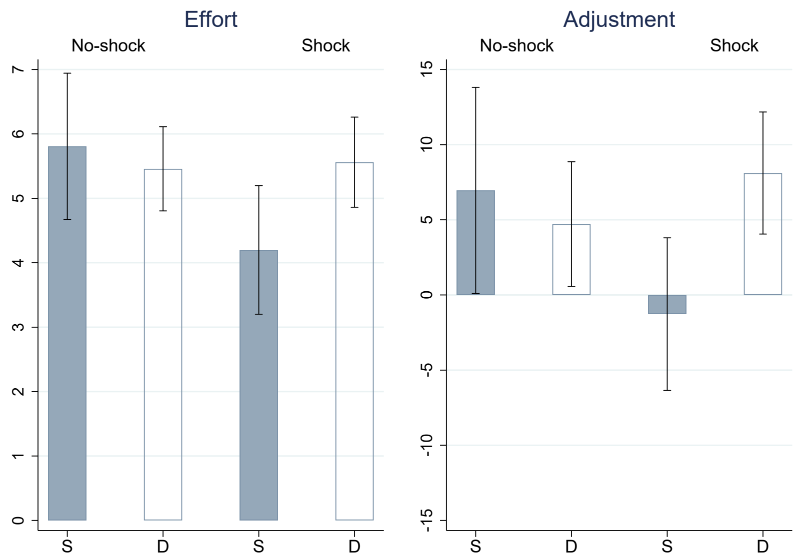
[8] As a result, av. welfare is not significantly different between the treatments ($W_{S|\text{no-shock}} = 39.96$, $W_{D|\text{no-shock}} = 33.75$; $p = 0.33$), as is the av. payoff of the principal ($PP_{S|\text{no-shock}} = 20.68$, $PP_{D|\text{no-shock}} = 16.46$; $p = 0.32$), and the av. payoff of the agent ($PA_{S|\text{no-shock}} = 19.28$, $PA_{D|\text{no-shock}} = 17.28$; $p = 0.41$).

Table 2: Averages of decision variables and payoffs

		Wage	Effort	Adjustment	Principal's payoff	Agent's payoff	Welfare
S	No-shock	35.19	5.81	6.96	20.68	19.28	39.96
		(3.07)	(0.49)	(2.97)	(2.47)	(3.49)	(5.00)
	Shock	29.21	4.20	−1.28	10.10	13.71	23.81
		(3.00)	(0.43)	(2.20)	(3.18)	(2.66)	(3.70)
D	No-shock	35.61	5.46	4.72	16.46	17.28	33.75
		(2.56)	(0.31)	(1.96)	(2.57)	(1.71)	(2.44)
	Shock	35.48	5.56	8.11	18.15	21.74	39.89
		(2.38)	(0.33)	(1.92)	(2.14)	(1.86)	(3.18)

Standard errors in parenthesis are based on 9 independent observations in the S treatments and on 18 independent observations in the D treatments.

Figure 1: Effort and adjustment, with error bars representing the 95% conf. intervals



Result 1 *In an environment without unobservable random shocks, extending the relationship length has no significant effect on average effort and average adjustment.*

Table 3: Panel models of adjustment and effort, no-shock treatments

	(1)	(2)	(3)	(4)
	S	D	S	D
Dep. variable:	adjustment		effort	
Wage	0.08	−0.07	0.07***	0.08***
	(0.09)	(0.11)	(0.01)	(0.01)
Effort - desired effort	2.95***	3.09***		
	(0.64)	(0.65)		
Desired effort			0.11**	0.16***
			(0.05)	(0.04)
Risk aversion	0.06	−0.01	0.00	0.00
	(0.06)	(0.06)	(0.01)	(0.01)
Inv. period	−13.82**	−4.60	−0.80**	−1.17*
	(6.82)	(5.58)	(0.34)	(0.71)
Constant	11.47***	15.22***	2.66***	1.70***
	(3.77)	(4.84)	(0.79)	(0.50)
Observations	360	360	360	360

Standard errors in parentheses are clustered on the group level and calculated via bootstrap; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inv. period runs from 1 to 1/10. Risk aversion runs from 1 to 100, with higher numbers indicating less risk aversion. Wage runs from 0 to 100. Effort and desired effort run from 0 to 14. Adjustment runs from -50 to 50.

Result 1 is in contrast to findings in the previous literature (see, for instance, Falk et al., 1999; Gächter and Falk, 2002; Brown et al., 2004). This is probably due to the fact that the previous literature uses the *two-stage* gift-exchange game as the basic game, while our basic game has three stages. Adding a third stage allows the principal to punish or reward the agent's effort choice within a given round, and this has been shown to have a pronounced effect on efficiency (see, for instance Fehr et al., 1997; Fehr and Schmidt, 2004; Fehr et al., 2007). This might explain why extending the relationship length does not have a significant additional effect.

Next we look at the determinants of behavior, by estimating different panel models where standard errors are clustered at the matching group level and calculated using a bootstrap method (Cameron et al., 2008). Following a backward-induction logic, we start with the adjustment stage in which the principal can reward (punish) the agent for high (low) effort by choosing a positive (negative) adjustment. In line with previous literature (Fehr et al., 1997; Rubin and Sheremeta, 2016), we assume that the principal's reciprocity motive is a relative one; i.e., it is a function of the agent's effort, relative to the desired effort. Therefore, we investigate the determinants of the principal's behavior in the third stage by regressing the adjustment on the difference between effort and desired effort (plus some control variables) – see columns (1) and (2) of Table 3. As expected, the difference between effort and desired effort has a significant positive impact on adjustment in both treatments. Inverse period has a negative effect on adjustment in both environments, however the effect is only significant in the static treatment. That is, controlling for all other reported variables, adjustment is higher in later periods in the static treatment. (It should be noted, however, that the two coefficients do not differ significantly from each other – see the interaction term of 'Inverse period x T_D' in column (1) of Table 13 in Appendix A.) A quick glance at the development over time (see figures 3 - 6 in

Appendix A) suggests that this result is mainly driven by the fact that wage is lower in later periods; also the fitted values in Figure 7 in Appendix A bolster this interpretation. In fact, when including the interaction term ‘Wage x inverse period’ into Table 3, the significant effect of inverse period on adjustment disappears – see columns (1) and (2) of Table 14 in Appendix A – while none of the other coefficients changes substantially.

In columns (3) and (4) of Table 3, we investigate the determinants of the agent’s effort choice in stage 2, by regressing effort on wage and desired effort and on some control variables. As expected, average effort is significantly higher when the wage is higher, and desired effort has a significant positive impact on effort in both treatments. Inverse period has a significant negative effect on effort in both treatments. That is, controlling for all other reported variables, effort is higher in later periods both in the shock and the no-shock treatment. Again, this effect seems to be driven by the wage – see figures 3 - 6 in Appendix A – and, in fact, the significant effect of inverse period on effort disappears when including the interaction term ‘Wage x inverse period’ into Table 3 – see columns (3) and (4) in Table 14 in Appendix A.

Next we search for direct evidence for the presence of a repeated-game effect. The repeated-game effect would predict that in a dynamic relationship, the agent’s effort in the current period is positively related to the adjustment in the previous period: The agent responds to a generous (low) adjustment in the previous period by exerting more (less) effort in the current period. We would not expect such a relationship in the static treatment, since the agent is paired with a new partner in each period. The repeated-game effect would also predict that the wage in the current period is positively related to the output in the previous period: The principal responds to the high output in the previous period by offering a more (less) generous wage in the current period. Again, no such relationship is expected for the static treatment. We display those predictions in columns (1) to (4) of Table 4.

Table 4: Overview expected effects in the respective treatments

	Correlation	$S_{\text{no-shock}}$	S_{shock}	$D_{\text{no-shock}}$	D_{shock}	$NRG_{\text{no-shock}}$	NRG_{shock}
Repeated-game effect	current effort & past-period adjustment	✗✔	✗✗	✔✔	✔✔		
	current wage & past-period output	✗✗	✗✗	✔✔	✔✔		
Noise canceling effect	current effort & past-period shock		✗✗		✔✔		✔✔

Note: Expected correlations are displayed with black checkmarks or crosses – checkmark if we expect a correlation, cross if we expect no correlation – while evidence is displayed in colors – red if the evidence is not in line with the expected correlation, green if the evidence is in line with the expected correlation.

In practice, we find the following. In the dynamic no-shock treatment, there is a significant positive correlation between current effort and previous adjustment – see column (2) of Table 5. However, the effect is not large (the coefficient is of size 0.02), and, contrary to our prediction, it is also present (and of similar size) in the static no-shock treatment (although only significant at the 10% level – see columns (1) and (3) of Table 5). This latter finding suggests that the positive correlation between current effort and previous adjustment in the no-shock treatments is not due to the repeated-game effect but rather due to experience: when the effort in the previous period was rewarded by a high adjustment in the previous period, agents are willing to provide high effort also in the current period.

Columns (1) and (2) of Table 6 provide more convincing evidence for the existence of a repeated-game effect. We regress wage on past-period output and past-period adjustment, as well as on some control variables. In the dynamic treatment, the current wage is significantly higher when the previous output was higher, while there is no statistically significant relationship between current wage and past-period output in the static treatment.

Table 5: Panel model of effort, controlling for past-period behavior, no-shock treatments

	(1)	(2)	(3)
	S	D	S and D
Dep. variable:		effort	
Wage	0.08***	0.08***	0.08***
	(0.01)	(0.01)	(0.01)
Desired effort	0.09	0.18***	0.14***
	(0.07)	(0.05)	(0.04)
$\text{Adjustment}_{\text{t-1}}$	0.01*	0.02***	0.02***
	(0.01)	(0.01)	(0.01)
T_D			−0.28
			(0.39)
$\text{Adjustment}_{\text{t-1}}$ x T_D			0.00
			(0.01)
Risk aversion	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)
Inv. period	−1.52	−4.22***	−2.95***
	(1.19)	(1.37)	(0.92)
Constant	2.76***	2.08***	2.46***
	(0.84)	(0.56)	(0.53)
Observations	324	324	648

Standard errors in parentheses are clustered on the group level and calculated via bootstrap; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inv. period runs from 1 to 1/10. Risk aversion runs from 1 to 100, with higher numbers indicating less risk aversion. Wage runs from 0 to 100. Effort and desired effort run from 0 to 14. Adjustment runs from -50 to 50. T_D is a dummy variable equal to one if the treatment is dynamic and zero otherwise.

Result 2 *In the no-shock treatments, we find some direct evidence for the presence of a repeated-game effect in the wage determination in the dynamic but not in the static interaction. There is also some evidence for a repeated-game effect in effort provision, but the effect is weak and (contrary to the prediction) also present in the static interaction.*

Summarizing the results of this section, we conclude that there is some direct evidence for the presence of a repeated-game effect in the data of the no-shock treatments (as summarized in

Table 6: Panel model of wage, controlling for past-period behavior, no-shock treatments

	(1) S	(2) D
Dep. variable:	wage	
$\text{Output}_{\text{t-1}}$	0.34	2.91***
	(0.30)	(0.41)
$\text{Adjustment}_{\text{t-1}}$	0.02	−0.01
	(0.04)	(0.06)
Risk aversion	0.06*	0.00
	(0.03)	(0.04)
Inv. period	33.42***	5.22
	(8.39)	(9.33)
Constant	22.33***	18.82***
	(3.51)	(3.59)
Observations	324	324

Standard errors in parentheses are clustered on the group level and calculated via bootstrap; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inv. period runs from 1 to 1/10. Risk aversion runs from 1 to 100, with higher numbers indicating less risk aversion. Wage runs from 0 to 100. Adjustment runs from -50 to 50. Effort runs from 0 to 14. $\text{Output}_{\text{t-1}}$ is the output of the previous period. In the no-shock treatments, output corresponds to effort.

Result 2). However, the effect seems to be insufficient to make the dynamic relationship more efficient than the static one (Result 1).

3.2 The impact of shocks on behavior

As shown by Rubin and Sheremeta (2016) and Davis et al. (2017), in static interactions the presence of unobservable random shocks has a pronounced negative impact on effort and adjustment. In our experimental design this result is reflected in the comparison between the $\text{S}_{\text{no-shock}}$ and the S_{shock} treatment: While the presence of shocks has no significant impact on the av. wage ($w_{S|\text{no-shock}} = 35.19$, $w_{S|\text{shock}} = 29.21$; MWU-test: $p = 0.20$, $n_{S|\text{no-shock}} = n_{S|\text{shock}} = 9$), the av. effort is reduced significantly ($e_{S|\text{no-shock}} = 5.81$, $e_{S|\text{shock}} = 4.20$; $p = 0.02$), as is av. adjustment ($a_{S|\text{no-shock}} = 6.96$, $a_{S|\text{shock}} =$ -1.28; $p = 0.06$).[9,10]

Turning to the D treatments, we find that the presence of unobservable random shocks has neither a significant impact on av. wage, nor on av. effort, nor on av. adjustment ($w_{D|\text{no-shock}}$

[9]The principal's average payoff is lower in presence of a shock ($PP_{S|\text{no-shock}} = 20.68$, $PP_{S|\text{shock}} = 10.10$; $p = 0.02$), while the agent's av. payoff is not significantly different ($PA_{S|\text{no-shock}} = 19.28$, $PA_{S|\text{shock}} = 13.71$; $p = 0.15$), and overall, av. welfare is lower under the shock ($W_{S|\text{no-shock}} = 39.96$, $W_{S|\text{shock}} = 23.81$; $p = 0.05$).

[10]Rubin and Sheremeta (2016) find that the presence of shocks decreases the effort and the wage while it leaves the adjustment (statistically) unaffected. Note, however, that the effect size of introducing the shock is very similar in the two settings; it leads to a decrease of 17.72% in the wage in Rubin and Sheremeta and to a decrease of 16.99% in our setting; it leads to a decrease in the effort of 26.72% in Rubin and Sheremeta and to a decrease of 27.71% in our setting; and it leads to a decrease in the adjustment of 444.23% in Rubin and Sheremeta and to a decrease of 118.37% in our setting. Also, when we compare the shock and the no-shock treatment between the two sets of experiments directly, there is neither a difference in the wage, nor in the effort, nor in the adjustment between the two studies. For the wage level the MWU test yields $p = 0.31$ for the comparison of the no-shock treatments and $p = 0.31$ for the comparison of the shock treatments; the respective values for effort and adjustment are $p = 0.40$ and $p = 0.31$ for the comparison of the no-shock treatments and $p = 0.23$ and $p = 0.22$ for the comparison of the shock treatments.

$= 35.61$, $w_{D|\text{shock}} = 35.48$; $e_{D|\text{no-shock}} = 5.46$, $e_{D|\text{shock}} = 5.56$; $a_{D|\text{no-shock}} = 4.72$, $a_{D|\text{shock}} = 8.11$; pairwise comparisons, MWU-tests, all $p > 0.22$; $n_{D|\text{no-shock}} = n_{D|\text{shock}} = 18$).[11]

We next compare the difference between the D_{shock} and the S_{shock} treatment to the difference between the $\text{D}_{\text{no-shock}}$ and the $\text{S}_{\text{no-shock}}$ treatment. We expect that extending the relationship length has a significant positive effect on average wage and average adjustment in the treatments with random shocks. Instead, from Result 1 we know that extending the relationship length has no significant effects on our variables of interest in the treatments without random shocks. As a result, we would expect that compared to the treatments without random shocks, extending the relationship length has a significantly more positive impact on average effort and average adjustment in the treatments with unobservable random shocks. This is indeed the case – compared to the no-shock treatments, the extension of the relationship length has a significantly more positive impact on av. effort in the shock treatments ($e_{D|\text{shock}}$ - $e_{S|\text{shock}} = 1.36$, $e_{D|\text{no-shock}}$ - $e_{S|\text{no-shock}} = -0.35$; diff-in-diff OLS regression, $p = 0.04$, see column (2) of Table 15 in Appendix A, with $n_{D|\text{no-shock}} = n_{D|\text{shock}} = 18$ and $n_{S|\text{no-shock}} = n_{S|\text{shock}} = 9$), and the same is true for the av. adjustment ($a_{D|\text{shock}}$ - $a_{S|\text{shock}} = 9.39$, $a_{D|no\,shock}$ - $a_{S|\text{no-shock}} = -2.23$; $p = 0.02$, see column (3) of Table 15).[12]

We summarize our findings regarding the impact of random shocks as follows:

Result 3 *The presence of unobservable random shocks has a pronounced negative effect on average effort and average adjustment when the relationship is static but no significant effect on average effort and average adjustment when the relationship is dynamic. Comparing the effect of extending the relationship length between the environment with random shocks and the environment without random shocks we find a significantly more pronounced positive effect on effort and adjustment in the presence of random shocks than in the absence of random shocks.*

We proceed by investigating the determinants of behavior. We start with the basic determinants. Each column of Table 7 includes data from both the shock and the no-shock treatment. In columns (1) and (2), we regress the adjustment on the difference between output and desired effort, on wage, on a 'T_{shock}' dummy (equal to one if the treatment includes shocks and zero otherwise), on the respective interaction terms, and on control variables. There is no significant difference in the impact of 'output - desired effort' between the shock and the no shock treatments, and as expected, the difference between output and desired effort positively correlates with the adjustment in all treatments.[13] The interaction term 'Wage x T_{shock}' is negative and (marginally) significant in the static relationship ($p = 0.10$). This means that there is a stronger negative correlation between wage and adjustment in the S_{shock} treatment than in the $\text{S}_{\text{no-shock}}$ treatment. However, wage does not have a significant impact on adjustment in either treatment – see the separate regressions for the shock and the no-shock treatments in columns (1) and (2) of Table 3 and columns (1) and (2) of Table 16 in Appendix A.[14]

[11] As a result, the principal's av. payoff, the agent's av. payoff, and av. welfare are not significantly different between the two treatments ($PP_{D|\text{no-shock}} = 16.46$, $PP_{D|\text{shock}} = 18.15$; $PA_{D|\text{no-shock}} = 17.28$, $PA_{D|\text{shock}} = 21.74$; $W_{D|\text{no-shock}} = 33.75$, $W_{D|\text{shock}} = 39.89$; all $p > 0.11$).

[12] As a consequence, the impact of extending the relationship length is also significantly more positive in the shock as compared to the no-shock treatments for av. welfare, av. principal's payoff and av. agents' payoff ($W_{D|\text{shock}}$ - $W_{S|\text{shock}} = 16.08$, $W_{D|\text{no-shock}}$ - $W_{S|\text{no-shock}} = -6.21$; $p < 0.01$; $PP_{D|\text{shock}}$ - $PP_{S|\text{shock}} = 8.05$, $PP_{D|\text{no-shock}}$ - $PP_{S|\text{no-shock}} = -4.22$; $p = 0.03$; $PA_{D|\text{shock}}$ - $PA_{S|\text{shock}} = 8.03$, $PA_{D|no,\ shock}$ - $PA_{S|\text{no-shock}} = -2.00$; $p = 0.04$, see columns (4) - (6) of Table 15).

[13] See the separate regressions for the shock and the no-shock treatments – columns (1) and (2) of Table 3 and columns (1) and (2) of Table 16 in Appendix A.

[14] Inverse period has a negative effect on adjustment in both sets of treatments, and – as in the no-shock treatments – the effect is only significant for the static interaction. Also, as in the no-shock treatments, including an interaction term between wage and inverse period into the determinants of adjustment shows that the significant effect of inverse period disappears, while the other variables change only marginally – see columns (1) and (2) of

Table 7: Panel model of adjustment and effort; shock and no-shock treatments

	(1)	(2)	(3)	(4)
	S	D	S	D
Dep. variable:	adjustment		effort	
Wage	0.07	−0.07	0.07***	0.08***
	(0.10)	(0.11)	(0.01)	(0.01)
Output - desired effort	2.92***	3.09***		
	(0.66)	(0.68)		
Desired effort			0.11**	0.17***
			(0.05)	(0.04)
T_{shock}	−0.47	−2.72	−0.88	−0.21
	(5.16)	(5.23)	(0.54)	(0.47)
Wage x T_{shock}	−0.23*	0.16	0.01	0.01
	(0.14)	(0.14)	(0.01)	(0.01)
(Output - desired effort) x T_{shock}	0.08	−0.72		
	(0.83)	(0.83)		
Desired effort x T_{shock}			−0.06	−0.01
			(0.07)	(0.06)
Risk aversion	−0.02	−0.01	0.01	0.00
	(0.04)	(0.05)	(0.01)	(0.00)
Inv. period	−8.77**	−5.25	−0.55**	−0.74*
	(4.45)	(4.31)	(27.27)	(0.44)
Constant	14.59***	15.57***	1.98***	1.64***
	(3.99)	(4.42)	(10.20)	(0.44)
Observations	720	720	720	720

Standard errors in parentheses are clustered on the group level and calculated via bootstrap; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inv. period runs from 1 to 1/10. Risk aversion runs from 1 to 100, with higher numbers indicating less risk aversion. Wage runs from 0 to 100. Effort and desired effort run from 0 to 14. Adjustment runs from -50 to 50. T_{shock} is a dummy equal to one if the treatment is with shock and zero otherwise. In the no-shock treatments, output corresponds to effort; in the shock treatments, output corresponds to effort plus the shock which runs from -2 to 2.

Table 8: Panel model of effort, controlling for past-period behavior and shock, shock treatments

	(1)	(2)
	S	D
Dep. variable:	effort	
Wage	0.08***	0.09***
	(0.01)	(0.01)
Desired effort	0.03	0.13**
	(0.03)	(0.05)
$\text{Adjustment}_{\text{t-1}}$	0.01	0.03***
	(0.01)	(0.01)
$\text{Shock}_{\text{t-1}}$	0.12	−0.21*
	(0.09)	(0.12)
Risk aversion	0.02**	0.00
	(0.01)	(0.01)
Inv. period	−0.33	−0.19
	(1.46)	(1.08)
Constant	0.37	1.36**
	(0.67)	(0.65)
Observations	324	324

Standard errors in parentheses are clustered on the group level and calculated via bootstrap; $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Inv. period runs from 1 to 1/10. Risk aversion runs from 1 to 100, with higher numbers indicating less risk aversion. Wage runs from 0 to 100. Effort and desired effort run from 0 to 14. $\text{Adjustment}_{\text{t-1}}$ is the adjustment of the previous period and runs from -50 to 50. $\text{Shock}_{\text{t-1}}$ is the shock of the previous period and runs from -2 to 2.

To investigate the determinants of the agent's effort choice, we regress effort on wage, desired effort, the T_{shock} dummy, interaction terms of these variables, and control variables – see columns (3) and (4) of Table 7. The impact of wage on effort does not differ significantly between the two treatments, and, as expected, the impact of wage is positive in all settings.[15] The impact of desired effort on effort does not differ between the shock and the no-shock treatment (the interaction term 'desired effort x T_{shock}' is not statistically significant), and separate regressions for the shock and the no-shock treatments show that desired effort has a significant positive impact only in $S_{no\text{-}shock}$, $D_{no\text{-}shock}$ and D_{shock}, and not in S_{shock} treatment – see columns (3) and (4) of Table 3 and columns (3) and (4) of Table 16 in Appendix A.[16]

Next we investigate whether there is direct evidence for the active part of the noise-canceling effect – the insurance component – and for the repeated-game effect in dynamic relationships with random shocks. The insurance component predicts that in dynamic relationships, the agent reacts to a negative (positive) shock in the previous period with higher (lower) effort in the current period. In a static relationship, since the agent is paired with a new partner in each period, there is no reason to expect such 'smoothing' behavior. The correlations predicted by the repeated-game effect are as described in section 3.1; all predicted correlations are summarized in Table 4.

In Table 8 we regress the effort on wage, desired effort, the adjustment of the previous period, the size of the shock of the previous period, and some other variables, for each of the two shock treatments separately. The past-period shock correlates negatively with current-period effort in D_{shock}, but not in S_{shock}, which is in line with our hypothesis that there is an insurance component in dynamic but not in static relationships plagued by random shocks. We also see that the past-period adjustment correlates positively with current-period effort in the treatment D_{shock}, but not in S_{shock}, which is in line with the presence of a repeated-game effect in the effort provision in dynamic but not in static relationships plagued by random shocks – again, as predicted.

For further evidence on the repeated game effect, we regress the current wage on past-period output and past-period adjustment – see Table 9. In line with the presence of a repeated game effect in the wage determination, we find a strong positive correlation between current-period wage and past-period output in the treatment D_{shock}, while there is no significant evidence for such relation in S_{shock}.

Result 4 *In the shock treatments, we find direct evidence for the insurance effect in effort provision in dynamic but not in static interactions, and we find direct evidence for the repeated-game effect, both in the wage determination and in the effort provision, in dynamic but not in static interactions.*

Summarizing the results of this section, we conclude that there is direct evidence for the active part of the noise-canceling effect (the insurance effect) and for the repeated-game effect in dynamic interactions plagued by random shocks, but not in their static counterpart and that shocks have a significantly less pronounced negative effect on the agent's effort provision in dynamic than in static interactions. Since the repeated-game effect is also present (and of similar size) in dynamic interactions unaffected by random shocks (see the interaction terms

Table 17 in Appendix A.

[15] See the separate regressions for the shock and the no-shock treatments – columns (3) and (4) of Table 3 and columns (3) and (4) of Table 16 in Appendix A.

[16] Also here, inverse period has a significant negative effect on effort in both sets of treatments. That is, controlling for all other reported variables, effort is higher in later periods. Again, the effect is mainly driven by the no-shock treatments (compare columns (3) and (4) of Table 3 to columns (3) and (4) of Table 16 in Appendix A), and it becomes insignificant when including the interaction term 'Wage x inverse period' – see columns (3) and (4) of Table 17 in Appendix A.

Table 9: Panel model of wage, controlling for past-period behavior, shock treatments

	(1)	(2)
	S	D
Dep. variable:	wage	
$\text{Output}_{\text{t-1}}$	0.50	2.49^{***}
	(0.37)	(0.26)
$\text{Adjustment}_{\text{t-1}}$	0.02	0.02
	(0.04)	(0.03)
Risk aversion	0.09	-0.11^{**}
	(0.07)	(0.05)
Inv. period	13.76^{***}	11.36^{*}
	(4.58)	(6.64)
Constant	18.57^{***}	23.84^{***}
	(5.33)	(3.65)
Observations	324	324

Standard errors in parentheses are clustered on the group level and calculated via bootstrap; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inv. period runs from 1 to 1/10. Risk aversion runs from 1 to 100, with higher numbers indicating less risk aversion. Wage runs from 0 to 100. $\text{Adjustment}_{\text{t-1}}$ is the adjustment of the previous period and runs from -50 to 50. $\text{Output}_{\text{t-1}}$ is the output of the previous period. In the shock treatments, output corresponds to effort plus the shock which runs from -2 to 2. Effort runs from 0 to 14.

in tables 19 and 20 in Appendix A), and since it is insufficient to increase efficiency in those interactions, one is tempted to conclude that the noise-canceling effect is the main driver for our result that efficiency is higher in dynamic interactions plagued by random shocks than in their static counterparts but not higher in dynamic interactions not plagued by random shocks than in their static counterparts. In the next section, we address the question whether noise-canceling alone is sufficient to neutralize the negative impact of random shocks.

4 The impact of giving regular feedback in form of rewards and punishments

In this section we address the question whether noise-canceling *alone* is enough to neutralize the negative impact of random shocks. To investigate this issue, we run two additional treatments, one with and the other without unobservable random shocks.[17] In both of these additional treatments the interaction is dynamic – that is, a principal-agent pair remains intact for one block. In contrast to the two D treatments in our main design, in the two additional treatments, principals and agents interact under the *same* contract over the whole block. That is, the first stage is placed at the beginning of each block: the principal offers a wage and states a desired effort, which is valid for each of the five periods in the block. The second stage, in which the agent chooses an effort, and – depending on the treatment – a shock is realized, is played for five periods. In the treatments with a shock, the agent learns the realization of the shock at the end of the respective period. The third stage only takes place at the end of a block. The principal observes the average outcome, and then chooses an adjustment level for all periods

[17] All treatments were planned ahead, at the same point in time.

in the block.[18] Again, one period is randomly selected for payment and a new principal-agent pair is formed after the first block.

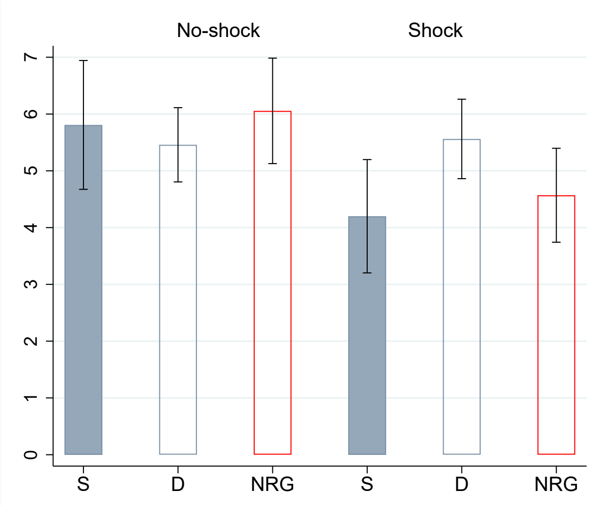
In this setup, the two components of the noise-canceling effect – that is, the learning component and the insurance component – potentially are still active, while the repeated-game effect is turned off: the principal does not have an opportunity to reward (punish) the agent for high (low) output in the present period by offering a high (low) wage in the next period and the agent cannot react to a high (low) adjustment in the current period by exerting high (low) effort in the next period. We therefore term the two additional treatments the no-repeated-game-effect (NRG) treatments.

Table 10: Averages of decision variables and payoffs, NRG treatments

	Wage	Effort	Adjustment	Principal's payoff	Agent's payoff	Welfare
No-shock	49.08	6.06	−5.69	9.10	18.03	27.82
	(2.24)	(0.21)	(4.68)	(5.82)	(3.87)	(6.90)
Shock	41.58	4.57	−1.67	4.72	23.53	28.93
	(3.18)	(0.39)	(3.13)	(5.68)	(3.81)	(4.65)

Standard errors in parenthesis are based on 18 independent observations

Figure 2: Effort in all treatments, with error bars representing the 95% conf. intervals



The predicted correlations for the NRG treatments are summarized in Table 4, and the averages of the main variables are displayed in Table 10 and Figure 2. To investigate the impact of removing the repeated-game effect in dynamic gift-exchange relationships without random shocks, we compare the $D_{\text{no-shock}}$ treatment to the $NRG_{\text{no-shock}}$ treatment. Removing the repeated-game effect in the absence of shocks increases av. wage and decreases av. adjustment ($w_{D|\text{no-shock}} = 35.61$, $w_{\text{NRG}|\text{no-shock}} = 49.08$; MWU-test: $p < 0.01$; $a_{D|\text{no-shock}} = 4.72$, $a_{\text{NRG}|\text{no-shock}} = \text{-}5.69$; $p = 0.08$; $n_{D|\text{no-shock}} = n_{\text{NRG}|\text{no-shock}} = 18$); however, it does not have a statistically significant effect on av. effort ($e_{D|\text{no-shock}} = 5.46$, $e_{\text{NRG}|\text{no-shock}} = 6.06$; $p = 0.26$).[19]

[18] Importantly, output is not observed by the principal each period; instead, she only observes the average output at the end of a block, before deciding on the adjustment.

[19] Av. welfare is not significantly different between the two treatments ($W_{D|\text{no-shock}} = 33.75$, $W_{\text{NRG}|\text{no-shock}} = 27.82$; $p = 0.62$), as is the av. payoff of the principals ($PP_{D|\text{no-shock}} = 16.46$, $PP_{\text{NRG}|\text{no-shock}} = 9.10$; $p = 0.66$),

Next we investigate the impact of removing the repeated-game effect in dynamic gift-exchange relationships with unobservable random shocks by comparing the D_{shock} treatment to the NRG_{shock} treatment. Removing the repeated-game effect in the presence of shocks does not impact av. wages ($w_{D|\text{shock}} = 35.48$, $w_{\text{NRG|shock}} = 41.58$; MWU-test: $p = 0.15$, $n_{D|\text{shock}} = n_{\text{NRG|shock}} = 18$), while it significantly decreases av. effort and av. adjustment ($e_{D|\text{shock}} = 5.56$, $e_{\text{NRG|shock}} = 4.57$; $p = 0.08$; $a_{D|\text{shock}} = 8.11$, $a_{\text{NRG|shock}} = -1.67$; $p < 0.01$; $n_{D|\text{shock}} = n_{\text{NRG|shock}} = 18$).[20]

To complete the picture we next analyze within the dynamic relationships whether shocks have the same impact when the repeated-game effect is turned off, by comparing the difference between the $D_{\text{no-shock}}$ and the D_{shock} treatment to the difference between the $NRG_{\text{no-shock}}$ and the NRG_{shock} treatment. From the previous section we know that in the D treatments the presence of unobservable random shocks has neither a significant impact on av. wage, nor on av. effort, nor on av. adjustment. In the NRG setting, the presence of unobservable random shocks has no significant impact on av. wage and adjustment ($w_{\text{NRG|no-shock}} = 49.08$, $w_{\text{NRG|shock}} = 41.58$; $p = 0.14$; $a_{\text{NRG|no-shock}} = -5.69$, $a_{\text{NRG|shock}} = -1.67$; MWU-test, $p = 0.57$; $n_{\text{NRG|no-shock}} = n_{\text{NRG|shock}} = 18$), while av. effort is significantly lower in the presence of a shock ($e_{\text{NRG|no-shock}} = 6.06$, $e_{\text{NRG|shock}} = 4.57$; $p = 0.03$).[21] As a result, the impact of unobservable random shocks on av. wage and av. adjustment is not significantly different between the two settings ($w_{D|\text{shock}}$ - $w_{D|\text{no-shock}} = -0.13$, $w_{\text{NRG|shock}}$ - $w_{\text{NRG|no-shock}} = -7.50$; diff-in-diff OLS regression: $p = 0.16$, see column (1) in Table 18 in Appendix A, with $n_{D|\text{no-shock}} = n_{D|\text{shock}} = n_{\text{NRG|no-shock}} = n_{\text{NRG|shock}} = 18$; $a_{D|\text{shock}}$ - $a_{D|\text{no-shock}} = 3.39$, $a_{\text{NRG|shock}}$ - $a_{\text{NRG|no-shock}} = 4.03$, $p = 0.92$, see column (3) of Table 18), while the impact of shocks on effort is more negative in the in the NRG treatments, compared to the D treatments ($e_{D|\text{shock}}$ - $e_{D|\text{no-shock}} = 0.10$, $e_{\text{NRG|shock}}$ - $e_{\text{NRG|no-shock}} = -1.49$; $p = 0.04$, see column (2) of Table 18).[22]

The above results suggest that unobservable random shocks have a pronounced negative impact on effort in static relationships and in dynamic relationships where the repeated-game effect is turned off. Comparing these two environments we find that the impact of unobservable random shocks on av. effort, av. wage and av. adjustment is not significantly different between them ($e_{S|\text{shock}}$ - $e_{S|\text{no-shock}} = -1.61$, $e_{\text{NRG|shock}}$ - $e_{\text{NRG|no-shock}} = -1.49$; diff-in-diff OLS regression: $p = 0.90$, see column (2) of Table 21 in Appendix A; $w_{S|\text{shock}}$ - $w_{S|\text{no-shock}} = -5.98$, $w_{\text{NRG|shock}}$ - $w_{\text{NRG|no-shock}} = -7.50$, $p = 0.81$, column (1) of Table 21; $a_{S|\text{shock}}$ - $a_{S|\text{no-shock}} = -8.23$, $a_{\text{NRG|shock}}$ - $a_{\text{NRG|no-shock}} = 4.02$, $p = 0.15$, column (3) of Table 21).[23] We summarize this evidence to Result 5:

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and the av. payoff of the agents ($PA_{D|\text{no-shock}} = 17.28$, $PA_{\text{NRG|no-shock}} = 18.03$; $p = 0.82$). The difference in principals' payoffs is very large (80.98%; $PP_{D|\text{no-shock}} = 16.46$, $PP_{\text{NRG|no-shock}} = 9.10$; MWU-test, $p = 0.66$). The fact that the difference is not significant is probably driven by the large variance in the NRG treatment (st. dev. in $D_{\text{no-shock}}$: 10.90; st. dev. in $NRG_{\text{no-shock}}$: 24.67).

[20] Av. welfare is significantly lower in the NRG treatment than in the D treatment ($W_{D|\text{shock}} = 39.89$, $W_{\text{NRG|shock}} = 28.93$; $p = 0.04$), which is driven by the difference in the av. payoff of the principals ($PP_{D|\text{shock}} = 18.15$, $PP_{\text{NRG|shock}} = 4.72$; $p = 0.03$), while the av. payoff of the agents is not significantly different between the treatments ($PA_{D|\text{shock}} = 21.74$, $PA_{\text{NRG|shock}} = 23.53$; $p = 0.99$).

[21] The principals' av. payoff, the agents' av. payoff, and av. welfare are not significantly different between the two treatments ($PP_{\text{NRG|no-shock}} = 9.10$, $PP_{\text{NRG|shock}} = 4.72$; $PA_{\text{NRG|no-shock}} = 18.03$, $PA_{\text{NRG|shock}} = 23.53$; $W_{\text{NRG|no-shock}} = 27.82$, $W_{\text{NRG|shock}} = 28.93$; all $p > 0.28$).

[22] The impact of the shock on the av. payoff of the principal, the av. payoff of the agent, and on av. overall welfare does not differ significantly between the two conditions ($W_{D|\text{shock}}$ - $W_{D|\text{no-shock}} = 6.15$, $W_{\text{NRG|shock}}$ - $W_{\text{NRG|no-shock}} = 1.11$; $p = 0.59$; $PP_{D|\text{shock}}$ - $PP_{D|\text{no-shock}} = 1.69$, $PP_{\text{NRG|shock}}$ - $PP_{\text{NRG|no-shock}} = -4.36$; $p = 0.49$; $PA_{D|\text{shock}}$ - $PA_{D|\text{no-shock}} = 4.46$, $PA_{\text{NRG|shock}}$ - $PA_{\text{NRG|no-shock}} = 5.49$; $p = 0.86$), see columns (4) - (6) of Table 18 in Appendix A.

[23] Also the impact of unobservable random shocks on the av. payoff of the principal, on the av. payoff of the agent, and on av. overall welfare does not differ significantly between the two conditions ($W_{S|\text{shock}}$ - $W_{S|\text{no-shock}} = 16.16$, $W_{\text{NRG|shock}}$ - $W_{\text{NRG|no-shock}} = 1.11$; $p = 0.18$; $PP_{S|\text{shock}}$ - $PP_{S|\text{no-shock}} = -10.58$, $PP_{\text{NRG|shock}}$ - $PP_{\text{NRG|no-shock}} = -4.36$; $p = 0.61$; $PA_{S|\text{shock}}$ - $PA_{S|\text{no-shock}} = -5.58$, $PA_{\text{NRG|shock}}$ - $PA_{\text{NRG|no-shock}} = 5.49$; $p = 0.19$, see columns (4) - (6) of Table 21 in Appendix A).

Result 5 *Without shocks, average wage is higher and average adjustment is lower in the no-repeated-game-effect treatment than in the dynamic treatment. Average effort is not significantly different between the treatments, however. In the presence of unobservable random shocks, average wage is not significantly different between the no-repeated-game-effect and the dynamic treatment, while average adjustment and average effort are lower in the no-repeated-game-effect treatment. The effect of introducing unobservable random shocks on average effort, average wage and average adjustment is not significantly different between the static and the no-repeated-game-effect treatment.*

Table 11: Panel model of adjustment and effort, shock and no-shock treatments, NRG treatments

	(1)	(2)
Dep. variable:	adjustment	effort
Wage	-0.10	0.06^{***}
	(0.20)	(0.02)
Output - desired effort	6.07^{***}	
	(0.91)	
Desired effort		-0.01
		(0.12)
T_{shock}	-19.31^{*}	-3.21^{**}
	(11.30)	(1.35)
Wage x T_{shock}	0.23	0.02
	(0.24)	(0.02)
(Output - desired effort) x T_{shock}	-5.53^{***}	
	(1.18)	
Desired effort x T_{shock}		0.16
		(0.14)
Risk aversion	-0.02	0.01
	(0.08)	(0.01)
Inv. period	-65.93^{**}	0.08
	(27.27)	(0.46)
Constant	24.46^{**}	2.76^{**}
	(10.20)	(1.19)
Observations	144	720

Standard errors in parentheses are clustered on the group level and calculated via bootstrap; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inv. period runs from 1 to 1/10. Risk aversion runs from 1 to 100, with higher numbers indicating less risk aversion. Wage runs from 0 to 100. Effort and desired effort run from 0 to 14. Adjustment runs from -50 to 50. In the no-shock treatments, output corresponds to effort; in the shock treatments, output corresponds to effort plus the shock which runs from -2 to 2. T_{shock} is a dummy equal to one if the treatment is with shock and zero otherwise. In treatments NRG_{shock} and $NRG_{no\text{-}shock}$, we replace output with average output from the five periods (since that is what the principals observe). Since in these two treatments, the principals only take 2 adjustment decisions (one in period 5, one in period 10) we only have 144 observations in column (1).

Again, we proceed by investigating the determinants of behavior, starting with the adjustment stage. Each column in Table 11 includes the shock and the no-shock treatments. In column (1), we regress adjustment on wage, the difference between output and desired effort, the T_{shock} dummy, interaction terms as well as some control variables. 'Output' is av. output from the corresponding block, since that is what the principal observes. The impact of wage on adjustment is not significantly different between the two treatments, and it has no significant impact

Table 12: Panel model of effort, controlling for past-period behavior and shock, only NRG shock treatment

	(1)
Dep. variable:	effort
Wage	0.08***
	(0.02)
Desired effort	0.13
	(0.10)
$\text{Shock}_{\text{t-1}}$	−0.13**
	(0.06)
Risk aversion	−0.01
	(0.02)
Inv. period	1.07
	(1.65)
Constant	0.61
	(1.47)
Observations	324

Standard errors in parentheses are clustered on the group level and calculated via bootstrap; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inv. period runs from 1 to 1/10. Risk aversion runs from 1 to 100, with higher numbers indicating less risk aversion. Wage runs from 0 to 100. Effort and desired effort run from 0 to 14. $\text{Shock}_{\text{t-1}}$ is the shock of the previous period and runs from -2 to 2.

on adjustment in either treatment (see the separate regressions for the no-shock and the shock treatment in columns (1) and (2) of Table 22 in Appendix A). Controlling for the different variables, the av. adjustment is significantly lower in the presence of a shock.[24] More importantly, there is a significant difference in the impact of 'Output - desired effort' on adjustment between the two treatments, see the significant coefficient on 'Output - desired effort x T_{shock}' ($p < 0.01$) in Table 11; this is confirmed by separate regressions for the no-shock and the shock treatment – see columns (1) and (2) of Table 22 in Appendix A.

In column (2) of Table 11 we regress effort on wage, desired effort, the T_{shock} dummy, interaction terms of these variables, and control variables. Av. effort is lower in the presence of shocks. There is no significant difference in the impact of wage or desired effort on effort; while wage has a significant positive impact in both treatments (see columns (3) and (4) of Table 22 in Appendix A, $\text{NRG}_{\text{no-shock}}$, $p < 0.01$; $\text{NRG}_{\text{shock}}$, $p < 0.01$), when analyzing the two treatments separately, desired effort shows up weakly significant in the shock treatment, and not significant in the no-shock treatment – see again columns (3) and (4) of Table 22 in Appendix A, $\text{NRG}_{\text{no-shock}}$, $p = 0.84$; $\text{NRG}_{\text{shock}}$, $p = 0.09$.

Next we investigate whether there is direct evidence for the active part of the noise-canceling effect (the insurance component), by controlling for past-period shocks in the shock treatments – see Table 12. Indeed, we find evidence for the noise-canceling effect: past-period shock correlates significantly negatively with current-period effort. We record this as Result 6:

Result 6 *In the no-repeated-game-effect treatment we find direct evidence for the active component of the noise-canceling effect (the insurance component) in the effort provision.*

[24] This is different from what we find in the MWU-test. This is potentially due to the fact that we control for several factors in the regression.

Summarizing the results of this section we conclude that there is some direct evidence for the presence of an insurance effect in effort provision in the data of the no-repeated-game-effect treatment plagued by random shocks. However, the effect seems to be insufficient to neutralize the negative impact of random shocks on efficiency.

Taken together, our results indicate that neither the repeated-game effect alone nor the noise-canceling effect alone is sufficient to alleviate the detrimental effects of unobservable random shocks. What is needed to eliminate the negative effects of shocks is an environment in which both the repeated-game effect and the noise-canceling effect can be active.

5 Conclusion

Rubin and Sheremeta (2016)'s result that reciprocal behavior is heavily depressed if unobservable random shocks blur the relation between effort and output in a static gift-exchange relationship challenges the relevance of gift-exchange for real world employment relationships. This paper has investigated the robustness of this finding by varying the relationship duration from a static interaction between the principal and the agent to a dynamic interaction, and by studying the importance of giving regular feedback in the form of rewards and punishments, in addition with setting the wage payment and having the posibility to observe output regularly.

We have shown that the negative impact of random shocks on effort provision is contained if the employment relation is dynamic (the same principal-agent pair interacts over several periods) provided that the principal has the possibility to give regular feedback in the form of rewards and punishments and by adapting the wage – together with the regular observation of the output. This allows for a repeated-game effect that is important to neutralize the negative impact of unobservable random shocks on reciprocal behavior.

Repeated interaction between the same principal-agent pair, no complete verifiability of the realized effort, but a regular observation of the output, together with the regular opportunity to give feedback by means of paying a bonus or fine, or adapting the wage payment – this is the setting that is most often observed in reality. All in all, our results suggest that reciprocal relationships in these settings are quite robust against the presence of unobservable random shocks.

A possible takeaway from our main finding that a dynamic setup eliminates the negative effect of shocks on gift-exchange is that firms might want to focus on building repeated relationships rather than trying to limit the impact of shocks by investing in greater supervision and more accurate measurement systems.

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A Additional tables and figures

Table 13: Panel model of adjustment, comparison $S_{\text{no-shock}}$ and $D_{\text{no-shock}}$

	(1)
Dep. variable:	$S_{\text{no-shock}}$ and $D_{\text{no-shock}}$ adjustment
Wage	0.06
	(0.11)
Effort - desired effort	2.83***
	(0.69)
T_D	3.37
	(6.26)
Wage x T_D	−0.13
	(0.15)
(Effort - desired effort) x T_D	0.30
	(0.94)
Risk aversion	0.07
	(0.07)
Inv. period	−13.40*
	(7.55)
Risk aversion x T_D	−0.07
	(0.09)
Inv. period x T_D	8.84
	(9.34)
Constant	11.88***
	(3.92)
Observations	720

Standard errors in parentheses are clustered on the group level and calculated via bootstrap; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inv. period runs from 1 to 1/10. Risk aversion runs from 1 to 100, with higher numbers indicating less risk aversion. Wage runs from 0 to 100. Effort and desired effort run from 0 to 14. Adjustment runs from -50 to 50. T_D is a dummy equal to 1 if the treatment is D and zero otherwise.

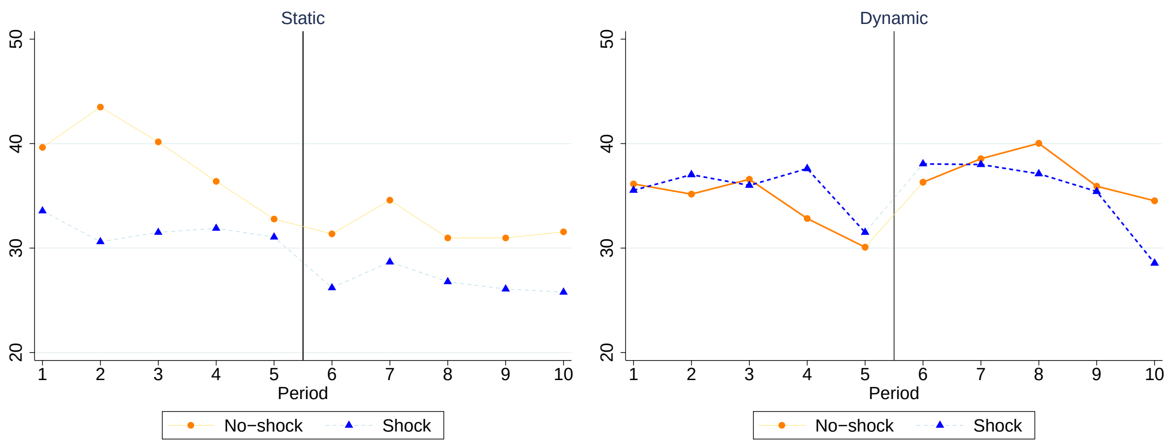


Figure 3: Average wage

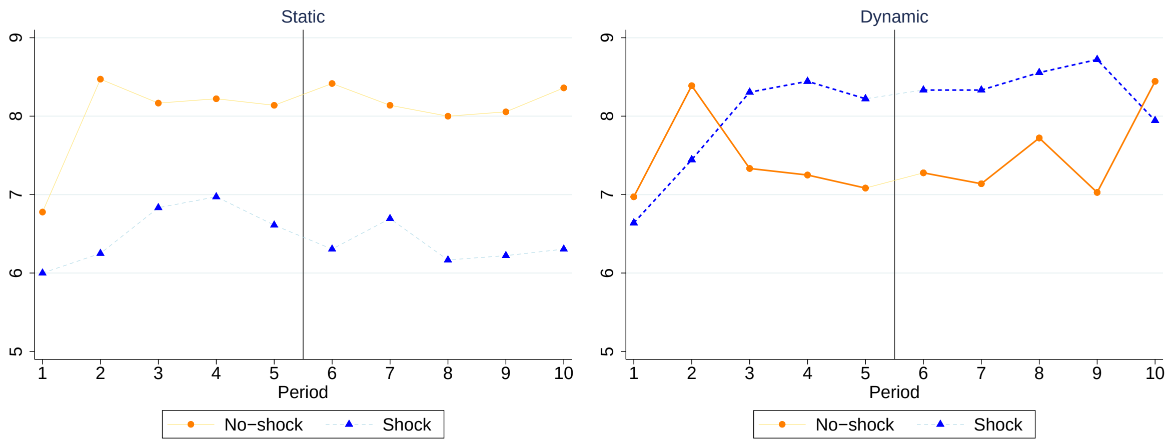


Figure 4: Average desired effort

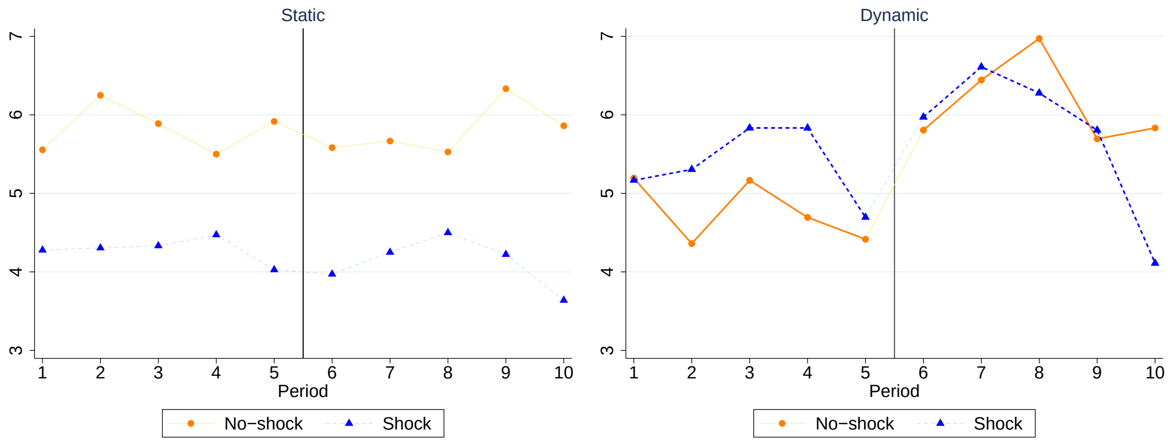


Figure 5: Average effort

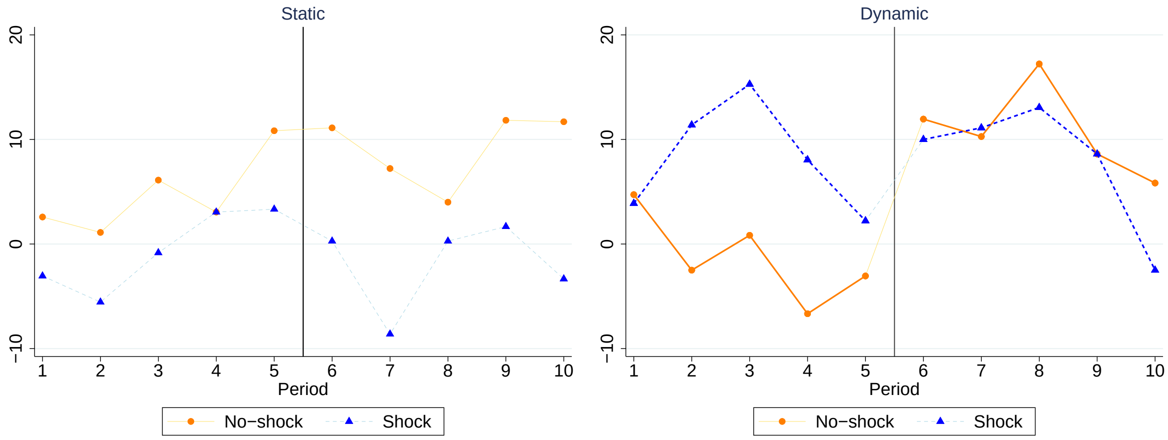


Figure 6: Average adjustment

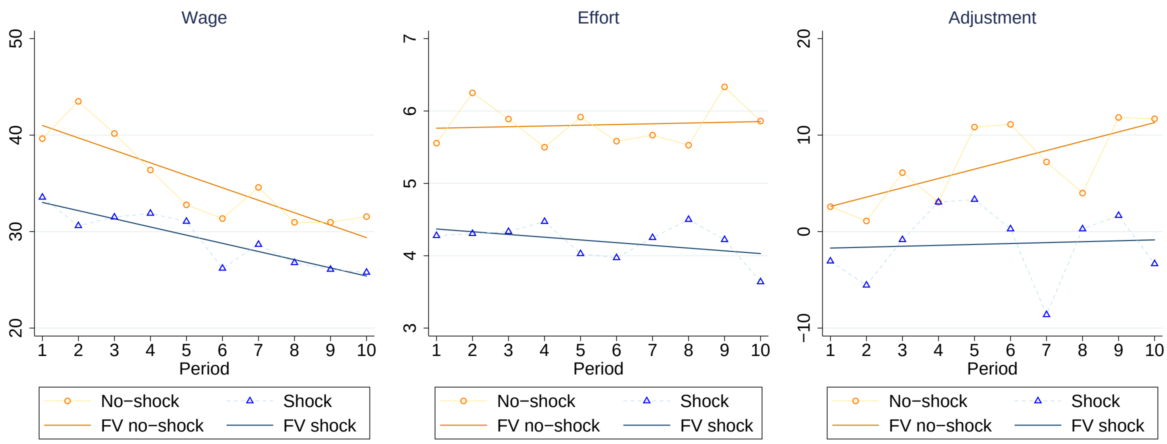


Figure 7: Fitted values for the static treatments, wage, effort, and adjustment

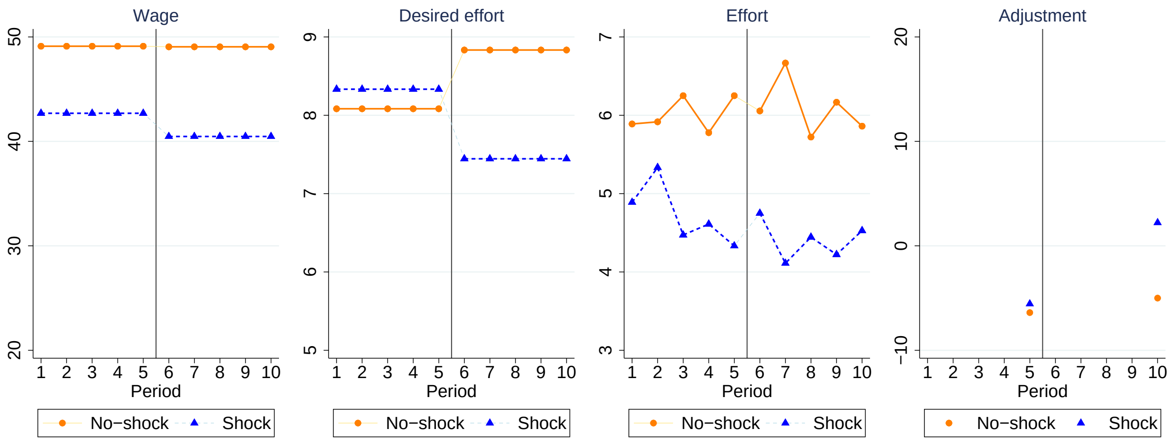


Figure 8: No-repeated-game-effect treatments

Table 14: Interaction term 'Wage x inv. period' included in Table 3

	(1)	(2)	(3)	(4)
	S	D	S	D
Dep. variable:	adjustment		effort	
Wage	0.18	-0.01	0.08^{***}	0.09^{***}
	(0.14)	(0.16)	(0.01)	(0.02)
Wage x inv. period	-0.29	-0.22	-0.02	-0.02
	(0.32)	(0.31)	(0.02)	(0.04)
Effort - desired effort	2.98^{***}	3.09^{***}		
	(0.65)	(0.65)		
Desired effort			0.11^{**}	0.16^{***}
			(0.05)	(0.04)
Risk aversion	0.06	-0.01	0.00	0.00
	(0.06)	(0.06)	(0.01)	(0.01)
Inv. period	-2.80	3.23	-0.13	-0.56
	(13.66)	(9.57)	(0.87)	(1.40)
Constant	8.34	13.01^{**}	2.46^{***}	1.54^{**}
	(5.67)	(6.23)	(0.87)	(0.69)
Observations	360	360	360	360

Standard errors in parentheses are clustered on the group level and calculated via bootstrap; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inv. period runs from 1 to 1/10. Risk aversion runs from 1 to 100, with higher numbers indicating less risk aversion. Wage runs from 0 to 100. Effort and desired effort run from 0 to 14. Adjustment runs from -50 to 50.

Table 15: OLS regressions investigating differences between the S and D treatments

	(1)	(2)	(3)	(4)	(5)	(6)
	mean wage	mean effort	mean adjustment	mean payoff principal	mean payoff agent	mean welfare
T_{shock}	-5.98	-1.61^{**}	-8.23^{**}	-10.58^{**}	-5.58	-16.16^{***}
	(4.75)	(0.65)	(3.83)	(4.52)	(3.86)	(5.85)
T_D	0.42	-0.35	-2.23	-4.22	-2.00	-6.22
	(4.11)	(0.56)	(3.32)	(3.91)	(3.34)	(5.06)
T_{shock} x T_D	5.85	1.71^{**}	11.62^{**}	12.27^{**}	10.04^{**}	22.30^{***}
	(5.82)	(0.79)	(4.69)	(5.53)	(4.72)	(7.16)
Constant	35.19^{***}	5.81^{***}	6.96^{**}	20.68^{***}	19.28^{***}	39.96^{***}
	(3.36)	(0.46)	(2.71)	(3.19)	(2.73)	(4.13)
Observations	54	54	54	54	54	54

Standard errors in parentheses are based on 9 (18) independent observations per treatment, to run an analysis similar to the MWU-tests as for the other results. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. T_{shock} is a dummy equal to one if the treatment is with shock and zero otherwise; T_D is a dummy equal to one if the treatment is dynamic and zero otherwise.

Table 16: Panel models of adjustment and effort, only shock treatments

	(1) S	(2) D	(3) S	(4) D
Dep. variable:	adjustment		effort	
Wage	-0.16	0.09	0.07^{***}	0.09^{***}
	(0.10)	(0.08)	(0.01)	(0.01)
Output - desired effort	3.02^{***}	2.38^{***}		
	(0.54)	(0.48)		
Desired effort			0.05	0.16^{***}
			(0.03)	(0.04)
Risk aversion	-0.10^{**}	-0.01	0.02^{**}	0.00
	(0.05)	(0.07)	(0.01)	(0.01)
Inv. period	-3.89	-5.92	-0.31	-0.30
	(4.52)	(6.23)	(0.41)	(0.48)
Constant	17.18^{***}	13.24^{***}	0.43	1.31^{**}
	(4.09)	(4.52)	(0.67)	(0.61)
Observations	360	360	360	360

Standard errors in parentheses are clustered on the group level and calculated via bootstrap; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inv. period runs from 1 to 1/10. Risk aversion runs from 1 to 100, with higher numbers indicating less risk aversion. Wage runs from 0 to 100. Effort and desired effort run from 0 to 14. Adjustment runs from -50 to 50. In the shock treatments, output corresponds to effort plus the shock which runs from -2 to 2.

Table 17: Interaction term ‘Wage x inv. period’ included in Table 7

	(1)	(2)	(3)	(4)
	S	D	S	D
Dep. variable:	adjustment		effort	
Wage	0.13	0.00	0.08***	0.09***
	(0.12)	(0.14)	(0.01)	(0.01)
Wage x inv. period	−0.17	−0.26	−0.04**	−0.02
	(0.17)	(0.23)	(0.02)	(0.03)
Output - desired effort	2.91***	3.09***		
	(0.66)	(0.68)		
Desired effort			0.11**	0.16***
			(0.05)	(0.04)
T_{shock}	−0.30	−2.45	−0.84	−0.20
	(5.21)	(5.22)	(0.53)	(0.47)
Wage x T_{shock}	−0.24*	0.16	0.01	0.01
	(0.14)	(0.14)	(0.01)	(0.01)
(Output - desired effort) x T_{shock}	0.08	−0.73		
	(0.83)	(0.84)		
Des. eff. x T_{shock}			−0.07	−0.01
			(0.06)	(0.06)
Risk aversion	−0.02	−0.01	0.01	0.00
	(0.04)	(0.05)	(0.01)	(0.00)
Inv. period	−2.74	3.93	0.92	−0.07
	(6.82)	(8.03)	(0.75)	(0.87)
Constant	12.89***	12.81**	1.57**	1.44***
	(4.61)	(5.49)	(0.73)	(0.53)
Observations	720	720	720	720

Standard errors in parentheses are clustered on the group level and calculated via bootstrap; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inv. period runs from 1 to 1/10. Risk aversion runs from 1 to 100, with higher numbers indicating less risk aversion. Wage runs from 0 to 100. Effort and desired effort run from 0 to 14. Adjustment runs from -50 to 50. T_{shock} is a dummy equal to one if the treatment is with shock and zero otherwise. In the no-shock treatments, output corresponds to effort; in the shock treatments, output corresponds to effort plus the shock which runs from -2 to 2.

Table 18: OLS regressions investigating differences between the D and NRG treatments

	(1) mean wage	(2) mean effort	(3) mean adjustment	(4) mean payoff principal	(5) mean payoff agent	(6) mean welfare
T_{shock}	-7.50^{**}	-1.49^{***}	4.03	-4.38	5.49	1.11
	(3.70)	(0.53)	(4.43)	(6.22)	(4.24)	(6.53)
T_D	-13.47^{***}	-0.60	10.42^{**}	7.37	-0.75	5.93
	(3.70)	(0.53)	(4.43)	(6.22)	(4.24)	(6.53)
T_{shock} x T_D	7.37	1.59^{**}	-0.64	6.06	-1.03	5.04
	(5.23)	(0.74)	(6.26)	(8.79)	(5.99)	(9.24)
Constant	49.08^{***}	6.06^{***}	-5.69^{*}	9.10^{**}	18.03^{***}	27.82^{***}
	(2.62)	(0.37)	(3.13)	(4.40)	(3.00)	(4.62)
Observations	72	72	72	72	72	72

Standard errors in parentheses are based on 18 independent observations per treatment, to run an analysis similar to the MWU-tests as for the other results. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. T_{shock} is a dummy equal to one if the treatment is with shock and zero otherwise; T_D is a dummy equal to one if the treatment is dynamic and zero otherwise.

Table 19: Difference in the effect of adjustment$_{t\text{-}1}$ and output$_{t\text{-}1}$ on wage between shock and no-shock treatments, only D

	(1)
Dep. variable	wage
Adjustment$_{t\text{-}1}$	0.00
	(0.05)
Output$_{t\text{-}1}$	2.86***
	(0.36)
T$_{shock}$	−0.24
	(3.78)
Adjustment$_{t\text{-}1}$ x T$_{shock}$	0.02
	(0.06)
Output$_{t\text{-}1}$ x T$_{shock}$	−0.27
	(0.44)
Risk aversion	−0.06*
	(0.03)
Inv. period	8.34
	(5.59)
Constant	21.63***
	(3.22)
Observations	648

Notes: Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. T$_{shock}$ is a dummy equal to one if the treatment is with shock and zero otherwise. Wage runs from 0 to 100. Inv. period runs from 1 to 1/10. Risk aversion runs from 1 to 100, with higher numbers indicating less risk aversion. Adjustment$_{t\text{-}1}$ is the adjustment of the previous period and runs from -50 to 50. Output$_{t\text{-}1}$ is the output of the previous period. In the no-shock treatments, output corresponds to effort; in the shock treatments, output corresponds to effort plus the shock which runs from -2 to 2.

Table 20: Difference in the effect of adjustment$_{t\text{-}1}$ and shock$_{t\text{-}1}$ on effort between the shock and no-shock treatments, only D

Dep. variable	(1) effort
Wage	0.09***
	(0.00)
Desired effort	0.15***
	(0.03)
Adjustment$_{t\text{-}1}$	0.02***
	(0.01)
T$_{shock}$	−0.22
	(0.29)
Adjustment$_{t\text{-}1}$ x T$_{shock}$	0.01
	(0.01)
Shock$_{t\text{-}1}$	−0.19
	(0.13)
Risk aversion	0.00
	(0.00)
Inv. period	−2.16**
	(0.95)
Constant	1.82***
	(0.42)
Observations	648

Notes: Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. T$_{shock}$ is a dummy equal to one if the treatment is with shock and zero otherwise. Wage runs from 0 to 100. Inv. period runs from 1 to 1/10. Risk aversion runs from 1 to 100, with higher numbers indicating less risk aversion. Effort and desired effort run from 0 to 14. Adjustment$_{t\text{-}1}$ is the adjustment of the previous period and runs from -50 to 50. Shock$_{t\text{-}1}$ is the shock of the previous period.

Table 21: OLS regressions investigating differences between the S and NRG treatments

	(1) mean wage	(2) mean effort	(3) mean adjustment	(4) mean payoff principal	(5) mean payoff agent	(6) mean welfare
T_{shock}	-7.50^{**}	-1.49^{***}	4.03	-4.38	5.49	1.11
	(3.64)	(0.55)	(4.87)	(6.89)	(4.81)	(7.30)
T_S	-13.89^{***}	-0.25	12.65^{**}	11.58	1.25	12.14
	(4.46)	(0.68)	(5.97)	(8.44)	(5.89)	(8.94)
T_{shock} x T_S	1.52	-0.12	-12.26	-6.20	-11.07	-17.27
	(6.31)	(0.96)	(8.44)	(11.94)	(8.33)	(12.64)
Constant	49.08^{***}	6.06^{***}	-5.69	9.10^{*}	18.03^{***}	27.82^{***}
	(2.57)	(0.39)	(3.45)	(4.88)	(3.40)	(5.16)
Observations	54	54	54	54	54	54

Standard errors in parentheses are based on 9 (18) independent observations per treatment, to run an analysis similar to the MWU-tests as for the other results. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. T_{shock} is a dummy equal to one if the treatment is with shock and zero otherwise; T_S is a dummy equal to one if the treatment is static and zero otherwise.

Table 22: Panel model of adjustment and effort, separately for the shock and no-shock treatments, NRG treatments

Dep. variable:	(1) no-shock adjustment	(2) shock adjustment	(3) no-shock effort	(4) shock effort
Wage	-0.13	0.12	0.06^{***}	0.08^{***}
	(0.20)	(0.16)	(0.02)	(0.02)
Output - desired effort	6.35^{***}	0.60		
	(0.89)	(0.78)		
Desired Effort			-0.02	0.15^{*}
			(0.11)	(0.09)
Risk aversion	-0.16^{*}	0.13	0.03^{*}	-0.01
	(0.09)	(0.12)	(0.02)	(0.02)
Inv. period	-56.50	-76.52^{*}	-0.26	0.42
	(35.05)	(41.48)	(0.45)	(0.79)
Constant	33.47^{***}	0.39	1.73	0.60
	(11.67)	(10.91)	(1.47)	(1.43)
Observations	72	72	360	360

Standard errors in parentheses are clustered on the group level and calculated via bootstrap; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inv. period runs from 1 to 1/10. Risk aversion runs from 1 to 100, with higher numbers indicating less risk aversion. Wage runs from 0 to 100. Effort and desired effort run from 0 to 14. Adjustment runs from -50 to 50. In the no-shock treatments, output corresponds to effort; in the shock treatments, output corresponds to effort plus the shock which runs from -2 to 2. In treatments NRG_{shock} and $NRG_{\text{no-shock}}$, we replace output with average output from the five periods (since that is what the principals observe). Since in these two treatments, the principals only take 2 adjustment decisions (one in period 5, one in period 10) we only have 72 observations in column (1) and column (2).

B Instructions

[[[The experiment was run in Austria; the displayed instructions and screenshots are translated from German.]]]

B.1 Title page *[[[for all treatments the same]]]*

Dear participants,

welcome to today's experiment.

Please read the instructions for the experiment carefully. All statements in the instructions are true, and all participants receive exactly the same instructions. Your earnings in the experiment depend on your decisions and potentially the decisions of others. If you have a question, please raise your hand. Your question will then be answered privately. The experiment as well as the data analysis is anonymous.

We ask you not to talk to other participants and to use only the resources and devices that are provided by the conductors of the experiment. Please switch off all electronic devices. In addition, at the computer you are only allowed to use features that are necessary for the experiment. If you do not comply with these rules, you won't be paid in this experiment and you are not allowed to participate in any further experiments.

For today's experiment, funds are provided by the Austrian Science Fund.

The currency used in the experiment is tokens. Tokens will be converted to Euros at a rate of 10 tokens to 1 Euro. You have already received a €9.00 participation fee. Your earnings from the experiment will be incorporated into your participation fee. At the end of today's experiment, you will be paid privately in cash.

The experiment consists of two parts. In total, the two parts will last for around 75 minutes. The two parts of the experiment are completely independent from each other. That is, your payment for part x only depends on decisions that you take in part x, and does not depend on decisions you take in the other part of the experiment.

At the beginning of each part you receive the corresponding instructions. We will read the instructions out loud and will give you time for questions. For a better understanding, in the following we will only use male designations. Those should be understood gender neutral. Thank you a lot for your attention and for participating in today's experiment.

B.2 Treatment S

[[[These are the instructions for the no-shock treatment. When instructions are adapted to the shock treatment, we mark the respective parts with squared brackets.]]]

PART 1

The role assignment

This part consists of 10 periods. In each period you are anonymously assigned to a group, which consists of two participants: participant A and participant B. At the beginning of the first period you will be randomly assigned either as participant A or participant B. You will remain in the same role throughout part 1 of the experiment. So, if you are assigned as participant B in the first period, then you will stay as participant B throughout the 10 periods of part 1.

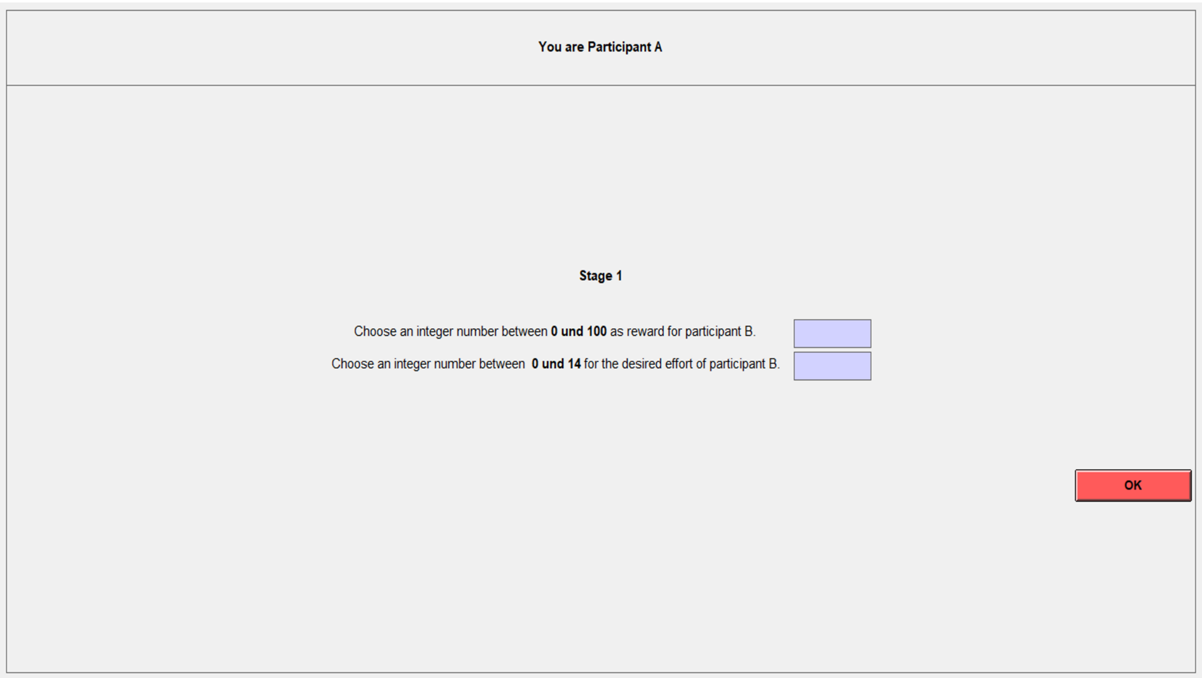
Independently on your role, at the beginning of each period you are randomly assigned another participant in the other role. That is, if you are participant B, for each period you get assigned another participant A.

Each period will proceed in three stages.

Stage 1

In stage 1, participant A will choose a reward (any integer number between 0 and 100) and a desired effort (any integer number between 0 and 14) for participant B.

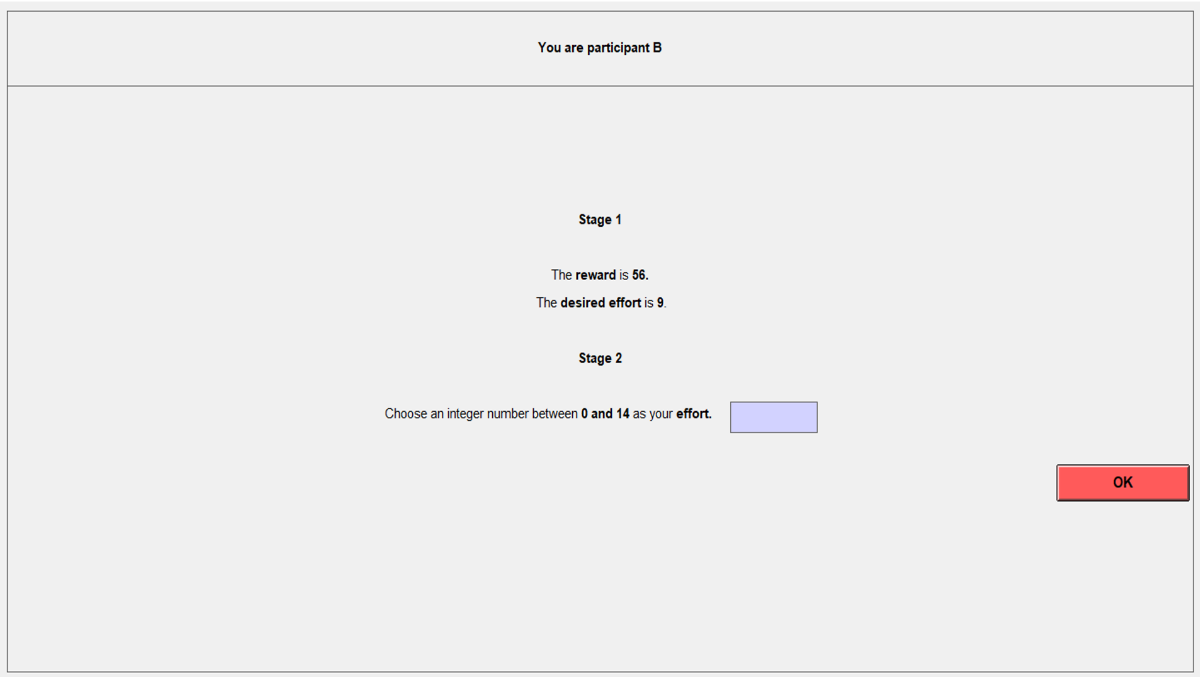
An example of the decision screen in stage 1 for participant A is shown below.



Stage 2

On the screen, participant B is shown the reward and the desired effort chosen by participant A. Then, participant B will choose an effort level (any integer number between 0 and 14).

An example of the stage 2 decision screen for participant B is shown below.



For each effort level chosen by participant B there is an associated cost of effort. The cost of effort can be found in the following table:

Effort	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Cost of effort	0	1	2	5	8	13	18	25	32	41	50	61	72	85	98

Note that as effort rises from 0 to 14, costs rise exponentially.

Stage 3

After participant B chooses the effort level, the performance of participant B is determined as follows: Participant B's performance = effort of participant B. Then the computer will display to participant A the performance of participant B on the screen.

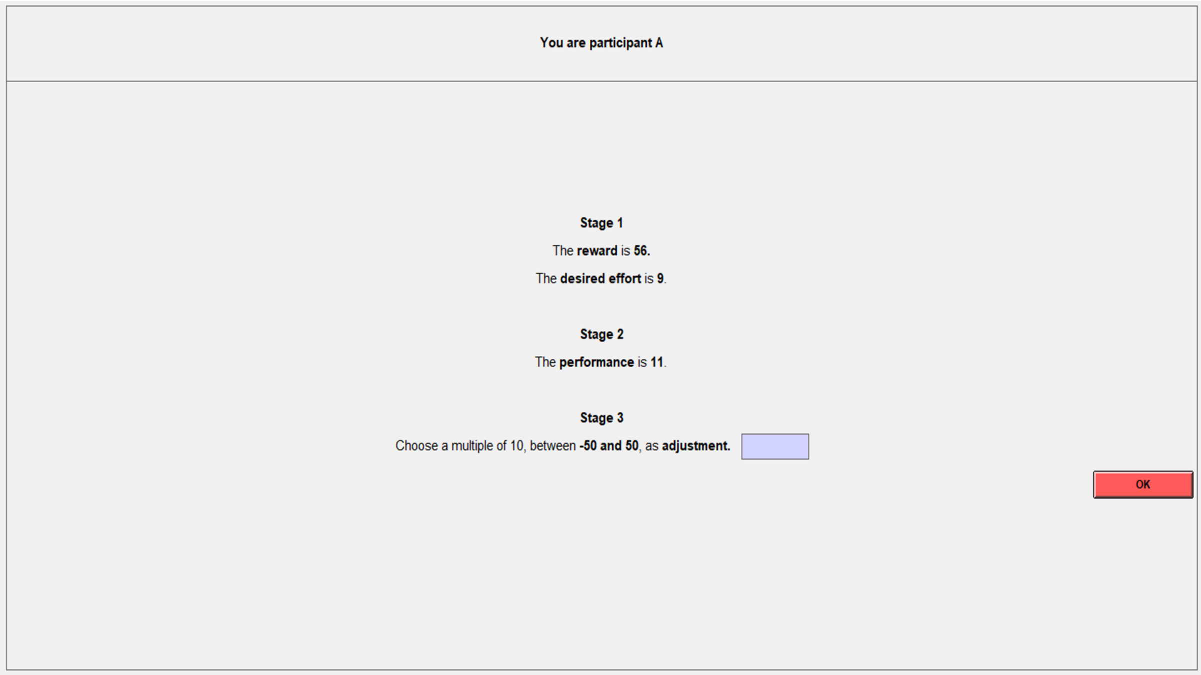
[[[Shock treatment: After participant B chooses the effort level, the computer will add to effort a random number to determine the performance of participant B:

Participant B's performance = effort + random number.

The random number chosen by the computer can take a value of -2, -1, 0, 1, or 2. Each number is equally likely to be drawn. Following the draw of the random number Participants B's performance will be shown to Participant A. Participant A will not know Participant B's actual effort or the random number drawn by the computer.]]]

Then, in the third stage, participant A will choose an adjustment level. The adjustment level must be a multiple of 10, between -50 and 50.

An example of the stage 3 decision screen for participant A is shown on the next picture.



For each adjustment level chosen by participant A there is an associated cost of adjustment. The cost of adjustment can be found in the following table:

Adjustment	-50	-40	-30	-20	-10	0	10	20	30	40	50
Cost of adjustment	5	4	3	2	1	0	1	2	3	4	5

Earnings of participant A

The earnings of participant A depend on the reward chosen by participant A in the first stage, the performance of participant B in the second stage and the adjustment chosen by participant A in the third stage. Specifically, the participant A's earnings are calculated by the following formula:

Participant A's earnings = 10*(performance participant B) – (reward) – (cost of adjustment)
[[[= 10*(effort of part. B+random number) – (reward) – (cost of adj.)]]]

Note that higher participant B's effort implies higher participant B's performance, and thus higher participant A's earnings. On the other hand, a higher reward or a higher cost of adjustment implies lower participant A's earnings.

Earnings of participant B

The earnings of participant B depend on the reward chosen by participant A in the first stage, the cost of the effort chosen by participant B in the second stage and the adjustment chosen by participant A in the third stage. Specifically, participant B's earnings are calculated by the following formula:

Participant B's earnings = (reward) – (cost of effort) + (adjustment)

Note that a higher reward chosen by participant A implies higher participant B's earnings. On the other hand, a higher effort implies higher effort costs and therefore lower participant B's earnings. If participant A chooses a positive adjustment level for participant B then participant B's earnings increase by that adjustment level. However, if participant A chooses a negative adjustment level then participant B's earnings decrease by that adjustment level.

Example 1

Assume the following scenario. In the first stage, participant A chooses a reward of 50 and a desired effort of 7. In the second stage, participant B chooses an effort of 6. [[[Then the computer selects 2 as a random number,]]] so the performance of participant B is 6 [[[8 (6+2)]]]. Then the computer displays to participant A that participant B's performance is 6 [[[8]]]. After observing this information, in the third stage, participant A chooses an adjustment of -40.

Therefore, participant A's earnings = 10*6 − 50 − 4 = 6, since participant B's performance is 6, the reward is 50, and the cost of adjustment of -40 is 4. Finally, participant B's earnings = 50 − 18 − 40 = -8, since the reward is 50, the cost of effort of 6 is 18, and the adjustment is -40.

[[[Therefore, participant A's earnings = 10*8 − 50 − 4 = 26, since participant B's performance is 8, the reward is 50, and the cost of adjustment of -40 is 4. Finally, participant B's earnings = 50 − 18 − 40 = -8, since the reward is 50, the cost of effort of 6 is 18, and the adjustment is -40.]]]

Example 2

Assume the following scenario. In the first stage, participant A chooses a reward of 40 and a desired effort of 6. In the second stage, participant B chooses an effort of 9. [[[Then the computer selects -2 as a random number,]]] so the performance of participant B is 9 [[[7 (9-2)]]]. Then the computer displays to participant A that participant B's performance is 9 [[[7]]]. After observing this information, in the third stage, participant A chooses an adjustment of 30.

Therefore, participant A's earnings = 10*9 − 40 − 3 = 47, since participant B's performance is 9, the reward is 40, and the cost of adjustment of 30 is 3. Finally, participant B's earnings = 40 − 41 + 30 = 29, since the reward is 40, the cost of effort of 9 is 41, and the adjustment is 30.

[[[Therefore, participant A's earnings = 10*7 − 40 − 3 = 27, since participant B's performance is 7, the reward is 40, and the cost of adjustment of 30 is 3. Finally, participant B's earnings = 40 − 41 + 30 = 29, since the reward is 40, the cost of effort of 9 is 41, and the adjustment is 30.]]]

End of the period

At the end of each period, the computer will display to both participants the following information: the reward chosen by participant A, the desired effort chosen by participant A, the performance of participant B, the adjustment chosen by participant A, as well as individual earnings for that period. An example of the outcome screen is shown below.

At the end of each period, the computer will calculate individual earnings.

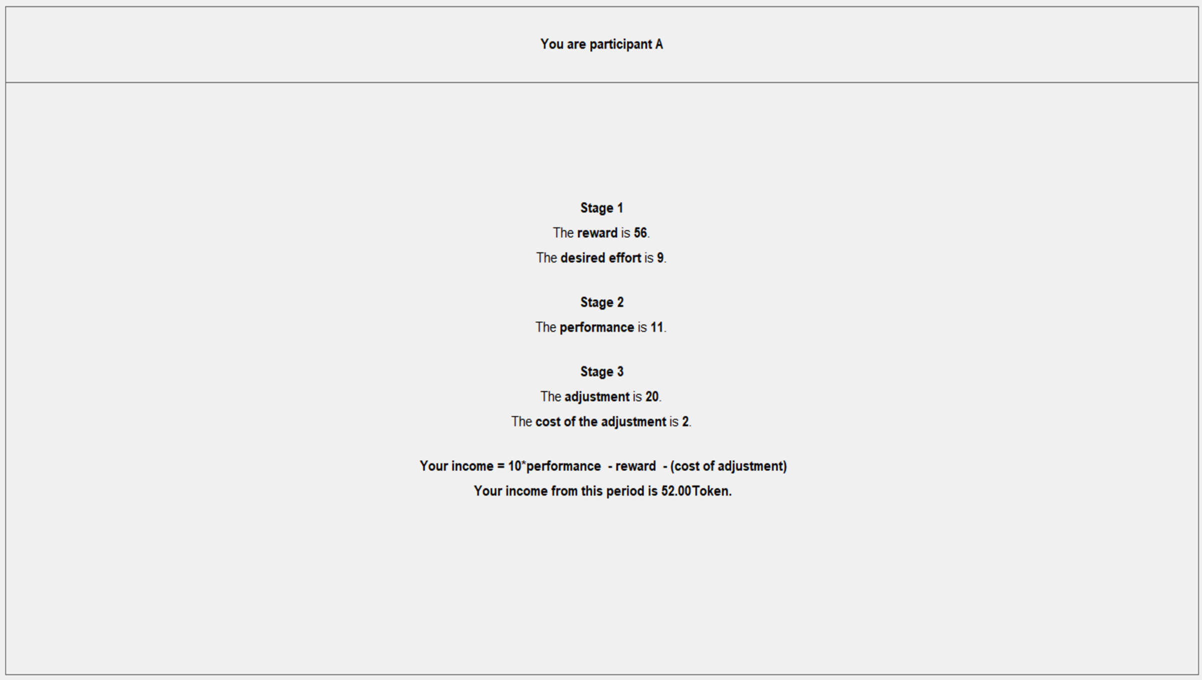
An example is shown on the following picture.

Once your earnings are displayed on the screen, please record your earnings for the period in your personal record sheet under the appropriate heading.

Important notes

Remember you have already received a €9.00 participation fee. In part 1 of the experiment, depending on a period, you may receive either positive or negative earnings. At the end of part 1 we will randomly select 1 out of 10 periods for actual payment and convert the income thereof to a payment in Euros. If the earnings are negative, we will subtract them from your total earnings. If the earnings are positive, we will add them to your total earnings.

Are there any questions?



Control questions *[[[implemented in z-Tree]]]*

Question 1: Assume the following scenario. Participant A chooses a reward of 30 and a desired effort of 8. In the second stage, participant B chooses an effort of 7, so the performance of participant B is 7. Then the computer displays to participant A that participant B's performance is 7. After observing this information, in the third stage, participant A chooses an adjustment of 40. What are participant A's earnings? _______(correct: 10*7-30-4 = 36) What are participant B's earnings? _______ (correct: 30-25+40 = 45)

Question 2: Assume the following scenario. Participant A chooses a reward of 40 and a desired effort of 5. In the second stage, participant B chooses an effort of 1, so the performance of participant B is 1. Then the computer displays to participant A that participant B's performance is 1. After observing this information, in the third stage, participant A chooses an adjustment of -50. What are participant A's earnings? _______(correct: 10*1-40-5 = -35) What are participant B's earnings? _______ (correct: 40-1-50 = -11)

[[[Question 1: Assume the following scenario. Participant A chooses a reward of 30 and a desired effort of 8. In the second stage, participant B chooses an effort of 7. Then the computer selects 1 as a random number, so the performance of participant B is 8 (7 +1). Then the computer displays to participant A that participant B's performance is 8. After observing this information, in the third stage, participant A chooses an adjustment of 40. What are participant A's earnings? _______(correct: 10*8-30-4 = 46) What are participant B's earnings? _______ (correct: 30-25+40 = 45)

Question 2: Assume the following scenario. Participant A chooses a reward of 40 and a desired effort of 5. In the second stage, participant B chooses an effort of 1. Then the computer selects -1 as a random number, so the performance of participant B is 0 (1 -1). Then the computer displays to participant A that participant B's performance is 0. After observing this information, in the third stage, participant A chooses an adjustment of -50. What are participant A's earnings? _______(correct: 10*0-40 -5 = -45) What are participant B's earnings? _______ (correct: 40-1-50 = -11)]]]

PART 2

On your computer screen you will see a square composed of 100 numbered boxes, like shown below.

1	2	3	4	5	6	7	8	9	10
11	12	13	14	15	16	17	18	19	20
21	22	23	24	25	26	27	28	29	30
31	32	33	34	35	36	37	38	39	40
41	42	43	44	45	46	47	48	49	50
51	52	53	54	55	56	57	58	59	60
61	62	63	64	65	66	67	68	69	70
71	72	73	74	75	76	77	78	79	80
81	82	83	84	85	86	87	88	89	90
91	92	93	94	95	96	97	98	99	100

Behind one of these boxes hides a mine; all the other 99 boxes are free from mines. You do not know where this mine lies. You only know that the mine can be in any place with equal probability.

Your task is to decide how many boxes to collect. Boxes will be collected in numerical order. So you will be asked to choose a number between 1 and 100.

At the end of the experiment we will randomly determine the number of the box containing the mine. If you happen to have harvested the box where the mine is located – i.e. if your chosen number is greater than or equal to the drawn number – you will earn zero. If the mine is located in a box that you did not harvest – i.e. if your chosen number is smaller than the drawn number – you will earn in euro an amount equivalent to the number you have chosen.

[[[Extra sheet]]]

Effort	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Cost of effort	0	1	2	5	8	13	18	25	32	41	50	61	72	85	98

Adjustment	-50	-40	-30	-20	-10	0	10	20	30	40	50
Cost of adjustment	5	4	3	2	1	0	1	2	3	4	5

Participant B's performance = effort [[[+ random number]]]

Participant A's earnings = 10*(performance participant B) − (reward) − (cost of adjustment) [[[= 10*(effort of part. B + random number) − (reward) − (cost of adj.)]]]

Participant B's earnings = (reward) − (cost of effort) + (adjustment)

B.3 Treatment D

[[[These are the instructions for the no-shock treatment. When instructions are adapted to the shock treatment, this is adapted accordingly, like in the S shock / no-shock treatments.]]]

PART 1

The role assignment

This part consists of 10 periods. In each period you are anonymously assigned to a group, which consists of two participants: participant A and participant B. At the beginning of the first period you will be randomly assigned either as participant A or participant B. You will remain in the same role throughout part 1 of the experiment. So, if you are assigned as participant B in the first period, then you will stay as participant B throughout the 10 periods of part 1.

Independently on your role, at the beginning the first and at the beginning of the second block you are randomly assigned another participant in the other role. For one block (five periods), respectively, you are assigned the same partner. That is, if you are participant B, for the first five periods you get assigned one participant A; for the following five periods you get assigned another participant A.

[[[Stage 1, Stage 2, and Stage 3 is as in the S treatment.]]]

B.4 Treatment NRG

[[[These are the instructions for the no-shock treatment. When instructions are adapted to the shock treatment, this is adapted accordingly, like in the S shock / no-shock treatments.]]]

PART 1

The role assignment

This part consists of 10 periods. In each period you are anonymously assigned to a group, which consists of two participants: participant A and participant B. At the beginning of the first period you will be randomly assigned either as participant A or participant B. You will remain in the same role throughout part 1 of the experiment. So, if you are assigned as participant B in the first period, then you will stay as participant B throughout the 10 periods of part 1.

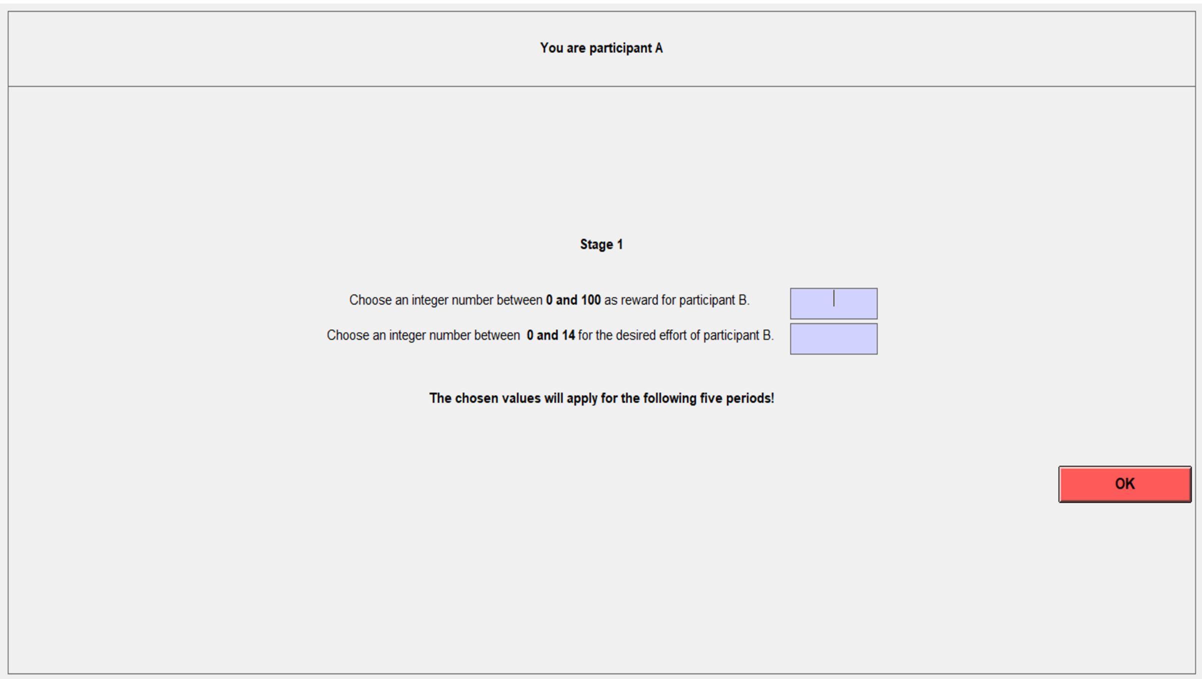
Independently on your role, at the beginning the first and at the beginning of the second block you are randomly assigned another participant in the other role. **For one block (five periods), respectively, you are assigned the same partner.** That is, if you are participant B, for the first five periods you get assigned one participant A; for the following five periods you get assigned another participant A.

Each period will proceed in three stages.

Stage 1

In stage 1, in periods 1 and 6, participant A will choose a reward (any integer number between 0 and 100) and a desired effort (any integer number between 0 and 14) for participant B. **The values chosen in this stage will apply for the following five periods, respectively!**

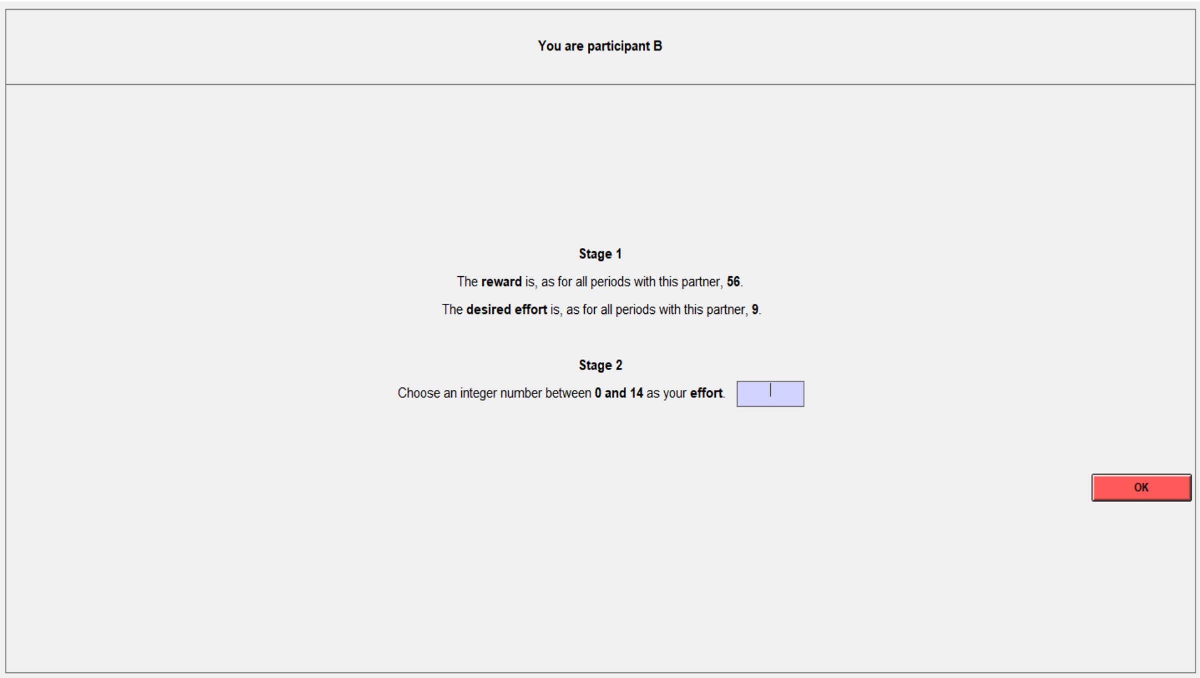
An example of the decision screen in stage 1 for participant A is shown below.



Stage 2

On the screen, participant B is shown the reward and the desired effort chosen by participant A. Then, participant B will choose an effort level (any integer number between 0 and 14). **Participant B chooses an effort level in each of the periods.**

An example of the stage 2 decision screen for participant B is shown below.



For each effort level chosen by participant B there is an associated cost of effort. The cost of effort can be found in the following table:

Effort	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Cost of effort	0	1	2	5	8	13	18	25	32	41	50	61	72	85	98

Note that as effort rises from 0 to 14, costs rise exponentially.

Stage 3

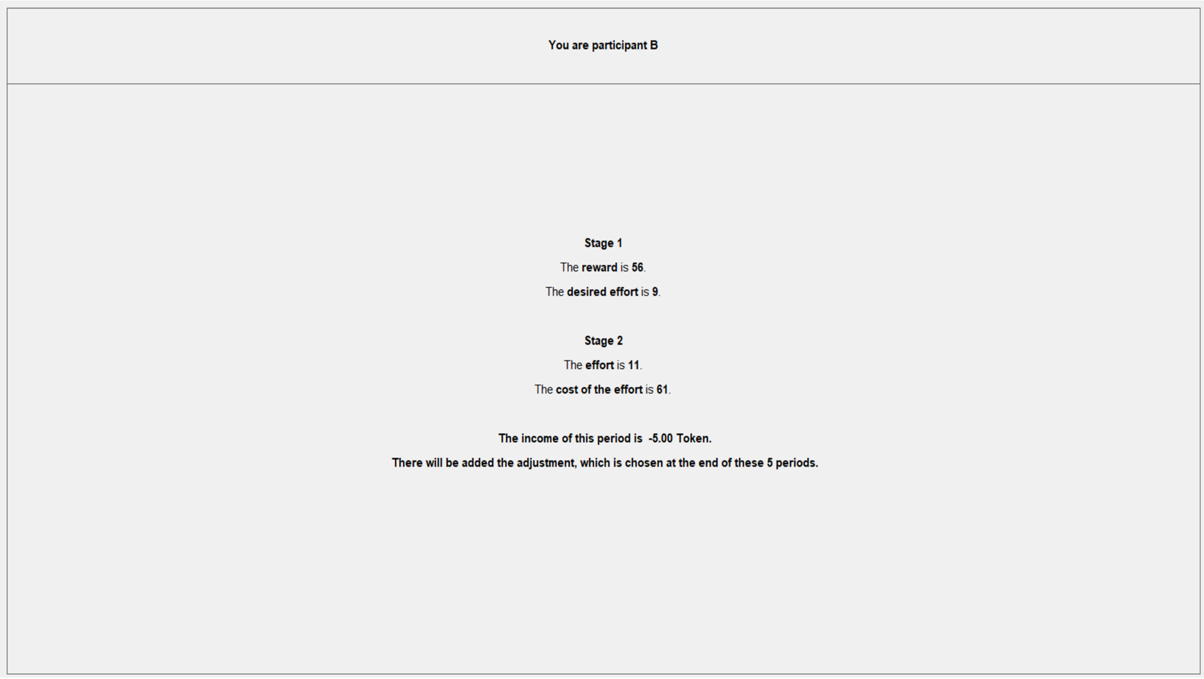
After participant B chooses the effort level, the performance of participant B is determined as follows:

Participant B's performance = effort of participant B.

End of the period

At the end of each period, participant B will be shown the following information on the screen: the reward chosen by participant A, the desired effort chosen by participant A, the performance of participant B, and the earnings for that period – without the "adjustment" (more regarding the adjustment in the next stage). An example for the screen is shown on the following picture.

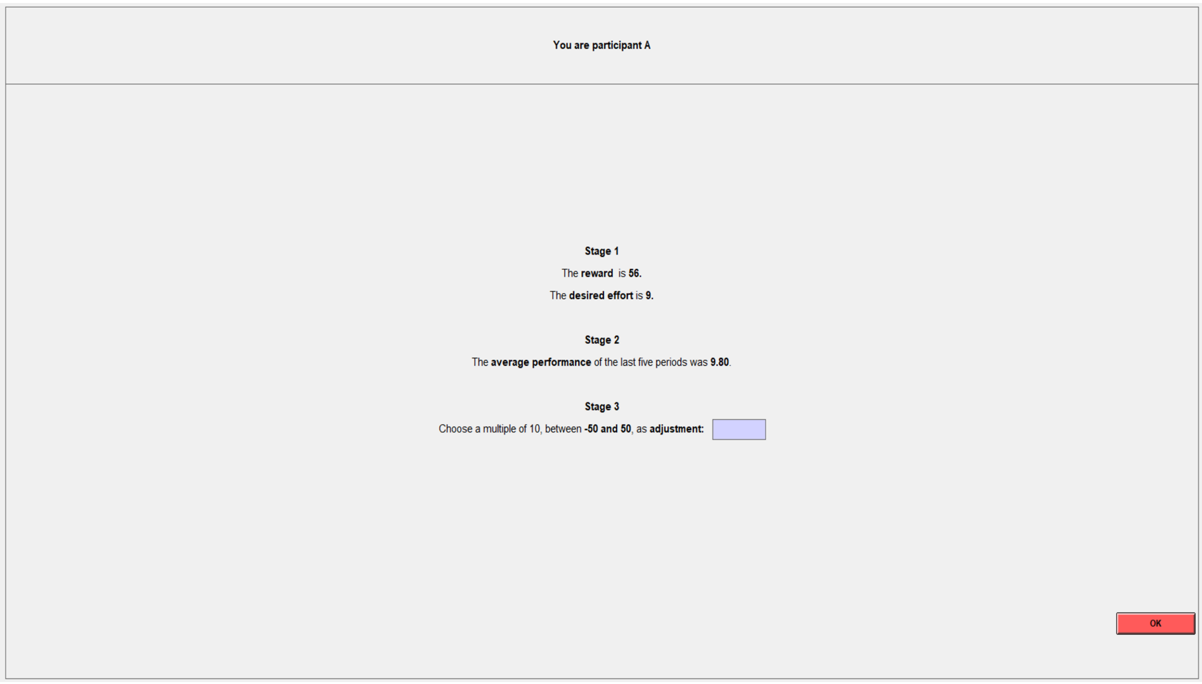
Once your earnings are displayed on the outcome screen as shown below you should record your earnings for the period on your personal record sheet under the appropriate heading.



End of the block: the adjustment

At the end of each block – hence, at the end of periods 5 and 10 – the computer shows participant A **the average performance** of the previous five periods. Then participant A can choose an adjustment level. The adjustment level must be a multiple of 10, between -50 and 50.

An example of the decision screen at the end of the block is shown below. **Participant A chooses an adjustment twice: At the end of period 5, and at the end of period 10.**



For each adjustment level chosen by participant A there is an associated cost of adjustment. The cost of adjustment can be found in the following table:

Adjustment	-50	-40	-30	-20	-10	0	10	20	30	40	50
Cost of adjustment	5	4	3	2	1	0	1	2	3	4	5

Earnings

At the end of the experiment **one of the 10 periods** is randomly chosen for payment. In this period, the reward of participant A, the effort and the resulting performance of participant B, and the adjustment paid at the end of the respective block as well as the arising costs are used to calculate the incomes. In detail, the incomes are composed as follows:

Earnings of participant A in the respective period

The earnings of participant A depends on three factors: on the reward chosen by participant A, the performance of participant B, and the adjustment chosen by participant A:

Participant A's earnings = 10*(performance participant B) – (reward) – (cost of adjustment)

Higher participant B's effort implies higher participant B's performance, and thus higher participant A's earnings. On the other hand, a higher reward or a higher cost of adjustment implies lower participant A's earnings.

Earnings of participant B in the respective period

The earnings of participant B depend on three factors: the reward chosen by participant A, the effort chosen by participant B; and the adjustment chosen by participant A:

Participant B's earnings = (reward) – (cost of effort) + (adjustment)

A higher reward chosen by participant A implies higher participant B's earnings. On the other hand, a higher effort implies higher effort costs and therefore lower participant B's earnings. If participant A chooses a positive adjustment level for participant B then participant B's earnings increase by that adjustment level. However, if participant A chooses a negative adjustment level then participant B's earnings decrease by that adjustment level.

Example 1

Assume the following scenario. At the beginning of the first block, participant A chooses a reward of 50 and a desired effort of 7.

In period 1, participant B chooses an effort of 5, hence the performance is 5.
In period 2, participant B chooses an effort of 7, hence the performance is 7.
In period 3, participant B chooses an effort of 4, hence the performance is 4.
In period 4, participant B chooses an effort of 6, hence the performance is 6.
In period 5, participant B chooses an effort of 8, hence the performance is 8.

After the first block, hence at the end of period 5, the computer displays to participant A that participant B's average performance is 6. Then, participant A chooses an adjustment of -40. The computer chooses period 4 for payment.

The earnings from participant A in this period is: 10*6 – 50 – 4 = 6: participant B's performance is 6, the reward is 50, and the cost of the adjustment is 4.

The earnings from participant B in this period is: 50 – 18 – 40 = -8: the reward is 50, the cost of effort of 6 is 18, and the adjustment is -40.

Example 2

Assume the following scenario. At the beginning of the second block, participant A chooses a reward of 40 and a desired effort of 6.

In period 6, participant B chooses an effort of 9, hence the performance is 9.
In period 7, participant B chooses an effort of 7, hence the performance is 7.
In period 8, participant B chooses an effort of 9, hence the performance is 9.
In period 9, participant B chooses an effort of 11, hence the performance is 11.
In period 10, participant B chooses an effort of 9, hence the performance is 9.

After the second block, hence at the end of period 10, the computer displays to participant A that participant B's average performance is 9. Then, participant A chooses an adjustment of 30. The computer chooses period 8 for payment.

The earnings from participant A in this period is: 10*9 – 40 – 3 = 47: participant B's performance is 9, the reward is 40, and the cost of the adjustment is 3.

The earnings from participant B in this period is: 40 – 41 + 30 = 29: the reward is 40, the cost of effort of 9 is 41, and the adjustment is 30.

Important notes

Remember you have already received a €9.00 participation fee. In part 1 of the experiment, depending on a period, you may receive either positive or negative earnings. The income from the randomly chosen period will be converted in Euros. If the earnings are negative, we will subtract them from your total earnings. If the earnings are positive, we will add them to your total earnings. Are there any questions?

Control questions *[[[implemented in z-Tree]]]*

Question 1: A) Assume that in period 1 you get assigned the role of participant A. Will your role change in period 2? B) Assume in period 1 you get assigned the role of participant B. For how many periods will you be with the same participant A?

Question 2: Assume the following scenario. Participant A chooses a reward of 30 and a desired effort of 8.
In period 1, participant B chooses an effort of 6. Hence the performance is 6. [[[Then the computer selects 1 as a random number, so the performance of participant B is 7 (6 +1).]]]
In period 2, participant B chooses an effort of 7. Hence the performance is 7. [[[Then the computer selects 0 as a random number, so the performance of participant B is 7 (7 +0).]]]
In period 3, participant B chooses an effort of 9. Hence the performance is 9. [[[Then the computer selects -2 as a random number, so the performance of participant B is 7 (9 -2).]]]
In period 4, participant B chooses an effort of 10. Hence the performance is 10. [[[Then the computer selects 1 as a random number, so the performance of participant B is 11 (10 +1).]]]
In period 5, participant B chooses an effort of 8. Hence the performance is 8. [[[Then the computer selects 2 as a random number, so the performance of participant B is 10 (8 +2).]]]

Then the computer displays to participant A that participant B's average performance is. After observing this information, participant A chooses an adjustment of 20.

Assume period 2 is chosen for payment.

What are participant A's earnings? _______(correct: 10*7-30-2 = 38) What are participant B's earnings? _______ (correct: 30-25+20 = 25)

C Declarations

C.1 Funding

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C.2 Conflicts of interest/Competing interests

Both Rudolf Kerschbamer and Regine Oexl have seen and approved the final version of the manuscript being submitted. The article is our original work, it has not received prior publication and it is not under consideration for publication elsewhere. There are no financial or personal conflicts of interests with respect to our research.

C.3 Availability of data and material

Data is available from the authors upon request.

C.4 Code availability

The software code is available from the authors upon request

C.5 Authors' contributions

'Not applicable'

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Rudolf Kerschbamer, Regine Oexl

The effect of random shocks on reciprocal behavior in dynamic principal-agent settings

Abstract
Previous work has shown that unobservable random shocks on output have a detrimental effect on effort provision in short-term ('static') employment relationships. Given the prevalence of long-term ('dynamic') relationships in firms, we investigate whether the impact of shocks is similarly pronounced in gift-exchange relationships where the same principal-agent pair interacts repeatedly. In dynamic relationships, shocks have a significantly less pronounced negative effect on the agent's effort provision than in static relationships. In an attempt to identify the drivers for our results we find that the combination of a repeated-game effect and a noise-canceling effect is required to avoid the detrimental effects of unobservable random shocks on effort provision.

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