Need-Based Transfer Systems are More Vulnerable to Cheating When Resources are Hidden

Scott Claessens¹, Jessica D. Ayers², Lee Cronk³, & Athena Aktipis^{2,4-7*}

Affiliations:

¹Department of Psychology, University of Auckland, New Zealand

²Department of Psychology, Arizona State University, US

³Department of Anthropology, Rutgers University, US

⁴Center for Evolution and Medicine, Arizona State University, US

⁵Arizona Cancer Evolution Center, Arizona State University, US

⁶Biodesign Center for Biocomputation, Security and Society, Arizona State University, US

⁷Interdisciplinary Cooperation Initiative, Arizona State University, US

*Correspondence concerning this article should be addressed to Athena Aktipis

Email: aktipis@asu.edu

Address: 950 S. McAllister Ave, Tempe, AZ 85281-1104

Abstract

Need-based transfer systems function to pool risk. Such arrangements are bound by two simple rules: Ask for help only when in need and, if you are able, give help to others who ask. But there is a temptation for individuals to break these rules for short-term personal profit. Here, we study one factor that may enforce honesty in need-based transfer relationships: the visibility of resources. Across three experiments employing a novel experimental economic game, breaking of both need-based transfer rules increased when resources were hidden rather than visible. Participants with hidden resources were (1) more likely to request help when not actually in need (greediness), and (2) more likely to not fulfill requests from others for help, even when they had sufficient resources available to help (stinginess). These findings highlight the visibility of resources as one potential limitation of cooperative risk pooling systems.

Keywords: Need-based transfers; cooperation; risk pooling; cheating; free-riding

Humans often inhabit unpredictable, volatile environments. In such risky environments, people are vulnerable to such unforeseen events as earthquakes, hurricanes, droughts, and diseases which may drastically reduce or entirely wipe out accumulated resources. Risk pooling (*i.e.*, taking on some of another party's risk in exchange for their willingness to take on some of one's own risk) is one method that humans across the world employ to deal with these ecological challenges (Cronk, Berbesque, *et al.*, 2019).

One example of risk pooling can be found among the Maasai of East Africa. Maasai pastoralists own large herds of cattle, sheep, and goats, but in the volatile ecology of the African savannah these herds may be hit by drought, disease, or theft at any time. *Oсотua*, a Maasai gift-giving system, acts to pool this risk with others. Literally translated as 'umbilical cord,' *osotua* is characterised by transfers of livestock between social partners with no expectation of repayment. Two very simple rules underlie *osotua* stock friendships. Rule 1: ask for livestock only if you are in need. Rule 2: give livestock if you are asked and able to do so without threatening your own survival. Recent agent-based models of these computationally simple rules have shown that they lead to successful risk pooling in volatile environments, increasing average herd survival over and above rules involving debts and repayments (Aktipis, Cronk, & de Aguiar, 2011; Aktipis *et al.*, 2016; Hao, Armbruster, Cronk, & Aktipis, 2015).

Could these principles of 'need-based transfers' be applied outside of the Maasai, to resources other than livestock? For example, other small-scale societies may measure their wealth in cash or small game animals rather than large herds. However, there is a key difference between these different subsistence types. On the one hand, herds of cattle, sheep, and goats are visible to all risk pooling partners. This observability makes both genuine need and an inability to help difficult to fake, and so cheating is discouraged. On the other hand, money and small game are concealable. With these kinds of resources, need and inability to

help become fakeable, and so cheating is likely to increase. If this cheating is frequent enough, the need-based transfer system will fail as a viable risk pooling relationship.

Here, we ask whether the visibility of resources affects the probability of cheating in need-based transfer relationships. Lab and field experiments have previously shown that anonymity increases cheating in public-goods scenarios (Hardy & Van Vugt, 2006; Yoeli, Hoffman, Rand, & Nowak, 2013). This is the first empirical exploration of factors that increase cheating in need-based transfer systems. Cheating takes subtly different forms in public-goods versus need-based transfer systems. In public-good provisioning or traditional social contract scenarios, cheating involves receiving a benefit without paying a required cost (Cosmides & Tooby, 1992). In need-based transfer systems, however, cheating is more nuanced: individuals can break one or both of the underlying rules by (1) asking for help when not in need (*i.e.*, greediness) and/or (2) refusing to give when asked and able (*i.e.*, stinginess). We hypothesise that both kinds of cheating should increase in frequency when resources are concealable.

Cronk *et al.* (2019) ran an experiment in which participants were paired with each other and given resources that grew but were also occasionally hit by disasters. Participants could see their own resources and those of the other player at all times, and they did share resources with each other in patterns consistent with a need-based transfer system. To investigate whether the visibility of resources affects propensity to cheat in need-based transfer systems, we created a simplified version of that experimental economic game to simulate the lives of Maasai pastoralists. In a live interaction, pairs of individuals manage herds of cattle. Their herds grow over time, but are also subject to unpredictable disasters, potentially reducing their herd size below a minimum ‘survival threshold.’ Individuals must request help from their partners to survive. We manipulated whether or not individuals could observe the resource holdings of their partner during the game. We predicted that the probability of

cheating would increase for both need-based transfer rules when resources were hidden as opposed to when they were visible.

Experiment 1

Methods

Participants. We recruited 100 participants on Amazon Mechanical Turk (<http://www.mturk.com>; henceforth MTurk). This sample size was chosen as the largest feasible sample size for interactive online sessions on MTurk. 18 participants dropped out (6 could not be connected to a partner, and 12 abandoned the game due to long wait times), leaving a final sample of 82 participants (41 females). All participants were over 18 years old ($M = 36$ years, $SD = 9$ years, range = 20-62 years) and from the United States. To qualify for the experiment, participants were required to have had at least 50 HITs (Human Intelligence Tasks) approved on MTurk, with a HIT approval rating of 97% and above. Participants did not have prior experience with the game or its purpose prior to participating in the experiment.

Procedure. Participants played an interactive incentivised economic game in real-time with one other participant on MTurk, run using the software oTree (Chen, Schonger, & Wickens, 2016). In this game, players managed a herd of cattle over time. Each round acted as a 'year' of time. Both players began the game with 70 cattle. Each year, their herds grew by a multiplication factor drawn from a statistical distribution based on real data on East African pastoralists (Dahl & Hjort, 1976). However, herds were also subject to random disasters. If a disaster occurred, the player's herd shrank by a multiplication amount drawn from a separate distribution. Disasters could occur for just one player or both players simultaneously. After these births and deaths, players had the opportunity to ask their partners for cattle and respond to their partner's request (see Supplementary Methods for more details).

After any cattle transfers were completed, the year ended. Players were required to keep their herds above the minimum survival threshold of 64 cattle at the end of every year. If their herd size was beneath this threshold, red warning text appeared notifying the player that they had three years to increase their herd size above the threshold or they would ‘die’ and be removed from the game. A player whose partner died could continue playing and earning money but could no longer ask anyone for cattle. The game lasted for 25 rounds, though to avoid endgame effects participants were informed only that the game would last somewhere between 20 and 30 rounds.

On average, participants spent 23 minutes completing the experiment ($SD = 8$ minutes, range = 8-45 minutes). They were paid a show-up fee of USD \$1.00 for participating and could earn bonus payments according to their performance (total number of cattle at the beginning of each year, \$0.01 per 10 cattle). On average, participants earned \$2.81 in total ($SD = \0.69, range = \$1.39-\$4.02).

Design. The experiment consisted of two between-subjects conditions: (a) a control condition, where resources in the game were visible ($n = 42$), and (b) an experimental condition, where resources in the game were hidden ($n = 40$). Participants learned about their condition before being connected to another player. Those in the visible condition were informed that their herd size would be visible to their partner at every stage of the game and, likewise, that they would always be able to see their partner’s herd size. They were also told that the occurrence of disasters would be visible to all players. Conversely, those in the hidden condition were informed that their herd size would never be visible to their partner and that they would never be able to see their partner’s herd size. They were also told that the occurrence of disasters would only be known by the player that experienced them.

Statistical analysis. To determine whether individuals broke the rules of need-based transfers, we compared requesting and giving behaviour between the two conditions. We

fitted Bayesian mixed-effects logistic regressions to the data. The slopes for condition were included as fixed effects, in accordance with our between-subjects design. Intercepts and slopes for the round number were included as random effects, grouped by participants nested within dyads. Estimated parameters are reported on the log odds scale, and 95% CIs are credible intervals for posterior distributions. We also report Bayes factors (BFs) for comparisons of posterior predicted probabilities between conditions, applying conventional cutoffs: below 0.33 (moderate support for the null hypothesis that the probabilities are equal) and above 3 (moderate support for the alternative hypothesis that the probabilities differ; Lee & Wagenmakers, 2014). All statistical analyses were conducted in R v3.6.1 (R Core Team, 2018) using the *brms* package (Bürkner, 2017). Hamiltonian Monte Carlo estimation was run with Stan (Stan Development Team, 2018). Figures were produced with the *ggplot2* package (Wickham, 2009). Data, code, full model fits, details about prior distributions, and MCMC chain diagnostics are accessible at <https://osf.io/wt2mq/>.

Results

Breaking of rule 1: Do not ask for cattle unless you are in need. To determine whether individuals broke the first rule of need-based transfers when resources were hidden as opposed to visible, we estimated the probability of making a request while above the minimum survival threshold (*i.e.*, greediness) in each condition. Logistic regression revealed that the probability of making a request when above the threshold was greater in the hidden condition than the visible condition ($\beta = 1.30$, 95% CI [0.38 2.18]). The model predicts that, when above the minimum threshold, individuals in the visible condition request from their partner with median probability .10. In the hidden condition, this probability increases to .29 (median posterior probability difference = .19, 95% CI [.05 .35], BF = 11.59; Figure 1a and 1b). Participants in the hidden condition were almost three times as likely to request resources when they were not in need compared to participants in the visible condition. We

also found that greediness declined over the rounds ($\beta = -0.11$, 95% CI [-0.19 -0.05]) likely because participants accumulated cattle as the game progressed and so felt less need to cheat.

Breaking of rule 2: Give cattle if you are asked and able. Next, we asked whether individuals broke the second rule of need-based transfers when resources were hidden as opposed to visible. We estimated the probability of not fulfilling a request (*i.e.*, giving less than what was asked) when the participant was able to do so without going beneath the minimum survival threshold themselves (*i.e.*, stinginess). Logistic regression revealed that the probability of not fulfilling a request when able was slightly greater in the hidden condition ($\beta = 1.20$, 95% CI [-0.38 2.75]) and declined over the rounds ($\beta = -0.11$, 95% CI [-0.33 0.06]). The model predicts that, when individuals are asked for cattle and able to give that amount, the median probability of not fulfilling the request is .49 in the visible condition and .76 in the hidden condition (median posterior probability difference = .25, 95% CI [-.08 .54], $BF = 2.80$; Figure 1c and 1d). However, the Bayes factor below three suggests that these model predictions are uncertain. Thus, the model tentatively suggests that participants are stingier in the hidden condition than the visible condition, though more data are needed.

Figure 1. Greediness and stinginess in Experiment 1 (MTurk). (a) Median model-predicted probability of requesting when above the minimum survival threshold (*i.e.*, greediness) with shaded 95% posterior credible intervals, across both conditions. (b) Lines represent the number of players alive in the game. Light bars represent the number of players above the threshold. Dark bars represent the number of players *requesting* when above the threshold. (c) Median model-predicted probability of not fulfilling a request when able to do so without dropping beneath the minimum survival threshold (*i.e.*, stinginess) with shaded 95% posterior credible intervals, across both conditions. (d) Lines represent the number of players alive in the game. Light bars represent the number of players able to fulfill their partner's request without dropping beneath the threshold. Dark bars represent the number of players who *do not fulfill* their partner's request when able. Code to reproduce this plot at <https://osf.io/wt2mq/>.

Discussion

These results suggest that individuals are more likely to break the rules of need-based transfers when resources can be hidden from their partners compared to when resources are visible. Participants in the hidden condition were more likely to request from their partners when they were not in need. The data also tentatively suggest that participants were more likely to not fulfill the requests of their partners (if they could safely do so) in the hidden condition, though the Bayes factor was slightly below three. In other words, participants were greedier and, arguably, stingier in the hidden condition. In order to ensure that these findings

held under more controlled settings, we next attempted to replicate this experiment under laboratory conditions with a within-subjects design.

Experiment 2

Methods

Participants. 80 Arizona State University students (27 female) were recruited to participate in the experiment, all of whom were over 18 years old ($M = 21$ years, $SD = 5$ years, range = 18-55 years). This sample size was deemed appropriate as it was sufficient to estimate the effects of condition in Experiment 1. Participants did not have prior experience with the game or its purpose prior to participating in the experiment.

Procedure and Design. Experiment 2 was largely identical to Experiment 1, with a few important changes. First, participants completed the experiment in the laboratory, rather than online. All participants arrived at a waiting area prior to their scheduled participation time and were asked to refrain from communicating. If there was an odd number of participants, one participant was asked to reschedule. Participants were then seated at computers one-at-a-time and truthfully informed that (1) they would be playing a two-person computer game with a participant in another room, (2) the research assistant did not know who their partner was, and (3) they would not be told who their partner was during the game. Second, unlike Experiment 1, participants played two games in the same session rather than just one. We utilised a within-subjects design, randomly counterbalancing the order of the visible and hidden games. Participants were informed that they would play both games with the same partner. Third, participants were compensated for their time differently. In Experiment 1, MTurk participants were paid directly for their performance in the game. Here, participants received either course credit or a flat \$8 payment (plus a \$4 fee if asked to reschedule). However, to continue incentivising play in the games, we truthfully informed participants that their performance would also determine the number of tickets entered into a

raffle on their behalf for a \$20 gift voucher that they could win after data collection had been completed. On average, participants spent 32 minutes completing the experiment ($SD = 7$ minutes, range = 15-49 minutes).

Statistical analysis. The same Bayesian mixed-effects logistic regressions from Experiment 1 were fitted to the data. However, slopes for condition were included as random effects, grouped by participants nested within dyads, in accordance with our within-subjects design. As before, estimated parameters are reported on the log odds scale, 95% CIs are credible intervals for posterior distributions, and Bayes factors determine whether posterior predicted probabilities are different or equal across conditions. Data, code, full model fits, details about prior distributions, and MCMC chain diagnostics are accessible at <https://osf.io/wt2mq/>.

Results

Breaking of rule 1: Do not ask for cattle unless you are in need. Our first model suggested that the probability of making a request when above the minimum survival threshold (*i.e.*, greediness) declined over the rounds ($\beta = -0.10$, 95% CI [-0.16 -0.05]) but, in contrast to Experiment 1, was *equal* across conditions ($\beta = 0.18$, 95% CI [-0.82 1.10]; median posterior probability difference = .02, 95% CI [-.07 .16], BF = 0.32). However, a follow-up model interacting condition with counterbalancing order revealed a substantial order effect. When participants played the visible game first, the median model-predicted probability of greediness was .11 in the visible condition and .08 in the hidden condition (median posterior probability difference = -.03, 95% CI [-.11 .06], BF = 0.31; Figure 2a) suggesting that the probability of greediness was equal across conditions. By contrast, when participants played the hidden game first, the probability of greediness was .13 in the visible condition and .34 in the hidden condition (median posterior probability difference = .20, 95% CI [.01 .43], BF = 3.05) indicating an increase in greediness in the expected direction. These hidden-first

participants were almost three times as likely to request when above the threshold in the hidden condition compared to the visible condition.

Breaking of rule 2: Give cattle if you are asked and able. Replicating Experiment 1, our first model revealed that the probability of not fulfilling a request when able (*i.e.*, stinginess) declined over the rounds ($\beta = -0.13$, 95% CI [-0.26 -0.04]) and was greater in the hidden condition than the visible condition ($\beta = 0.95$, 95% CI [0.15 1.71]). A follow-up model revealed a similar order effect as for greediness. When participants played the visible game first, the median model-predicted probability of stinginess was .32 in the visible condition and .48 in the hidden condition (median posterior probability difference = .15, 95% CI [-.08 .37], BF = 1.43; Figure 2c) indicating only anecdotal evidence for a difference between conditions. By contrast, when participants played the hidden game first, the probability of stinginess was .42 in the visible condition and .69 in the hidden condition (median posterior probability difference = .26, 95% CI [.03 .46], BF = 7.07) providing strong evidence for an increase in stinginess across conditions. These hidden-first participants were almost twice as likely to not fulfill their partners' requests in the hidden condition compared to the visible condition.

Figure 2. Greediness and stinginess in Experiment 2 (Lab). (a) Median model-predicted probability of requesting when above the minimum survival threshold (*i.e.*, greediness) with shaded 95% posterior credible intervals, across both conditions and counterbalancing orders. (b) Lines represent the number of players alive in the game. Light bars represent the number of players above the threshold. Dark bars represent the number of players *requesting* when above the threshold. (c) Median model-predicted probability of not fulfilling a request when able to do so without dropping beneath the minimum survival threshold (*i.e.*, stinginess) with shaded 95% posterior credible intervals, across both conditions and counterbalancing orders. (d) Lines represent the number of players alive in the game. Light bars represent the number of players able to fulfill their partner's request without dropping beneath the threshold. Dark bars represent the number of players who *do not fulfill* their partner's request when able. Code to reproduce this plot at <https://osf.io/wt2mq/>.

Discussion

The results of this laboratory experiment replicate and extend the findings from Experiment 1. Again, we find that individuals are more likely to break the rules of need-based transfers when resources can be hidden from their partners, compared to when resources are visible. Interestingly, however, this effect only holds when individuals have no prior experience with a partner (*i.e.*, they play the hidden game first). When participants played the visible game first, the probabilities of greediness and stinginess did not increase

when they proceeded to play the hidden game with the same partner, suggesting that earlier rounds acted to build trust between the two interdependent parties and suppress later cheating.

Experiments 1 and 2 have shown that breaking of both need-based transfer rules increases when resources can be hidden. Breaking of rule 1 (*i.e.*, greediness) can readily be interpreted as active cheating for short-term personal profit. However, breaking of rule 2 (*i.e.*, stinginess) is not necessarily evidence of cheating. A hesitancy to fulfill partners' requests may instead be a rational response to the suspicion that the other player is being greedy. Experiments 1 and 2 do not allow us to disentangle these two possible explanations of stingy behaviour: *suspicion* vs. *cheating*. To illuminate the motivation for stingy behaviour, we compared play in a standard control version of the game (in which both players' resources were visible) with an asymmetric version of the game (in which Player 1's resources were visible and Player 2's resources were hidden). Our focus is on Player 2's behaviour. If stinginess is the result of suspicion, then Player 2's stinginess should not increase in the asymmetric game, as Player 1's resources remain visible. Conversely, if stinginess is the result of active cheating, then Player 2's stinginess should increase in the asymmetric game.

Experiment 3

Methods

Participants. We recruited 98 participants on the online platform Prolific (<http://prolific.ac>). This sample size was deemed appropriate as it was sufficient to estimate the effects of condition in Experiment 1. 14 participants dropped out (5 could not be connected to a partner, and 9 abandoned the game due to long wait times), leaving a final sample of 84 participants (51 females). All participants were over 18 years old ($M = 31$ years, $SD = 10$ years, range = 18-65 years) and from the United States. To qualify for the experiment, participants were required to have a minimum 95% approval rate on Prolific.

Participants did not have prior experience with the game or its purpose prior to participating in the experiment.

Procedure and Design. Participants were paid a show-up fee of \$3.25 for participating and could earn bonus payments throughout. On average, participants earned \$4.99 in total ($SD = \0.75, range = \$3.70-\$6.13). Experiment 3 was identical to Experiment 1, except that the visibility of resources was manipulated for Player 2 only in each dyad. Player 1's resources were always visible. In a between-subjects design, this resulted in (a) a control condition where Player 2's resources were visible (number of Player 2s = 20), and (b) an experimental condition where Player 2's resources were hidden (number of Player 2s = 22).

Statistical analysis. As before, Bayesian mixed-effects logistic regressions were fitted to the data. Because Player 1's resources were always visible and we manipulated whether Player 2's resources were visible or not, we analysed data for Player 2s only. Estimated parameters are reported on the log odds scale, 95% CIs are credible intervals for posterior distributions, and Bayes factors determine whether posterior predicted probabilities are different or equal across conditions. Data, code, full model fits, details about prior distributions, and MCMC chain diagnostics are accessible at <https://osf.io/wt2mq/>.

Results

We asked whether Player 2s were stingier when their resources were hidden as opposed to visible, even when Player 1's resources remained visible. Replicating the previous experiments, logistic regression revealed that the probability of not fulfilling a request when able (*i.e.*, stinginess) was slightly greater in the hidden condition ($\beta = 0.85$, 95% CI [-0.45 2.00]). The model predicts that, when Player 2s are asked for cattle and able to give that amount, the median probability of not fulfilling the request is .37 in the visible condition and .58 in the hidden condition (median posterior probability difference = .20, 95% CI [-.10 .45], $BF = 2.06$; Figure 4a and 4b). The smaller sample size from focusing on half the sample

(Player 2s only) results in more uncertain model predictions and thus a reduced Bayes factor. Nevertheless, the effect is in the expected direction, with Player 2s in the hidden condition being almost twice as likely not to fulfill requests when asked.

Figure 3. Stinginess in Experiment 3 (Prolific). (a) Median model-predicted probability of not fulfilling a request when able to do so without dropping beneath the minimum survival threshold (*i.e.*, stinginess) with shaded 95% posterior credible intervals. (b) Lines represent the number of players alive in the game. Light bars represent the number of players able to fulfill their partner's request without dropping beneath the threshold. Dark bars represent the number of players who *do not fulfill* their partner's request when able. Code to reproduce this plot at <https://osf.io/wt2mq/>.

Discussion

In this final experiment, Player 1's resources remained visible while the visibility of Player 2's resources was manipulated. We found that there was an increase in the probability of stinginess among Player 2s when their resources were hidden, even when Player 1's resources remained visible. Although uncertain due to the small sample size in our restricted analyses, this effect size is comparable to that found in previous experiments. This suggests that simply manipulating the visibility of one's resource holdings, even when others' are visible, is enough to induce stingy behaviour. In other words, this experiment tentatively supports the *cheating* hypothesis of stingy behaviour over the *suspicion* hypothesis.

General Discussion

In volatile environments, need-based transfer systems such as osotua allow individuals to pool risk with others (Aktipis *et al.*, 2011, 2016; Hao *et al.*, 2015). Two simple rules underlie such relationships: (1) ask only when in need and (2) give when asked and able. But, as with any social dilemma, the temptation to break these rules and exploit the generosity of others is ever present. Here, we found that people were more likely to break need-based transfer rules when resources were hidden as opposed to visible. When others could not evaluate their resource holdings, individuals tended to be greedier (*i.e.*, more likely to ask for help even when they were not in need) and stingier (*i.e.*, more likely to not fulfill requests from their partner even if they could do so without going under the survival threshold themselves). Experiment 3 provided initial evidence that stingy behaviour is due to active cheating and not simply suspicion of one's partner, but more data will be needed to confirm this.

These results may explain the collapse of real-world risk pooling systems when resources can be concealed. Consider, for example, demand-sharing, a cooperative risk pooling system among foragers in which individuals are forced via social pressure to distribute any surplus food from hunts to other camp members (Peterson, 1993). While individuals could free-ride on demand-sharing arrangements by refusing to give and hiding resources, such cheating is made difficult by the visibility of large game. However, increased monetisation in egalitarian societies has often led to the collapse of such demand-sharing systems. For example, after traders began buying meat from Mbuti hunter-gatherers, the amount of meat shared within camps decreased (Ichikawa, 1991). Similarly, the introduction of cash and commoditization into Baka communities in Southeastern Cameroon encouraged market economy exchange and money hoarding, resulting in declines in demand-sharing of

meat (Kitanishi, 2006; Townsend, 2015). Thus, when resources become easily concealed, as monetisation allows, cooperative risk pooling arrangements can be difficult to maintain.

We expected that the same effect would occur in our economic game: hidden resources would result in the complete collapse of the need-based transfer system. However, while the probability of cheating tended to increase when resources could be hidden, individuals in the hidden condition still continued to give at least some cattle to their partners, and we did not find consistent links between the visibility of resources and survival rate (see Supplementary Results). Why didn't the cooperative system collapse entirely when resources could be hidden? This is likely because individuals were still aware that they were interdependent with their only risk pooling partner (Aktipis *et al.*, 2018; Roberts, 2005) and that some cattle transfers must still occur if they were to survive the game. Thus, at least in dyads, there is a conflict between breaking the rules to earn the most short-term profit and ensuring that one's partner survives to keep them available as a long-term risk pooling partner. However, this tension may be lessened, and the probability of cheating increased, if individuals have more outside options (*i.e.*, group size > 2), as the target of any cheating is just one of many interdependent risk pooling partners. Future research could test this prediction by extending our methodology to larger groups of players.

Despite this temptation to cheat, several mechanisms may stabilise need-based transfer systems with easily concealable resources. First, as highlighted by Experiment 2, repeated interactions can develop trust between individuals, reducing the temptation to cheat later. Such repeated bond-based transfers, as seen in gifts between Ju/'hoansi *hxaro* partners (Wiessner, 1982), establish and maintain relationships between risk pooling partners. Second, the system may be imbued with cultural significance, such as sacredness (Cronk & Aktipis, 2018). Consider, for example, the Ik of Uganda. As small-scale farmers, hunters, and gatherers, what few resources the Ik possess are much easier to hide than the livestock of the

346 Maasai. Nevertheless, they do have a strong ethic of sharing with those in need. This is
347 supported by an Ik belief that the landscape is inhabited by earth spirits known as *kijawikå*.
348 The Ik believe that the *kijawikå* bring misfortune to people who fail to share with others and
349 reward those who are generous. Resources may be hidden to people, but the *kijawikå* can be
350 counted on to see them no matter where they are (Cronk, Berbesque, *et al.*, 2019).

351 In conclusion, we have shown across several experiments that if resources are hidden,
352 individuals are greedier and stingier in economic games modelling risk pooling scenarios.
353 Thus, while *osotua* is an effective means of pooling the risks associated with Maasai
354 livestock herding, similar need-based transfer systems may be less viable in societies in
355 which resources are more easily concealed.

Ethics Statement

Ethical approval was granted by Arizona State University Institutional Review Board for Experiment 1 (ref: STUDY00007432), Experiment 2 (ref: MOD00008507), and Experiment 3 (ref: STUDY00007432).

Acknowledgements

Aktipis and Cronk's participation was made possible by a grant from the John Templeton Foundation. Any opinions, findings, conclusions, or recommendations expressed in this article are those of the authors and do not necessarily reflect the views of the John Templeton Foundation.

Author Contributions

S. Claessens, L. Cronk, and A. Aktipis conceived of and designed the experimental economic game. J. D. Ayers collected data in the laboratory at Arizona State University and helped set

up data collection on Prolific. S. Claessens wrote the code for the game, collected data on Amazon Mechanical Turk and Prolific, conducted all statistical analyses, and wrote the manuscript. All authors approved the final version of the manuscript for submission.

Open Practices Statement

The experiments reported in this manuscript were not formally pre-registered. Open data, analysis code, and materials can be found on our Open Science Framework repository:

<https://osf.io/wt2mq/>.

References

- Aktipis, A., Cronk, L., Alcock, J., Ayers, J. D., Baciú, C., Balliet, D., ... Winfrey, P. (2018). Understanding cooperation through fitness interdependence. *Nature Human Behaviour*, 2, 429–431.
- Aktipis, A., Cronk, L., & de Aguiar, R. (2011). Risk-pooling and herd survival: An agent-based model of a Maasai gift-giving system. *Human Ecology*, 39, 131–140.
- Aktipis, A., de Aguiar, R., Flaherty, A., Iyer, P., Sonkoi, D., & Cronk, L. (2016). Cooperation in an uncertain world: For the Maasai of East Africa, need-based transfers outperform account-keeping in volatile environments. *Human Ecology*, 44, 353–364.
- Bürkner, P.-C. (2017). brms: An R package for Bayesian multilevel models using Stan. *Journal of Statistical Software*, 80, 1–28.
- Chen, D. L., Schonger, M., & Wickens, C. (2016). oTree-An open-source platform for laboratory, online, and field experiments. *Journal of Behavioral and Experimental Finance*, 9, 88–97.
- Cosmides, L., & Tooby, J. (1992). Cognitive adaptations for social exchange. In J. Barkow, L. Cosmides, & J. Tooby (Eds.), *The Adapted Mind: Evolutionary Psychology and the*

- Generation of Culture* (pp. 163–228). New York: Oxford University Press.
- Cronk, L., & Aktipis, A. (2018). Sacredness as an implied threat of supernatural punishment: The case of need-based transfers. *Religion, Brain & Behavior*, 8, 282–285.
- Cronk, L., Aktipis, A., Gazzillo, S., White, D., Wutich, A., & Sopher, B. (2019). Common knowledge promotes risk pooling in an experimental economic game. *PLoS ONE*, 14, e0220682.
- Cronk, L., Berbesque, J. C., Conte, T., Gervais, M., Iyer, P., McCarthy, D., ... Aktipis, A. (2019). Managing risk through cooperation: Need-based transfers and risk pooling among the societies of the Human Generosity Project. In L. R. Lozny & T. H. McGovern (Eds.), *Global Perspectives on Long Term Community Resource Management*. Springer.
- Dahl, G., & Hjort, A. (1976). *Having Herds: Pastoral Herd Growth and Household Economy*. Department of Social Anthropology, University of Stockholm.
- Hao, Y., Armbruster, D., Cronk, L., & Aktipis, A. (2015). Need-based transfers on a network: A model of risk-pooling in ecologically volatile environments. *Evolution and Human Behavior*, 36, 265–273.
- Hardy, C. L., & Van Vugt, M. (2006). Nice guys finish first: The competitive altruism hypothesis. *Personality and Social Psychology Bulletin*, 32, 1402–1413.
- Ichikawa, M. (1991). The impact of commoditization on the Mbuti of Eastern Zaire. In N. Peterson & T. Matsuyana (Eds.), *Cash, Commoditisation and Changing Foragers, Senri Ethnological Studies* (pp. 135–162).
- Kitanishi, K. (2006). The impact of cash and commoditization on the Baka hunter-gatherer society in Southeastern Cameroon. *African Study Monographs*, 33, 121–142.
- Lee, M. D., & Wagenmakers, E. J. (2014). *Bayesian Cognitive Modeling: A Practical Course*. New York: Cambridge University Press.

- Peterson, N. (1993). Demand sharing: Reciprocity and the pressure for generosity among foragers. *American Anthropologist*, 95, 860–874.
- R Core Team. (2018). *R: A Language and Environment for Statistical Computing*. Retrieved from <https://www.r-project.org/>
- Roberts, G. (2005). Cooperation through interdependence. *Animal Behaviour*, 70, 901–908.
- Stan Development Team. (2018). *RStan: the R interface to Stan*. Retrieved from <http://mc-stan.org/>
- Townsend, C. (2015). *The Emergence of Inequality in a Former Hunter-Gatherer Society: A Baka Case Study*. Doctoral Dissertation, University College London.
- Wickham, H. (2009). *ggplot2: Elegant Graphics for Data Analysis*. Springer-Verlag New York.
- Wiessner, P. (1982). Risk, reciprocity and social influences on !Kung San economics. In E. Leacock & R. Lee (Eds.), *Politics and History in Band Societies* (pp. 61–84). New York: Cambridge University Press.
- Yoeli, E., Hoffman, M., Rand, D. G., & Nowak, M. A. (2013). Powering up with indirect reciprocity in a large-scale field experiment. *Proceedings of the National Academy of Sciences of the United States of America*, 110, 10424–10429.

Supplementary Materials

“Need-Based Transfer Systems are More Vulnerable to

Cheating When Resources are Hidden”

(Claessens *et al.*)

Supplementary Methods

Additional Information Regarding the Economic Game

Before the game, participants first read a consent form, provided some basic demographic information (age, gender, and primary language), and read some instructions about the game that included information about their payment. After reading about the experimental manipulation, participants completed an example round of the game with the computer and answered several comprehension questions about the game to test their understanding. Participants were paid \$0.02 for each comprehension question they correctly answered, but they had unlimited tries to get these questions right. For online experiments, participants were also paid for waiting to be connected to a partner (\$0.05 per minute, maximum 10 minutes waiting time).

During the experiment, herd sizes increased by a multiplication amount drawn independently from a Gaussian distribution ($M = 0.034$, $SD = 0.0253$). The probability of a disaster occurring in any given year was 20%. If a disaster occurred, the player's herd decreased by a multiplication amount drawn independently from a separate Gaussian distribution ($M = 0.15$, $SD = 0.05$). Gaussian distribution parameters for these 'births and deaths' replicated those used in previous modelling work (Aktipis, Cronk, & de Aguiar, 2011), which were themselves drawn from real data on East African pastoralists (Dahl & Hjort, 1976).

When requesting help and responding to requests, if participants stated that they would like to ask for help, they then specified how many cattle they wanted to ask their partner for (any positive number). Their partner then saw the request and could transfer any cattle amount they wanted to (anything from nothing to their current herd size).

If a participant's partner dropped out during the game (*i.e.*, stopped responding), the game could not proceed, but participants were given the option to quit the game and skip to the end if they had to wait too long.

To uncover the reasons for making certain decisions in the game, after the game participants were asked a series of questions about fairness and empathy for their partner. There was also an open-ended question where participants could say why they gave cattle, if they ever gave. This was followed by some brief feedback questions about gameplay. None of these post-game questions were compulsory, but participants were paid an extra \$0.01 for each question answered.

Supplementary Results

Game Control Checks

In each experiment, we checked whether stochastic aspects of gameplay (*i.e.*, number of disasters, total number of cattle lost due to disasters) differed across conditions to ensure that these variables did not confound the results. For these models, random effects were not included (for more details, see <https://osf.io/wt2mq/>).

In Experiment 1, logistic regression revealed that disasters were very slightly less likely to occur in the hidden condition than the visible condition ($\beta = -0.27$, 95% CI [-0.51 -0.02]; median posterior probability difference = -.04, 95% CI [-.08 .00]). Gaussian regression indicated no difference between the total number of cattle lost due to disasters in the two conditions ($\beta = 0.22$, 95% CI [-5.50 6.07]).

In Experiment 2, logistic regression revealed that the probability of disasters did not differ between conditions ($\beta = -0.03$, 95% CI [-0.21 0.14]). However, Gaussian regression revealed that slightly fewer cattle were lost in the hidden condition, on average ($\beta = -6.70$, 95% CI [-11.98 -1.30]). This average difference of 7 cattle was very small, and so we did not feel the need to control for it directly in our modelling.

In Experiment 3, logistic regression revealed that the probability of disasters did not differ between conditions ($\beta = 0.00$, 95% CI [-0.26 0.25]). Gaussian regression indicated no difference between the total number of cattle lost due to disasters in the two conditions ($\beta = -3.05$, 95% CI [-9.25 3.44]).

Survival Analyses

In Experiments 1 and 2, we also conducted survival analyses to determine if the visibility of resources or the frequency of cheating were associated with differences in survival rate. We expected that when resources are hidden as opposed to visible, individuals should survive fewer rounds. We also predicted that survival rate should decrease as individuals cheat more in the games. To test these predictions, we used survival analysis, a regression method which estimates survival rate while accounting for the right-censored nature of the data (*i.e.*, right-censored participants survived all 25 rounds, the maximum amount, but may have died if the game had continued). We used the Weibull family in the *brms* package (Bürkner, 2017) and included random intercepts and slopes grouped by participant and/or dyad where necessary. Model parameters are reported on the log scale, and Bayes factors are for the hypothesis that these parameters differ from zero (for more details, see <https://osf.io/wt2mq/>).

In Experiment 1, the survival analysis predicted that, if participants were allowed to play for more than 25 rounds, those in the Visible condition would have survived 28 rounds (median) while those in the Hidden condition would have survived 50 rounds ($\beta = 0.57$, 95% CI [-0.28 1.48], BF = 0.47; median posterior difference = 17.86 rounds, 95% CIs [-9.87 142.14]). This is the opposite direction as expected, with individuals surviving for longer when resources can be hidden, although the 95% credible intervals cross zero and the Bayes factor provides anecdotal evidence for *equal* survival rates across conditions. With regards to cheating, we ran two separate models with two separate predictors: (1) proportion of rule 1

breaking (out of all possible rule 1 breaking occurrences), and (2) proportion of rule 2 breaking (out of all possible rule 2 breaking occurrences). We found that as individuals broke rule 1 more, their survival rate decreased slightly ($\beta = -0.87$, 95% CIs [-1.90 0.11], BF = 1.22) but 95% credible intervals cross zero and the Bayes factor suggests only anecdotal evidence for a difference. We also found that as individuals broke rule 2 more, their survival rate increased slightly ($\beta = 0.49$, 95% CIs [-0.62 1.65], BF = 0.40) but 95% credible intervals cross zero and the Bayes factor suggests anecdotal evidence for no difference in survival rate. In sum, Experiment 1 does not reveal clear effects of the visibility of resources or cheating on the rate of survival.

Turning to Experiment 2, we find that participants in the Visible condition are predicted to survive 68 rounds (median) while participants in the Hidden condition are predicted to survive 30 rounds ($\beta = -0.82$, 95% CI [-1.45 -0.29], BF = 14.67; median posterior difference = -34.22 rounds, 95% CIs [-282.52 -1.16]). This supports our predictions: survival rate is lower in the Hidden condition, with 95% credible intervals below zero and strong support for the hypothesis that the survival rates differ. However, this difference in survival rate between conditions does not clearly map onto differences in cheating: we do not find evidence that proportion of rule 1 breaking ($\beta = 0.50$, 95% CI [-0.56 1.71], BF = 0.41) or proportion of rule 2 breaking ($\beta = -0.17$, 95% CI [-0.97 0.66], BF = 0.20) are associated with a difference in survival rate.

Supplementary Discussion

Experiment 1: Limitations of Online Interactive Games

Online experiments are increasingly being used to study economic decision making, as they are inexpensive, quick to run, and easy to implement. However, online interactive games come with their own set of methodological issues (Arechar, Gächter, & Molleman, 2018). Dropouts are common in online studies, and even more likely in interactive games as

participants must wait for others to continue. Selective attrition can systematically vary between experimental conditions, inflating Type I error rates (Zhou & Fishbach, 2016). We mitigated these problems in Experiment 1 in several ways: (1) after the introduction screens, participants entered a waiting lobby and were quickly paired to the next available participant, (2) participants knew that they were playing with another real player, who was waiting for them, and (3) participants could leave the game if they had to wait too long for their partner. With these measures in place, we observed a dropout rate of only 18% – less than the 30-50% rate cited by others (Zhou & Fishbach, 2016) – and dropouts were balanced across conditions. Nevertheless, these measures introduced new problems. Quick, enthusiastic players were likely paired in the waiting lobby first, biasing the formation of dyads. Furthermore, participants were unable to be randomised to conditions: two distinct MTurk sessions were required to ensure that participants were paired with others in the same condition. Experiment 2 aimed to mitigate these shortcomings by replicating the findings from Experiment 1 in the laboratory.

References

- Aktipis, A., Cronk, L., & de Aguiar, R. (2011). Risk-pooling and herd survival: An agent-based model of a Maasai gift-giving system. *Human Ecology*, 39, 131–140.
- Arechar, A. A., Gächter, S., & Molleman, L. (2018). Conducting interactive experiments online. *Experimental Economics*, 21, 99–131.
- Bürkner, P.-C. (2017). brms: An R package for Bayesian multilevel models using Stan. *Journal of Statistical Software*, 80, 1–28.
- Dahl, G., & Hjort, A. (1976). *Having Herds: Pastoral Herd Growth and Household Economy*. Department of Social Anthropology, University of Stockholm.
- Zhou, H., & Fishbach, A. (2016). The pitfall of experimenting on the Web: How unattended

selective attrition leads to surprising (yet false) research conclusions. *Journal of Personality and Social Psychology*, 111, 493–504.