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Hot–cold reciprocation gaps

Riccardo Ghidoni[1,2], Sigrid Suetens[3] and Jierui Yang[3,4]

[1]Department of Economics, University of Bologna, Bologna, Italy
[2]IZA@LISER Network, Luxembourg
[3]Department of Economics, CentER, Tilburg University, Tilburg, Netherlands
[4]Business School, Xiangtan University, Xiangtan, China
Corresponding author: Jierui Yang; Email: jierui.yang3@xtu.edu.cn

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Abstract

We study whether and why the mode of decision-making affects reciprocation in a binary trust game, using three preregistered online experiments. Trustees – some of whom were exogenously induced to be surprised by trust – either decided whether to reciprocate after observing their partner's choice (*hot* mode) or committed in advance to a contingent response plan (*cold* mode). In our *Baseline* experiment, once participants gained experience, reciprocation became more likely under *hot* than under *cold* decision-making, especially when trust was unlikely – consistent with a model in which positive surprise at being trusted increases the utility of reciprocating. Results from the other two experiments show that surprise promotes reciprocation only when trust reflects an intentional choice by the trustor and when cognitive load in the decision environment is not too high.

Keywords: contingent decision mode; direct decision mode; experiment; psychological game; reciprocity; strategy method

JEL Codes: C70; C90; D70

1. Introduction

Understanding when and why individuals reciprocate trust is central to analyses of cooperation in many economic and social interactions.[1] This article investigates whether the mode of decision-making matters. In the case of a direct mode, individuals know they are trusted (or not) before effectively deciding to reciprocate trust (or not). In contrast, with contingent decision-making, the reciprocation decision is pre-planned; it is made before observing whether they have been trusted. Although many environments allow individuals to respond directly, others, such as interactions mediated by intelligent machines, naturally involve programmed, contingent decision-making. Moreover, some individuals may be characterized by a stronger tendency than others to behave according to an internally planned, contingent strategy such as reciprocation to trust. Do these different decision modes lead to differences in reciprocation? In other words, is there a hot–cold reciprocation gap, and what mechanism can generate it?

Direct and contingent decision modes differ in the scope for an emotional response (e.g. Loewenstein, 2000). Under the direct mode, individuals who are pessimistic about trust are positively

[1]See Akerlof (1982), Brown et al. (2004), Fehr et al. (1993), Greif (1993), Kehoe and Levine (1993), Kreps (1990), and Thomas and Worrall (1990) for examples in the contexts of trade, employer–employee relationships, and lending.

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surprised when someone actually trusts them, which may prompt them to reciprocate.[2] Such a response cannot arise under the contingent mode for the simple reason that individuals do not know whether they are being trusted when deciding on a plan of action.[3] Yet it is not obvious that such an effect appears in practice. For someone who feels it is a duty to reciprocate, the temptation to breach trust looms more strongly with direct than with contingent decision-making, thus requiring a stronger feeling of duty or more self-control to overcome the temptation in the former than in the latter case (Chen & Schonger, 2024).

We report the results of online experiments designed to investigate the effect of the decision mode on reciprocation in a binary trust game and to shed light on the underlying mechanism. Our *Baseline* experiment ($N = 395$) used a setting in which a first mover chose whether or not to trust a second mover, who then decided whether to reciprocate or defect conditional on being trusted. The design and hypothesis are motivated by a utility function that incorporates an emotional–psychological component capturing the potential for positive surprise induced by being trusted (see also Geanakoplos et al., 1989; Ruffle, 1999).[4] The experiment included treatments with direct and contingent elicitation of reciprocation. In the direct (*Hot*) treatments, second movers chose whether to reciprocate *after* observing the first mover's choice and could therefore react emotionally to positive surprise. In the contingent (*Cold*) treatments, second movers specified a response plan *before* knowing whether they had been trusted, thereby removing any role for surprise in influencing reciprocation.

To induce exogenous variation in the extent of positive surprise within the *Hot* treatment, we varied the aggregate trust rate across treatments. First movers were divided into two groups: one with a trust rate of 69% and one with a trust rate of 28%. Second movers were matched exclusively with first movers from either the high- or the low-trust group and were informed about the trust rate of the group they were paired with. We conjectured that trust would generate stronger positive surprise in the low-trust condition than in the high-trust condition, in line with the idea that unexpectedness increases the intensity of surprise.[5] A manipulation check confirmed this conjecture. Our main hypothesis, derived from the model of positive surprise, was that the hot–cold reciprocation gap should be larger when positive surprise is more likely, namely in the low-trust condition of the *Hot* treatment.

We find no difference in reciprocation between the *Hot* and *Cold* treatments in the first round, but behavior diverged thereafter. While the hot reciprocation rate remained largely stable across rounds, the cold reciprocation rate declined over time, ultimately leading to a 10.3 percentage–point hot–cold gap in the final rounds ($P = 0.037$).[6] Consistent with the surprise hypothesis, the divergent pattern was more pronounced in the low-trust condition – where second movers were more likely to be surprised by trust – than in the high-trust condition.

We implemented an additional experiment to examine the role of intentions, which play a central role in models of reciprocity (Dufwenberg & Kirchsteiger, 2004). Following Cox (2004), we removed the intentional nature of trust by programming the first movers' choices. This allowed us to test

[2]More precisely, in our setting surprise is defined relative to the second mover's prior belief about being trusted: a lower prior increases the intensity of the surprise experienced upon observing trust.

[3]Individuals may also experience emotions based on expectations about their partner's behavior, but because surprise is inherently tied to unexpected realized outcomes, these emotions are not equivalent to surprise induced by the actual choices of others. Surprise may lead individuals to update their cognitive representations of others and adjust their actions accordingly (Reisenzein et al., 2019).

[4]Our approach is equivalent to that of Aina et al. (2020), who allow for anger in an ultimatum game. It differs from Khalmetski et al. (2015), who assume that individuals derive utility from surprising others. Unlike in Ruffle (1999), disappointment from not being trusted cannot influence second movers here, as they have no decision to make when trust is withheld.

[5]The degree of unexpectedness is deemed to be a crucial driver of the intensity of surprise in the social psychology literature (e.g., Meyer et al., 1997; Reisenzein et al., 2019).

[6] Second movers were matched with a different first mover in each round.

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whether positive surprise – and any resulting reciprocation – can arise from mechanically generated 'trust,' or whether it requires an intentional act by the first mover. Aside from this change, the design was identical to our *Baseline* experiment. We find that removing intentions eliminates the hot–cold reciprocation gap, indicating that the intentionality of trust is essential for positive surprise to translate into higher reciprocal behavior.

A third experiment followed the same set-up as our *Baseline* experiment but additionally elicited second-order beliefs from second movers, as these have been linked to reciprocation through guilt aversion (Charness & Dufwenberg, 2006). We first elicited first movers' beliefs about the overall reciprocation rate in the experiment and then asked second movers, before each round, to guess the average belief of all first movers who chose to trust. This design allowed us to verify that hot and cold decision-making did not generate different second-order beliefs, conditional on the aggregate trust rate, thereby ruling out psychological mechanisms other than surprise. We find no such differences. However, the elicitation of second-order beliefs per se altered behavior in the trust game. Relative to the *Baseline* experiment, hot reciprocation rates were somewhat lower but remained fairly stable across rounds, whereas cold reciprocation rates started off and remained substantially higher, reversing the hot–cold reciprocation gap in initial rounds. Evidence from a post-experimental questionnaire indicates that belief elicitation substantially increased perceived cognitive load of the experiment. We conjecture that this additional cognitive burden led relatively more participants to instinctively reciprocate in the *Cold* treatments – where formulating a response plan is more demanding – compared to the *Hot* treatments (see Section 4.4 for a discussion).

The paper contributes to the literature on how the decision mode affects choices in sequential-move games involving trust and reciprocation, such as trust games, conditional cooperation games, and sequential prisoner's dilemma games. From the survey by Brandts and Charness (2011), we know that the effect on the reciprocation rate, which is the focus of our study, can go either way and is not well understood.[7] Most studies report statistically insignificant effects. Especially in games where a continuum of trust and reciprocation levels are possible – as in conditional public goods games and standard trust games – or in the case of repeated interaction, the decision mode has little effect on reciprocation choices (e.g. Fischbacher et al., 2012; Johnson & Mislin, 2011; Reuben & Suetens, 2012). Casari and Cason (2009) found that trustees sent back more money under direct elicitation. Brandts and Charness (2000) and García-Pola et al. (2020) are most closely related to our paper in terms of design (between subjects) and choice setting (one-shot setting with binary choices). In García-pola et al. (2020), participants stopped substantially earlier in hot than in cold (symmetric) centipede games while in Brandts and Charness (2000), reciprocation rates in sequential prisoner's dilemma games do not differ statistically between hot and cold. Opposite effects are also reported by Solnick (2007). Our paper explores mechanisms behind hot–cold reciprocation gaps and contributes by showing hot–cold gaps are the outcome of two countervailing forces: surprise, which pushes behavior toward more reciprocation under direct decision-making, and cognitive load, which tends to reverse this pattern by pushing individuals to adopt a reciprocal strategy when elicited contingently.

The remainder of the paper is organized as follows. Section 2 outlines the theoretical framework and derives the main testable predictions. Section 3 presents the design of the experiments and the associated hypotheses. Section 4 reports the main results and examines the underlying mechanisms. Section 5 concludes.

2. Theoretical background

Consider a two-player binary trust game in which both players earn 25 if the first mover does not trust the second mover; both earn 35 if the first mover trusts the second mover and the latter reciprocates; and the first mover earns 12 while the second mover earns 50 if the first mover trusts and the second

[7] In contrast, in dilemma games involving punishment, the decision mode has a clearer effect: direct decisions generate higher levels of punishment than contingent decisions (discussed in Brandts & Charness, 2011).

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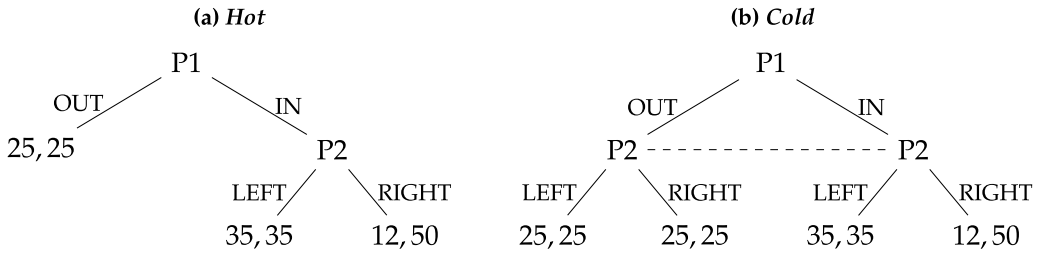


Fig. 1 Binary trust games
Notes: Panel a (b) describes the trust game if choices are elicited using *Hot* (*Cold*) elicitation. P1 (P2) denotes the first mover (second mover). Payoffs for the first mover (second mover) are shown on the left (right). The first mover's trust decision is IN, and the second mover's reciprocation decision is LEFT.

mover defects. The second mover's choices are elicited either under hot decision-making (the *Hot* game) or under cold decision-making (the *Cold* game), as illustrated in Fig. 1. In the *Hot* game, the second mover makes her choice after observing whether the first mover trusted her (i.e., chose IN). In the *Cold* game, she makes a choice, conditional on being trusted. Hence, her optimal choice depends on her expectation about the first mover's choice in the latter game but not in the former game. Let p denote the ex ante probability with which the second mover expects the first mover to trust her.

To derive testable predictions, we first examine the best reply of a payoff-maximizing second mover and then introduce our psychological typology.[8] If the second mover is a payoff maximizer, the payoffs in Fig. 1 coincide with her utility. In the *Hot* game, her best reply to trust is to defect irrespective of p. In contrast, in the *Cold* game, the best reply depends on p: if $p > 0$, defecting yields a strictly higher expected payoff than reciprocating, making defection optimal; if $p = 0$, the expected payoffs of defecting and reciprocating are identical, leaving her indifferent between the two actions.[9]

We analyze next the trust game as a psychological game by allowing the second mover to gain additional utility from reciprocating when she is surprised by the trust shown by the first mover.[10] Such psychological utility can arise only in the *Hot* game, because only in that setting does the second mover observe the first mover's actual choice before deciding. We assume that the extent to which the second mover cares about the first mover's payoff depends on her surprise sensitivity denoted by $\theta \geq 0$ and the surprise level denoted by $S(p)$.[11] The function S is decreasing in p, such that the lower the expectation that the first mover will trust her, the more surprised the second mover is when the first mover does. To focus on the role of surprise, we assume that utilities are equal to monetary payoffs, apart from the surprise component.

Hence, the second mover's utility is:

$$U_2 = \pi_2 + \theta S(p)\,\pi_1 \tag{1}$$

It follows that, in the *Hot* game, the second mover reciprocates if $\theta > \hat{\theta} \equiv \frac{50-35}{S(p)(35-12)}$, with $S(p) > 0$. In contrast, in the *Cold* game, she finds it optimal to defect because surprise cannot arise,

[8] Theoretical predictions are based on best-reply behavior by second movers, as the asynchronous structure of the experimental design prevents first movers from learning, leaving no scope for convergence to an equilibrium. Equilibrium calculations are reported in Section A of the Appendix.

[9] In a subgame-perfect Nash equilibrium, the probability that the second mover reciprocates cannot exceed 0.57, as a higher probability would induce trust and render defection optimal.

[10] This assumption is reminiscent of that made by Geanakoplos et al. (1989), who posit that a player in a repeated prisoner's dilemma becomes more sympathetic toward a partner who unexpectedly cooperated earlier in the game.

[11] Allowing for heterogeneity in surprise sensitivity across players does not affect the main theoretical predictions.

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that is, $S(p) = 0$. If she assigns a strictly positive probability to being trusted, then she will defect, and if she believes that the first mover will not trust her, then she is indifferent between the two actions.[12]

In summary, if at least some second movers are motivated by positive surprise and as long as $p > 0$, two predictions follow.[13] First, for a given p, the reciprocation rate will be (weakly) higher in the *Hot* game than in the *Cold* game. Second, the reciprocation gap between the *Hot* game and the *Cold* game is expected to narrow as p increases.

3. The experiments

3.1. Design and hypothesis

We refer to our main experiment as *Baseline*. Participants were assigned the role of either first or second mover. First movers decided whether to trust a randomly matched second mover in a trust game with payoffs as in Fig. 1. Second movers were informed about the trust rate among the first movers from whose population their partners were drawn and then made their choices. Second movers were randomly assigned, in a between-subjects design, to one of four treatment conditions varying along two dimensions: the decision mode (*Hot* or *Cold*) and the induced prior belief about trust, p (*Low* or *High*).

We exogenously varied p to manipulate the scope for positive surprise. To implement the *Low* and *High* conditions, the pool of first movers was divided into two groups: one with a low trust rate (0.28) and one with a high trust rate (0.69).[14] Second movers were then matched exclusively with first movers from either the low- or the high-trust group. We conjectured that a higher trust rate would induce a more optimistic prior about trust, thereby reducing the level of surprise when encountering a trusting first mover in *Hot*.

After completing 20 trust games with different first movers, second movers answered a brief survey that included a question on the surprise they experienced. The response served as a manipulation check for the variation in trust rates within *Hot*. Specifically, we asked them to rate their level of surprise ('not surprised,' 'somewhat surprised,' or 'surprised') at the trust shown by the first mover in games where trust was chosen. In *Hot*, participants assigned to the *High* condition were significantly less likely to report being surprised: the percentage reporting 'surprised' or 'somewhat surprised' was 9–18 points lower than in the *Low* condition, depending on the specification ($P \leq 0.045$; see Table D.1 in the Appendix). This confirms that the induced variation in prior beliefs successfully altered the scope for positive surprise.

The hypotheses follow directly from the predictions of the surprise model in Section 2:

Surprise hypothesis. (a) The reciprocation rate is (weakly) higher in *Hot* than in *Cold* in both *Low* and *High*. (b) The difference in reciprocation rates between *Hot* and *Cold* is (weakly) larger in *Low* than in *High*.

3.2. Intentions and second-order beliefs

Intentions. We implemented an additional experiment – the *No Intentions* experiment – that replicated the setup described above, except that first movers' choices were programmed rather than made

[12] A simple extension that would yield a positive rate of reciprocation even in the *Cold* game is to incorporate other-regarding preferences into the utility function (e.g., Charness & Rabin, 2002; Fehr & Schmidt, 1999). This extension, however, does not alter the comparative-static predictions.

[13] The condition $p > 0$ holds in our experiment, as second movers are informed about the (strictly positive) trust rate in the population of first movers they are matched with. This design feature is discussed in Section 3.1.

[14] Before running the experiment, we conducted pilot sessions to calibrate the payoff parameters. In these sessions, the payoffs for trust and reciprocation were 42 and 37, respectively (instead of 35). The goal of this preregistered procedure was to achieve an aggregate trust rate of about 40%, allowing us to split first movers into two subgroups with sufficiently different trust rates (see Section 3.3).

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by actual participants. The programmed trust decisions exactly matched the trust rates used in the *Low* and *High* treatments of the *Baseline* experiment. All other aspects of the design were identical. Hence, the key difference between this experiment and the main one lies in the presence of an intentional act of trust. This design allows us to test whether surprise is triggered only by an intentional expression of trust, or whether it can also arise when 'trust' is mechanically generated (see Cox, 2004).

Second-order beliefs. Psychological mechanisms based on second-order beliefs – such as guilt aversion and intention-based reciprocity – are well-known drivers of reciprocation.[15] Because second-order beliefs may be positively affected by the communicated trust rate, they likely differ between the *Low* and *High* conditions, potentially affecting reciprocation through guilt aversion or intention-based reciprocity. Although this possibility does not per se invalidate our identification strategy for testing the surprise hypotheses, a key identifying assumption is that second-order beliefs do not differ between *Hot* and *Cold* within both *Low* and *High*.

To assess this assumption, we implemented another experiment identical to the *Baseline* experiment – with the same *Low* and *High* trust rates – but adding an incentivized second-order belief elicitation stage. We refer to this as the *Beliefs* experiment. First, we elicited first-order beliefs from first movers about the overall reciprocation rate in the experiment before they had made their trust decision. Next, we elicited second-order beliefs from second movers by asking them, before each round, to guess the average first-order belief of all first movers who chose to trust.[16] To minimize any influence of the belief elicitation on reciprocation decisions, second movers did not receive feedback about the accuracy of their beliefs after each round.

3.3. Procedures

All experiments were programmed in Qualtrics and conducted on Prolific. Section B in the Appendix details the implementation timeline, its correspondence with our preregistrations, and the number of participants recruited for each experiment. Participation was restricted to students based in the UK, and we targeted a sample of 100 second movers per treatment.[17] To ensure comprehension of the instructions, participants were required to correctly answer a set of control questions before proceeding; they could make up to six attempts, and those who failed were excluded. The instructions for the *High* treatments are provided in Section C of the Appendix.

In the *Baseline* and *Beliefs* experiments, the design was asynchronous: first movers completed the task before second movers were recruited. Each first mover made a single-trust decision without knowing which treatment group their matched second mover would later be assigned to. These decisions served solely to incentivize second movers' behavior and are therefore not analyzed. Each second mover then played 20 rounds of the game, each time with a different first mover, and was informed about the trust rate in the population of first movers they were matched with (either 0.28 or 0.69). After each round, second movers in *Cold* received feedback on the first mover's choice, and all participants received feedback on their payoffs. In the *No Intentions* experiment, first-mover actions were pre-programmed to match the trust rates of the *Low* and *High* conditions.

[15]For guilt aversion, the intuition is that the more optimistic a guilt-averse second mover believes the first mover is about reciprocation, the more likely she is to reciprocate (Charness & Dufwenberg, 2006). In contrast, intention-based reciprocity predicts that the more optimistic a second mover thinks the first mover is about reciprocation, the *less* inclined she will be to reciprocate, because trust from an optimistic first mover is perceived as less generous than trust from a pessimistic one (Dufwenberg & Kirchsteiger, 2004; Rabin, 1993). A similar mechanism arises in the model of surprising gifts in Khalmetski et al. (2015).

[16]We employed a simple incentive scheme to keep the task easy to understand and avoid lengthening the experiment excessively. First movers whose guess was within 5% of the actual reciprocation rate earned 20 additional tokens. Second movers earned 20 additional tokens if their belief in a randomly selected round (different from the one determining the payoff for their reciprocation choice) was within 5% of the average first-order belief of first movers who chose to trust them.

[17]Power calculations assumed a hot–cold difference of 15 percentage points. Detecting such an effect with 80% power at a 5% significance level required 100 second movers per treatment.

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To ensure an equal start and an equal number of observations in *Hot* and *Cold* – and thus sufficient statistical power – we arranged the games so that in the first round all second movers faced a trusting first mover.[18] We shall interpret first-round choices of second movers as their instinctive responses. After the 20 rounds, participants completed a short survey including socio-demographic questions (gender, age, and education), measures of social preferences, perceived task complexity, and the level of surprise experienced. One round was then randomly selected for payment to both the second mover and their matched first mover. Second movers completed the experiment in about 8 to 14 minutes and earned £2.80 on average, including a £0.60 participation fee.

3.4. Sample compositions and balance

Table 1 reports descriptive statistics on the composition of the estimation samples by experiment and treatment. Across all experiments, participants were predominantly undergraduate students, and the share of female participants was slightly higher than that of males. To assess balance in socio-demographic characteristics across treatments within each experiment, we compare gender, age, and education using post-experimental survey data. Within each experiment, socio-demographic differences across treatments are small and generally not statistically significant. The only exception is in the *Beliefs* experiment, where the share of participants with at most a high-school diploma is significantly higher in the *Hot–High* treatment than in the other three conditions. To account for this imbalance, we control for educational background when estimating treatment effects on reciprocation as a robustness check. Small differences in the number of observations reflect attrition. Among second movers who entered the decision-making stage, the attrition rates ranged from 0.1 to 0.2 across treatments and were balanced in all experiments ($P > 0.422$ according to Wald tests from probit regressions; see Section B in the Appendix for details).

Across experiments, some socio-demographic differences emerge, most notably between *No Intentions* and *Baseline* (see Table D.2 in the Appendix). Participants in *No Intentions* were, on average, about three years older and less often female. Participants in *Beliefs* were, on average, two years younger than those in *Baseline*. These moderate differences likely reflect the staggered implementation of the experiments and the exclusion of previous participants; note that the *Beliefs* experiment was conducted before the other experiments. While there is no theoretical reason to

Table 1. Sample characteristics and treatment balance

	Hot–Low	*Hot–High*	*Cold–Low*	*Cold–High*	*P*-value
(a) Baseline experiment					
Female	0.696	0.657	0.589	0.636	0.472
Age	26.608	26.101	25.095	26.101	0.612
Education					
High school or below	0.127	0.152	0.084	0.091	0.406
Bachelor	0.578	0.596	0.653	0.667	0.510
Master	0.186	0.162	0.221	0.202	0.758
PhD or above	0.088	0.091	0.032	0.030	0.114
Other	0.020	0.000	0.011	0.010	0.590

(*Continued*)

[18]We also ensured that the sequence of trust decisions faced by second movers across the 20 rounds was comparable across treatments. Specifically, we generated 20 random sequences of trust decisions and assigned roughly five second movers per treatment to each sequence.

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Table 1. (*Continued.*)

	Hot–Low	*Hot–High*	*Cold–Low*	*Cold–High*	*P*-value
Observations	102	99	95	99	
(b) Beliefs experiment					
Female	0.687	0.723	0.693	0.700	0.939
Age	24.443	24.241	24.132	23.727	0.920
Education					
High school or below	0.122	0.205	0.088	0.082	0.018
Bachelor	0.652	0.571	0.684	0.573	0.199
Master	0.148	0.170	0.158	0.236	0.303
PhD or above	0.078	0.036	0.044	0.100	0.171
Other	0.000	0.018	0.026	0.009	0.343
Observations	115	112	114	110	
(c) *No Intentions* experiment					
Female	0.538	0.485	0.653	0.543	0.107
Age	29.837	28.687	28.317	29.681	0.660
Education					
High school or below	0.096	0.172	0.178	0.117	0.252
Bachelor	0.548	0.556	0.554	0.606	0.837
Master	0.250	0.222	0.228	0.170	0.586
PhD or above	0.077	0.040	0.040	0.106	0.178
Other	0.029	0.010	0.000	0.000	0.129
Observations	104	99	101	94	

Notes: The table reports averages of socio-demographic variables across treatments for each estimation sample, separately for each experiment. The last column shows *P*-values from joint Wald tests of equality across treatments. For each experiment, *P*-values are obtained from regressions of the corresponding variable on treatment indicators: linear probability models for the dummy variables (*Female* and education categories) and OLS for *Age*.

expect that demographic or timing differences systematically bias treatment comparisons, we include socio-demographic controls in the few analyses that pool data across experiments.

4. Results

4.1. Hot–cold reciprocation gap and *surprise*

We begin with the *Baseline* experiment. Figure 2 shows reciprocation rates in the *Hot* and *Cold* treatments, separately for the *Low* and *High* conditions and across first, last two and all rounds.[19] In the first round, reciprocation is slightly higher in *Hot* than in *Cold*, but the difference is negligible. Over time, however, a hot–cold reciprocation gap appears and widens: in the final rounds, the reciprocation rate under Low is 17.73 percentage points higher in Hot than in Cold, whereas the gap under High is only 6.58 percentage points Pooling all rounds, reciprocation remains higher in *Hot*, though the aggregate difference is small because it combines early rounds – where rates are similar – with later rounds – where they diverge.

To formally test the surprise hypothesis, we estimate linear probability models with the choice to reciprocate as the dependent variable. Results are reported in Table 2. In the first specification, we

[19]Throughout the analysis we focus on rounds 19–20, where behavior is most likely to reflect post-learning choices while still providing sufficient observations in the *Hot* condition. Using only the final round yields highly comparable point estimates but substantially larger standard errors due to the limited number of observations in *Hot*. Table D.6 in the Appendix shows that results are robust to alternative round windows within the second half of the experiment.

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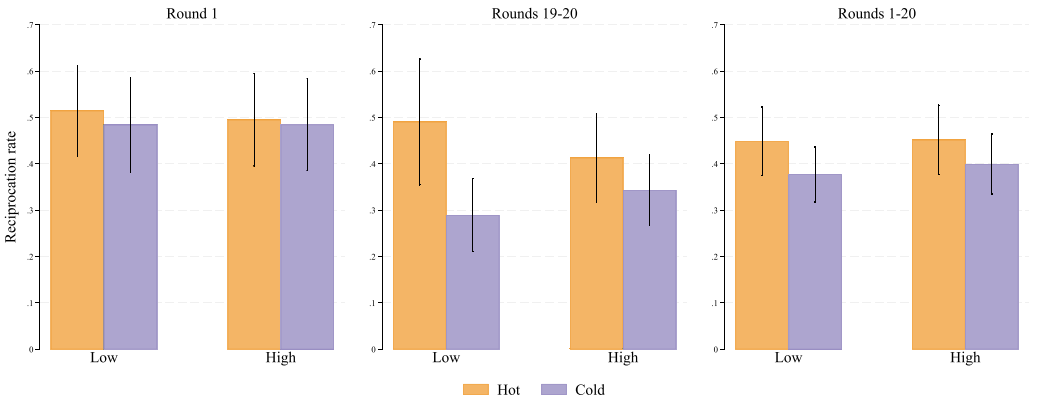


Fig. 2 Reciprocation rates in the *Baseline* experiment
Notes: Each bar reports predicted reciprocation rates by treatment with 95% confidence intervals, estimated from linear probability models including three treatment dummies (*Hot–Low* as the reference). Confidence intervals are based on standard errors clustered at the individual level (for rounds 19–20 and 1–20).

Table 2. Treatment effects on reciprocation in the *Baseline* experiment

	Round 1		Rounds 19 − 20		Rounds 1 − 20	
	(1)	(2)	(3)	(4)	(5)	(6)
Hot	0.005	0.010	0.103**	0.177**	0.056	0.065
	(0.050)	(0.071)	(0.049)	(0.081)	(0.036)	(0.048)
High	− 0.019	− 0.013	0.013	0.044	0.012	0.016
	(0.050)	(0.071)	(0.046)	(0.055)	(0.035)	(0.044)
Hot × High		− 0.011		− 0.112		− 0.014
		(0.100)		(0.102)		(0.069)
Controls	✓	✓	✓	✓	✓	✓
Observations	395	395	579	579	5,815	5,815
Adjusted *R*2	0.030	0.027	0.016	0.017	0.014	0.013

Notes: Estimates from linear probability models. The dependent variable equals 1 if the participant chose to reciprocate. Standard errors are reported in parentheses; in Columns (3)–(6), they are clustered at the individual level. All regressions control for age, gender, and education. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

test part (a) of the hypothesis – that reciprocation is more likely in *Hot* than in *Cold* within both *Low* and *High* – by regressing the reciprocation decision on the indicators *Hot* and *High*. In the second specification, we test part (b) of the hypothesis – that the hot– cold reciprocation gap is larger under *Low* than under *High* – by including the interaction term between *Hot* and *High*.[20] All regressions control for age, gender, and education.

Consistent with part (a) of the surprise hypothesis, the *Hot* effect is positive in both *Low* and *High* treatments. In the last two rounds, the non-interacted specification yields a *Hot* coefficient of 0.103 ($P = 0.037$), whereas the interacted model implies a *Hot* effect of 0.177 under *Low* ($P = 0.030$) and 0.065 under *High* ($P = 0.289$). In line with part (b), the estimated interaction term *Hot* × *High* is negative – as predicted – but imprecisely estimated ($P = 0.275$).

Results are robust to using probit models and to excluding socio-demographic controls (see Tables D.3 and D.4 in the Appendix, respectively). Treatment effects also remain strong – and become statistically significant across all rounds – when controlling for individual reciprocity attitudes, measured using the validated Preference Survey Module by Falk et al. (2023), which are strongly and

[20]Econometrically, this specification is equivalent to that underlying Fig. 2; the treatment effects plotted in the figure correspond to linear combinations of coefficients from the interacted model.

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positively correlated with the decision to reciprocate (see Table D.5 in the Appendix). We do not include these measures in our main specifications because they were elicited after the experimental tasks and therefore constitute post-treatment variables that may themselves be affected by the treatment.

In summary, in the *Baseline* experiment, the *Hot* condition led to higher reciprocation after learning, particularly in *Low*, consistent with the surprise intuition. In line with this interpretation, second movers in *Hot–Low* reported being more surprised by trust than those in *Hot–High*, as shown in Table D.1 in the Appendix. Taken together, these findings support the main implication of the surprise hypothesis. The gap between conditions widens with experience, especially under *Low*, suggesting that surprise can shape reciprocation behavior. We return to the dynamics across rounds and the role of experience in Section 4.3.

4.2. The role of intentions

We next turn to the *No Intentions* experiment, in which the decision to trust was pre-programmed by the experimenter according to earlier first-mover choices. Figure 3 shows that the hot– cold reciprocation gap observed in the *Baseline* experiment disappears: the share of prosocial choices is now similar across all treatments. Regression estimates confirm this result – none of the treatment effects or interactions are statistically significant ($P \geq 0.269$, Table D.7 in the Appendix), and differences across round blocks are small. This finding indicates that the surprise mechanism operates only when trust is perceived as a deliberate act by another person.

This interpretation is consistent with the significantly lower reciprocation rates observed across all treatments and rounds of *No Intentions* relative to the *Baseline* experiment, echoing previous evidence that reciprocation differs from purely outcome-based preferences (Cox, 2004). In the *Cold* condition, participants in *No Intentions* reciprocated roughly 6–8 percentage points less often than in Baseline ($P \leq 0.096$ in regressions, see Table D.8 in the Appendix), and the gap widened to about 11 percentage points in the Hot condition ($P \leq 0.018$). The somewhat larger drop in *Hot* suggests that the influence of surprise diminishes when trust is not perceived as an intentional choice by another person, thereby reducing the motivation to reciprocate.

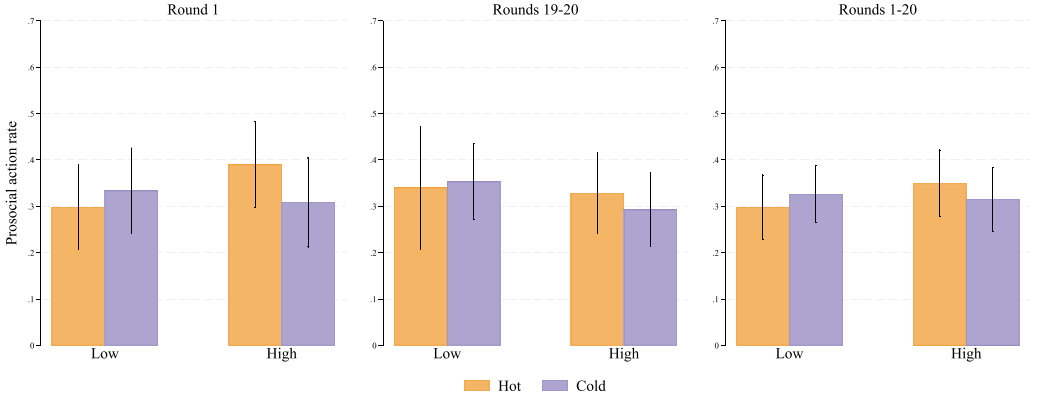


Fig. 3 Reciprocation rates in the *No Intentions* experiment
Notes: Each bar reports predicted prosocial action rates by treatment with 95% confidence intervals, estimated from linear probability models including three treatment dummies (*Hot–Low* as the reference). Confidence intervals are based on standard errors clustered at the individual level (for rounds 19–20 and 1–20).

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4.3. Second-order beliefs and complexity

Finally, we report on the *Beliefs* experiment, in which second-order beliefs were elicited to assess whether alternative psychological mechanisms – such as guilt aversion or intention-based reciprocity – could confound our interpretation. As shown in Fig. 4, second-order beliefs do not differ significantly between the *Hot* and *Cold* conditions after controlling for the aggregate trust rate. In the last two rounds, second movers in the *Low* condition guessed, on average, that first movers expected a reciprocation rate of 33.90% in *Hot* and 33.28% in *Cold* ($P = 0.811$), whereas in *High* they expected 56.16% and 58.77%, respectively ($P = 0.291$).[21] This result shows that the difference in reciprocation between *Hot* and *Cold* cannot be explained by a difference in second-order beliefs.[22]

Nonetheless, the pattern of reciprocation in the *Beliefs* experiment runs opposite to that observed in the *Baseline* experiment and to the prediction of the surprise hypothesis: reciprocation is higher in the *Cold* than in the *Hot* treatment. As shown in Fig. 5, reciprocation rates in *Cold* consistently exceed those in *Hot*, particularly in the first round ($P = 0.001$). This result remains robust when controlling for the second-order beliefs reported by second movers, which, in both the *Hot* and *Cold* conditions, are significantly and positively correlated with the decision to reciprocate, consistent with models of guilt aversion (Table D.9 in the Appendix).[23]

If we compare reciprocation rates in the *Beliefs* experiment to those in the *Baseline* experiment, it becomes clear that the difference in the hot–cold gap is entirely driven by different choices in the *Cold* treatments. As illustrated in Fig. 6, while reciprocation rates under *Hot* are similar in the two experiments, the cold reciprocation rate is more than 10 percentage points higher in Beliefs than in Baseline (see also Table D.8 in the Appendix). Moreover, whereas hot reciprocation rates remain relatively stable across rounds, cold reciprocation rates show a marked declining trend (see Table D.13 in the Appendix).[24] Given the comparable *Hot* rates across experiments, the upward shift in cold

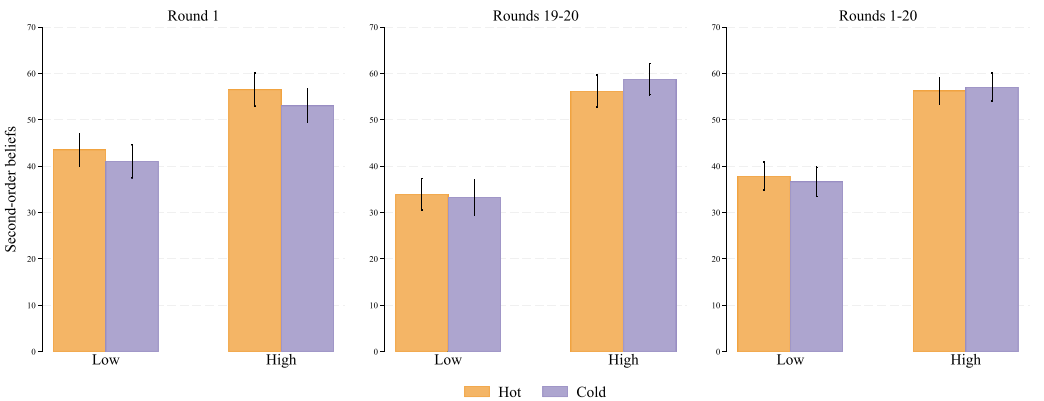


Fig. 4 Second-order beliefs in the *Beliefs* experiment

Notes: Each bar reports predicted second-order beliefs with 95% confidence intervals, estimated from linear regressions including three treatment dummies (*Hot–Low* as the reference). Confidence intervals are based on standard errors clustered at the individual level (for rounds 19–20 and rounds 1–20).

[21] The reported *P*-values refer to Wald tests based on the linear regressions used to generate Fig. 4.

[22] Under both *Hot* and *Cold*, beliefs are stable across rounds in *High*, while they exhibit a slight decreasing trend in *Low* (Figure E.3 in the Appendix).

[23] We do not include second-order beliefs in the main regressions because they may constitute a 'bad' control: beliefs are themselves endogenous to the treatment and could mediate part of the behavioral response, biasing the estimated treatment effects.

[24] Table D.12 reports estimates from regressions with treatment–round interactions separately for each experiment. The results confirm that reciprocation declines significantly faster under *Cold* than under *Hot* in both experiments ($P = 0.050$ in *Baseline* and $P = 0.014$ in *Beliefs*), whereas no systematic trend emerges in *No Intentions*. Conditional on decision-making

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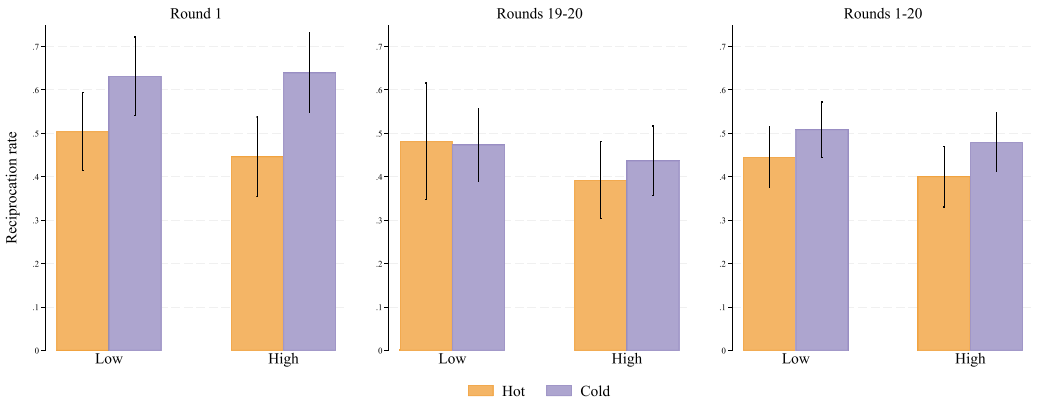


Fig. 5 Reciprocation rates in the *Beliefs* experiment

Notes: Each bar reports predicted reciprocation rates by treatment with 95% confidence intervals, estimated from linear probability models including three treatment dummies (*Hot–Low* as the reference). Confidence intervals are based on standard errors clustered at the individual level (for rounds 19–20 and 1–20).

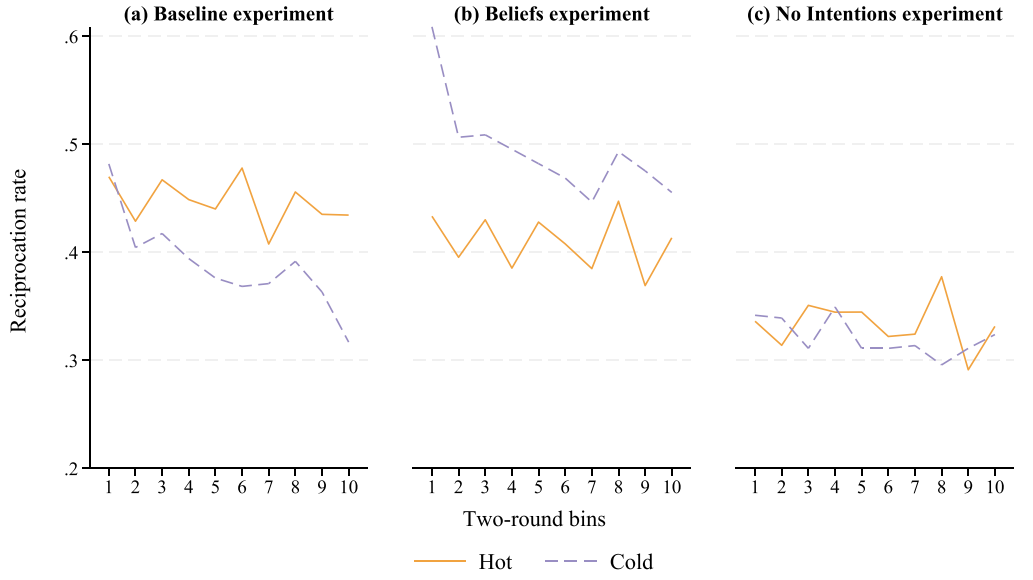


Fig. 6 Trends in reciprocation rates

Notes: Each line shows average reciprocation rates by experiment, pooling *High* and *Low* conditions. Rates are averaged over two consecutive rounds (two-round bins) to reduce noise, particularly in *Hot*, where fewer choices are made. Unbinned trends are shown in Figure E.2 in the Appendix.

reciprocation rates – driven by levels rather than trends – reverses the sign of the hot–cold reciprocation gap: in *Baseline*, the gap is positive and widens over time, whereas in *Beliefs* it is negative and narrows in later rounds. Thus, the difference between the two experiments primarily reflects higher cold reciprocation levels in the *Beliefs* experiment.

mode, reciprocation trends are also similar across rounds under the *High* and *Low* treatments (see Figure E.1 and Table D.12 in the Appendix).

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4.4. Difference between the *baseline* and *beliefs* experiments

This section further discusses the difference between the *Baseline* and *Beliefs* experiments. The reversal in the hot–cold reciprocation gap is unlikely to be driven by a difference in sample composition or by a general downward time trend in reciprocation between the two experiments. The first explanation can be ruled out because samples are well balanced across key socio-demographic characteristics; any small residual imbalances are accounted for by the controls included in the regressions (Tables D.2, D.8, and D.13 in the Appendix). Although the *Baseline* experiment was conducted about five years after the *Beliefs* experiment, we can also rule out the downward trend explanation, as reciprocation rates in the *Hot* condition did not differ significantly across experiments.

A plausible explanation lies in differences in cognitive load between the *Baseline* and *Beliefs* experiments. We find that the belief-elicitation stage implemented before each reciprocation decision – the only difference between the two experiments – increased cognitive load.

Specifically, participants perceived the latter experiment as more complex than the former experiment according to a post-experimental questionnaire. This can be seen in Fig. 7, which shows self-reported complexity measured on an 11-point Likert scale (see also Table D.14 in the Appendix). Why could the higher complexity have had a larger effect on reciprocation in *Cold* than in *Hot*? We conjecture that cold decision-making is less straightforward than hot decision-making and therefore cognitively more demanding, which might be particularly consequential in combination with other cognitively demanding tasks (such as belief elicitation in the *Beliefs* experiment). As can be seen in Fig. 6, reciprocation rates in *Cold* show a starkly declining trend, in contrast to *Hot*, suggesting that second movers in *Cold* 'unlearn' to reciprocate, which in turn suggests that the initially high reciprocation rates might have largely been due to instinctive reflexes (see Section 5 for a discussion).

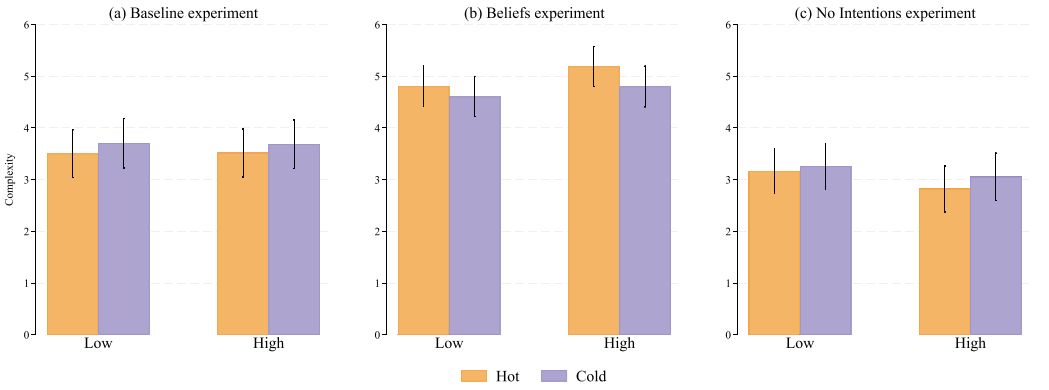


Fig. 7 Differences in self-reported complexity
Notes: Each bar reports predicted self-reported complexity score (0–10 scale) with 95% confidence intervals, estimated from linear regressions including three treatment dummies (*Hot–Low* as the reference).

5. Conclusion

Understanding the conditions under which people reciprocate another person's trust has been a recurring theme in the literature. We conducted experiments to compare reciprocation rates in a binary trust game between direct and contingent decision modes, using a design that can shed light on the effect of being positively surprised by a partner's trust. In a real-world economic context, our direct mode corresponds to settings in which individuals respond to trust after observing it, such as trust in workplace, professional-service, or trade relationships. Our contingent mode of decision-making captures situations in which reciprocal responses are determined and programmed ex ante

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and implemented automatically. Examples include automated bonus or discount mechanisms on e-commerce platforms and telematics-based insurance pricing (Reimers & Shiller, 2019).[25] Our results show that, in conditions where cognitive resources are not depleted, individuals are more likely to reciprocate trust with direct than with contingent decision-making. In the case of cognitive depletion, however, the hot–cold reciprocation gap may reverse.

The findings are overall consistent with the presence of two countervailing mechanisms: positive surprise and a duty to reciprocate. Positive surprise about trust induces reciprocation under hot decision-making and may thus lead to a positive hot–cold reciprocation gap, but this may be counteracted by the presence of a duty to reciprocate. Specifically, individuals who experience actual trust and feel a duty to reciprocate may require a stronger sense of duty to overcome the temptation to defect than individuals who merely imagine that someone trusts them, thus leading to a negative hot–cold reciprocation gap (Chen & Schonger, 2024, see Section F of the Appendix for a simple illustration in our context).[26] Our findings further suggest that cognitive depletion may affect the relative strength of these two mechanisms, with the duty mechanism possibly gaining relative weight in contexts with stronger cognitive depletion. While this mechanism is not directly identified in our data, the interpretation is supported by indirect evidence from participants' self-reported task complexity. This interpretation is broadly in line with the notion that some individuals tend to cooperate or reciprocate instinctively, as a rule of thumb, in contexts where such behavior entails a monetary loss, for example because they apply a strategy from daily life, where sanctioning mechanisms are in place or interactions are repeated (Rand et al., 2012, 2014; Reuben & Suetens, 2018).

Our findings are related to de Melo et al. (2019) who asked participants in a prisoner's dilemma experiment to program an automaton (defect or cooperate) that would act on their behalf against another participant. Programming the strategy generated a higher cooperation rate than direct play with other participants. This negative hot–cold gap might have been driven by the complexity of the task in the cold treatment, where participants most likely took several elements into account when programming their strategy and formed beliefs about their partner's cooperation. This might have depleted cognitive resources among some, spurring them to act out of duty. Our findings also suggest that when trust decisions are simple and observable, as in small-scale labor relations and trade, removing or automating human contact may weaken reciprocity, which in turn harms mutual trust. In complex environments like financial or insurance contracting, digital platforms with dense information, ex ante decision rules may better sustain reciprocity.

As we have discussed in the introduction, our findings help to understand the mixed evidence in the literature on direct versus contingent decision-making in trust and related games. Interestingly, evidence is also mixed in the literature on negative reciprocation, where the second mover responds to a possibly unfair choice by the first mover (see Brandts & Charness, 2011, for an overview). A number of studies find that second movers in a hot decision mode punish unfair choices more severely than in a cold decision mode, or are more likely to reject them in an ultimatum game (Aina et al., 2020; Brandts & Charness, 2003; Brosig et al., 2003; Falk et al., 2005; Oxoby & McLeish, 2004). In contrast, a meta-analysis shows that ultimatum games with hot responses have a lower rejection rate (Oosterbeek et al., 2004). The mixed evidence can be understood by viewing emotional responses and duty to reciprocate as mechanisms leading to opposite effects. A hot response in this context may be motivated by anger or frustration and is therefore associated with a stronger reaction to unfairness than a cold response (Aina et al., 2020), whereas feelings of duty to reciprocate may induce individuals to respond more strongly to unfair choices under cold decision-making. Future research might be devoted to testing this conjecture directly.

[25]Insurers can monitor their consumers by using telematics devices. Telematics pricing often includes pre-programmed reward or penalty rules according to how the consumers behave.

[26]Akdeniz and van Veelen (2021, 2023) show that the presence of a duty or commitment to reciprocate is key for understanding human morality.

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Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/eec.2026.10059

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Replication package. The replication material for this study is available at https://doi.org/10.34894/5FMX0C.

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