Decision Times Reveal Private Information in Strategic Settings: Evidence from Bargaining Experiments[1]

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June 1, 2023

Abstract

People respond quickly when they have a clear preference and slowly when they are close to indifference. The question is whether others exploit this tendency to infer private information. In two-stage bargaining experiments, we observe that the speed with which buyers reject sellers' offers decreases with the size of the foregone surplus. This should allow sellers to infer buyers' values from response times (RT), creating an incentive for buyers to manipulate their RT. We experimentally identify distinct

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The authors thank Michael Durdak, Anushka Das, Arushi Sharma, Nicholas Kudlapur, and Alexandrea Howard for research assistance, and Yaron Azrieli, Colin Camerer, Fadong Chen, Ernst Fehr, Todd Hare, Paul J. Healy, John Kagel, Dan Levin, Kirby Nielsen, Alexander Ritschel, members of the OSU Theory/Experimental group, and conference and seminar participants at ESA 2016, ISDN 2017, BEEC 2018, NPE 2018, BRIC 2018, SJDM 2018, Virginia Tech Decision Workshop 2018, Stanford University, University of Zurich, Duke University, Texas A&M, University of Southern California, CERGE-EI, CFPB, Penn State Smeal, University of Konstanz, University of Basel, University of Warwick, and University of Nottingham for their helpful comments and conversations. All subjects gave written consent, and studies were approved by the OSU Institutional Review Board (2013B0583; 2022E0396). IK acknowledges funding from NSF 1554837 and NSF 2148982, and the Cattell Sabbatical Fund.

conditions under which subjects do, and do not, exhibit such strategic behaviour. These results provide the first insight into the possible use of RT as a strategic variable.

Keywords: bargaining, response times.

JEL: C91, D01, D87.

1 Introduction

Many economic interactions entail a rich set of potentially valuable non-choice information. We typically infer values and beliefs from choices, but choices leave other physical traces such as facial expressions, hesitation, or brain activity. Decision time or response time (RT), that is, the time it takes an agent to decide, is one readily available measure that may reveal hidden information and can be assessed by both researchers and interacting agents themselves. Availability of RT data, as well as advancements in joint modeling of choices and RTs, have provoked a growing interest in the study of RTs in economics (Clithero, 2018; Spiliopoulos and Ortmann, 2018).

One outcome of this research is the understanding that RTs are not random, nor are they explicitly chosen by the agent. Instead, RTs are best modeled as stochastic outputs of a noisy evidence accumulation process. In this process, the agent has no control over the quality of the evidence but does decide on a stopping rule. The combination of the endogenous stopping rule with the exogenous evidence determines the choice outcome and RT in a particular choice instance. For a given stopping rule, stronger evidence results in shorter RT, in a first-order-stochastic-dominance sense (Wilcox, 1993; Moffatt, 2005; Chabris *et al.*, 2009; Dickhaut *et al.*, 2009; Krajbich *et al.*, 2010; De Martino *et al.*, 2013; Webb, 2018; Dai and Busemeyer, 2014; Rodriguez *et al.*, 2014; Caplin and Martin, 2016; Clithero, 2018; Bhui, 2019). In an economic setting, stronger evidence equates to being farther from indifference. Under these assumptions, one can on average infer an agent's strength of preference from their RT (Echenique and Saito, 2017; Konovalov and Krajbich, 2019; Alós-Ferrer *et al.*, 2021).[2]

If the relationship between strength-of-preference and RT from individual choice extends to strategic situations, RT could help to alleviate problems of asymmetric information. Because RT is a natural by-product of the decision process, it should be available in any economic setting. By paying attention to RT, market analysts (and agents in the markets) should have better information than they would otherwise. Thus, RT has the potential to eliminate, or at least reduce, problems associated with asymmetric information.

Bargaining is one prominent setting where RT could be used to address asymmetric information. Laboratory evidence indicates that the relationship between strength-of-preference and RT is evident in purchasing behaviour; subjects purchasing consumer goods are slow (fast) to accept (reject) low value goods or goods with high prices (Krajbich *et al.*, 2012). This relationship could be exploited by the other party during bargaining. There are many bargaining situations where RT is observable, for example the millions of exchanges that occur online each year (Backus et al. 2020). Evidence from eBay bargaining data indicates that RT is informative: sellers are slow

[2] Strength of preference can be established by looking at the probability of making the same choice in identical (or very similar) choice instances. For instance, Konovalov & Krajbich (2019) examine two datasets where each decision problem was faced twice. Later-reversed vs. later-repeated choices had median RTs of 1.36s vs. 1.17s in time-constrained intertemporal choice, and 2.36s vs. 1.40s in unconstrained risky choice.

(fast) to accept low (high) offers and fast (slow) to reject low (high) offers on their goods (Cotet and Krajbich 2021).

Even though field data indicate that RT is informative, one might expect some agents, in some cases, to try to manipulate their RT. Anyone who has experienced face-to-face trades in a street market or bazaar knows that sellers sometimes bargain more aggressively if a potential buyer hesitates while rejecting the initial price offer. A strategic buyer may therefore try to reject quickly, signaling less interest in the good. Conversely, a quick acceptance may reveal to the seller that he could have set a higher price and still made the sale. In line with this idea, Srivastava and Oza (2006) explored hypothetical bargaining scenarios and found that people were more satisfied when offers were accepted after a delay rather than immediately.

Consider the following two-stage bargaining game, where the buyer can either accept the first offer, or reject it and receive a second offer (with profits that are discounted by a factor δ due to the delay). Even with a positive surplus, the buyer may reject the first offer in the hopes that the second offer will be low enough to compensate for the loss due to the delay (Sobel and Takahashi, 1983).

Suppose a buyer receives an initial price offer of \$30 and chooses to reject it. With no strategic RT considerations, a buyer who values the good at \$50 would take more time to reject than a buyer who values it at \$35. The buyer with the \$50 value would find it difficult to reject a \$20 surplus, while the buyer with the \$35 value would find it easy to reject a \$5 surplus. While these buyers' choices would be equally (un)informative about their private values, their RTs would reveal extra information. If the seller knew the relationship between RT and preference, he could update his belief about the buyer's value from her RT and adjust the next price offer, implementing price discrimination and increasing the probability of trade.[3] But if the buyer was aware that the seller was price discriminating, she might try to reject quickly to receive a better deal, pretending to be a low-value type.

However, what makes RT particularly interesting is that it is not cheap talk. Because it is a natural by-product of the decision process, manipulating RT also affects choice outcomes. Specifically, speeding up the decision process results in more choice errors (Palmer *et al.*, 2005; Milosavljevic *et al.*, 2010). Thus, when "deciding how to decide", the agent must balance the utility from signaling with the utility from making accurate choices. In the example above, the buyer might erroneously accept a "bad" offer or reject a "good" offer by speeding up.[4]

There are two key assumptions that must hold for agents to use RTs strategically. First, the agents must know what information to infer from RT (strength of preference). Second, they must pay attention to RT amid the other information in the environment (and correctly estimate it). The first assumption is relatively straightforward to test in a laboratory experiment by prompting subjects with RT information and seeing whether they respond as predicted. On the other hand, the second assumption is not straightforward to test, since it likely depends on the specifics of the bargaining environment, i.e. the presence and salience of other information.

We find evidence for the assumption that individuals know what to infer from RT. In one experiment we explicitly displayed buyers' RTs numerically on the computer

[3] In a more general multi-stage bargaining game, this would instead decrease delay costs due to additional bargaining rounds.

[4] By "good/bad offer" we mean an offer whose probability of being accepted is increasing/decreasing with deliberation.

screen before sellers made their second offers, to carefully test whether subjects knew what information to infer from RT. This was an idealized setting, as it ensured that subjects were paying attention to RT but were not cued about how to use it.[5] In this experiment we find clear evidence that our subjects used RT as predicted. In another experiment, we let subjects bargain without access to RT before bargaining with access to RT. In between we made a change to a single line in the instructions: '…the seller will now see the buyer's decision in real time'. Here we also see clear evidence that our subjects used RT as predicted.

In both experiments, we find that buyers were fast to reject bad offers and slow to reject good offers. Like our earlier example, on average a price of 40 was rejected faster by a buyer with a value of 50 than by a buyer with a value of 60. Sellers used this information against the buyers by offering smaller price drops when their first offers were rejected slowly. In the same example, the seller might offer a second price of 30 to the slow buyer but a price of 20 to the fast buyer. Buyers responded by speeding up their rejections when they knew that their RTs were being observed. Despite this adjustment, buyers' RTs still reflected the surplus from the offers.

This last result is important, as it indicates that RTs continue to convey useful information even when they are a strategic element of the bargaining process. This is consistent with the data from naturally occurring market data (Cotet and Krajbich, 2021). Those data consist of millions of eBay bargaining transactions from 2012 to 2013 and reveal that sellers' RTs reflect their strength of preference even with experienced sellers.

[5] We believe that this explicit presentation is more representative of online interactions, where timestamps are routinely displayed.

On the other hand, we find mixed evidence that agents pay attention to RT. In a condition where subjects bargained with access to RT right from the start, we find little evidence that they used RT strategically or used others' RT to infer private information. The lack of strategic RT use in this test could be due to violations of either of the assumptions above. It could be that our subjects did not know what information to infer from RT, or it could be that RT simply wasn't salient enough in that treatment. The other experimental treatments provide substantial evidence for the latter. To further address this issue, we conducted an online survey about bargaining behaviours. Here, nearly half of the respondents indicated that they pay attention to RT when bargaining.

Overall, our results clearly indicate that RT indeed reflects strength of preference in strategic situations and that agents respond to this information in the expected direction. However, our results also indicate that inexperienced agents may not know to pay attention to RT or might not be proficient at estimating it.

2 Literature

There is a long history of experiments in bargaining. Many early bilateral bargaining experiments reported that basic theoretical models fail to predict outcomes in many simple settings (Ochs and Roth, 1989; Güth and Tietz, 1990; Rapoport *et al.*, 1990; Weg *et al.*, 1990; Bolton, 1991; Palfrey and Rosenthal, 1991; Kennan and Wilson, 1993; Hoffman *et al.*, 1994; Babcock and Loewenstein, 1997), including cases with asymmetric information (Forsythe *et al.*, 1991; Rapoport *et al.*, 1995; Reynolds, 2000; Cason and Reynolds, 2005). Some studies have reported a significant difference in outcomes between face-to-face and anonymized bargaining (Radner and Schotter, 1989; Valley *et al.*, 2002;

Kagel and Roth, 2016), suggesting an important role of communication and non-verbal signals; these signals potentially include RT. In recent years, using process data such as chat logs and RT has become more common as these data provide a window into the decision-making processes during bargaining (Bhatt *et al.*, 2010; Bradfield and Kagel, 2015; Camerer *et al.*, 2019).

Here we use the bargaining setting merely as an illustration of a more general phenomenon: RT can reveal private information such as preferences or beliefs and thus can be used strategically. The link between RT and preferences has been established through the application of "sequential sampling models" (SSM), often also referred to as drift-diffusion (DDM) or evidence-accumulation models (Bogacz et al. 2009; Gold and Shadlen 2001; Ratcliff and McKoon 2008; Usher and McClelland 2001), which have been used in cognitive psychology for decades, primarily to model perception and memory. SSMs have also been successfully used to model choices and RTs in a wide variety of economic domains, including risk and uncertainty (Busemeyer, 1985; Busemeyer and Townsend, 1993; Hunt *et al.*, 2012; Glickman *et al*., 2019; Zilker and Pachur, 2021; Zhao *et al.,* 2020), intertemporal choice (Dai and Busemeyer, 2014; Rodriguez *et al.*, 2014; Zhao *et al.,* 2019), social preferences (Hutcherson et al. 2015; Krajbich et al. 2015a; Krajbich et al. 2015b), and food and consumer choice (Krajbich *et al.*, 2010, 2014; Milosavljevic *et al.*, 2010; Krajbich and Rangel, 2011; De Martino *et al.*, 2013; Philiastides and Ratcliff, 2013).

From a theoretical point of view, the DDM, under certain conditions such as costly time, constant difficulty, and stochastic utility sampling, is the optimal choice policy (Webb, 2018; Woodford, 2014; Caplin and Martin, 2016; Tajima *et al.*, 2016;

Fudenberg *et al.*, 2018). Other approaches such as the directed cognition model have also proposed links between RT and choice (Gabaix and Laibson, 2005; Gabaix *et al.*, 2006; Chabris *et al.*, 2009). A small number of studies have shown that private information (e.g. preferences) can be extracted from RTs alone, without using choices (e.g. in a situation where choices are uninformative). Chabris et al. (2009) and Chabris et al. (2008) use the directed cognition model and RT distributions at the group level to estimate aggregated intertemporal preferences, with a structural estimation approach. Schotter and Trevino (2021) use the longest RTs to estimate threshold strategies in a global game and predict subjects' choices out of sample (in the second half of the experiment), beating the theoretical equilibrium prediction. Konovalov and Krajbich (2019) show that it is possible to use the DDM and more simple methods to recover individual utility-function parameters from RTs alone, without using choice data, in three different choice domains (intertemporal choice, risky choice, and a dictator game). Frydman and Krajbich (2022) show that people can use others' RTs to extract beliefs in a social learning (information cascades) setting. Cotet and Krajbich (2021) show that eBay sellers' RTs reflect the quality of the offers they receive from potential buyers. Finally, Echenique and Saito (2017), Alós-Ferrer, Fehr, and Netzer (2021), and Alaoui and Penta (2022) provide theoretical frameworks for inference of preferences from RT. Others, such as Wilcox (1993) and Moffatt (2005), have investigated other factors influencing RTs such as complexity, with people taking more time to make a choice if a lottery environment involves more outcomes.

Several recent studies have investigated the role of time passing in economic decisions, specifically in experimental settings. A few studies have shown that some types of preferences such as time discounting might be related to subjective time perception (Read, 2001; Prelec, 2004; Zauberman *et al.*, 2009; Bradford *et al.*, 2019). Others have demonstrated that time pressure can affect decisions in variety of settings, including bargaining, social, and risky decision making (Kocher and Sutter, 2006; Kocher *et al.*, 2013, 2019; Lindner and Sutter, 2013; De Paola and Gioia, 2016; Chen and Krajbich, 2018; Karagözoğlu and Kocher, 2019). Despite these efforts, examinations of RT and time pressure remain uncommon in economic experiments.

Our paper builds on the earlier studies, hypothesizing that RTs can also reveal buyers' private values (assigned by the researcher) in strategic settings. Although there is a small literature in psychology on how people perceive others' RTs in social dilemmas and hypothetical bargaining situations (Critcher *et al.*, 2013; Van de Calseyde *et al.*, 2014; Evans and van de Calseyde, 2017), we are not aware of any research that investigates the strategic manipulation of RT.

It is important to emphasize that our approach is different from the literature on strategic delays, impasse, and the role of deadlines in bargaining (e.g. Admati and Perry 1987; Cho 1990; Cramton 1992; Ghosh 1996; Babcock and Loewenstein 1997; Bac 2000; Gneezy, Haruvy, and Roth 2003; Winoto, McCalla, and Vassileva 2007). In such settings, one of the players can strategically delay their acceptance decision across bargaining periods (leading to a decrease in the pie size or players' profits but signaling the player's type). We consider buyers' RT in a single decision (period), and this RT does not directly affect players' payoffs or anything else in the game, except for what it signals to others.

2 Predictions

2.1. Baseline equilibrium predictions

The simple model of one-sided bargaining with two agents and asymmetric information is well-established in the literature. The setting we implement has the following theoretical predictions (Sobel and Takahashi, 1983).

There is a single buyer with a value (reservation price) of v, and a single seller that owns a good with a reservation price of 0. Both agents have a discount factor of $\delta = 0.8$, which is common knowledge, and only the buyer knows her value. The value is uniformly distributed on the interval $[0,1]$, which is common knowledge. The game proceeds as follows. In stage one, the seller makes a price offer p_1, and the buyer either accepts or rejects it. Upon acceptance, the seller receives a payoff of p_1, and the buyer receives a payoff (surplus) of $v - p_1$. If the buyer rejects the offer, stage two starts. The seller makes a second offer p_2, and the buyer again has an opportunity either to accept or reject it. If the buyer rejects this offer, both receive zero payoffs, and the game ends. If the buyer accepts the offer, she receives $\delta(v - p_2)$, and the seller receives δp_2.

We will use the perfect Bayesian equilibrium as a solution concept following Sobel and Takahashi (1983) (see their Section 3, Theorem 3 and the follow-up example that matches our setting, for a discussion of its derivation and uniqueness[6]). The seller sets the first price to

[6] Another potential equilibrium is the commitment equilibrium, where the seller simply announces prices of 0.5 and 0.5 for both stages. As Sobel and Takahashi point out, this equilibrium is only viable if the buyer believes that the seller would not deviate from the announced strategy by lowering her price in the second stage. All trade will then happen only in stage one; given that in our experimental setting two stages are forced, it is unlikely that such commitment would be credible.

$$p_1^* = \frac{(2-\delta)^2}{2(4-3\delta)} = 0.45. \quad (1)$$

The buyers with the cutoff type $\bar{v} = \frac{2-\delta}{4-3\delta} = 0.75$ are indifferent between accepting and rejecting the offer, with all $v > \bar{v}$ accepting it (25% of all buyers). The rest of the buyers reject the offer and proceed to the second stage, where the sellers update their beliefs and make the equilibrium price offer of

$$p_2^* = \frac{2-\delta}{2(4-3\delta)} = 0.375. \quad (2)$$

Buyers with values above 0.375 accept this offer, while the rest reject it and earn nothing. The seller's expected profit is 0.225.

2.2. Baseline RT predictions

Let us assume that each buyer's decision evolves in continuous time. We assume that the buyer first sees the good and the initial price simultaneously at time t = 0. The buyer then attempts to learn her value v for the good and the price p_1, accumulating evidence gradually over time. Following the notation in Fudenberg, Strack, and Strzalecki (2018), this accumulated evidence follows a drift-diffusion process denoted as:

$$Z_t = \mu t + B_t, \#(3)$$

$$Z_0 = 0. \#(4)$$

Here B_t is standard Brownian motion and μ is the rate of evidence accumulation, known as the drift rate, which is a function of both v and p_1:

$$\mu(v, p_1) \in \mathbb{R}. \#(5)$$

This process continues until Z_t crosses a boundary b_t, with $(b_t \geq 0)$, triggering an action, $a \in \{accept, reject\}$. More specifically, the buyer chooses $a = accept$ if $Z_\tau = b_\tau$ and $a = reject$ if $Z_\tau = -b_\tau$, where the response time (RT) is $\tau(b, \mu) = \inf\{t: |Z_t| = b_t\}$.

The distribution T of τ depends on two factors, the endogenous boundary b_t and the exogenous drift rate μ. Narrower boundaries (smaller b_t) reduce the evidence needed to decide and result in shorter τ. Higher magnitude drift rates (larger $|\mu|$) reflect stronger evidence in favor of the better option and result in shorter τ. We assume only that b_t is continuous over time, though a standard assumption in the literature is that it is constant over time (see Appendix E). The latter is the assumption in the standard drift-diffusion model (DDM).

Given this setup, we further assume that $\mu(v, p_1)$ is strictly increasing in *v* and strictly decreasing in p_1. That is, the higher a buyer's true value, the more likely she is to accept, and the higher the price, the less likely she is to accept. $\mu = 0$ corresponds to the case where the buyer is equally likely to accept or reject (assuming an unbiased starting point $Z_0 = 0$).

From this point onwards, we only consider cases where the buyer rejects the offer, because that is the only scenario in which the seller has another decision to make.

PROPOSITION 1. *Given two drift rates μ_1 and μ_2 and the corresponding distributions of stopping times T_1 and T_2 (conditional on $a = reject$): $0 < \mu_1 < \mu_2 \Leftrightarrow T_1 < T_2$ in a first-order stochastic dominance sense.*

PROOF: See Appendix D.

Proposition 1 tells us that for any choice of b_t, buyers with different values of μ will exhibit different RT distributions. Thus, by observing a buyer's rejection time, the seller can estimate v (knowing the price p_1 that they offered and the fact that $\mu(\cdot)$ increases in v). A sufficient condition for this to work is the standard DDM assumption that agents choose their boundary b_t before the decision, i.e., b_t does not depend on v or on p_1.

From this setup, we make the following predictions for the baseline case where buyers' RT are hidden from the seller, or when agents are unaware that the other party is aware of RT:

HYPOTHESIS 1. *When buyers' RT are hidden from the sellers, their RT will be increasing with v and decreasing with p_1 for rejections, while RT will be decreasing with v and increasing with p_1 for acceptances. If sellers are given access to the buyers' RT (without the buyers' awareness), they will make lower second offers to buyers with faster RT. If buyers can manipulate their RT (without the sellers' awareness), they will reject with faster RT, to signal a lower v to the seller.*

2.3. Binary RT model

To proceed with an analysis of the strategic RT considerations we first simplify the model by assuming that the buyer has a binary choice to either quickly learn whether their value is above or below the price or slowly learn their precise value, which incurs a cost. Using this simplified model, we re-derive predictions for when RT is costly but unobservable and then derive comparative statics for when RT is made observable.

The game proceeds as follows. The seller announces the first price p_1. After observing the price, the buyer immediately learns her coarse type, specifically whether her value is above or below this price ($v \leq p_1$ or $v > p_1$). The buyer then has four possible actions involving two possible RT (fast or slow) and two possible actions (accept or reject). She could either (1) accept or (2) reject the offer with a fast RT, or incur a cost $c > 0$ to learn her precise value v and then (3) accept or (4) reject with a slow RT. For the sake of tractability, we assume that the cost is common knowledge.

These assumptions are based on the idea that offers with negative surplus are "no-brainers"; there is no reason to accept them. Such decisions can be made very quickly (see Section 2.2), without time-consuming strategic considerations.

If the buyer rejects, the game proceeds to the second stage, where the seller offers a single price p_2. We assume that buyers know their true values by the second stage. In other details, the game is identical to the baseline model.

Let us consider two possible cases: first, when the seller cannot observe the buyer's RT (fast or slow); second, when RT is observable.

2.3.1. Unobservable RT

In the first case, the seller does not observe the buyer's RT. Thus, the buyer's decision of whether to learn their value is dictated by the cost *c*.

If $c > 0$, it is a dominant strategy for low-value buyers ($v \leq p_1$) to always reject fast. The baseline equilibrium with high-value buyers responding slowly and learning their values and the seller setting the prices to $p_1 = 0.45$ and $p_2 = 0.375$ can be sustained given $0 \leq c < 0.01$ (for $\delta = 0.8$, and $v \sim U[0,1]$ used in the experiment; we will ignore the borderline cases).[7] If $c > 0.01$, high-value buyers have the incentive to deviate and reject fast (in expectation, choosing to go slow yields $0.29 - c$, fast acceptance yields 0.275, and fast rejection yields 0.28).

There are no equilibria where all buyers (both high- and low-value) reject the first offer. If they did, it would imply an optimal second-stage price of 0.5, in which case the seller would have an incentive to offer a first-stage price that entices some of the high-value buyers. For example, a first-stage price of 0.5 would be more attractive to both the sellers and the buyers with values greater than 0.5. For $\delta = 0.8$, there are also no equilibria where high-value buyers always accept fast, as then the optimal low p_2 creates an incentive for them to reject fast.[8]

We then consider the following mixed-strategy equilibrium, where high-value buyers mix between fast acceptance and fast rejection. In the first stage, the seller offers p_1^*. All buyers with $v \leq p_1^*$ reject the offer with fast RT. All buyers with $v > p_1^*$ reject the offer (with fast RT) with probability α and accept the offer (with fast RT) with probability $(1 - \alpha)$. In the second stage, the seller offers a single price p_2 (given that RTs and types are unobservable).

[7] To keep high-value buyers (their value in expectation is $\frac{1+p_1}{2}$) from deviating, their expected payoff from fast rejection (and fast acceptance) should be lower than the expected payoff from going slow: $\delta(\frac{1+p_1}{2} - p_2) < \frac{1-\bar{v}}{1-p_1}\left(\frac{1+\bar{v}}{2} - p_1\right) + \frac{\bar{v}-p_1}{1-p_1}\delta\left(\frac{\bar{v}+p_1}{2} - p_2\right) - c$.

[8] For some values of δ, the separating equilibrium exists. If only low-value buyers ($v < p_1$) reject fast, the seller sets the second price for fast rejections to $p_2 = p_1/2$. The seller would then maximize $\{(1 - p_1)p_1 + \delta p_1 \frac{p_1}{2}\}$, yielding $p_1^* = \frac{1}{2-\delta}$. High-value buyers with $E(v) = \frac{1+p_1}{2}$ would not deviate to fast rejection if $E(v) - p_1 > \delta(E(v) - p_2)$. This condition only holds if $\delta < 0.38$.

Upon observing a fast rejection, the seller could be facing both high- and low-value types. The seller then maximizes the following utility function:

$$\max_{p_2}\{(1-\frac{p_2}{p_1+\alpha(1-p_1)})\cdot p_2\},\#(6)$$

where $\frac{p_2}{p_1+\alpha(1-p_1)}$ is the probability that the second offer will be rejected. This leads to the optimal second-stage price:

$$p_2=\frac{\alpha+p_1(1-\alpha)}{2}.\#(7)$$

Now let us consider the first-stage offer. High-value buyers (with value $v>p_1$, i.e. expected value $\frac{1+p_1}{2}$) are playing a mixed strategy and thus indifferent between accepting and rejecting the first offer:

$$\frac{1+p_1}{2}-p_1=\delta\left(\frac{1+p_1}{2}-p_2\right).\#(8)$$

Substituting p_2 from (7) into (2), we solve for the equilibrium first-stage price

$$p_1=\frac{1-\delta(1-\alpha)}{1+\alpha\delta}.\#(9)$$

Now, the seller is maximizing their profits by choosing the first-stage price p_1:

$$\max_{\mathrm{p}_1}\{(1-p_1)(1-\alpha)p_1+\delta((1-p_1)\alpha p_2+(p_1-p_2)p_2)\}.\#(10)$$

By maximizing this expression (taking a derivative with respect to p_1) and then substituting expressions (7) and (9), we obtain the equilibrium value of α:

$$\alpha^*=\frac{\delta^3-4\delta^2-\delta+2}{2\delta^3-2\delta^2-2\delta}.\#(11)$$

Substituting (11) into (9) and (7) yields the equilibrium value for the first offer

$$p_1^*=\frac{1+\delta^2}{(3-\delta)(1+\delta)}\#(12)$$

and the second offer

$$p_2^* = \frac{1-2\delta}{(\delta-3)\delta}. \#(13)$$

In our setting, with $\delta = 0.8$ and $v \sim U[0,1]$, the (rounded) equilibrium values are $\alpha^* = 0.46$, $p_1^* = 0.41$, $p_2^* = 0.34$, with 31% of first offers being accepted. Interestingly, this equilibrium yields roughly the same profit to the seller as the original case (0.225 rounded). This equilibrium will only be sustained if high-value buyers do not have the incentive to deviate and go slow, learning their precise value. This requires the cost of time to be high enough ($c > 0.015$)[9], which also rules out mixed-strategy equilibria where high-value buyers randomize between going fast and slow.

To sum up, a positive cost of time can create an incentive for high-value buyers to speed up their decisions and randomize between acceptance and rejection, although the original equilibrium can still be sustained if the cost of time is low enough. Thus, introducing a cost of time can lead to deliberately stochastic choice.

2.3.2. Observable RT

Now, let us consider the case where the seller observes the buyer's RT. If the buyer rejects, the game proceeds to the second stage, where the seller makes an offer p_2^S to slow buyers and p_2^F to fast buyers. Again, we assume buyers know their true values by the second stage.

[9] Given the price offers in equilibrium, $\bar{v} = 0.71$ (after learning their precise value, only buyers with $v > \bar{v}$ would accept the first offer). In expectation, high-value buyers receive higher surplus from going fast if $\delta\left(\frac{1+p_1}{2} - p_2\right) > \frac{1-\bar{v}}{1-p_1}\left(\frac{1+\bar{v}}{2} - p_1\right) + \frac{\bar{v}-p_1}{1-p_1}\delta\left(\frac{\bar{v}+p_1}{2} - p_2\right) - c$. This inequality holds if $c > 0.015$.

Is it possible that the baseline equilibrium without the cost of time is still sustained? Let us consider the separating case first. Suppose that in equilibrium high-value buyers go slow and learn their true values, while low-value buyers always reject fast. If only low-value types (with $v \leq p_1$) reject fast, the optimal p_2^F is $\frac{p_1}{2}$, and the optimal p_2^S must be higher than p_1. In this case, no high-value buyer has the incentive to reject after learning their value, as they can simply accept p_1 instead. Knowing that their only slow option is to accept, buyers would prefer to accept fast, saving them the cost c. However, high-value buyers cannot always accept fast in equilibrium, as the low p_2^F creates an incentive to reject fast (given $\delta = 0.8$, see footnote 9).

Let us now consider the pooling cases. Suppose that, as in the original equilibrium (with $c = 0$), the seller offers $p_1 = 0.45$, all buyers go slow (incurring c), learn their precise values, high-value buyers with $v > 0.75$ accept the first offer, and the second-stage price $p_2^S = 0.375$ is offered to all the remaining buyers. Off the equilibrium path the seller must set the price for fast buyers to $p_2^F > p_2^S + \frac{c}{\delta}$, so that low-value buyers have no incentive to deviate and reject fast[10]. Given $\delta = 0.8$, for even a small cost ($c > 0.005$) this price makes fast rejection less attractive to high-value buyers (yielding less than the $0.28 - c$ from going slow)[11] than fast acceptance (yielding $\frac{1+p_1}{2} - 0.45 = 0.275$), so the seller should not expect to face a high-value buyer upon observing a fast rejection (intuitive criterion). This would make the high price following fast rejection a

[10] To keep low-value buyers (whose value in expectation is $p_1/2$) from deviating, their expected payoff from rejecting fast should be lower than the expected payoff from going slow: $\delta\left(\frac{p_1}{2} - p_2^F\right) < \delta\left(\frac{p_1}{2} - p_2^S\right) - c$.

[11] Given $p_2^F > p_2^S + \frac{c}{\delta}$, the buyer's expected utility must be lower than expected value ($\frac{1+p_1}{2} - p_2^S - \frac{c}{\delta}$).

non-optimal choice for the seller, with the optimal price after fast rejections being $p_2^F = p_1/2$. This would create an incentive for low-value buyers to deviate from slow to fast rejection, and the equilibrium would not be sustainable.

However, a version of the mixed strategy equilibrium described in Section 2.3.1 can be sustained with observable RTs. As shown above, the equilibrium strategies would be $\alpha^* = 0.46$, $p_1^* = 0.41$, $p_2^F = 0.34$, with 31% of first offers being accepted. Off the equilibrium path, the seller believes, upon observing a slow decision, that she is facing a high-value buyer with probability 1 and thus sets $p_2^S = \frac{1+p_1}{2}$. Note that this profile would be an equilibrium under any cost of time c.

To summarize, introducing the cost *c* can force high-value buyers to respond fast and randomize between acceptance and rejection. If their RTs are unobservable by the seller, this would only happen if the cost of time is high enough ($c > 0.015$). If it is low ($c < 0.01$), the baseline equilibrium can still be sustained. If RTs are observable, the original equilibrium is not stable, and the mixed strategy equilibrium can be possible under all $c > 0$. We formalize our predictions in the following hypotheses.

HYPOTHESIS 2A. *Assume that buyers incur a high cost* ($c > 0.015$) *to learn their values. Relative to the baseline predictions (Section 2.1), we expect lower first and second offers from the sellers and higher acceptance rates from the buyers, in both observable and unobservable RT conditions. Both low- and high-value buyers respond fast.*

HYPOTHESIS 2B. *Assume that buyers incur a low cost* $(0 < c < 0.01)$ *to learn their values. In the unobservable RT condition, we expect high-value buyers to make slow decisions, low-value buyers to make fast decisions, and sellers to make offers that correspond to the baseline equilibrium (Section 2.1). In the observable RT condition, we expect all buyers to make fast decisions, and sellers to make lower offers than at baseline.*

Let us also consider a special case with naive buyers who do not consider the possibility of manipulating their RT or are not able to do so. Let us restrict the set of strategies and assume that high-value buyers $(v > p_1)$ always respond slow (and always incur the time cost), and low-value buyers always respond fast, so that sellers can implement price discrimination. Given that only low-value buyers respond fast, the optimal p_2^F is $p_1/2$. High-value buyers would never reject the first offer in equilibrium after going slow, as the optimal second-stage price for slow buyers would be higher than p_1 ($\frac{1+p_1}{2}$ to be precise). Given that high-value buyers always accept, and low value buyers always reject, the seller seeks to maximize:

$$\max\left\{(1-p_1)p_1 + \delta\frac{p_1}{2}\frac{p_1}{2}\right\}.$$

This yields the optimal first stage price $p_1 = 0.625$, $p_2^F = 0.3125$, $p_2^S = 0.8125$. 37.5% of buyers accept the first offer, and the seller achieves higher profits from price discrimination, earning 0.3125.

HYPOTHESIS 2C. *Assume that buyers are naïve and so cannot manipulate their RT, and sellers are aware of this. Relative to the baseline case, we expect sellers to* *increase their first price offers, decrease their second price offers following fast rejections, and off the equilibrium path, increase their second price offers following slow rejections. High-value buyers make slow decisions (acceptances) and low-value buyers make fast decisions (rejections).*

3 Experimental design

3.1 Experimental overview

We conducted two experiments, both utilizing a simple one-sided bargaining game based on the model described in Section 2.1, which was first discussed in Sobel and Takahashi (1983) and tested in the laboratory by Reynolds (2000). An overview of the experiments is presented in Table 1.

In the "Live" experiment, each subject bargained in both (H)idden and (V)isible treatments (order counterbalanced across sessions/subjects). In the V treatment, sellers were notified as soon as the buyer decided, while in the H treatment, sellers were not notified when the buyer decided. Thus, in the V treatment, both players knew that the buyer's RT was observable. Each bargaining treatment also had a subsequent selling task in which each subject assumed the role of seller and tried to guess the values of various buyers from (only) the preceding bargaining task, based on the offers they rejected and their rejection RT. In summary, the Live experiment consisted of four tasks: two bargaining tasks and two accompanying selling tasks.[12]

[12] Before proceeding to this second half of the experiment, subjects received brief instructions explaining that they were now going to repeat what they had done before, but this time either observing the buyers in real time (2V), or not (2H). The purpose of having subjects go through both treatments was to allow us

In the "Explicit-RT" experiment, each subject bargained only in the H(idden) treatment. Like in the Live experiment, there was a subsequent selling task. However, in this experiment's selling task, we provided subjects with a written measure of the time that it took each buyer to reject their offer (rather than estimating it live). Finally, this experiment had a third buying task in which each subject assumed the role of buyer and tried to guess the offers made by sellers in the preceding selling task, based on the buyer's value, the rejected offer, and the rejection RT. Here again, we provided RT to the subjects in writing.

We programmed both experiments in zTree (Fischbacher, 2007). Experimental sessions lasted for about an hour. We read instructions (see Appendix F) out loud before every part, while subjects followed along with a paper copy. Each part started with two unpaid practice periods. The RTs were recorded using zTree's EventTime function. RTs in the first bargaining part were restricted to 11 seconds.[13] Less than 2% of the trials exceeded the limit.

to conduct within-subject tests. An unintended but fortunate consequence was to highlight the presence of RT in 2V.

[13] The intended number was 10 seconds, which was increased due to a software error.

Table 1. Structure of experimental treatments.

Experimental Overview			
	Live experiment		Explicit-RT experiment
Timeline	"Hidden first" 48 subjects	"Visible first" 42 subjects	All 66 subjects
1	Bargaining 1H: Bargaining game with RT hidden from sellers	Bargaining 1V: Bargaining game with RT visible to sellers	Bargaining 1H: Bargaining game with RT hidden from sellers
2	Selling 1H: Seller's task with data from Bargaining 1H	Selling 1V: Seller's task with data from Bargaining 1V	Selling 1H: Seller's task with data from Bargaining 1H
3	Bargaining 2V: Bargaining game with RT visible to sellers	Bargaining 2H: Bargaining game with RT hidden from sellers	Buying 1H: Buyer's task with data from Selling 1H
4	Selling 2V: Seller's task with data from Bargaining 2V	Selling 2H: Seller's task with data from Bargaining 2H	

Notes: The Live experiment involved two types of sessions: in the "hidden first" sessions, buyers' RTs were hidden from sellers in block 1 of the experiment and visible to sellers in block 3, whereas in the "visible first" sessions, block 1 had buyers' RTs revealed to sellers, and in block 3 RTs were hidden. After each bargaining game, subjects completed an accompanying selling task, with RTs visible (blocks 2 and 4). The Explicit-RT experiment mirrored the "hidden first" sessions from the Live experiment but had a distinct buying task in the third block.

We recruited subjects from the Ohio State University economics subject pool. In the Live experiment, 90 subjects participated in the experiment: 48 in three "Visible first" sessions and 42 in three "Hidden first" sessions. In the Explicit-RT experiment, 66 subjects participated in four sessions.

In each experiment, subjects received a $5 show-up fee and the sum of their earnings from each part in the experiments, earning $18.60 and $15.30 on average, respectively. All subjects gave written consent, and studies were approved by the OSU Institutional Review Board (2013B0583).

3.2 Tasks common to both experiments

Both experiments contained a bargaining task and a selling task. Next, we describe the common features of these two tasks, while noting any differences between the two experiments.

3.2.1 Bargaining task

The bargaining task consisted of 20 periods. In each period we randomly and anonymously (stranger matching) assigned subjects into pairs. Each pair played the following two-stage bargaining game (as laid out in Section 2.1). First, sellers, who had a "voucher" that was of zero value to them, made a price offer p_1, limited to the range between 0 and 100 experimental currency units (ECU; 7 ECU = $1), to their buyer.

Their decision time was unrestricted. Once all the sellers made their offers, the buyers learned their values v for the vouchers (drawn from a uniform integer distribution from 0 to 100) and their seller's price offer p_1.

Buyers had 11 seconds to accept or reject the offer by clicking on two corresponding buttons.[14] In both H(idden) and V(isible) treatments, the buyers' decisions were only revealed at the end of the 11 seconds, regardless of when they decided. However, in the V treatment (Live experiment only), once a buyer clicked on the accept/reject button, the paired seller immediately saw a message notifying them that the buyer had decided (but not what they had decided). In the H treatment there was no such notification, so here the 11-second period served to conceal buyers' RT.

Instructions for the V and H treatments were the same except that in the V treatment it was additionally noted that 'Once the buyer clicks one of the buttons, the paired seller will see a message on their screen that says "The buyer has made their decision."' Buyers and sellers had the same instructions and so both were aware of all design features.

If the buyer accepted the offer, they received a surplus equal to $v - p_1$, and the seller received p_1. If the buyer rejected, both subjects proceeded to the second stage, where the seller made another offer to the buyer. If the buyer accepted, both received payoffs discounted by $\delta = 0.8$, and if the offer was rejected, both received 0 profits. In all other aspects, stage 2 of the bargaining process was analogous to stage 1, and there were no differences between treatments.

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[14] We chose this timing based on unlimited-time pilot sessions where we established that this was (roughly) the shortest amount of time we could give people without it being a binding constraint. There was a timer on the screen, as is standard in Z-Tree experiments. Violating the time limit in either treatment carried a penalty of 5 ECUs. In practice this was a non-binding constraint; less than 1% of trials violated the time limit (11 trials total) and mean RTs ranged from 4 to 7 s (92% of buyer's choices were completed under 7 s). To help subjects avoid running out of time, in both conditions there was a timer in the right upper corner of the screen, as is the default in z-Tree and thus in most experiments.

At the end of each bargaining period, subjects learned their own profits, but buyers' values remained private. We then re-matched subjects into new pairs. Every subject kept their role (buyer or seller) throughout the 20 periods. At the end of the experiment, we randomly selected one period in this part for payment.

3.2.2 Selling task

After 20 periods of the bargaining task, all subjects next completed the selling task. We use Selling H and Selling V to denote the selling tasks following the H(idden) and V(isible) treatments respectively.

In the selling task, all subjects assumed the role of sellers playing against the database collected in the immediately preceding bargaining task (so in the H selling task subjects only observed decision from the H bargaining task; same for the V treatments). The selling task lasted 20 periods.[15] In each period, we presented subjects with a buyer's first-stage rejection decision.[16] However, in this case, sellers in both H and V treatments were able to observe the buyer's RT.[17] The task was to make a new offer to the buyer, which would be automatically accepted by the computer if it was lower than the buyer's value and rejected otherwise. At the end of each period, subjects

[15] Due to a technical issue, in two sessions of the Live experiment additional data were collected in the Selling task (8 periods of Selling 2H and 1 period of Selling 2V). All these data were included in the analyses and the results in these blocks are not affected if the additional data are excluded.

[16] The rejections were drawn with replacement, but we did not analyze repeat trials (1-2 per subject on average; this did not affect the results). Subjects never encountered trials that they were a part of in the bargaining task.

[17] In the Live experiment subjects observed the decisions in real time (with the message "The buyer is making a decision…" presented on the screen), just like in Bargaining V. In the "explicit-RT" experiment, we provided RT to the subjects in writing.

learned the buyer's true value and their own profit.[18] At the end of the experiment, we randomly selected one of these periods for payment.

These decisions had no impact on the original bargaining outcomes; earnings in this task were independent from earnings in the bargaining task and went solely to the sellers. We did this to ensure that buyers' RT from the H treatment could not affect their earnings. This method also has the advantage of removing any potential social-preference considerations.

The purpose of this task was to allow us to analyze sellers' price discrimination strategies against buyers who had strategic RT considerations (Bargaining V) and those who didn't (Bargaining H). We could not do this during the initial bargaining task since sellers did not have access to buyers' RT in the H treatment. This design is closely related to the designs from Alaoui and Penta (2016) and Alaoui, Janezic, and Penta (2020), which allow one to disentangle lower- and higher-order belief effects. In this case, the comparison between H and V treatments for the selling task allowed us to elicit the seller's second order beliefs about the buyers' strategic behaviour conditional on the buyers potentially manipulating their RT in the bargaining phase.

3.3 Buying task in the Explicit-RT experiment

In the third part of the Explicit-RT experiment, all subjects assumed the role of buyers playing against the database collected in the immediately preceding selling task. We call this task the buying task. The goal of the task was to test whether subjects in

[18] One might be concerned that subjects used this feedback to learn the relationship between RT and value. However, we see no evidence of learning across periods of the selling tasks (Figs. A3 & A6). Thus, it is unlikely that the improvement we see in the Live experiment (see Results) is due to this feedback.

the buyer role would manipulate RTs to receive a better offer, given that the seller was unaware of this possibility.

In each of 30 periods, subjects chose between one of two bargaining situations from the preceding selling task. We presented both situations on the screen at the same time, with buyers' values, first price offers, and RTs in both cases. The subjects' goal was to pick the better deal, based on the expected offer from the selling task. After selecting one of the situations, subjects learned the selling-task offer and received a surplus equal to the value minus the actual second price offer from that trial of the selling task.[19] At the end of the experiment, we randomly selected one of these periods for payment.

As in the selling task, these decisions had no impact on other subjects; earnings here were independent from earnings in the bargaining and selling tasks and went solely to the buyers. As in the selling task, we did this to ensure that a subject's decisions from the previous two parts could not affect their earnings in this part.

3.4 Bargaining survey

To assess whether people pay attention to RT outside of experimental settings, we also conducted a short online Qualtrics survey (N = 200) on Prolific using U.S. respondents who were paid a fixed $2. In this survey, there were a variety of questions about bargaining behaviours, as well as an attention check.[20] We excluded respondents who failed the attention check (n = 3) or who indicated that they never bargained as

[19] Note that we only selected trials generated from situations that the subject had not previously been a part of. The situations were drawn with replacement.
[20] See Appendix F for the full survey.

either a seller or a buyer (n = 18), leaving us with a total of N = 179 valid respondents. Our key question asked: 'If you bargain, what do you pay attention to when bargaining? (Select all that apply)'.[21] The possible answers included the size of the offer, the timing of the offer, words or phrases used, emotional state, facial expressions or body language, user ratings/profile, and demographics (see Appendix F for the full questionnaire). Subjects gave informed consent prior to starting the survey and the study was approved by the OSU Institutional Review Board (2022E0396).

4 Results

To preview the results, we find in all cases that buyers' RTs did reflect their values and first price offers, consistent with Hypotheses 1 & 2C. However, in the Explicit-RT experiment and the second half of the Live experiment, buyers' RTs were shorter in the V(isible) treatment, consistent with the RT manipulation in Hypothesis 2B.

In the Explicit-RT experiment and the second half of the Live experiment we observed several results for the sellers. Sellers' second price offers increased with buyers' RT whenever RT was visible, consistent with the price discrimination in Hypotheses 1 & 2C. Also, sellers' first price offers were higher in the V vs. H(idden) treatments, as in Hypothesis 2C (but contrary to Hypothesis 2B). Finally, in the selling task, sellers' offers were less responsive to RT in the V vs. H treatments, consistent with less price discrimination due to less informative RTs from the buyers (consistent with Hypothesis 2B but not 2A).

[21] The order of these answers was randomized across subjects.

In the first half of the Live experiment, both buyers and sellers behaved as if they were unaware of RT, suggesting that in some cases people may need to learn to pay attention to RT. An overview of the basic behavioural results can be found in Appendix B, Table B1.

4.1. Buyers' RT as a function of surplus

In line with Hypotheses 1 & 2C, buyers rejected the first offer more slowly (and accepted more quickly) as their surplus increased (Figure 1). Conditional on acceptance, this effect was reversed (Table B2). These effects occurred in both H and V treatments.

RESULT 1*. In all treatments, buyers' RTs were significantly increasing with value and decreasing with price for rejections (all* $p < 0.01$*), and significantly decreasing with value and increasing with price for acceptances (*$p < 0.001$*, Figure 1; Tables B2-3)*.

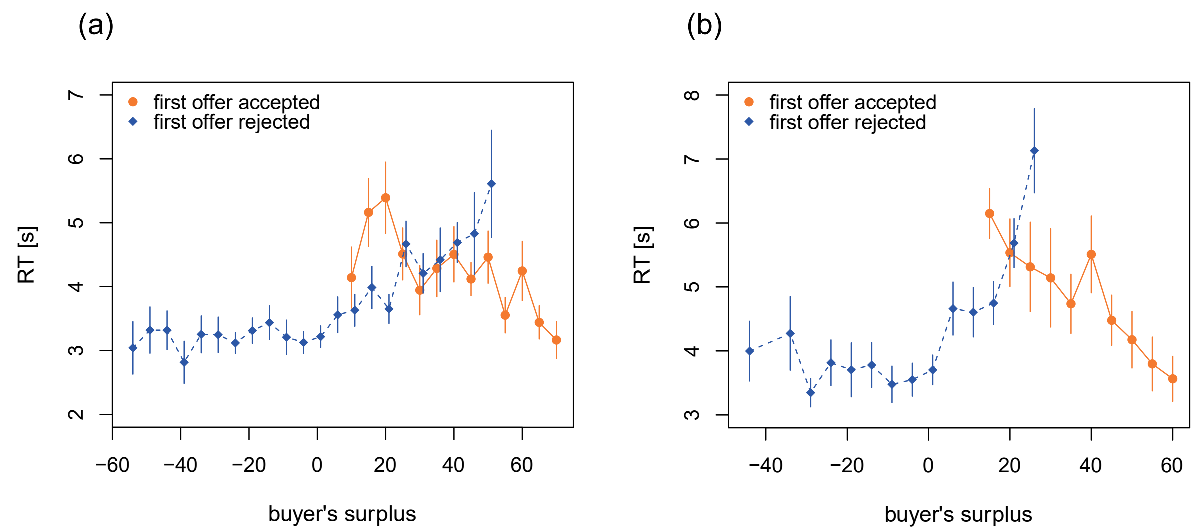

Fig 1. Response time (in seconds) as a function of buyer's surplus.

Notes: Buyer's surplus is value minus the first offered price, plotted in bins of size 5 for the bargaining task in (a) Live experiment (all treatments), and (b) Explicit-RT experiment. Bins with fewer than 10 subjects removed for display purposes. Bars denote s.e. clustered at the subject level.

As noted in Section 2.3, a likely first step in the buyer's decision is determining whether their value is above or below the price. To check for this, we included a dummy variable *easy* in the regressions, which was equal to 1 if the value was lower than the price (Table B3). Pooling all treatments, *easy* was indeed a significant predictor of RT.[22]

RESULT 2*. Across all treatments, buyers' RTs were significantly lower when the buyer's value was below the price (Live:* $p = 0.01$*, Explicit-RT:* $p < 0.001$*;Table B2-3).*

[22] About 68% of first offers were rejected, and out of all rejections 63% were "easy" rejections with negative surplus for the buyer in the case of acceptance (Table B1).

4.2. Sellers' second offers as a function of RT

The results so far indicate that a seller could potentially use a buyer's first rejection RT to infer their value. Next, we examine sellers' behaviour to see whether they conditioned their second price offers on the buyers' rejection RT. If buyers are naïve and sellers recognize that (as per Hypothesis 2C), sellers' second price offers p_2 would be monotonically increasing with buyers' RT in the V treatment of the bargaining task and in both V and H treatments of the Selling task. Alternatively, if Hypothesis 2A or 2B were correct, offers would be unrelated to buyers' RT in the V treatments and only related to RT in Selling H (as per Hypothesis 1).

We find the relationship between p_2 and RT in some cases but not others. In particular, we find it in the Explicit-RT experiment, as well as Bargaining 2V, Selling 2H, and Selling 2V of the Live experiment. We do not find it in Bargaining 1V, Selling 1H, or Selling 1V. In other words, we observe it in the second half of the Live experiment, but not the first half.

RESULT 3. *In the Explicit-RT experiment and second half of the Live experiment, sellers made higher second offers to buyers with longer RTs ($p < 0.001$) in both selling tasks and the Visible bargaining task, controlling for the first offer and other factors (Figure 2a-b and Table B4).*[23] *However, sellers did not display this behaviour in the first half of the Live experiment (Figure B2, Tables B5-6).*

[23] In the Explicit-RT experiment, 57/66 subjects exhibited a positive relationship between RT and second price (controlling for first price), and this was significant for 44/57 at $p < 0.1$ (with only 20 observations per subject); none of the negative coefficients were significant.

Naturally, there was no such positive correlation in the Hidden bargaining task of either the Explicit-RT experiment (p = 0.93, OLS fit, controlling for the first offer and period, Table B4), or the Live experiment (p = 0.18, OLS fit, controlling for the first offer and period, Table B4), as RTs were concealed from sellers (Figure 2). We also did not observe any significant difference between former buyers and former sellers.

We also suspected that sellers might be less inclined to use RT to inform their second offers in the V selling task compared to the H selling task, since buyers whose RTs were visible may have tried to manipulate them, reducing their informational value (as per Hypothesis 2B). To test this idea, we compared the selling tasks from the second half of the Live experiment.

RESULT 4. *In Hidden selling, sellers showed a significant relationship between their new offers and buyers' RTs ($p = 0.001$, Table B5, Figure 2d), while in Visible selling this relationship was marginally weaker ($p = 0.1$, Table B5, Figure 2d).*

These results confirm that sellers understood the relationship between RT and buyers' values and used that information to make better offers. When the relationship between RT and buyers' values was potentially distorted due to strategic behaviour by the buyers, sellers used the RT information less.

4.3. Sellers' first offers in Hidden vs. Visible bargaining

The results so far indicate that sellers could, and often did, use a buyer's rejection RT to infer their value and price discriminate against them with their second offers. If buyers were aware of this behaviour, they might try to manipulate their RT, reducing or eliminating its informational content. Next, we examine whether sellers seemed to anticipate this behaviour in the V treatment of the bargaining task. If Hypothesis 2B were correct, the sellers should lower their first offer anticipating buyers' RT manipulation. If Hypothesis 2C were correct and sellers believe that buyers are naïve, then sellers should raise their first offers to profit from price discrimination.

RESULT 5. *In the second half of the Live experiment, comparing treatments, sellers' first offers were significantly higher in Visible compared to Hidden bargaining (Table B1, 45.6 vs. 38.9, $p = 0.06$, Wilcoxon rank sum test at the subject level).*

This result is in line with Hypothesis 2C and suggests that sellers believed that buyers were naïve in their RT usage. We found no such difference in first offers in the first half of the Live experiment (45.2 vs 42.7, $p = 0.52$, Wilcoxon rank sum test at the subject level).

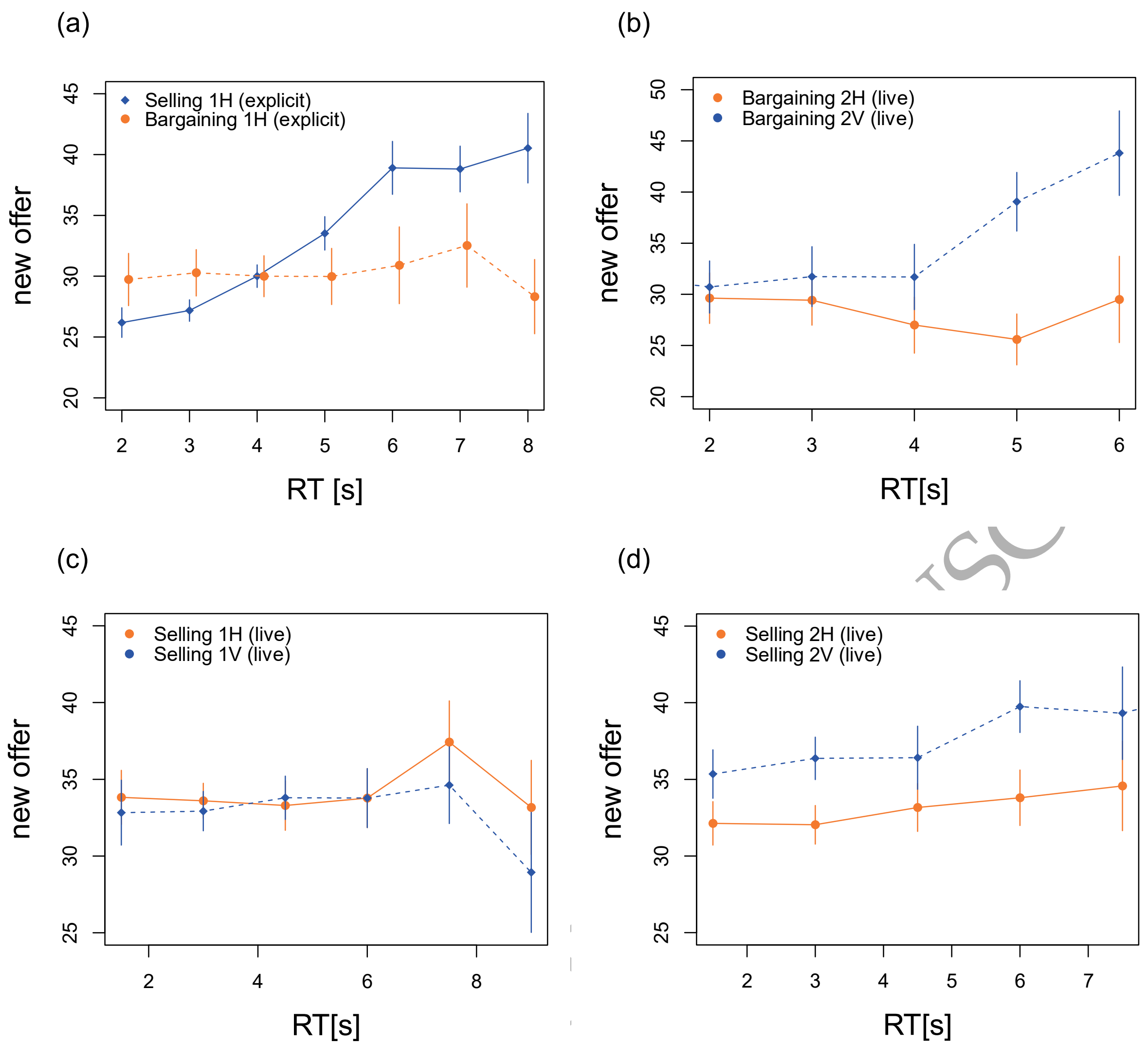

Fig. 2. New offer as a function of the buyer's RT across all conditions.

Notes: (a) Explicit RT experiment. Sellers made higher offers to slower buyers in the selling task, when their RTs were displayed on the screen, but not in the bargaining game, when buyers' RTs were hidden. (b) Live bargaining task, second half of the experiment. (c) Live selling task, first half of the experiment. (d) Live selling task, second half of the experiment. In all panels: bins with fewer than 10 subjects are removed for display purposes. Bars denote s.e. clustered at the subject level.

4.4. Buyers' manipulation of RT

As discussed in Section 2.3, if sellers are price discriminating against buyers using their RT, then buyers may react by choosing faster (as per Hypothesis 2B). Thus, we hypothesized that in the V treatment of the bargaining task buyers might choose faster than in the H treatment, and that in the buying task of the Explicit-RT experiment, buyers would choose situations with faster rejections (as per Hypothesis 1).

We first compare buyers' RT in the V vs. H treatments of the bargaining task in the second half of the Live experiment. We indeed find that buyers chose faster in the V treatment than in the H treatment, consistent with Hypothesis 2B.

RESULT 6. *In the second half of the Live experiment, when RT was visible, buyers' responses were faster (p = 0.01, Table B2) than when RT was hidden.*

In an additional robustness check, we also included buyers' RT from the first half as a baseline. There was a main effect of RT decreasing in the second half compared to the first half, regardless of treatment. However, this decrease in RT was significantly larger for buyers whose RTs were visible in the second half (Table B2).[24]

Next, we examined behaviour in the buying task of the Explicit-RT experiment. In each period, we presented buyers with two situations from the selling task, including the first price offer, the RT, and the value of the buyer. When they picked one of the situations, we revealed the second offer made by the seller in the selling task, and they received a surplus equal to $\max\ \{v - p_2, 0\}$.

To maximize their surplus, subjects should choose situations with a higher value, a lower first offer, and a shorter RT (as per Hypothesis 1). The goal of this treatment

[24] When considering the second stage, there were again no differences in terms of second-stage offers (p = 0.15), or second-stage acceptance rates (p = 0.23).

was to test whether subjects understood this. For a formal test, we regressed subjects' choices (1 = choose left) on the differences (left minus right) in displayed values ($\beta = 0.13$, $p < 0.001$), prices ($\beta = -0.06$, $p < 0.001$), and RTs ($\beta = -0.08$, p = 0.01)(Table B7).

RESULT 7. *In the buying task, buyers preferred faster rejections (p = 0.01, Table B7).*

Together, these results indicate that our subjects understood that shorter RTs would yield lower offers from sellers, netting them higher profits. Thus, our subjects appeared to be aware of the strategic advantage of making faster decisions.

4.5. Effects of RT information on earnings

We've established that sellers often price discriminate based on buyers' RT and that, in response, buyers may somewhat speed up their decisions. We next ask what impact this strategic behaviour has on buyers' and sellers' earnings. Based on the model in Section 2.3, we hypothesized that sellers would benefit the most from the availability of RT information (as predicted by Hypothesis 2C).

We first examine the H selling tasks where buyers had no reason to manipulate their RT. This should provide an upper bound on how well sellers can use RT information to inform their second offers.

RESULT 8. *In the Explicit-RT experiment, sellers earned more in the Hidden Selling task, where they had access to buyers' RT, than in the Hidden Bargaining task, where they did not (*$p = 0.003$*).*

Specifically, in the Explicit-RT experiment, sellers in the bargaining game earned 11.6 ECU on average (conditional on rejection and excluding a small number of trials where subjects rejected positive-profit offers in the second stage), and subjects in the selling task earned 14.1 ECU on average (the difference is significant at $p = 0.003$, Wilcoxon rank-sum test at the subject level).[25] In the first half of the Live experiment, sellers earned the same amount in bargaining H and selling H tasks (12.05 vs 12.2 ECU, $p = 0.98$, Wilcoxon rank-sum test at the subject level). In the second half of the Live experiment, sellers earned 11.3 ECU in the H selling task and 11.2 ECU in the H bargaining task (the difference was not statistically significant: $p = 0.85$, Wilcoxon rank-sum test at the subject level).

We also compared buyers' and sellers' earnings between the two treatments of the bargaining task in the second half of the Live experiment. We expected sellers to earn more in the V treatment because of their ability to price discriminate in the second stage. We also expected buyers to earn less in the V treatment for the same reason.

RESULT 9. *In the second half of the Live experiment, compared to Hidden, in Visible bargaining buyers earned less (21.1 vs. 18.4, p = 0.1, Wilcoxon rank sum test at*

[25] Former buyers earned more than former sellers (14.7 vs 13.5), but this difference was not statistically significant (p = 0.3, Wilcoxon rank-sum test at the subject level).

the subject level) and sellers significantly more, conditional on rejection, (Table B1, 13.7 vs. 10.7, p = 0.01, Wilcoxon rank sum test at the subject level).

Surprisingly, we observed the opposite result in the second iteration of the selling task: sellers earned more in the V treatment (19.7 ECU) than in the H treatment (15 ECU; $p < 0.001$). It appears that this may be due to sellers offering systematically lower prices in the H treatment to all RT levels (Figure 2d).

Thus, sellers earned more (and buyers less) when RT information was available. However, to some extent, when buyers were aware that their RT was observed, sellers were less able to increase their profits through price discrimination.

4.6 Generalizing beyond the lab

In this section, we address potential concerns about the generalizability of our experiments beyond the lab and describe the results of our online survey.

First, one could argue that RTs are difficult to estimate in practice, and thus they might pose little relevance for economic transactions outside of the laboratory. However, many online economic transactions (browsing, purchasing, and bargaining) have digital timestamps that allow for exact estimation of RT. For instance, bid data from eBay auctions confirm that timing is an important variable in strategic interactions (Roth and Ockenfels, 2002) and bargaining data from eBay indicate that RT conveys information about private values (Cotet & Krajbich 2021). Additionally, in brick-and-mortar transactions, RT may take a back seat to more salient measures of hesitation, such as verbal signals, facial expressions, or body language (Stillman *et al.*, 2020).

On the other hand, the field is not the ideal place to study the basics of how people interpret RT, since RT in the field can be contaminated by multi-tasking, the quality of internet connections, the ability to track time, etc. Lab experiments allow us to single out RT and focus on their uncontaminated effects on behaviour. They also allow us to exogenously manipulate the parameters of the interaction (such as the agent's true valuations, often unobservable in the field, see Cotet and Krajbich (2021)).

Second, one could argue that lab interactions are not "real" economic interactions, and that these effects would not be observed in the field or business practice. We wish to reiterate that these are real, incentivized economic transactions, which are just special, simple, tractable cases of a more general phenomenon (Svorenčík, 2020). Until proven otherwise, there is no cause for assuming that behaviour observed in the lab will not arise in the field.

There is also evidence from psychology that people consider RT to be an important factor when evaluating moral character (Critcher *et al.*, 2013), job offers, housing offers, and car mechanics (Van de Calseyde *et al.*, 2014). Similar to our Live experiment, Gates et al. (2021) had subjects watch videos of people making choices after delays of 3, 5, 7, or 9 seconds. They found that people inferred stronger preferences with shorter RT. Finally, in a field setting, Van de Calseyde, Keren, and Zeelenberg (2014) examined data from the TV talent show *The Voice*, where aspiring singers choose from a set of voice coaches who expressed interest in them during an audition. The authors found that contestants were much more likely to select coaches who were faster at indicating their interest.

Finally, to address this issue directly, in the context of bargaining, we conducted a short online survey about people's bargaining behaviours (as described in Section 3.4). Our key question asked: 'If you bargain, what do you pay attention to when bargaining? (Select all that apply)'.[26] The most common answer was 'the amount of the offer' (n = 153, 85%). However, the next most common answer was 'the time it takes the other person to respond to your offers' (n = 79, 44%).[27] Thus, nearly half of our respondents pay attention to timing when bargaining.

These survey results complement our lab results. The lab study uses a simulated bargaining environment to examine how agents strategically use RT when it is salient. The survey reveals the salience of RT in bargaining outside of the lab. Combined, these results suggest that many agents strategically use RT when bargaining, inferring stronger preferences from faster decisions. However, more work (ideally field experiments) is required to verify this particular use of RT outside of the lab.

5 Experimenter demand

Some readers may worry that the explicit presentation or mention of RT in our experiments could produce an experimenter demand effect, which is typically defined as a change in behaviour due to cues about what constitutes appropriate behaviour (Zizzo,

[26] The order of these answers was randomized across subjects.

[27] Other answers included "the specific words of phrases used by the other person" (n = 73, 41%), "the other person's user ratings or profile" (n = 70, 39%), "the other person's facial expressions or body language" (n = 59, 33%), "the other person's emotional state" (n = 35, 20%), and "the other person's demographics" (n = 14, 8%).

2010). It is critical to note that we did not in any way indicate to subjects how (if at all) they should use the RT information. In other words, even if we had explicitly told subjects to make use of RT, that would not have been sufficient to get them to use RT in a particular way. Specifically, we did not in any way signal to sellers that they should offer higher prices in response to slower rejections, nor did we signal to buyers that they should speed up their rejections. Subjects had to figure this out on their own. There are many plausible alternatives for how agents could interpret and manipulate RT.

In the Live experiment, RT was not at all salient; experiments in zTree display on-screen timers by default. The fact that sellers could observe buyers' RT was only mentioned in a single sentence of the "Visible RT" instructions (without any indication of why this might be important). This line in the instructions was necessary to let subjects know that they could observe their partners' RT.

In the Explicit-RT experiment, RT was very salient. The reason for this is that we felt that it was important to study how people respond to RT under idealized conditions. While we are interested in the link between subjects' *perceptions* of RT and their resulting behaviour, we can only observe the actual RT. Thus, our aim here was to eliminate any possible differences between actual and perceived RT. To put it simply, we tried to eliminate noise due to how well subjects track the passing of time (Eagleman, 2008).

This experimental technique is analogous to what is often used in studies on risk, where researchers explicitly present lottery probabilities because they are interested in how people respond to probability information, not how people extract probabilities from the environment (although there is a literature comparing experienced versus explicit probabilities (Hertwig *et al.*, 2004)). Virtually all of the risk-attitude elicitation methods in economics use explicitly presented probabilities (Holt and Laury, 2002; Andersen *et al.*, 2008; Andreoni and Sprenger, 2012).

6 Discussion

Here we have examined the role of response times (RT) and their observability in strategic settings, using a simple bargaining game. In two laboratory experiments, buyers' RTs reflected their potential surplus from accepting sellers' offers, sellers dropped their prices more after fast rejections, and buyers made decisions more quickly when their RT could be observed by the sellers.

Our results suggest that more experienced agents would indeed monitor and manipulate RT, and develop instruments (e.g., timestamps) to make RT more explicit. Late in the first experiment, buyers' RTs continued to reflect their private values and the sellers' prices, even when their RT was observable to the seller. This aligns well with data from millions of eBay bargaining transactions, where sellers' RTs reflect the quality of the offers they have received from potential buyers (Cotet and Krajbich, 2021). These relationships hold even with experienced sellers. Thus, the information contained in RT seems to persist, both in laboratory and field markets. While most of the results in this article are restricted to the lab, they indicate that explicit training or experience with the interaction between RT and valuation could be an important factor in bargaining. Those who are attuned to RT may be able to gain a strategic advantage over those who are not.

Setting aside individual differences, one might ask how potential manipulability of RT might affect the equilibrium of the bargaining game? We have outlined a few possibilities within a simplified model (Section 2.3). We see some evidence for Hypothesis 2A in the form of lower offers from sellers and more acceptances from buyers, relative to the baseline predictions (Appendix B). However, we see more compelling evidence for Hypotheses 2B and 2C in the differences between the observable- and unobservable-RT blocks. Thus, we infer that individuals in our experiment were affected by a small but positive cost of decision time.

In some cases, it might be beneficial for agents to conceal their RT or to announce it in advance: consider a hiring situation, where an employer might not want to reveal how much interest they have in a candidate, so they could announce their RT in advance (Becker *et al.*, 2010). In other cases where this pre-commitment strategy is not an option, a strategic agent could try to manipulate their RT to pretend to be another type. However, RT manipulation is not necessarily costless, since speeding up comes with an increased chance of making an error.

Of course, speeding up is not the only way to manipulate RT. In some cases, an agent might want to introduce a delay before indicating their decision. For example, a chess or poker player in a strong position might not want to alarm their opponent by moving too quickly. In repeated bargaining, where reputation is a factor, agents might want to delay their final decision to prevent their business partners from feeling dissatisfied with the outcome of the negotiations (Galinsky *et al.*, 2002). In dating, it is common advice not to follow up too quickly after the first date, to avoid looking desperate. Unlike speeding up decisions, which comes at the cost of accuracy, there is no inherent cost in delaying a decision, except in cases where the opportunity might disappear. An impatient bargainer or date might move on to find another partner if left waiting too long.

Although the relationship between strength-of-preference and RT might be well-known to agents who bargain on a regular basis (Section 4.6), our study is the first to isolate RT from other variables such as facial expressions, body language, etc. As online interactions become more common, RT is becoming easier to record and estimate, while those other factors may become less important. Consider the comparison between online vs. live poker; in online poker one can no longer look for other players' "tells", other than through their RT.

Auctions are another example where RTs can be informative. Although in theory the rate at which bids are made in an English auction should not affect the outcome, "bidding frenzy" is a well-known phenomenon: frequency of bids tend to increase product valuations (Häubl and Leszczyc, 2004). In online settings such as eBay, the timing of bids is easily observable. Even in sealed-bid auctions, when the bid submission process (but not the bids themselves) is observable by all bidders, RTs could be used as signals of interest in the good.

Besides obvious practical significance, our findings also have important methodological implications for experimenters. Typically, economics experiments do not report whether players are able to observe each other's RT. Our results indicate that this may be important to consider, since it may allow subjects to infer more information than the experimenter(s) intended. Consider the seller's task in our Explicit-RT experiment. A standard experimenter might analyze the relationship between sellers' second offers and buyers' values and find a significant correlation (controlling for the first offer). The experimenter, unaware that RT conveys information, would have to conclude that sellers are clairvoyant. In reality, the sellers simply had access to information the experimenter hadn't considered, because the experimenter was purely focused on choice outcomes. We thus suggest that experimenters should control for RT observability and report this in their experimental design sections.

Our results also indicate that the DDM and the link between RT and preferences is present in settings where time is not explicitly costly. In both experiments, buyers had to wait 11 seconds each period, regardless of when they decided. A similar phenomenon was observed in Frydman and Krajbich (2022). Limited attention means that time spent thinking about a decision is time that is not spent thinking about more pleasant, important, or fruitful things. Attention is a scarce resource and so people may not fully allocate it to their decisions.

Our results also have important theoretical implications for behavioural modeling. For some time now, RTs in economics have been used to distinguish between deliberative (slow) and intuitive (fast) decisions, often in a dual-process framework. In many simple games, people who eventually choose a better, more thoughtful strategy tend to take more time to make that decision, so RTs have been used to classify subjects by their strategic type. Some of this research has shown that people who contemplate a problem longer tend to play better strategies or strategies closer to theoretical predictions (Rubinstein, 2007, 2016; Arad and Rubinstein, 2012), or that short RTs tend to reflect clear errors (Rubinstein, 2013). Agranov, Caplin, and Tergiman (2015) demonstrate that, within a single decision, over time, sophisticated players tend to display higher cognitive levels. Contrary to the dual-process perspective, in our Hidden bargaining tasks, buyers rejected increasingly slowly as their values increased beyond 75. These RTs contradict the notion that slower decisions should be closer to the equilibrium predictions.[28]

The dual-process style analyses are typically done across subjects, where individual differences in the boundary function could produce a positive correlation between RT and accuracy, the standard speed-accuracy tradeoff. However, within subject, boundary functions are typically assumed to be constant across decisions, and so the primary determinant of RT is drift rate. This results in a negative correlation between RT and accuracy. Slower choices indicate weaker preference. In line with this, Gill and Prowse (2023) study 3-person p-beauty contests and find a positive relation between earnings and RT across subjects, but find the opposite effect within subject. Thus, the results in the literature, as well as those in our study, appear more consistent with single-process sequential sampling models than with dual-process models.

Our work extends the scope of sequential sampling models to strategic settings. This extension is analogous to the adoption of random utility theory (McFadden, 1973) into game theory, in the form of quantal response equilibrium (McKelvey and Palfrey, 1998; Goeree *et al.*, 2002; Rogers *et al.*, 2009).[29] Future research will need to further investigate the exact nature of the relationship between latent processes like preference

[28] For a more careful analysis, we ran the following test. First, we calculated expected buyer's surplus from acceptance (as value minus the first price $v - p_1$), and rejection (as $\delta(v - Ep_2)$, where the expected second price was predicted from an OLS regression of second prices on first prices). Second, we identified expected surplus from the actually chosen option and the unchosen option. We found that RTs were 0.91 seconds longer in "error" trials, where an option with lower expected surplus was chosen (4.86 s vs 3.95 s, $p < 0.001$, paired t-test at the subject level).

[29] Webb (2018) discusses the equivalence between random utility models and sequential sampling models.

formation, strategic considerations, individual differences in cognitive ability, and conflict (Wilcox, 1993; Gabaix and Laibson, 2005; Moffatt, 2005; Krajbich, Bartling, *et al.*, 2015; Caplin and Martin, 2016; Rubinstein, 2016; Alekseev, 2019; Alós-Ferrer and Buckenmaier, 2021).

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Supplementary Data

The data and codes for this paper are available on the Journal repository. They were checked for their ability to reproduce the results presented in the paper. The replication package for this paper is available at the following address: https://doi.org/10.5281/zenodo.8170197

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