Essays in Behavioural and Experimental Economics

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Contents

Introduction

At the center of neoclassical economic theory stands the assumption that individuals are rational profit-maximizers. Individuals are supposed to act as "homo economicus", that is they behave self interested, perfectly rational and have stable preferences. Furthermore, they are not limited in their processing power and react to financial incentives independent of the context. The field of behavioural economics has challenged these assumptions since the 1980s. Experimental data shows that individuals care for the welfare of others (Charness & Rabin, 2002), display time inconsistency (Thaler, 1981) and also show limitations in rationality over a multitude of contexts (Camerer, 1998). The usual approach is to test hypotheses, sometimes based on theoretical predictions, using specifically designed, randomized and incentivized experiments.

The behavioural economics literature has continuously expanded and now spans over a plethora of topics. This dissertation features three papers that cover the themes of dishonesty (chapter 2), coordination in markets (chapter 3) and preferences during a crisis (chapter 4). The papers are connected through the methodology explained earlier, in that they all use experimental data to provide insight into specific behavioural patterns. In addition to the variation in topics, the subject pools also vary. While university students have been selected for the experiment in chapter 3, chapters 2 and 4 use an online pool located in the U.S. Student pools allow for a lot of control over the experimental environment and tend to be more homogeneous due to similarities in income, age and education. The advantage of online pools is that participants can be more representative for economic behaviour as participants often earn their main income through these platforms. Additionally, online pools tend to be significantly larger than any student pool and can be accessed more easily.

Talk is cheap. Actions speak. But what if actual actions are not easily observable? Chapter 2 focuses on a situation where individuals are faced with making a choice over multiple options of unknown quality. When the agents that provide information about the quality are self interested there is an incentive to overstate the actual quality. In addition, there is also an intrinsical motivation to be honest and/or to appear honest and virtuous to others. The chapter experimentally investigates the effects of transparency in the scenario mentioned. Additionally, the paper includes behavioural motivations, specifically lying aversion and image concerns into a theoretical model in order to generate fitting predictions.

The chapter is connected to literature on signaling, dishonesty, image concerns, political competition and quality signaling in markets. In a standard signaling game as pioneered by Crawford and Sobel (1982) an informed sender sends a signal to a uninformed receiver who then makes a payout-relevant decision. Multi sender signaling games have been previously researched (Austen-Smith, 1993; Gilligan & Krehbiel, 1989; Krishna, 2001; Milgrom & Roberts, 1986b). The setup described above is special in that senders information is uncorrelated and that the receivers' choice is to select a single sender. Both has not sufficiently been researched in the literature. Literature on political competition (Heidhues & Lagerlöf, 2003; Martinelli, 2001; Schultz, 1996; Woon & Kanthak, 2019) analyzes a similar szenario but usually does not apply behavioural concepts such as lying aversion or image concerns. The same holds true for literature on quality signaling in markets, see e.g. (Kirmani & Rao, 2000; Milgrom & Roberts, 1986a; Rao et al., 1999). Finally, the study intersects with novel literature on the effect of transparency on dishonesty behaviour (Andreoni & Petrie, 2004; Behnk et al., 2014; Cain et al., 2005; Gächter & Fehr, 1999; Gneezy et al., 2018; Irlenbusch & Sliwka, 2005; Khalmetski & Sliwka, 2019; Rege & Telle, 2004). However the effects of transparency are usually not assessed in a competitive environment.

The study uses a game-theoretic model to generate predicitions on overall behaviour as well as the effects of transparency in the specific game described above. The predictions are then tested with a fitting experiment. Results show that cheating behaviour fits a profile of lying over a set of high quality signals. Transparency has a reducing effect on lying, especially in female participants. This behaviour is in line with at least some individuals' action being driven by image concerns. An additional finding is that changes in transparency have a more pronounced effect on dishonest behaviour.

Chapter 3 presents a study that investigates the effects of specific market design features on match-dependent externalities. The concept of externalities has been first discussed by Pigou (1924) and remains an important topic in economics. When private and social interests are misaligned, welfare can suffer significantly. Today, externalities are at the heart of many of societies' major problems such as pollution and greenhouse gas emissions.

A case that has been overlooked in previous literature is that it can matter who exactly trades with whom. When traders are separated geographically, buying from a seller that is close by will create a smaller need for transportation. But markets regularly do not provide information about the exact location of a seller or buyer. A typical approach to solving externality problems is to use a corresponding pigouvian tax, that reflects the social damages for each action. However with match-dependent externalities such a tax can be significantly more difficult to implement, as the tax needs to change conditional on trader locations. In our study we examine a double auction market both theoretically and through an experiment. Treatments vary the amount of information that is given to traders about their trading partners and the current level of congestion. Finally, one treatment implies a nodal pigouvian tax that exactly reflects the externality.

The effect of externalities in markets has been discussed, as in Plott (1983), where each trade between market participants creates social damage. However, to the authors' knowledge there exists no literature on an externality that depends on the specific matching between buyers and sellers. Apart from the literature above, the study also connects to literature on auctions with goods that have multiple attributes (Bichler, 2000; Che, 1993) as well as with literature on the effect of market framing on concerns for externalities (Bartling et al., 2019; Bartling et al., 2015; Falk & Szech, 2013).

One major example for such an externality can be found in electricity markets. Producers and consumers are spread geographically and are connected through a grid system. Each trade between grid nodes creates transportation losses and, if overall demand for transportation is high enough create congestion in the system. This leads to significant system costs, which are typically not reflected in the price. The problem is exasperated when a high amount of renewables are connected to the grid, as their decentralized placement leads to overall higher transmission needs. One possible solution is nodal pricing which implements different prices for each grid node, depending on the externality. But especially in electricity markets, which are characterized by a high amount of trades, the implementation of nodal prices is difficult and especially pricy. Consequently, it is important to evaluate the effects of nodal pricing compared to other measures, such as market platforms that provide local information.

We find that providing limited locational information can already significantly reduce the externality but also has a negative effect on trade surplus. The provision of information alone is not sufficient to increase welfare to the level of externality internalization.

Finally, the paper presented in chapter 4 investigates how the COVID-19 crisis impacted economic preferences in U.S. participants. The worldwide epidemic that began in early 2020 has had a tremendous impact on global economies. With COVID related deaths estimated around 10 million worldwide, global supply chains interrupted and social life snuffed out, the crisis has not only impacted individual health but also financial stability and welfare. Most countries initiated significant aid packages and additionally private donations supported both individuals as well as the healthcare sector. The aim of this study is to provide insight in how different aspects of the COVID-19 crisis influences individuals' preferences, specifically altruism, time preferences and efficiency concerns. A shift in preferences is of particular importance when deciding about the exact nature of aid packages. If for example the medical impacts of the crisis makes individuals more short sighted and as a result leads to more inefficient choices, this is an incentive to focus on medical relief rather than financial relief.

The measurement of individuals' preferences has a long history in both psychology and behavioural economics. Literature includes estimation of time preferences (Cohen et al., 2020), concerns for equity and efficiency (Jakiela, 2013) and other regarding preferences or altruism (for an overview see e.g. (Andreoni et al., 2010)). Recently the economic literature trends towards measuring these preferences simultaneously, as they are not necessarily independent. There is evidence towards a connection of altruism with both timing (Andreoni & Serra-Garcia, 2021) and efficiency (or the price of giving) (Andreoni & Miller, 2002; Fisman et al., 2007). In this study, we use an approach similar to Koelle and Wenner (2018) who measure these preferences simultaneously based on a set of convex budget choices as pioneered by Andreoni and Sprenger (2012). The paper is also connected to literature on the impact of disasters on crises. Negative experiences can influence individual risk preferences (Dohmen et al., 2016; Eckel et al., 2009; Guiso et al., 2018; Malmendier & Nagel, 2011). The effect on time preferences is less clear (Bauer & Kramer, 2016; Cassar et al., 2017), the same holds true for altruism (Fisman et al., 2015; Voors et al., 2012).

We recruit U.S. participants in three cohorts during the progress of the crisis. Our experiment measures multiple allocation decisions which we then use to estimate individual preferences. Additionally, we elicit individual affectedness by the pandemic and connect state level data such as incidence and unemployment. We continue by estimating how this data is correlated to the individual preferences. Our main finding is that being financially affected by the pandemic as well as having at least one COVID-19 health risk factor results in decreased efficiency concerns but at the same time increased altruism. We find no evidence that the number of cases per capita or the stringency of lockdown measures have any effects on preferences.

The three essays included in this dissertation all add to different fields within behavioural economics. Chapter 2 demonstrates that individuals react to transparency in a competitive multi sender environment. It also provides new insights into the impact of changes in transparency on dishonesty. The paper in chapter 3 is the first to investigate match-dependent externalities in markets. Results show that market platforms that feature additional information can help mitigate damages, but do not achieve efficient outcomes. Finally, chapter 4 investigates the effect of a global pandemic on individual preferences. The results may guide future government policy.

Competitive Multi Sender Cheap Talk with Noisy Outcomes: The Impact of Transparency

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Abstract

Individuals are often faced with selecting between multiple options of different quality. At the same time the actual quality is not directly observable before contracting. Instead, only cheap talk quality claims by competing agents are available. The competitive situation creates an incentive to overstate qualities. Additionally, the actual quality may not be perfectly observable ex post. Real world examples include employment negotiations, political campaigning and procurement contracting. The limited amount of studies that analyze this type of situation typically do not include any behavioural motivations such as lying aversion and concerns for image. The aim of this paper is to develop a theoretical framework that includes behavioural types and to test the impact of ex-post transparency on behaviour with an experiment. Results from the experiment show that individuals lie less under transparency. Additionally, there is a pronounced effect of changes of transparency.

JEL classification: D90, D72, C91

Keywords: Deception, Information Transmission, Transparency, Experiment, Decisions under Risk, Social Image

2.1 Introduction

Many economic settings require making a choice over multiple products or individuals with varying quality. Often, the actual quality is not directly observable before contracting. Instead, only possibly dishonest quality signals are available. In purchase decisions in a marketplace, employment contesting or political competition the parties selling the good/aspiring to be chosen have an incentive to exaggerate their quality. These 'cheap talk' statements make it much more difficult for a decision-maker to select the best possible choice. While this setup is similar to Akerlof's (1970) 'market for lemons', this paper focuses on behavioural effects and not on selection processes. The introduction of Individual lying aversion and image concerns can generate different behaviour than simple profit maximization. As even ex-post information on the quality can be vague, even low quality products/politicians/employees can be successful at their respective tasks, in the same way that high quality can result in unfavourable outcomes. This kind of ex-post uncertainty can have ambiguous effects on quality signaling when individuals are image concerned. This paper presents both theory and experimental evidence for this scarcely researched but relevant competitive signaling game. Additionally, the treatments aim at evaluating the effect that ex-post observabiltiy of quality has on behaviour. Additionally, treatments are also suited to evaluate the impact of within-changes in transparency.

The mentioned scenario can be formulated as a multi-sender signalling game with uncorrelated sender types. While the initial Crawford and Sobel (1982) paper has been expanded to a multiple sender framework (Austen-Smith, 1993; Gilligan & Krehbiel, 1989; Krishna, 2001; Milgrom & Roberts, 1986b) the focus there mainly lies on the information that can be extracted from the correlated signals of multiple senders. There are few studies that focus on receivers selecting one of the senders. Exceptions are Goeree and Zhang (2014) and Rantakari (2014), but both do not introduce behavioural elements into their analysis. This paper is the first (to the authors knowledge) to investigate a multi sender signaling game with uncorrelated sender types and how ex-post transparency can affect behaviour in such a setting.

Concerning the effects of transparency, (Cain et al., 2005) find that ex-ante transparency can give individuals a 'licence to lie' resulting in negative welfare effects. With regards to transparency in the context of a signaling game, (Behnk et al., 2014) show that ex-post transparency reduces deception in a single-sender framework. An important experimental approach for measuring dishonesty is the 'die under a cup' paradigm, as introduced by Fischbacher and Föllmi-Heusi (2013). Here, individuals secretly roll a die and are paid according to the reported number. Gneezy et al. (2018) find individuals prefer partial lies compared to maximum lies, when their behaviour can be observed by the experimenter. This is consistent with a theoretical model in which agents have an intrinsic valuation of being perceived as honest. Further, papers such as Khalmetski and Sliwka (2019) show theoretically that image concerns can create the behavioral patterns observed in 'die under a cup' experiments. In more broad sense, there is also a rich literature on how transparency, or ex-post revelation of action, can impact prosocial behaviour such as prosociality, see e.g. Andreoni and Petrie (2004), Gächter and Fehr (1999), Irlenbusch and Sliwka (2005), and Rege and Telle (2004).

A branch of literature from the field of political economy that focuses on signaling between politicians and their electorate is also closely related. However, as with most of the signaling literature, the focus is on how candidates convey their private information on a policy-relevant state to the electorate, see Heidhues and Lagerlöf (2003), Martinelli (2001), and Schultz (1996). A paper from the field that is closely connected to this paper is Woon and Kanthak (2019). They investigate lying over previously tested ability to complete a payout relevant task. Their focus however is mainly on the effect of incentives and selection of dishonesty into successful politicians, not on image motivations.

Finally, there is also marketing literature on signaling product quality. While it features some similarities with this paper, the main focus there is on specific methods of quality signaling (Kirmani & Rao, 2000; Milgrom & Roberts, 1986a; Rao et al., 1999). As with the political economy literature, these papers do not include behavioural preferences such as lying aversion or image concerns.

Results from the experiment show a dishonesty reducing effect of transparency on dishonesty. The effect is especially pronounced in female participants. Additionally, a change into transparency results in a consistent reduction in cheating. Surprisingly, the effect of changing out of transparency has a dishonesty reducing effect for females while the opposite is true for male participants.

This paper adds to the literature in that it analyzes a under-researched game of competing quality signaling with noisy outcomes. Additionally, it adds to the mostly behavioural literature on the effects of transparency on lying by testing decision in a competitive setting.

The rest of the paper is structured as follows: Section two will give a detailed explanation of the experimental design. Sections three and four present the experimental procedure and results. Section five closes up with the discussion of results.

2.2 Theory

Model with standard lying aversion

A population of senders and receivers play the following signaling game: there are two senders and one receiver in each group. Each round, each sender draws a uniformly distributed random type $t_i \in 0 \leq t_1, ..., t_M \leq 1$. The probability of drawing any t_i is then $P(t_i) = \frac{1}{M}$, with $M > 1$ as the total number of types. After observing the type, each sender chooses a signal $s_i \in 0 \leq t_1, ..., t_M \leq 1$. The receiver observes signals from two senders and selects one of them. A sender receives a payout normalized to one if he is selected and nothing otherwise. I further assume that senders are either payout-maximizers or truthtellers. The share of truthtellers in the sender population is given by α. A Senders' expected utility is then defined as:

$$EU_s(t_i, s_i) = (1 - \eta_i)P_s(s_i) - \eta_i I(s \neq t|s_i) \tag{2.1}$$

where $P_s(s_i)$ is the (equilibrium) probability of being chosen when sending signal s_i. The individual type is defined by $\eta_i \in \{0, 1\}$. When $\eta_i = 0$, the sender is a payout-maximizer. When $\eta_i = 1$, the sender is a truthteller and thus only interested in being honest. An indicator for honesty is introduced into the utility function by $I(s_i \neq t_i|s_i)$, which is 1 if $s_i \neq t_i$ and 0 otherwise. The receiver then receives a payout normalized to one with probability $P_w(t_i) = \frac{t_i}{\bar{t}+1}$, where t_i is the type of the selected sender. Receivers are further assumed to be risk neutral so that their expected utility solely depends on the selected senders' type t_i. Receivers' expected utility solely depends on the expected type given the chosen senders' signal:

$$EU_r(s_i) = E(t|s_i) \tag{2.2}$$

In any equilibrium, receivers will thus choose the sender who is sending the signal with the higher expected type, or chose randomly if the signals have the same expected type. Lemma 1 defines the equilibrium strategy of the senders.

Lemma 1: payout maximizers will apply a mixed strategy over an interval of signals $X_L \in \{t_p, ..., t_M\}$ such that the expected type associated with each signal in the interval is the same. Signals are sent with higher probability the further away they are from the average type. The lower bound t_p is weakly increasing in the share of truthtellers in the population, α. When α is sufficiently high, payout maximizers will pool into t_M.

The rationale is as follows: When t_M is not part of X_L, the signal t_M will have the highest expected type, as it is only sent by honest senders. This creates an incentive to signal t_M instead of any signal in X_L. So, t_M always has to be in X_L in equilibrium. The expected type for any given signal s can be formulated as: $E(t|s) = \sum_t P(t|s)t$, where $P(t|s)$ is the conditional probability that a sender is type t given he sent signal s. With $\epsilon(s)$ as the equilibrium probability of a strategic type sending signal s, the expected type is:

$$E(t|s) = \frac{s\alpha + (1-\alpha)\epsilon(s)\sum_t t}{\alpha + (1-\alpha)\epsilon(s)M} \tag{2.3}$$

For $\alpha = 1$, the expected type is equal to s, indicating a full revealing equilibrium. For $\alpha = 0$, the expected type for each signal collapses to $\frac{\sum_t t}{M}$, which is the average type. Then, signals hold no information, constituting a babbling equilibrium. For $0 < \alpha < 1$, the expected type is increasing in s and, as long as the signal is above the mean type, decreasing in $\epsilon(s)$:

$$\frac{\partial E(t|s)}{\partial \epsilon} = \frac{M\alpha(1-\alpha)[\frac{\sum_t t}{M} - s]}{(\alpha + (1-\alpha)\epsilon(s)M)^2} \tag{2.4}$$

This implies that $\epsilon(s)$ must increase the further away a signal is from the average signal in order to keep expected types within X_L constant. The intuition here is that when senders mix into a signal, the expected type shifts towards the average type of the pooling senders. This results in a reduction of the expected type for signals above the average type and an increase for signals below the average type. Further, pooling into a single signal $\bar{t}$ can only be an equilibrium strategy when the share of truthtellers in the population, α is high enough. When α is sufficiently low the expected type $E(t|t_M)$ would be below $E(t|t_{M-1})$ under a pooling strategy and thus cannot be an equilibrium. Instead, senders mix over X_L. Mixing over X_L can only be an equilibrium strategy if the expected type and thus win probability for senders P_s is identical for all signals on the interval. The equilibrium presented here is similar to cheating games with image concerns such as in Khalmetski and Sliwka (2019). However, the equilibrium here does not require image concerns to predict partial lies, the incentive to mix over an interval is created by the competition between senders.

Image concerns

Now, consider a model that also includes an additional behavioural type that is only concerned with being perceived as honest, image-maximizers. They choose their signal in order to maximize their reputational payout. As before, the types are exclusive, so there are no mixed types. Assume the following utiltiy function:

$$EU_s(t_i, s_i) = (1 - \eta_i - \mu_i)P_s(s_i) - \eta_i I(s \neq t|s_i) - \mu_i Pr(t \neq s|s_i) \tag{2.5}$$

where $\eta_i \in \{0, 1\}$, $\mu_i \in \{0, 1\}$ and $\eta_i + \mu_i \leq 1$. As before, assume that α is the share of truthtellers, and β is the share of image-maximizers. The optimal strategy for image-maximizers differs depending on the transparency of their action. Define $-\mu_i Pr(t_i \neq s_i|s_i)$ as the reputational payout.

When the type is ex-post observable any signal that is not identical to the type results in an image loss. The term for reputation is then identical to the lying identity. This creates identical strategies for truthtellers and image-maximizers. The payout-maximizers will still have the same equilibrium strategy as described in Lemma 1, but the interval size is decreasing in $\alpha + \beta$ instead of only alpha.

In the absence of transparency, image-maximizers have an incentive to lie if it results in a reputational payout. Lemma 3 describes the euilibrium strategy of image-maximizers without transparency:

Lemma 2: Under transparency, image-maximizers will behave identically to truthtellers and tell the truth. In the absence of transparency, image-maximizers will apply a mixed strategy over $X_I \in \{t_0, t_1, ..., t_r\}$ such that the reputational payout associated with each signal within the interval is the same. t_r can not be below t_{p-1}. When $(1 - \alpha - \beta)$ is sufficiently large, $t_r = t_{p-1}$. Then the equilibrium strategy is randomizing over X_I. When $t_r >= t_p$ image-maximizers will choose signals below t_p with equal probability and signals above or equal to t_p with lower probability the higher the signal.

The highest reputational payout is provided by signals that are chosen by the highest share of truthtellers. t_r can never be smaller than t_{p-1} as t_{p-1} would then provide a higher reputational payout than signals on X_I, as there would only be truthtellers signaling t_{p-1}. When β is sufficiently small, $t_r = t_{p-1}$. For larger β image maximizers will also mix over signals within X_L. In equilibrium, the reputational payout has to be equal for all signals within X_I. This implies that image concerned agents send signals within $\{t_1, t_{p-1}\}$ with equal probability. As payout-maximizers also mix within $\{t_p, ..., t_M\}$ image-maximizers must send signals within this interval with a probability that is decreasing with the probability of payout-maximizers sending the signal. As payout-maximizers' probability of sending a signal increases with the distance of the signal from the average type, image-maximizers' probability of sending a signal has the inverse relationship. Whether X_I and X_L overlap depends on the values of α and β. When the share of image concerned agents, β is sufficiently large, these types will also mix over signals within X_L.

Image maximizers choosing signals decrease the corresponding expected type in the same way as payout maximizers, the result is a shift of the expected type towards the average type. When $t^r > t^*$ any increase in β (with constant $1-\alpha$) will thus also increase X_L.

Hypotheses

H1: A significant number of senders will send either be the highest possible signal, or mix among a set of the highest signals.

Note that this explicitly predicts partial lies. This follows from *Lemma 1*.

H2: There are more completely honest signals under transparency than without transparency.

As image concerned senders are predicted to be honest under transparency, but chose signals in order to maximize reputational payout when there is no transparency. This follows from *Lemma 2*.

H3: In the absence of transparency, signals on the lower end of the typespace are sent with higher frequency than under transparency.

This also follows from the equilibrium presented in *Lemma 2*.

2.3 Experimental Design and Procedures

2.3.1 Experimental Design

In the experiment participants participate in a modified 3-player signaling game. In each group, there are two senders and one receiver. Senders get individual type draws, which can be interpreted as a probability. These draws are private information. Senders must then choose which signal to send to the receiver. Signals are limited to possible type draws. The receiver observes the two signals and must then choose one of the senders. Senders' payout are determined by being chosen by the receiver. For the receiver, payout is determined by a lottery with odds determined by the type of the chosen sender. Each participant is randomly matched with other participants each round, for a total of 20 rounds. Feedback on receiver choices and payout outcomes is not given until the end of the experiment. The main rationale behind a non-feedback, multiple rounds framework is to get enough information for each sender about the complete strategy, without any reputational concerns.

Treatments vary the degree of observability of actual behavior. Under the *hidden* condition, receivers never find out about the actual types of the chosen sender. Under *transparency* the type of the chosen sender is revealed to the receiver, along with the payout outcome. This makes detection of deception easy. For comprehensibility, the information is displayed at the end of round 20, along with all signals and choices. In addition, all participants find out about the lottery outcome for the receiver in each round.

In order to capture both the general effect of transparency, as well as any effects of switching from and to a transparent regime, treatments vary the condition under which round 1-10 and 11-20 are played. Table 2.1 gives an overview over all 4 treatments. Participants were not informed about the exact nature of the change that will happen after round 10 prior to the change.

Table 2.1: Treatment Overview

Treatment	Condition Round 1-10	Condition Round 11-20
TT	transparency	transparency
TH	transparency	hidden
HT	hidden	transparency
HH	hidden	hidden

The type is drawn from a uniform distribution over the set $\{0.1, 0.3, 0.5, 0.7, 0.9\}$. The type corresponds directly to the winning probability of the receiver. This type of coded type/signal space allows for small lies, while both keeping the instructions relatively simple and preventing transmission of additional information by selecting specific signals. The exclusion of values of 0 and 1 makes sure that it is never possible to determine a messages' truthfulness with certainty when there is no transparency. This prevents pooling on 'safe' signals. Senders were randomly assigned either of the two labels "Sender A" or "Sender B" each round, to counteract any default decisions on the receivers side.

2.3.2 Experimental Procedures

The experiment was conducted in February 2019.[1] The experiment was realized using O-tree (Chen et al., 2016). The participants were recruited using the pool of Amazon Mturk. The pool consists of around 500.000 participants mainly originating from the U.S. and India (Paolacci & Chandler, 2014). Individuals were required to be located in the U.S. and to not have participated in any prior experiment rounds.[2] As with all Mturk online experiments, there is still the possibility of selection into the experiment, as workers choose their tasks themselves. However, this does not interfere with the randomization process, as the task description was the same for all treatments and roles. With 120 participants per treatment, in total 480 individuals took part in the experiment. The dataset thus consists of 20 observations each of 320 senders and 160 receivers, resulting in 6400 sender choices and 3200 corresponding receiver choices.

Participants received a base pay of 1$ for participating. Out of the total of 20 rounds only

[1] A pilot of similar size was conducted in April/March 2018, data upon request

[2] additionally, participants had to have participated in at least 1000 HITs and have an approval rate of at least 97%

one was randomly chosen after the experiment to determine the additional payout. Senders received 6$ if they were chosen in the relevant round, while receivers earned 5$ if they won in the lottery. The reason for different payouts was to keep average earnings about the same for senders and receivers. While this is not a critical design choice, it generates fairness in terms of final payouts between the senders and receivers. If receivers manage to always select the sender with the higher message, their average win chance increases to 0.75. Senders' average chance of being selected is always 0.5, as only one of the senders can win in each round. This resulted in overall total earnings of 3.82$, 3.89$ for senders and 3.70$ for receivers.

After reading the instructions, participants had to answer a number of comprehension questions correctly. There were a total of 6 questions on the same page, and all had to be correct to proceed. If at least one answer was incorrect, participants were informed that the answers were not correct, but not which answer specifically was wrong. The number of wrong submissions before passing is represented by the 'wronganswers' variable, which can be used to correct for participant comprehension of the experiment. There were two attention test, after round 5, and after round 15. Failing in these tests is captured in variables 'attentionfails1' and 'attentionfails2'. Inclusion of the attention-tests or the number of wrong answers does not change the results in a meaningfuly way. After finishing all 20 rounds, participants were asked the following beliefs: Senders were asked which signal they think is most likely to be chosen by receivers. Receivers were asked which message they think is most likely to be true, and which message conveys the highest probability of winning the lottery. There was a survey following the experiment in which participants had to give information about their gender, age, nationality, income bracket, education level and self-reported risk-aversion. Table 2.2 shows the means of personal characteristics for each treatment.

Table 2.2: Balancing Table

Treatment	HH	HT	TH	TT	Total
gender	0.44	0.45	0.42	0.48	0.45
age	36.92	40.62	39.25	37.37	38.59
education	3.11	3.52	3.22	3.33	3.30
income	5.14	5.05	5.07	5.46	5.18
riskaversion	4.59	4.85	5.28	4.71	4.87
wronganswers	2.45	2.46	2.10	2.21	2.30
attentioncheck5	0.04	0.10	0.10	0.06	0.07
attentioncheck15	0.03	0.02	0.00	0.04	0.02

2.4 Results

2.4.1 Overall Behaviour

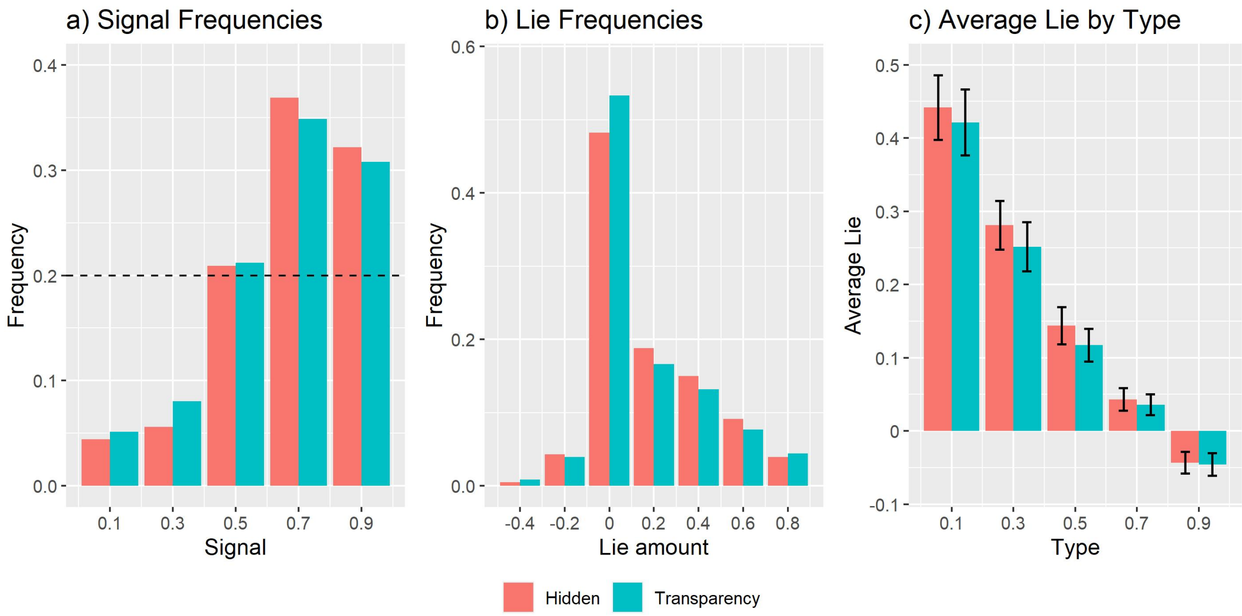


Figure 2.1: a): Average signal frequencies, dotted line represents honest equilibrium values, b): Lie Frequencies, c): Average lie dependent on type. Only observations from round 1-10 are used. Treatments are pooled based on transparency. Bars indicate 95% confidence intervals

The analysis will first focus on the results from the first 10 rounds of the experiment, in order to analyze both overall behaviour in the two sender signaling game and the baseline effect of transparency on dishonesty. Data from treatments TT and TH are thus pooled, the same is true for HT and HH. Figure 2.1/a shows the signalling strategy of senders. Overall, Signals 0.5 through 0.7 are sent more often than lower signals (two-sided binomial test, all pval<0.01). These results hold both for the whole dataset as well as for the subsets with and without transparency. Signals of 0.9 are sent less frequent than 0.7. The difference is 5% significant for the whole dataset and under transparency, but not without transparency.[3] Overall, the data indicates pooling on the signals 0.5, 0.7 and 0.9. This fits to senders

[3]See table A.2.1 in the appendix for the exact pvalues

applying a mixed strategy over the signal 0.5-0.9. This is also supported by the observed negative average lies when the type is 0.9 as seen in Figure 2.1/c. The observed negative average lie is significantly different from zero (one-sample ttest pval<0.01), independent of transparency. This is further supportive of senders mixing over an interval.

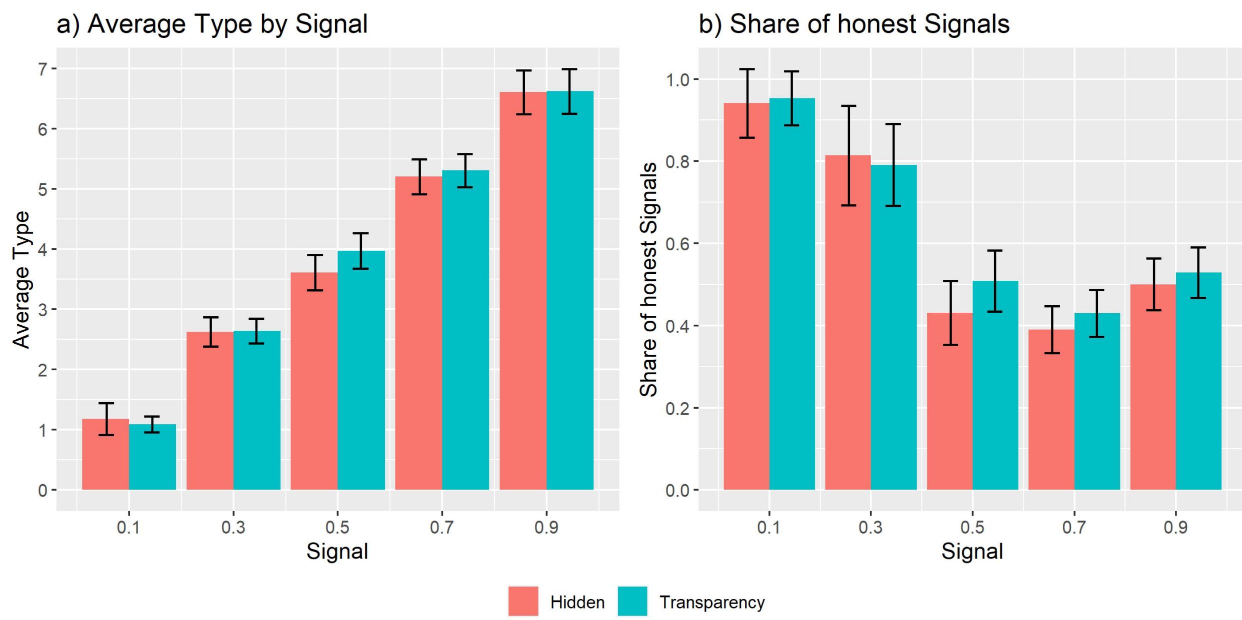


Figure 2.2: a: Average type associated with each signal, b: Share of honest signals. Only observations from round 1-10 are used. Bars indicate 95% confidence intervals

Figure 2.2 shows both the average type behind each signal (a) and the share of honest signals (b). The theory predicts identical average types for an interval of high signals. This is not supported by the data, as Average types are increasing in the signal for all signals, independent of transparency (Wilcoxon ranksum test pvals<0.05). Based on the experimental data, there is a clear financial incentive for receivers to always choose higher signals. In turn, payout maximizing senders should go for a signal of 0.9. With respect to the reputational payout, the share of honest signals shows an inverted u-shape, as seen in figure 2.2/b. Signals of 0.5 and 0.7 have the lowest share of honest signals (Wilcoxon ranksum test pvals<0.05). Individuals that focus on maximizing their image should prefer signals of 0.1, 0.3 and 0.9 over signals of 0.5 and 0.7.

2.4.2 Effects of Transparency

With respect to the effect of transparency, the data shows no difference in signal frequencies (all Pearson's Chi-squared pvals>0.05). The same holds for differences in individual average lies (Mann-Whitney-U-Test pval=0.259) and lies dependent on type (Mann-Whitney-U-Test pvals>0.05 for each type). Regression analysis does also not support any effect of transparency in rounds 1-10. The data does thus not support the higher frequency of lower signals, as predicted by H3. Table 2.3 shows results of both a random effects (model 1) and random effects tobit regression (model 4) with individual lie as the dependent variable. The regression does not show a significant effect of transparency on the lying decision. However, under transparency there is a slightly higher share of honest signals, with 0.533% of signals that are honest under transparency, compared to 0.482% (Mann-Whitney-U-Test pval = 0.043). This is limited evidence towards a higher share of honest signals under transparency as proposed under H2.

A common finding in the economic literature on cheating is that males tend to lie more than females, over a variety of contexts (see e.g. Erat and Gneezy (2012), Abeler et al. (2014), Houser et al. (2012)). With respect to image concerns there is very limited literature that focuses on gender differences, and often no effect of gender is found, see Tonin and Vlassopoulos (2013). Figure 2.3 shows average lies dependent on the type, for each gender separately. Females have lower average lies under transparency (Mann-Whitney-U-Test pval = 0.019). Males show no such difference (Mann-Whitney-U-Test pval = 0.4859). This is supported by significant negative coefficients of the dummy for transparency on models (3) and (6) in Table 2.3.

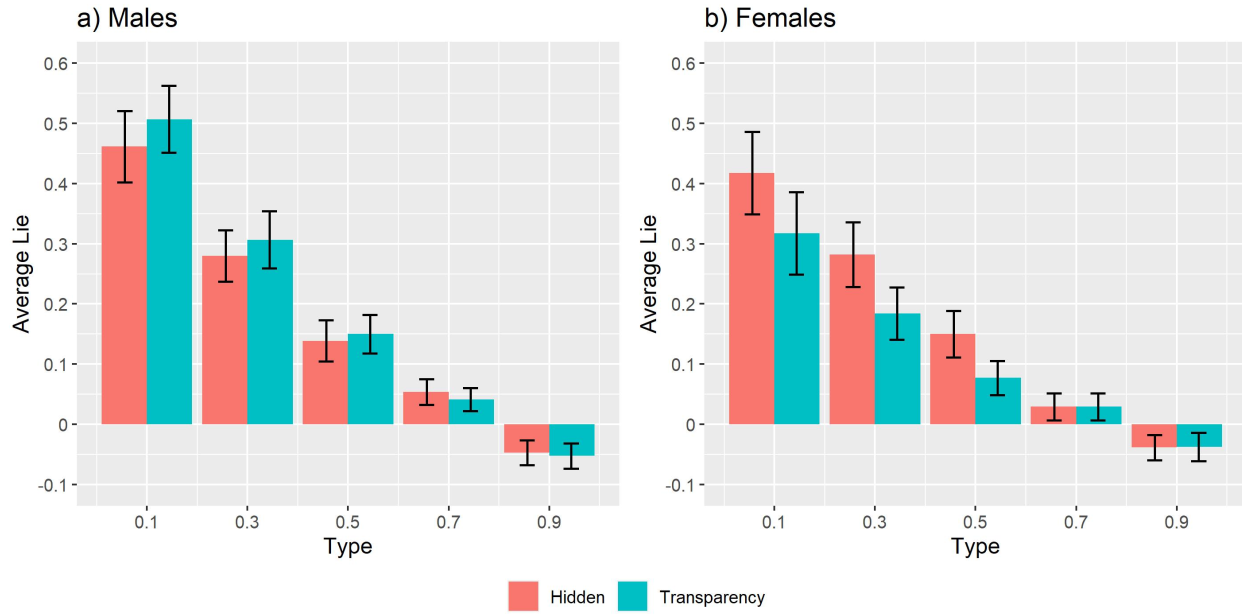


Figure 2.3: Average lie dependent on type, shown separately for males (a) and females (b). Only observations from round 1-10 are used. Bars indicate 95% confidence intervals

Table 2.3: RE Model Results

	Dependent Variable: lie					
	RE			RE Tobit		
Model	(1)	(2)	(3)	(4)	(5)	(6)
transparency	-0.0927	0.218	0.202	-0.225	0.600	0.559
	(0.161)	(0.213)	(0.212)	(0.358)	(0.470)	(0.464)
gender		-0.113	-0.122		-0.0579	-0.141
		(0.230)	(0.238)		(0.508)	(0.522)
transparency*gender		-0.693**	-0.689**		-1.845***	-1.793**
		(0.318)	(0.318)		(0.703)	(0.699)
Observations	3,200	3,200	3,200	3,200	3,200	3,200
Number of id	320	320	320	320	320	320
Controls	No	No	Yes	No	No	Yes
R^2	0.000159	0.0162	0.0241			
Note:	*** p<0.01, ** p<0.05, * p<0.1					
	Standard errors in parentheses, only observations from round 1-10 are used, controls include: age,income, education and self-assessed risk-aversion					

2.4.3 Switching Behaviour

In the context of this experiment it also makes sense to look into the effect hat a change of transparency could have on dishonesty. Literature on moral licensing in cheating games indicates that individuals compensate moral behaviour with dishonest behaviour (see e.g. Jordan et al. (2011), Clot et al. (2014), and for a meta analysis on moral licensing Blanken et al. (2015)). This can apply to a change in transparency as well: If individual behave honest under transparency, cheating could increase once behaviour is hidden, compared to a situation where behaviour was never transparent. In the same way, dishonesty could decrease more after a switch into transparency compared to a situation with persistent transparency. To the authors knowledge, there is no literature on the effects of changes in transparency on economic behaviour. These effects can have a significant impact when policies aimed at changing transparency are implemented.

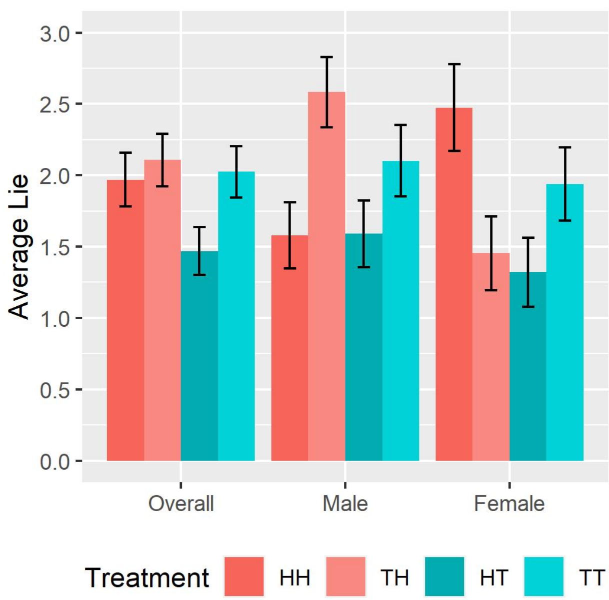


Figure 2.4: Average lie for each treatment. Only observations from round 11-20 are used. Bars indicate 95% confidence intervals

Figure 2.4 shows average lies in rounds 11-20, for each treatment. There is a pattern of lower lies under transparency when the previous rounds have been played without transparency, as seen by the lower bars on the HT treatment compared to TT . This finding is highly significant for the overall data (Mann-Whitney-U-Test pval = 0.008) but not for the gender subsets (Mann-Whitney-U-Test pvals m= 0.064, f=0.056).

Additionally, males lie more in the absence of transparency when previously actions have been transparent (TH>HH, Mann-Whitney-U-Test pval = 0.001), while the effect is the opposite for females (TH<HH, Mann-Whitney-U-Test pval = 0.014).

Table 2.4 shows corresponding regressions. again with both random effects and random effects tobit estimators. The regressions include the dummy *r11-20 for rounds 11-20, which is interacted with the treatment dummies. Note that the table shows individual regressions for gender subsets, as using triple interaction terms would make meaningful interpretation of the coefficients challenging. The regression confirms the earlier findings of changes in transparency, with significant negative effects of treatment HT*r11-20 and positive, significant coefficients on TH*r11-20 for males and on *r11-20 for females.

Table 2.4: RE Models with individual treatment dummies

	Dependent Variable: absolute lie					
	RE			RE Tobit		
	all	male	female	all	male	female
Model	(1)	(2)	(3)	(4)	(5)	(6)
r11-20	0.0254	-0.170	0.277*	0.208	-0.200	0.692**
	(0.106)	(0.147)	(0.151)	(0.219)	(0.299)	(0.320)
HT	-0.00231	0.549*	-0.692**	-0.104	1.373	-1.980*
	(0.225)	(0.290)	(0.337)	(0.693)	(0.887)	(1.033)
TH	-0.129	0.511*	-0.976***	-0.487	1.575*	-3.183***
	(0.224)	(0.284)	(0.343)	(0.692)	(0.872)	(1.053)
TT	0.00526	0.558*	-0.677**	-0.171	1.421	-2.157**
	(0.224)	(0.290)	(0.333)	(0.688)	(0.886)	(1.019)
HT*r11-20	-0.552***	-0.503**	-0.635***	-1.603***	-1.515***	-1.655***
	(0.144)	(0.203)	(0.203)	(0.306)	(0.414)	(0.452)
TH*r11-20	0.232	0.491**	-0.106	0.741**	1.329***	-0.0199
	(0.144)	(0.199)	(0.207)	(0.303)	(0.404)	(0.458)
TT*r11-20	0.0770	0.0382	0.0826	0.233	0.0706	0.409
	(0.144)	(0.203)	(0.201)	(0.298)	(0.408)	(0.433)
gender	-0.365**			-1.021**		
	(0.148)			(0.470)		
Observations	6,400	6,400	6,400	6,400	6,400	6,400
Number of id	320	320	320	320	320	320
R^2	0.0131	0.0326	0.0372			

Note: *** p<0.01, ** p<0.05, * p<0.1
Standard errors in parentheses, controls include: age,income, education and self-assessed risk-aversion

2.4.4 Behavioural Types

Table 2.5: Share of Truthtellers. Onyl data from rounds 1-10 is used.

	Transparency	
Gender	*Hidden*	*Transparency*
Overall	0.170	0.221
Males	0.152	0.098
Females	0.191	0.373

In addition to the previous analysis, the data can be used to typify the participants into truthtellers and other types. Table 2.5 shows the shares of individuals that are completely honest, using only observations from round 1-10. Only the female subset shows a significant difference in the share of truthtellers (Mann-Whitney-U-Test pval = 0.017). Similar to the previous results, only females show a different behaviour under transparency. This fits to the theory in that image-concerned types are predicted to be completely honest under transparency. As shown in Figure 2.1 (b), there are some negative lies observed. Overall, 4.81% of signals are lower than the type. The highest share of negative lies is made by senders with a type of 0.9, with 79.22% of negative lies sent by sender with type 0.9. With no significant difference in negative lies between treatments, it is unclear if negative lies stem from image concerns or the maximization of payout.

2.4.5 Receiver Behaviour

Table 2.6: Win Probability(Sender) for each Signal

	Probability of being chosen		
Signal	Total	*Hidden*	*Transparency*
0.1	0.091	0.067	0.107
0.3	0.226	0.228	0.225
0.5	0.433	0.432	0.433
0.7	0.603	0.596	0.609
0.9	0.549	0.517	0.582

As the theory predicts pooling on either the highest signal or a interval of signals with identical probability of being chosen, it makes sense to look at the corresponding success probabilities. Table 2.6 shows the probabilities of being chosen given the signal overall and with an without transparency. Under transparency and overall the win probability is different from the probability of adjacent signals at least on the 1%-level (Mann-Whitney-U-Test) for all signals. Without transparency the difference between 0.7 and 0.9 is not significant (Mann-Whitney-U-Test pval=0.106). This, in combination with the increasing average types over signals shown in Figure 2.2 (a) shows that receivers do not manage to make the optimal choice with regards to expected payout. Unfortunately the data does not provide any hints towards the exact reasoning behind this pattern. It is possible that the setup of the experiment which limits learning results in off-equilibrium behaviour. It is also possible that the decisions are driven by an intent to punish dishonest senders by favouring lower signals.

2.5 Conclusion

This paper studies a competitive signaling game with random outcomes. There are many examples in everyday life where agents compete through claims over ex-ante unverifiable quality. Additionally, both honesty and dishonesty cannot be exactly determined ex-post. This kind of un-observability can lead to more dishonesty, resulting in decreased information transition and thus efficiency.

The theory and corresponding experiment presented in this paper investigate this scarcely researched signaling game. Experimental treatments are aimed at changing behaviour in image-motivated individuals. The theory presented fits the data overall. However, the effect of baseline transparency is limited to female participants. Additionally there are strong effects on lying generated by a change in transparency. Overall , a change into transparency has a reducing effect on dishonesty. Controversially, the effect of a change out of transparency on lying is positive for men and negative for women. These results add to the literature in that they contribute to limited evidence on gender differences in image concerns and is to the authors knowledge the only study that also investigates the effects of changes in transparency. The effects measured are lower bound estimates, as the online platform used for the data acquisition has a high degree of anonymity.

Finally, the theoretic framework, which has been developed after the experiment, introduces specific behavioural types into a competitive signaling environment in order to explain behaviour. Although the underlying scenario overlaps with many topics, from political competition to product marketing, literature on the topic usually does not include behavioural motivations such as image motivation or lying aversion.

2.6 Appendix

2.6.1 Appendix A: Supplementary Tables

Signals	Total	*Hidden*	*Transparency*
1vs3	0.019	0.362	0.028
3vs5	0.000	0.000	0.000
5vs7	0.000	0.000	0.000
7vs9	0.035	0.128	0.159

Table A.2.1: P-values from a two-sided binomial test for equal frequency.

2.6.2 Appendix B: Experimental Instructions

Experimental Instructions for Senders

Thank you for participating in our study! Please read the following information carefully!

Throughout the experiment, your identity will be completely anonymous and will not be disclosed to anyone else. All participants will receive these exact same instructions. Note: If only the male form is chosen, this is not meant to be gender-specific but serves solely for the better comprehensibility and legibility of the text.

About the experiment

The experiment will take about 20 minutes. For participating in the experiment you will earn $1 for sure and can earn up to $6 in bonus payments, depending on your and other participants' behavior.

There will be a total of 20 rounds, followed by a short survey that will not affect your payment in any way. At the end of the experiment, only one of the 20 rounds will be randomly selected, and the earnings in that round will determine your bonus payment.

You can review these instructions at any time by clicking the button on the bottom of the page.

Course of the experiment

Each participant will be randomly assigned a role – either the role of the sender or the role of the receiver. Roles will not change at any time. In each round, two senders and one receiver will be playing together.You will be randomly grouped with two other participants each round, so you won't always face the same sender(s) or receiver. The probability of being grouped with the same participants is very low.

Each round will take place as follows:

At the beginning of the round, each sender will get 10 lottery tickets. These lottery tickets have no value to the sender, but to the receiver. Each ticket can either be a winning ticket or a blank. The number of winning tickets out of the 10 is randomly assigned to each sender each round. There can be 1, 3, 5, 7 or 9 winning tickets out of the 10. The rest are always blanks. Only the sender will know how many of his 10 tickets are winning tickets.

Each sender sends a message to the receiver, indicating the number of winning lottery tickets he has (again taking values of 1, 3, 5, 7 or 9). The message does not have to indicate the true number of winning tickets.

Based on the messages by the two senders in his group, the receiver has to choose one of the senders. The chosen sender earns a bonus payment of $6. The other sender receives a bonus payment of $0.

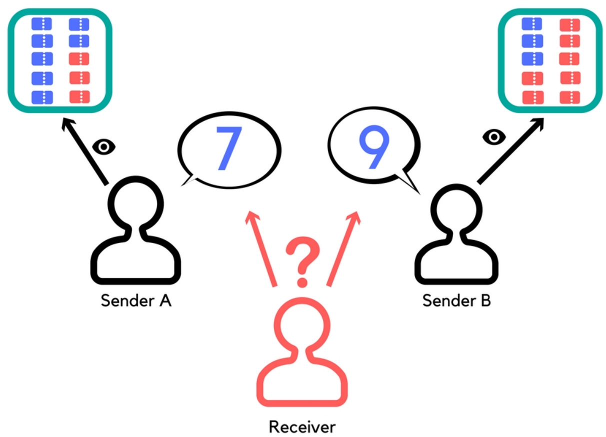
For the receiver, the bonus payment is determined by a lottery. The lottery box contains the 10 lottery tickets of the sender he has chosen. One out of the 10 lottery tickets is drawn and determines the receivers' bonus payment for this round – either $5 if it's a winning ticket or $0 if a blank is drawn. All tickets are discarded at the end of each round, so they don't carry over to the next round.

Hint: Remember that the senders' message does not have to indicate the true number of winning tickets.

The receiver will be informed about the result of each rounds' lottery after the final round. In some rounds the receiver may also learn how many winning tickets actually were in the box. If so, everyone will be informed about that prior to these rounds. Senders will learn in which rounds they have been chosen when they receive their bonus payment. Everyone will see a short summary of their choices after the last round.

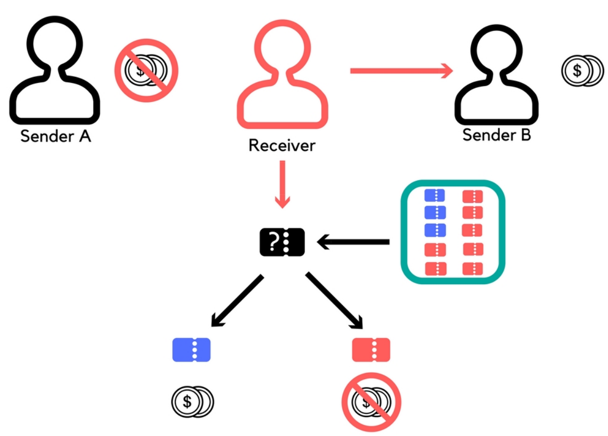
Example:

Sender A has received 7 winning tickets (blue), while sender B has received 3. Sender A chooses to send the message 7 while Sender B chooses to send message 9. After observing both messages, but not the actual number of winning tickets, the receiver can choose either A or B.



Example:

Let's assume the receiver choses sender B. Sender B will earn \$6, while sender A will earn nothing. Out of the 10 tickets of sender B, one will be randomly drawn. If it is a blue winning ticket, the receiver will earn \$5. If it is a red blank he will earn nothing.



Questions

Please answer the following comprehension questions. You have ten minutes to get the correct answers.

1. After observing the messages from both senders A and B, the receiver chooses Sender B. What can be said about the senders earnings for this round (excluding the $1 that everyone earns anyway)?

Sender A earns ...

Sender B earns ...

2. The receiver has chosen a sender with 7 winning tickets.

How high is the receivers' winning chance in percent?

How much $ does the receiver at least earn in this round?

3. The receiver gets the following messages from the senders:

Sender A: "I have 5 winning tickets"

Sender B: "I have 9 winning tickets"

Can the receiver know how many tickets each of the senders actually has?

Can the receiver know which sender has more tickets?

Internalizing Match-dependent Externalities

Authors: Andreas Lange and Johannes Ross

Abstract

External effects can be triggered through trade and depend on the locations of buyers and sellers who are matched. Inspired by electricity markets, we experimentally investigate markets in which net trades between two locations induce social costs. Based on a modified double auction setting, we compare the performance of market platforms that are location-blind with those where information on the location of (potential) trading partners or the level of the externality is given. We demonstrate that locational information can already reduce the externality. Imposing the full external costs on individual trades leads to maximal price differentiation between locations and further reduces net trades, while welfare improvements are limited. Reasons for not achieving the typically high efficiency of double auctions are discussed.

JEL classification: D40, D62, C91, Q4

Keywords: Match-dependent Externalities, Market Design, Double Auction, Electricity Markets, Experiment.

3.1 Introduction

Internalizing external effects is at the heart of many regulatory interventions. Externalities may arise from production or consumption, yet often they are created through trade and thereby depend on the match between buyers and sellers. A typical example are external effects arising from transporting products. Calls for "buying local" reflect awareness for these issues.[1] Yet, typical trading platforms do not necessarily provide information about the location of market participants.

Electricity markets are another example for such external effects: here, a trade between market participants can create an external effect on the grid if transmission lines are congested.[2] The externalities also include environmental effects if renewables are replaced by fossil-based production. However, markets participants may not be aware of the externalities created through their trade or may not be incentivized to take the external effect into account. While nodal pricing (Hogan, 1998; Schweppe et al., 2013) incorporates part of such grid costs, the zonal pricing in electricity markets prevalent in Europe essentially rules out match-dependent prices that depend on the location of installations and consumers in a partly congested grid. Specifically in electricity grids, trades can also alleviate the externality by reducing the congestion level, i.e. by resulting in transportation in the opposite direction of the congestion.

In this paper, we investigate how market design can counter such match-dependent externalities. We provide evidence from a laboratory auction experiment to show how information and internalization incentives impact the externalities, market outcomes, and resulting welfare. We find that providing information on locations of buyers and sellers already leads to spatial price separation. Full price separation is only achieved when individual market participants are forced to fully internalize the externality arising from their individual trades. Yet, price separation does not necessarily translate into increased welfare as the gains from addressing the externality comes at the cost of surplus (and efficiency) reductions.

With our experimental design, we modify the typical double auction environment to allow

[1] Buying locally is frequently advertized based on environmental benefits, among others (e.g., http://www.gogreen.org/blog/the-environmental-benefits-of-buying-locally, https://sustainableconnections.org/why-buy-local/, even though critical assessments exists, (e.g., Ferguson & Thompson, 2021).)

[2] The costs can be substantial. For example, the largest German grid operator spent more than one billion euros on redispatch or on compensating curtailed renewables https://www.reuters.com/article/us-tennet-germany-idUSKCN1PS0FI.

for location specific bids. Double auctions are typically a highly efficient market mechanism to match buyers and sellers (Friedman, 1984; Ketcham et al., 1984; Smith, 1962). Only few papers incorporate externalities in such market settings. The main focus is on flat per-trade externalities, as in Plott (1983), who finds no evidence for individuals reacting to the externality in the absence of internalization. Sutter et al., 2020 analyze a double auction market where trades have a negative impact on a third party. They find that the presence of an externality decreases the number of trades whereas the effect on prices depends on the market structure.

More generally, the literature suggests that market settings may reduce existing concerns for externalities (e.g., Bartling et al., 2019; Bartling et al., 2015; Falk & Szech, 2013). We are not aware of any study that allows for external effects to depend on exactly who is matched in a trade.

By providing information on the location of buyers and sellers, the units offered or requested in the auctions become spatially differentiated. With this, our study is related to the literature on auctions that allow for individual valuations of auction items to depend on multiple attributes (e.g., Bichler, 2000; Che, 1993). Our design differs as the locational dimension only affects the externality and not market participants' own valuations or costs. Guided by the electricity market setting, the externality in our setting depends on the number of net trades between the two locations. That is, trades in opposite directions can offset each other.

In our treatments, we vary the information on the location of (potential) trading partners, the current level of the externality, as well the way how the burden arising from the externality is distributed: in equal shares onto all market participants vs. onto the seller or buyer who triggers a change to the externality through concluding the trade. This latter internalization treatment is predicted to lead to maximal price differentiation between the locations by fully internalizing the externality. This treatment thus theoretically mimics nodal pricing.

Our results show the importance of providing proper internalization incentives into the market. Once the external costs are imposed on the causal trade, the number of net trades and thus the externality are significantly reduced. Yet, price differentials between locations already occur even if external costs are socialized across all market participants as long as information is given on the location of trading partners. We find that providing locational information or indicating the current level of externalities can already reduce the externality resulting from net trades.

Providing locational information or incentives for internalizing the external benefits into the market comes at a cost. As the benefits from trade depend on who one is selling to or buying from, i.e. on the locational match, the efficiency of markets for achieving the predicted surplus is impaired. Trading off these efficiency costs with the reduction of the externalities, the welfare gains from their internalization only materialize if externalizes are sufficiently steep.

The remainder of the paper proceeds as follows: section 3.2 provides the theoretical foundation, section 3.3 reports the experimental design, predictions, and experimental procedures, before we discuss the results in section 3.4. The final section concludes.

3.2 Theory

We model trades of a good that is homogeneous with respect to the private valuations by market participants, i.e. by buyers and sellers. Buyers and sellers are located at two locations A and B. In location $l \in \{A, B\}$, the aggregate (inverse) demand of n_l buyers is given by $P_l(X_l)$ (decreasing), while the costs of sellers $j = 1, \ldots, m_l$ are given by $C_l^j(y_l^j)$ (increasing convex). We denote aggregate supply in location l by $Y_l = \sum_j y_l^j$. Any feasible allocation needs to balance demand and supply, i.e. $X_A + X_B = Y_A + Y_B$. Guided by the example of electricity markets, we assume that externalities d depend on the net trade between locations, that is on the extent of transportation from A to B. This is given by the excess demand in B, $X_B - Y_B$, or equivalently by the excess supply in A, $Y_A - X_A$. Thus, $d = D(X_B - Y_B)$. Note that net trade can go in either direction, i.e. $X_B - Y_B$ can be positive or negative. For simplicity, we assume that $D(\cdot)$ is symmetric, i.e. $D(z) = D(-z)$, with $D(z)$ decreasing in z for $z < 0$, increasing for $z > 0$ and convex.

The welfare in any feasible allocation (satisfying $X_A + X_B = Y_A + Y_B$) is thus given by:

$$\int^{X_A} P_A(s)ds + \int^{X_B} P_B(s)ds - \sum_j C_A^j(y_A^j) - \sum_j C_B^j(y_B^j) - D(X_B - Y_B). \tag{3.1}$$

Welfare maximization, i.e. maximization of (3.1) requires

$$p_A = P_A(X_A) = C_A^{j\prime}(y_A^j) \tag{3.2}$$

$$p_B = P_B(X_B) = C_B^{j\prime}(y_B^j) \tag{3.3}$$

$$p_B - p_A = D'(X_B - Y_B). \tag{3.4}$$

Here, (3.2) and (3.3) state the typical conditions that the price needs to equal marginal costs within a location. The intuition is that no externality arises from a trade between buyers and sellers located at the same location. Yet, (3.4) shows the potential of a price differential between the two locations. If net trade flows from A to B, i.e. if $X_B > Y_B$, an additional trade from A to B increases the externality ($D'(X_B - Y_B) > 0$) such that the price (and marginal costs) within location B are larger then in A. The opposite effect happens if location A has positive excess demand and trade flows are reversed ($p_B - p_A = D'(X_B - Y_B) < 0$). The price differential thus optimally reflects the marginal externality arising the marginal trade between the two locations.

Assuming competitive market behavior, this welfare optimum can be decentralized by charging a tax τ for trade from A to B at a rate of $D'(X_B - Y_B)$. Then, a firm located in A is indifferent at the margin between selling within A (obtaining a price p_A) or selling to a buyer in B and thus obtaining a revenue of $p_B - \tau (= p_A)$ for the last trade. A similar argument holds for firms in B for whom a trade with buyers in A would essentially be subsidized: $p_B = p_A + \tau$. That is, selling within B gives a price of p_B, while selling into A would give them not only the price p_A, but also an additional subsidy τ.

Yet, such decentralization does not only necessitate imposing such a tax to internalize the externality, but also requires market participants to know the location of their trading partners. If this information is not present, prices cannot be differentiated between locations. In this case, uniform prices would result in equilibrium:

$$p = P_A(X_A) = P_B(X_B) = C_A^{j\prime}(y_A^j) = C_B^{j\prime}(y_B^j) \tag{3.5}$$

which then induces the damages D to be larger than optimal. Clearly, the same equilibrium allocation results even if the locations of trading partners are known, but no internalization of externalities is induced by taxes like described above. Within electricity markets, the grid costs, and thus also the components arising from congestions are typically rolled over to market participants. While this implies that part of the externalities are borne by those who conclude a specific trade, no effect on behavior can be expected if markets are competitive.

3.3 Experimental Design and procedures

3.3.1 Experimental Design

We embed the idea of match-dependent externalities in a multi-unit double auction experiment. Our market platform matches sellers with buyers and assigns prices to each trade.

Participants interact in groups of eight traders, comprised of four sellers and four buyers. Two sellers and two buyers are assigned a location A or B, respectively.[3] A trader always knows his own location, but not necessarily the location of other traders, that is the origin of bids made on the auction platform. The individual location is fixed across periods.

Sellers are endowed with five units of the tradable good, and buyers are not allowed to resell any units they bought. Each buyer (seller) is randomly assigned a vector of valuations (costs) for the respective five units per participant. For buyers, the valuation refers to the payout received when a unit is acquired on the market. For sellers, the cost shows the amount that is deducted from the selling price.

Table 3.1 shows the valuations and costs induced on particpants in the respective locations. These are noted in experimental units (EU). The valuations are designed to be lower in location A in order to make sure there is a market incentive to trade across locations.

Buyers and sellers can both submit bids on the market platform, and can accept bids from the opposite role. Figure 3.1 shows the implementation of the market platform for our baseline treatment.

As described in the previous section, the externality depends on the number of net trades from one location to the other. We assume symmetry such that the direction of net trades does not matter. The additional costs arising from the externality are identical for all traders within a group. In order to investigate the impact of the shape of the externality function,

[3]We chose to have two sellers and two buyers in each location in order to prevent locational market power. Typically, this suffices to result in competitive outcomes (e.g. Plott, 1982; Smith, 1982).

Table 3.1: Valuations of buyers (B1 and B2) and costs of sellers (S1 and S2) in locations A and B

	Location							
	A				B			
Unit	S1	S2	B1	B2	S1	S2	B1	B2
1	31	33	55	53	39	41	63	61
2	35	37	51	49	43	45	59	57
3	39	41	47	45	47	49	55	53
4	43	45	43	41	51	53	51	49
5	47	49	39	37	55	57	47	45

Table 3.2: Externality costs as a function of net trades between location A and B

Net trades $\lvert X_B - Y_B \rvert$	External costs $D(\lvert X_B - Y_B \rvert)$	
	linear	exponential
0	0	0
1	10	5
2	20	15
3	30	30
4	40	50
5	50	75
6	60	105

we vary between linear and exponential externality costs,[4] the specific values are shown in Table 3.2. In all but the *internalization* treatment the costs of the externality are shared between all market participants in equal parts, independent of the number of units traded. In order to prevent participants ending up with negative costs, traders receive 10 EU at the end of each round.

The market is specified further by the following rules: bids and bid acceptance are only allowed if the trader at least breaks even with the trade. We note that our design slightly differs from the traditional double auction setting (e.g., Friedman, 1984; Ketcham et al., 1984; Smith, 1962): bids that match with respect to price do not automatically result in a trade. Instead, bids have to be specifically accepted by another buyer/seller. This is simply done by clicking on the corresponding bid, see Figure 3.1 for an example of the market platform. Obviously, posting a bid gives no control over the location of the trading partner

[4]For the linear externality damage function, the optimal tax τ on trades from A to B is independent of the numnber of concluded net trades, while the exponential functions implies an increasing tax depending on the net trade level.

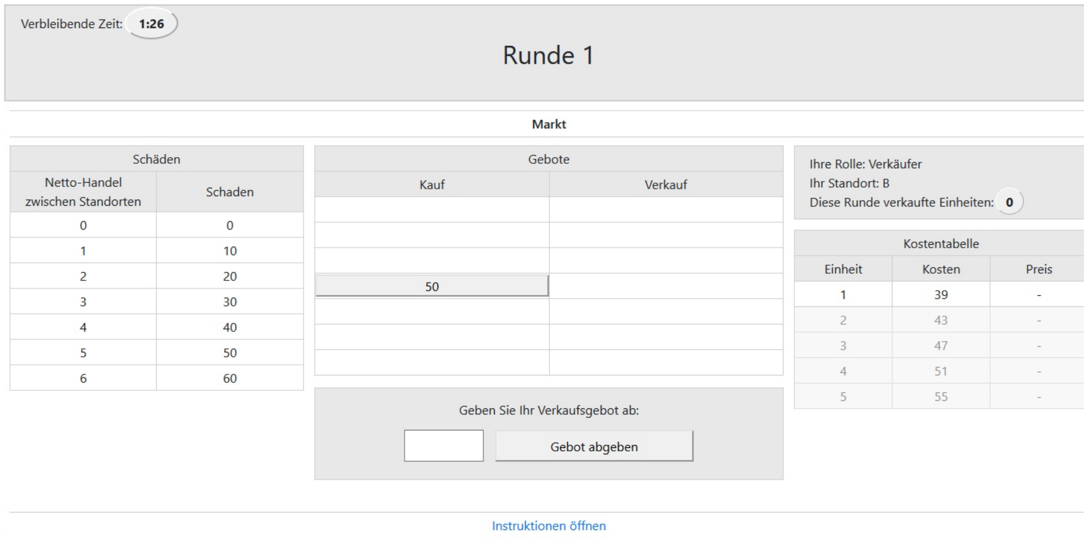


Figure 3.1: Screenshot of market platform,*baseline* treatment

who eventually accepts the bid. The accepting trader, however, can decide where to sell to/buy from if the necessary information is given. Thus it is necessary that lower offers or higher asks are not immediately replaced and bids of the same value are allowed, as they may originate from different locations. After a bid is accepted, the trade is logged for the two involved traders and the market is cleared of all bids. Each round lasts a total of two minutes. After that, the traders get an overview of their financial gains. This includes paid/received prices, valuations of the units bought or costs of units sold, and the costs arising from the externality.

Each group plays for a total of 16 rounds, with individual valuations and the externality cost structure changing after round 8. The traders know that the change will happen, but not in which way. Note again that neither a trader's role nor his location change at any time.

The treatments are aimed at changing the information that traders get from the trading platform. In the *baseline* treatment traders only know their own location. There is no additional information on the origin of any bid. Treatment *locational* introduces this information by displaying the location a bid is from next to any bid on the platform. This enables traders to take into account the effect that accepting a bid may have on the externality. Specifically, by accepting a bid from the trader's own location, no externality will arise. Accepting the bid from the other location may reduce or increase the externality. Treatment *indicator* additionally shows a box with information on the current net trades, the corresponding externality cost, and the share of the cost the trader has to pay. Thus, traders can anticipate the effect of each trade on the total externality as well as on their share of external costs. Finally, in treatment *internalization*, the financial effect of a trade on the externality costs is directly imposed on the accepting trader only.

3.3.2 Predictions

The valuations as given in Table 3.1 allow to specify the excess demand in the two locations. The right panel in Figure 3.2 illustrates excess demand for location B and excess supply for location A. In the *baseline* treatment, no information on locations of trading partners is given. We thus expect an equilibrium price of $p = 47$ with 4 net trades from A to B. Notwithstanding the differences to the traditional double auction setting mentioned earlier, we anticipate similar convergence properties towards the market equilibrium as in the extant literature. In particular, prices should not differ between the two locations.

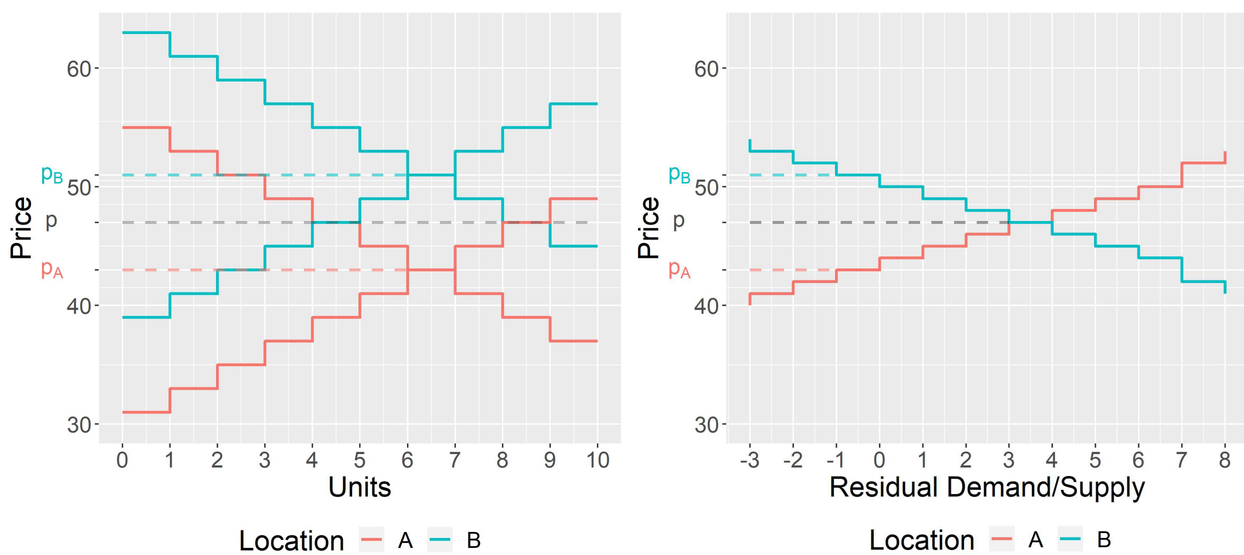


Figure 3.2: Market supply and demand (panel 1) and excess demand for location B and excess supply for location A (panel 2)

Treatment *internalization* imposes the full external cost onto the accepting trader. We thus expect a separation of prices in the two locations. Maximal price separation occurs under autarky, i.e. without any trade with price levels given by $p_A = 43$ and $p_B = 51$ as illustrated in Figure 3.2. For the linear externality variant, the marginal external costs triggered by any additional net trade are given by 10 units. Thus, we expect zero net trades as the price difference in autarky is smaller than the marginal external costs. For the exponential externality, the marginal external costs for the first trade are 5 such that we expect one net trade from A to B to occur in equilibrium under internalization. Figure 3.2 illustrates that the excess demand of one unit is decentralized with $p_A = 44$ and $p_B = 50$.[5]

The specific units that are traded in the equilibria in the *baseline* and the *internalization* treatments are illustrated by the shaded entries in Tables A.3.9-A.3.12.

In treatment *locational* and in treatment *indicator*, each player takes on one eights of the total external costs. Different from the *baseline* treatment, players know the location of their trading partners and thus may take this partial internalization into account. If they do, the equilibrium predictions involves prices differentiated by location, $p_A = 46$ and $p_B = 48$ and a net trade of three units.[6]

Given the sequential nature of the experimental double auction, not all units are necessarily traded at an identical price. In particular, in treatment *internalization*, the traders are charged different amounts for the externality, depending on when they trade. For example, the first trade between A and B necessarily imposes an externality of $D(1)$ which is charged to the accepting trader. In case that a second trade occurs in the same direction, the accepting trader will be charged $D(2) - D(1)$. Should a third trade occur in the opposite direction, the then accepting trader would receive a subsidy of $D(2) - D(1)$. We thus may see different price offers over time depending on the current level of net trades. At the same time, this dynamic may lower convergence rates towards the equilibrium.

[5] Note that due to the discrete trading options, the price difference is not exactly at the level of the marginal externality. The prices are limited to natural numbers.

[6] Note that the marginal external costs from an additional net trade are $10/8 = 1.125$ under linear externalities, while a fourth net trade would impose and externality of $20/8 = 2.5$ for the exponential externality, while relaxing the externality by trading the opposite direction reduces the externality by $15/8 = 1.875$. Given the prices p_A and p_B, there is no incentive for any market participant to trade an additional unit: sellers in A (B) would get p_A ($p_A + (D(3) - D(2))/8$) for selling an additional unit into A, $p_B - (D(4) - D(3))/8$ (p_B) for selling into B. Buyers in A (B) pay p_A ($p_A + (D(4) - D(3))/8$) when buying an additional unit in A, and $p_B - (D(3) - D(2))/8$ (p_B) when buying in B.

3.3.3 Experimental Procedures

The experiment was conducted online at the WiSo Forschungslabor at University of Hamburg, with participants being invited via hroot (Bock et al., 2014). The experimental software used was oTree (Chen et al., 2016). A total of 288 participants ran through the experiment. Four groups had at least one participant drop out, and have been repeated with new participants, resulting in 256 participants, balanced over all 4 treatments. We thus have data on a total of 32 groups of 8. The data is also balanced with respect to the order in which the externality cost structures are applied, within each treatment, for each group that starts with linear externality costs there is a group that starts with exponential costs.[7]

Participants received 5€ as show-up fee. In addition, one round was randomly selected at the end of the experiment for payout. For each EU 0.50€ were paid out, resulting in an average payout of 19.75€ per participant. With fixed timeouts for introduction and rounds, the maximum time spent on the experiment was about 60 minutes, resulting in above average wages for participants at the lab at University of Hamburg.

3.4 Results

The data-set consists of 37131 individual bids, with a total of 6320 units traded, resulting in 17.02% of bids being accepted. On average, each trader bought or sold 3.09 units.

In section 3.4.1, we first consider the treatment effects averaged across rounds. We consider the effects on prices, net trades (and the resulting externalities), and welfare. We additionally discuss in detail which units end up being traded to better understand the impact of treatments on surplus and welfare. Section 3.4.2 then considers the temporal effects, i.e. convergence properties.

3.4.1 Average treatment effects

Table 3.3 shows the prices that result in location A and B as well as the test statistics for price differentiation. We pool the average last bids over all periods with the same cost structure

[7]No order effects can be identified.

as one observation per group. This applies for all statistical tests in this section. The final average bids are also illustrated in Figure 3.3.

Table 3.3: Last price, average price, locational prices, price differences, by externality type and treatment. pvals are from Wilcoxon signed rank tests.

externality	treatment	p	p_{av}	p_A	p_B	pval p_A=p_B	$\lvert p_A$-$p_B \rvert$
linear	*baseline*	47.06	44.09	46.45	46.88	0.21	0.42
linear	*locational*	46.70	46.45	46.30	46.91	0.01	0.61
linear	*indicator*	46.48	45.89	45.39	46.70	0.04	1.31
linear	*internalization*	47.42	47.52	44.53	50.41	0.01	5.88
exp	*baseline*	46.58	44.89	46.41	46.75	0.04	0.34
exp	*locational*	47.39	47.08	46.89	47.95	0.02	1.06
exp	*indicator*	47.02	45.60	46.56	47.34	0.11	0.78
exp	*internalization*	44.78	46.53	43.84	47.73	0.01	3.89

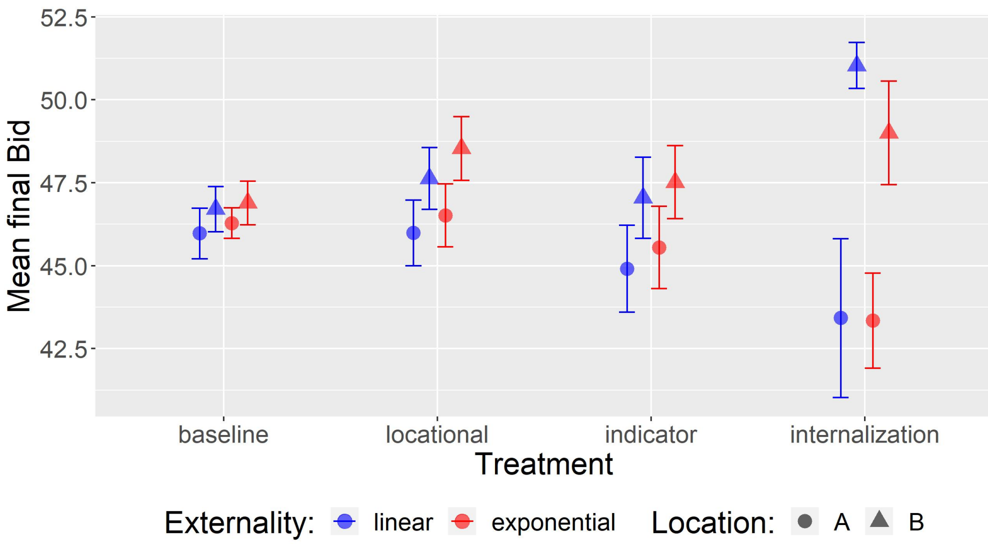


Figure 3.3: Average final bids for each treatment. Observations are grouped over all rounds on the group level. Bars indicate 95% confidence intervals.

In the *baseline* treatment, there are only minor differences between prices in the two locations ($|p_A - p_B|$ averaging to 0.42 and 0.36, respectively).[8] As predicted, the *internalization* treatments lead to significant price differentiation under both linear and exponential externality ($p < 0.01$, Wilcoxon signed rank). In fact, the difference is significantly larger than in the

[8]Even though small, the difference is significant under the exponential externality – this potentially could be driven by both induced values and costs being larger in B than in A.

baseline treatment for both variants of externality ($p < 0.01$, Mann-Whitney rank sum test). Table 3.3 reveals that the treatments *locational* and *indicator* already lead to significant price differentiation. However, the extent of differentiation does not significantly differ from the *baseline* treatment.[9]

While the price differentiation indicates that the treatments indeed affect the prices and thus the trade decisions within and between locations, the welfare properties are clearly determined by the units that are actually traded. Table 3.4 delineates the trades by locations. We first note that the total number of traded units varies in the treatments between 12.08 (internalization, linear) and 12.77 (locational, linear) and thus falls short of the predicted number of 14 traded units. We also note that trades occur in both directions, that is from A to B and from B to A, with more trades going from A to B as predicted. Within the *internalization* treatment, this also implies that some trades are taxed while others are subsidized.

Table 3.4: Number of actual trades

Externality	Treatment	Actual Trades				
		Total	A to A	B to B	A to B	B to A
linear	*baseline*	12.61	3.41	2.89	4.66	1.66
linear	*locational*	12.77	3.83	4.17	3.73	1.03
linear	*indicator*	12.14	4.25	3.44	3.39	1.06
linear	*internalization*	12.08	5.05	4.97	1.16	0.91
exp	*baseline*	12.47	3.19	3.02	4.91	1.36
exp	*locational*	12.34	3.83	4.05	3.36	1.11
exp	*indicator*	12.11	3.70	3.39	3.73	1.28
exp	*internalization*	12.23	4.91	4.84	1.56	0.92

The average net trades range from 3.00 (linear externality) and 3.55 (exponential externality) in the *baseline* treatments, to 0.31 and 0.64 in the *internalization* treatments as reported in Table 3.5 and illustrated in Figure 3.4. For the linear externality variant, the net trades in *locational* and *indicator* treatments are not significantly smaller than in the *baseline* treatment. Only *internalization* reduces net trades relative to all other treatments ($p < 0.01$, Mann-Whitney U test, see Table A.3.1). Different treatment effects result for the exponential externality: here, *locational* and *indicator* treatments already reduce the net trade relative to the *baseline* ($p = 0.004$ and $p = 0.074$, respectively). Full *internalization* further reduces the average net trade relative to all other treatments ($p < 0.01$, see Table A.3.2).

[9]Conversely, the differentiation in the *internalization* treatment is larger than in any other treatment ($p < 0.01$, Mann Whitney rank sum, for both externality variants).

We thus obtain that providing locational information or indicating the current level of externalities can already reduce the externality resulting from net trades. We note that the reduction is significant when it is particularly important under the exponential externality variant as here the marginal benefits from reducing the congestion are largest.

The reduction of the externality is traded off against reductions in surplus as reported in Table 3.5 and illustrated in Figure 3.5. As such, the treatment effects on welfare are less promising: under a linear externality, we do not find any positive treatment effect on the welfare level (see Table A.3.3). Under the exponential externality, however, both *locational* ($p = 0.05$) and full *internalization* ($p = 0.015$) have a positive effect on welfare relative to the *baseline* treatment (see Table A.3.4).

Table 3.5: Net trades, surplus, welfare and efficiency by externality type and treatment. Efficiency is equal to the percentage of the social optimum, including the externality, omitting comission.

externality	treatment	|net-trades|	externality D	trade surplus	welfare
linear	*baseline*	3.00	30.00	174.78	144.78
linear	*locational*	2.73	27.34	173.09	145.75
linear	*indicator*	2.39	23.91	166.75	142.84
linear	*internalization*	0.31	3.13	149.94	146.81
exp	*baseline*	3.55	43.67	174.50	130.83
exp	*locational*	2.34	24.06	169.03	144.97
exp	*indicator*	2.52	27.03	164.06	137.03
exp	*internalization*	0.64	3.60	154.53	150.94

To further investigate the welfare effects of the diverse treatments, it is instructive to compare the surplus and net trades, and the actual welfare with the predicted levels. In typical double auction settings (e.g., Friedman, 1984; Ketcham et al., 1984; Smith, 1962), high efficiency rates are observed.

The equilibrium prediction in our *baseline* treatment involves a surplus of 184, with four net trades occurring and leading to welfare level of 144 in the linear and 134 in the exponential externality variant. The actual surplus reaches 95% of the predicted values in the *baseline* treatment under both externality variants. As fewer units are traded, the actual welfare amounts to 100% (linear) and 98% (exponential) of the predicted welfare levels. Overall, the blind market in the *baseline* treatment thus confirms previous findings that double auctions generate a high efficiency close to equilibrium predictions.

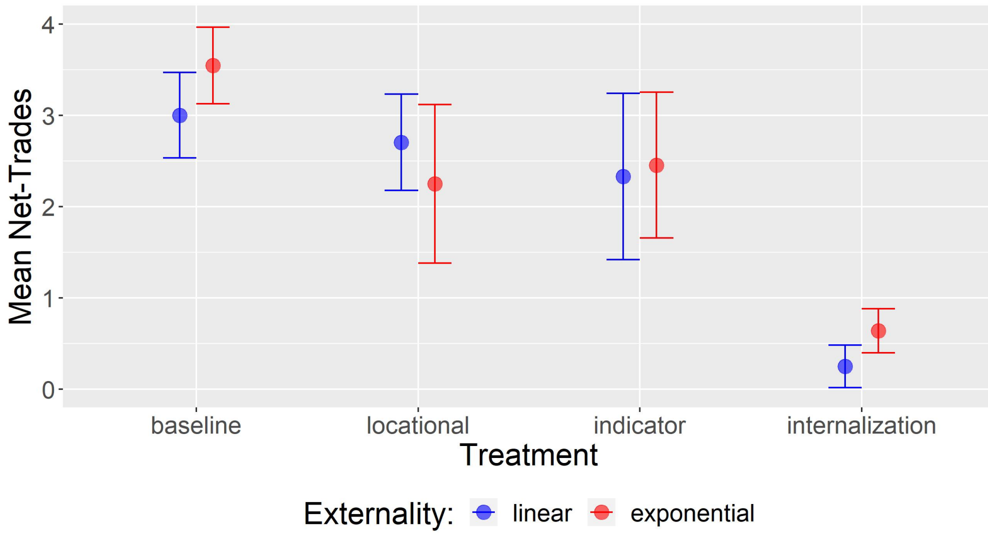


Figure 3.4: Average final externality for each treatment. Observations are grouped over all rounds on the group level. Bars indicate 95% confidence intervals.

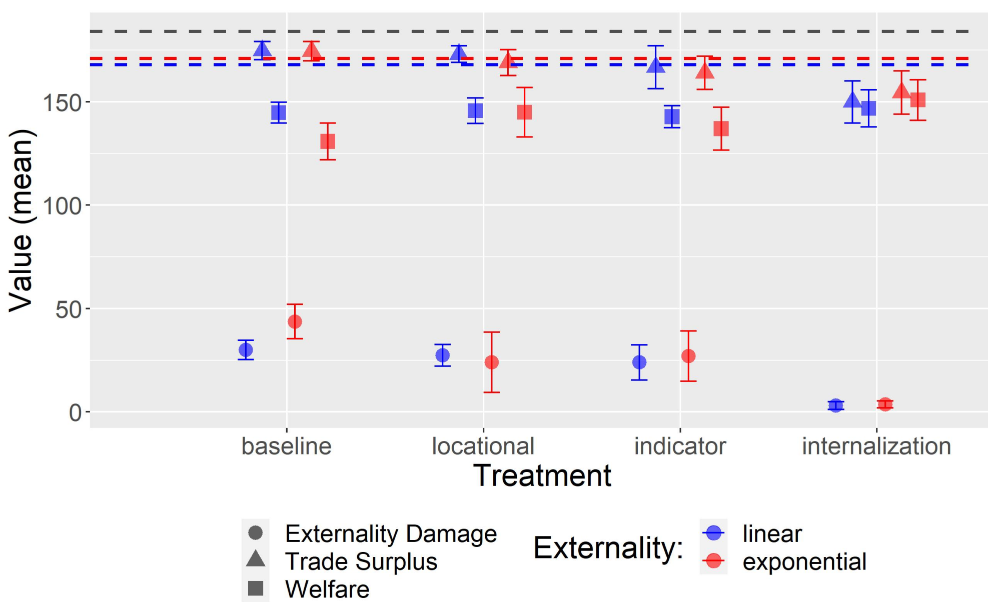


Figure 3.5: Overview over mean surplus, externality damage and welfare. Observations are grouped over all rounds on the group level. Bars indicate 95% confidence intervals.

However, the existence of the externalities obviously leaves room for beneficial interventions as the optimal welfare is 168 in the linear and 171 in the exponential externality variant. Yet, the *internalization* treatments realize "only" 87% (linear) and 88% (exponential) of the maximal welfare and thus cannot fully generate the predicted welfare gains. As the net trades (see Table 3.5) lead to only minor damages resulting from the externality, the missing welfare is largely due to not achieving the predicted surplus in the *internalization* treatment.

In order to explore this in more detail, we consider the likelihood that any individual unit is traded. Tables A.3.9 to A.3.12 show these probabilities (in brackets) and also indicate which units should be traded under the equilibrium predictions (shaded entries). Comparing the *baseline* treatments (Tables A.3.9 and A.3.10) with the *internalization* treatments (Tables A.3.11 and A.3.12) suggests that the separation of units that should vs. should not be traded is less pronounced in the latter. For example, the marginal seller in A and the marginal buyer in B that just should not trade have probabilities of trading of 11% (22%) and 17% (19%) in the *baseline* treatment under linear (exponential) externality. The corresponding probabilities under *internalization* are given by 44% (36%) and 33% (30%) and thus are substantially higher. This suggests that the likelihood of wrong matches between buyers and sellers, i.e. units that should not be traded, are more frequent under the *internalization* treatment. One potential reasons is the sequential nature of trades which leads to some trades being taxed, while others are subsidized which may make such wrong matches overly attractive.

3.4.2 Convergence over time

We investigate the time effects in two different ways: first, we discuss how the main variables (price differential, net trades, welfare) develop over the eight rounds. Second, we consider the evolution of within-round-variance of accepted bids, i.e. the variance of prices at which the units are traded within periods.

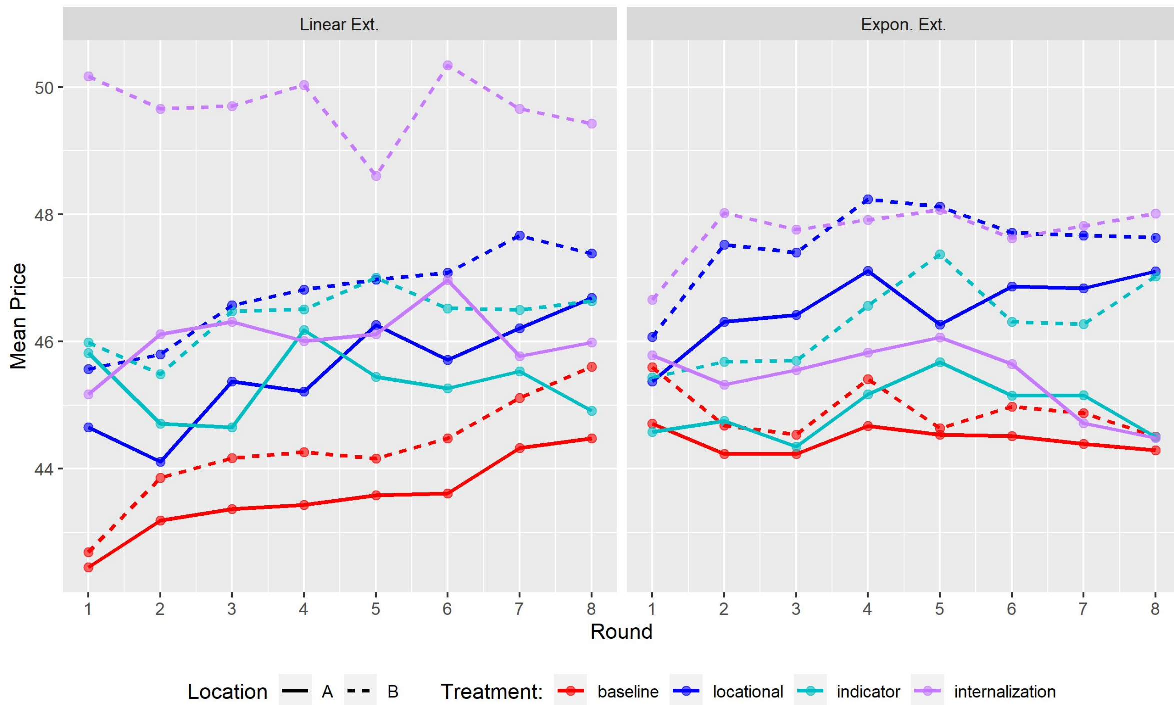


Figure 3.6: Overall av. price for each round/treatment, separated by location

We depict the price trends over the eight rounds in Figure 3.6. Under linear externality, with the exception of the *internalization* treatment prices show a slight upwards trend over time. This is more strongly pronounced in *baseline* and *locational*. With exponential externality, prices are overall stable after round one. Regarding the price separation, prices separate after round 1 and show a stable separation afterwards. In fact, the random effects regressions in Table A.3.13 suggest a slight (yet significant) reduction of the price differential over time under the linear externality. Table A.3.14 reports the corresponding random effects regressions on the number of net trades. Across all treatments, there is an increase in net trades over time, thus leading to larger externalities. Importantly, the *internalization* treatment breaks this positive trend and leads to stable net trades across periods, i.e. its time trend is significantly different from the *baseline* under linear externalities and *locational* and *indicator* treatments under exponential externalities. Correspondingly, welfare is relatively stable over time as reported in Table A.3.15.)))

Figure 3.7 displays the evolution of the variance of accepted bids over the eight rounds played under one externality variant. Figure 3.8 additionally separates the variance by location.

In general, we observe a steep drop in variance in the earlier rounds, especially after round one. This is in line with the behavior typically observed in double auction experiments as individuals learn about trading prices over time. However, while all treatments continue to show a downward trend, *internalization* treatments show persisting higher variances than the *baseline* and *locational* treatments (Wilcoxon rank sum test using only observations from rounds 5-8, pvals<0.05)[10]. That is, once market participants know about the contribution of the next trade to the externality, i.e. both the level as well as if it adds or reduces the externality, the prices at which the trades are concluded show a larger variance. This is consistent with traders incorporating the varying externality levels into their bids and asks. We note that this is not possible in the *baseline* and *locational* treatment as market participants there are not informed about the current net trade situation.

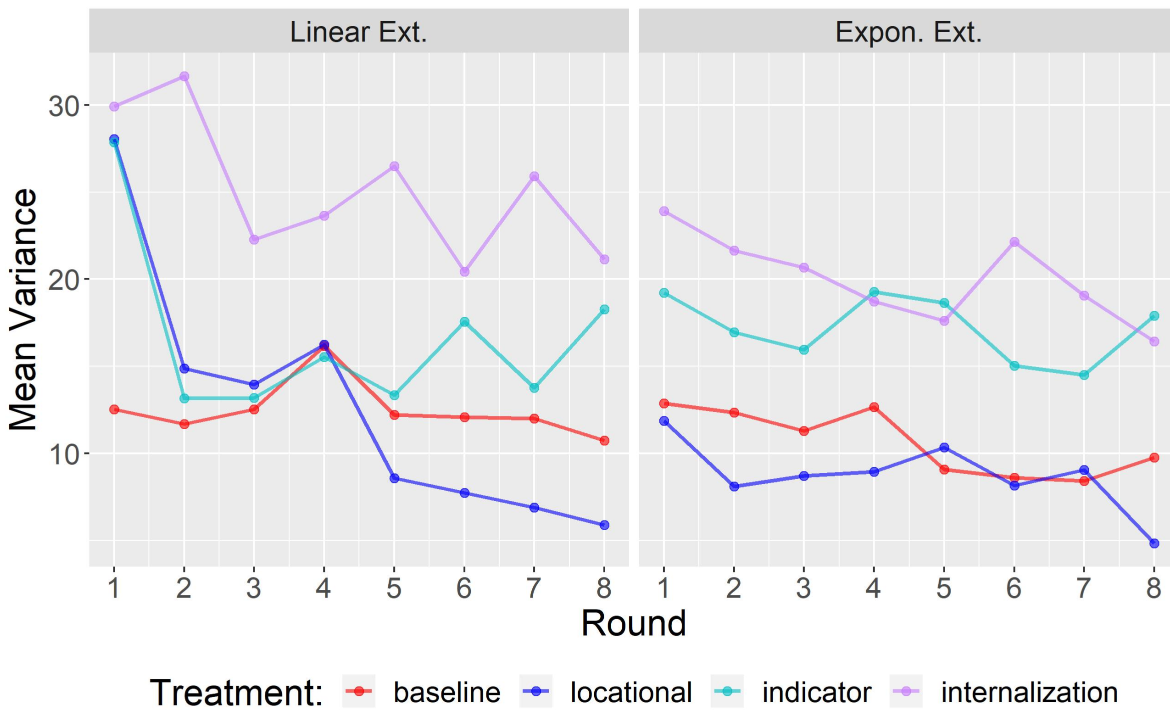


Figure 3.7: Variances of accepted prices for each round/treatment. Variances are calculated within each group and round. Points display average over group variances.

[10]For the exact test results, see tables A.3.5 and A.3.6 in the appendix

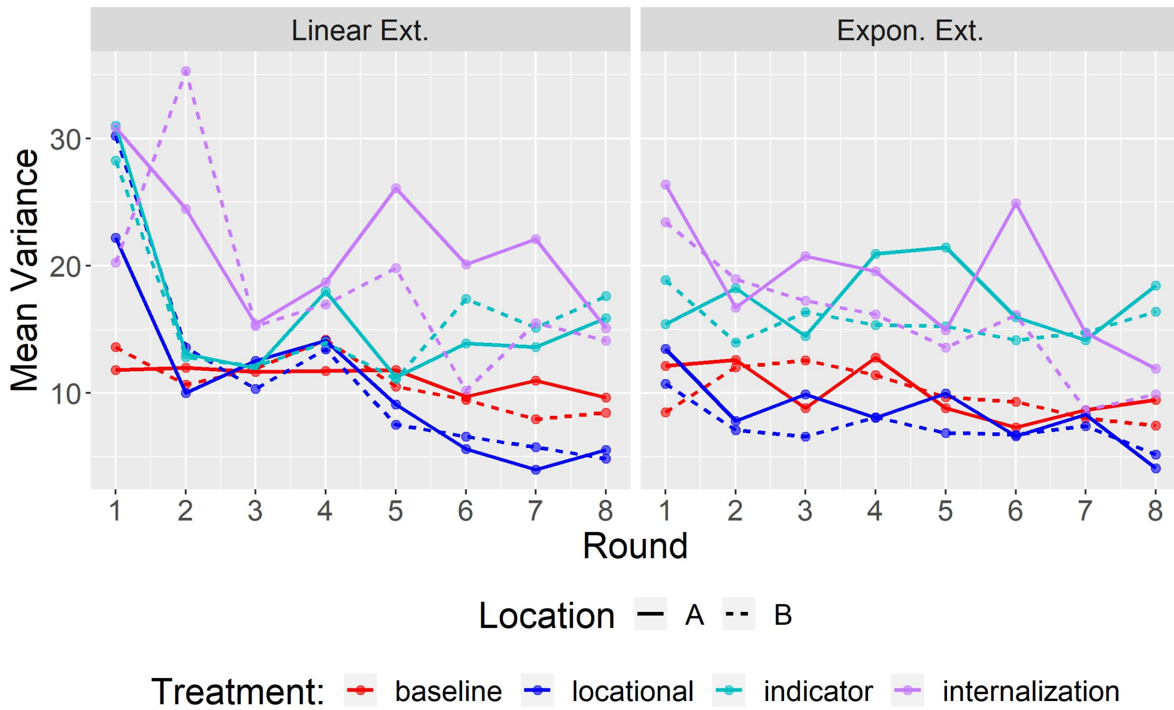


Figure 3.8: Variances of accepted prices for each round/treatment with additional separation by location. Variances are calculated within each group and round. Points display average over group variances.

3.5 Conclusion

This paper studies ways to deal with externalities that depend on the specific match between buyers and sellers in the marketplace. While our experimental design is inspired by electricity markets, such match-dependent externalities are also prevalent in transportation settings.

We employ a modified double auction platform to incorporate locational information into the market design. Our baseline treatment is inspired by typical market platforms that are location-blind and thus do not give market participants the information on externality-relevant characteristics of potential trading partners. In line with previous literature on double auction settings, the behavior is close to equilibrium predictions. Yet, in our settings this leaves substantial room for welfare improvements as the externality is not incorporated into individual decision making.

We demonstrate that locational information can already reduce the externality. Imposing the full external costs on individual trades leads to maximal price differentiation between locations and further reduces net trades. While the externality is thus countered, the welfare gains do not fully materialize.

Our data suggests that this could be driven by the sequentiality of trades in the double auction setting: if multiple units are traded between the locations, the external costs imposed on the individual trade vary which limits the convergence towards the optimal internalization of external effects in all trades.

Our paper adds to the literature on externalities arising in market settings. Most of this literature is concerned with externalities triggered through consumption, production, or trade. The role of externalities arising from the specific match between trading partners has largely been overlooked. Our experimental evidence provides first guidance to designing market platforms to deal with such match-dependent externalities.

3.6 Appendix

3.6.1 Appendix A: Supplementary Tables

Table A.3.1: Wilcoxon rank sum test pvals for difference in net-trades between treatments for linear externality

net-trades	lin baseline	lin *locational*	lin *indicator*	lin *internalization*
lin baseline	na	-	-	-
lin *locational*	0.874	na	-	-
lin *indicator*	0.317	0.399	na	-
lin *internalization*	0.001	0.001	0.004	na

Table A.3.2: Wilcoxon rank sum test pvals for difference between net-trades between treatments for exponential externality

net-trades	exp baseline	exp *locational*	exp *indicator*	exp *internalization*
exp baseline	na	-	-	-
exp *locational*	0.004	na	-	-
exp *indicator*	0.074	0.712	na	-
exp *internalization*	0.003	0.001	0.0059	na

Table A.3.3: Wilcoxon rank sum test pvals for difference in welfare between treatments for linear externality

welfare	lin baseline	lin *locational*	lin *indicator*	lin *internalization*
lin baseline	na	-	-	-
lin *locational*	0.793	na	-	-
lin *indicator*	0.599	0.645	na	-
lin *internalization*	1	1	0.563	na

Table A.3.4: Wilcoxon rank sum test pvals for difference in welfare between treatments for exponential externality

welfare	exp baseline	exp *locational*	exp *indicator*	exp *internalization*
exp baseline	na	-	-	-
exp *locational*	0.050	na	-	-
exp *indicator*	0.599	0.318	na	-
exp *internalization*	0.015	0.207	0.066	na

Table A.3.5: Wilcoxon rank sum test pvals for difference in variance between treatments for linear externality. Only observations from rounds 5-8 are used

Variance	lin baseline	lin *locational*	lin *indicator*	lin *internalization*
lin baseline	na	-	-	-
lin *locational*	0.234	na	-	-
lin *indicator*	0.798	0.195	na	-
lin *internalization*	0.0281	0.003	0.195	na

Table A.3.6: Wilcoxon rank sum test pvals for difference in variance between treatments for exponential externality. Only observations from rounds 5-8 are used

Variance	exp baseline	exp *locational*	exp *indicator*	exp *internalization*
exp baseline	na	-	-	-
exp *locational*	1	na	-	-
exp *indicator*	0.382	0.160	na	-
exp *internalization*	0.038	0.028	0.574	na

Table A.3.7: Wilcoxon rank sum test pvals for difference in variance between treatments for linear externality. Only observations from rounds 5-8 are used. Variances are calculated within each location

Variance	lin baseline	lin *locational*	lin *indicator*	lin *internalization*
lin baseline	na	-	-	-
lin *locational*	0.328	na	-	-
lin *indicator*	0.959	0.328	na	-
lin *internalization*	0.234	0.028	0.442	na

Table A.3.8: Wilcoxon rank sum test pvals for difference in variance between treatments for exponential externality. Only observations from rounds 5-8 are used. Variances are calculated within each location

Variance	exp baseline	exp *locational*	exp *indicator*	exp *internalization*
exp baseline	na	-	-	-
exp *locational*	0.798	na	-	-
exp *indicator*	0.382	0.130	na	-
exp *internalization*	0.160	<0.050	0.849	na

Table A.3.9: Valuations and cost and probabilities of actual trade for respective units in baseline treatment; shaded units are traded in equilibrium, linear externality.

	Location							
	A				B			
Unit	S1	S2	B1	B2	S1	S2	B1	B2
1	31(1.00)	33(1.00)	55(0.97)	53(0.98)	39(0.95)	41(0.97)	63(0.97)	61(1.00)
2	35(1.00)	37(1.00)	51(0.95)	49(0.95)	43(0.86)	45(0.92)	59(0.95)	57(0.98)
3	39(0.98)	41(0.97)	47(0.77)	45(0.36)	47(0.63)	49(0.17)	55(0.84)	53(0.94)
4	43(0.88)	45(0.72)	43(0.08)	41(0.00)	51(0.05)	53(0.00)	51(0.77)	49(0.72)
5	47(0.41)	49(0.11)	39(0.00)	37(0.00)	55(0.00)	57(0.00)	47(0.22)	45(0.17)

Table A.3.10: Valuations and cost and probabilities of actual trade for respective units in baseline treatment; shaded units are traded in equilibrium, exponential externality

	Location							
	A				B			
Unit	S1	S2	B1	B2	S1	S2	B1	B2
1	31(0.98)	33(1.00)	55(0.94)	53(0.94)	39(1.00)	41(0.97)	63(1.00)	61(1.00)
2	35(0.97)	37(1.00)	51(0.87)	49(0.84)	43(0.92)	45(0.83)	59(1.00)	57(1.00)
3	39(0.94)	41(0.98)	47(0.66)	45(0.23)	47(0.52)	49(0.20)	55(0.95)	53(0.91)
4	43(0.78)	45(0.89)	43(0.05)	41(0.02)	51(0.02)	53(0.00)	51(0.75)	49(0.69)
5	47(0.33)	49(0.22)	39(0.00)	37(0.00)	55(0.00)	57(0.00)	47(0.44)	45(0.19)

Table A.3.11: Valuations and cost and probabilities of actual trade for respective units in *internalization* treatment; shaded units are traded in equilibrium, linear externality

	Location							
	A				B			
Unit	S1	S2	B1	B2	S1	S2	B1	B2
1	31(0.81)	33(0.94)	55(0.97)	53(0.97)	39(1.00)	41(0.95)	63(0.98)	61(0.98)
2	35(0.77)	37(0.91)	51(0.95)	49(0.92)	43(0.97)	45(0.90)	59(0.86)	57(0.95)
3	39(0.72)	41(0.88)	47(0.73)	45(0.70)	47(0.81)	49(0.73)	55(0.75)	53(0.88)
4	43(0.45)	45(0.44)	43(0.38)	41(0.28)	51(0.36)	53(0.14)	51(0.38)	49(0.33)
5	47(0.17)	49(0.13)	39(0.05)	37(0.00)	55(0.03)	57(0.00)	47(0.02)	45(0.03)

Table A.3.12: Valuations and cost and probabilities of actual trade for respective units in *internalization* treatment; shaded units are traded in equilibrium, exponential externality

	Location							
	A				B			
Unit	S1	S2	B1	B2	S1	S2	B1	B2
1	31(1.00)	33(0.84)	55(0.92)	53(0.95)	39(1.00)	41(0.97)	63(0.98)	61(0.97)
2	35(0.97)	37(0.81)	51(0.89)	49(0.92)	43(0.95)	45(0.92)	59(0.95)	57(0.86)
3	39(0.91)	41(0.61)	47(0.81)	45(0.73)	47(0.75)	49(0.70)	55(0.83)	53(0.64)
4	43(0.59)	45(0.36)	43(0.41)	41(0.17)	51(0.23)	53(0.16)	51(0.69)	49(0.30)
5	47(0.20)	49(0.17)	39(0.02)	37(0.00)	55(0.02)	57(0.06)	47(0.14)	45(0.05)

Table A.3.13: Random effects estimation with price spread as dependent variable. Rounds are noted as 1-8.

	Price Spread			
	linear		exponential	
locational	0.91	1.72	1.47*	−0.21
indicator	1.41	1.61	1.34	0.61
internalization	6.88***	6.42***	5.00***	4.64***
round	−0.23**	−0.20	0.10	−0.05
round**locational*		−0.18		0.37
round**indicator*		−0.04		0.16
round**internalization*		0.10		0.08
Constant	1.78**	1.64	0.15	0.84
Observations	256	256	256	256
R^2	0.17	0.17	0.13	0.14

Note: *p<0.1; **p<0.05; ***p<0.01

Table A.3.14: Random effects estimation with net trades as dependent variable. Rounds are noted as 1-8.

	Net trades			
	linear		exponential	
locational	−0.30	−0.29	−1.30***	−2.17***
indicator	−0.67	−0.41	−1.09**	−1.96***
internalization	−2.75***	−2.17***	−2.91***	−2.93***
round	0.06***	0.11**	0.08***	−0.02
round**locational*		−0.001		0.19***
round**indicator*		−0.06		0.19***
round**internalization*		−0.13*		0.01
Constant	2.72***	2.50***	3.20***	3.65***
Observations	256	256	256	256
R^2	0.19	0.21	0.16	0.21

Note: *p<0.1; **p<0.05; ***p<0.01

Table A.3.15: Random Effects Estimation with final welfare as dependent variable. Rounds are noted as 1-8.

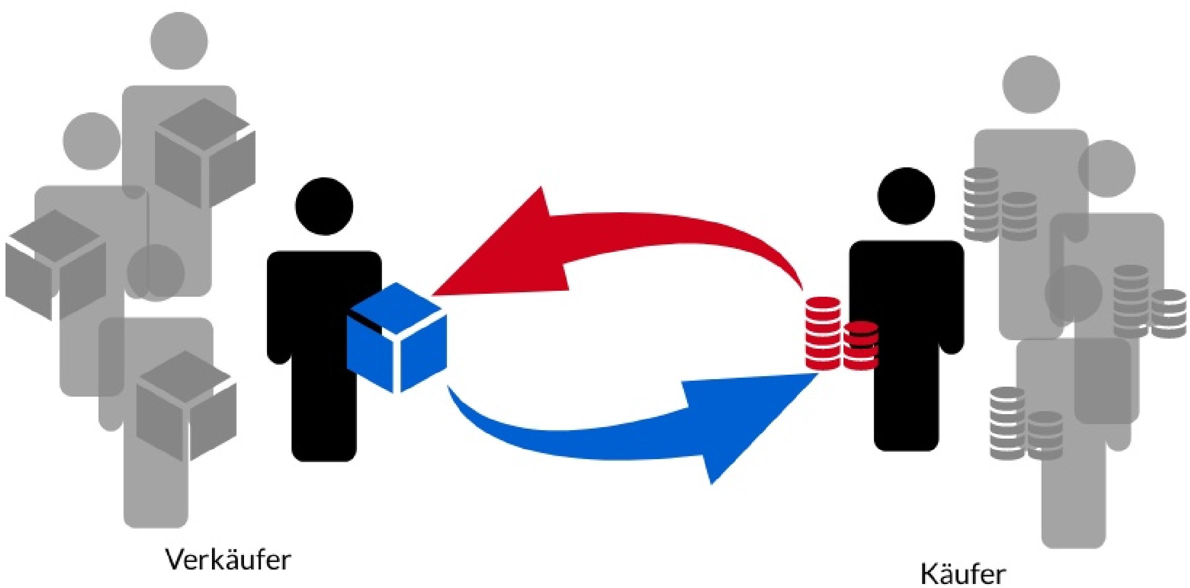
	Welfare			
	linear		exponential	
locational	2.45	1.64	18.93***	27.58***
indicator	5.09	2.76	15.34**	26.94***
internalization	23.77***	17.12***	37.58***	38.96***
round	−0.27	−0.82*	−0.54	0.66
round**locational*		0.18		−1.92
round**indicator*		0.52		−2.58**
round**internalization*		1.48**		−0.31
Constant	−6.92**	−4.47	−19.43***	−24.84***
Observations	256	256	256	256
R^2	0.18	0.20	0.10	0.12

Note: *p<0.1; **p<0.05; ***p<0.01

3.6.2 Appendix B: Instructions (translated from German)

Thank you for participating in this economic experiment. Please read these instructions carefully, you will have to answer test questions later. The instructions are identical for all participants. Your payout will depend on both yours' and other participants' behavior. You will be paid after the experiment.

In this experiment, you participate in a market where you are either a buyer or a seller. The currency on the market is not Euros, but EU. Sellers own goods that they can sell to buyers for EU.



In addition, buyers and sellers are in different locations, which can lead to costs. Once the experiment starts, you will see on your screen if you are a buyer or a seller, what location you are in, and a custom table that shows the value each purchase/sale has to you. Only you have this information; no other participant knows your table.

Buyers:
Buyers can acquire a maximum of five units of the good from any number of sellers in each

market period. Each unit results in a different payout for them. The example table shows that the first unit purchased has a value of 50 EU, the second unit 40, and so on (note that you will get a different table in the experiment). In addition, you will receive a commission of 0.5 EU for every purchase.

Payout Table (Example)	
Unit	Payout (in EU)
1	50
2	40
3	30
4	20
5	10

The profit from each purchase is calculated from the payout minus the price paid plus the commission:

$$\text{Profit} = \text{(Payout)-(Price)+(Commission)}$$

For example, if you bought one unit for 30 EU and one unit for 25 EU (using the table above), your profit is as follows (including the 0.5 EU commission per purchase):

$$\begin{aligned} &\text{Profit Unit 1:} && = 50 - 30 + 0.5 && = 20.5 \\ &\text{Profit Unit 2:} && = 40 - 25 + 0.5 && = 15.5 \\ &\text{Total Profit:} && = 20.5 + 15.5 && = 36 \end{aligned}$$

Sellers:
Sellers can sell a maximum of five units of the good in each market period. Each unit sold results in different costs for them. The example table shows that the first unit sold has a cost of 10 EU, the second unit has a cost of 20 EU, and so on (note that you will get a different table later in the experiment). In addition, you will receive a commission of 0.5 EU for every sale.

Cost Table (Example)	
Unit	Costs (in EU)
1	10
2	20
3	30
4	40
5	50

Your profit from each sale is calculated as the price received minus the cost of the unit plus the commission.

$$\text{Profit} = (\text{Price})\text{-}(\text{Costs})+(\text{Commission})$$

For example, if you sold one unit for 35 EU and one unit for 40 EU, your profit is as follows (including the 0.5 EU commission per sale):

$$\begin{aligned} &\text{Profit Unit 1:} && = 35 - 10 + 0.5 && = 25.5 \\ &\text{Profit Unit 2:} && = 40 - 20 + 0.5 && = 20.5 \\ &\text{Total Profit:} && = 25.5 + 20.5 && = 46 \end{aligned}$$

Market:
The market where you can buy/sell is made up of 4 buyers and 4 sellers. It is organized as follows: The market stays open for 2 minutes each round. A corresponding timer is displayed. As long as the market is open, buyers can bid and sellers can bid. A purchase bid indicates the willingness to pay the bid amount for a unit of the good. A sell bid indicates the willingness to sell a unit for the amount offered. Buyers can accept a bid to sell at any time and sellers can accept a bid to buy at any time. To accept a bid, simply click on the relevant bid. No bids can be submitted or accepted in which participants would lose money. All bids are visible to all participants at all times. Below is a screenshot of the market surface:

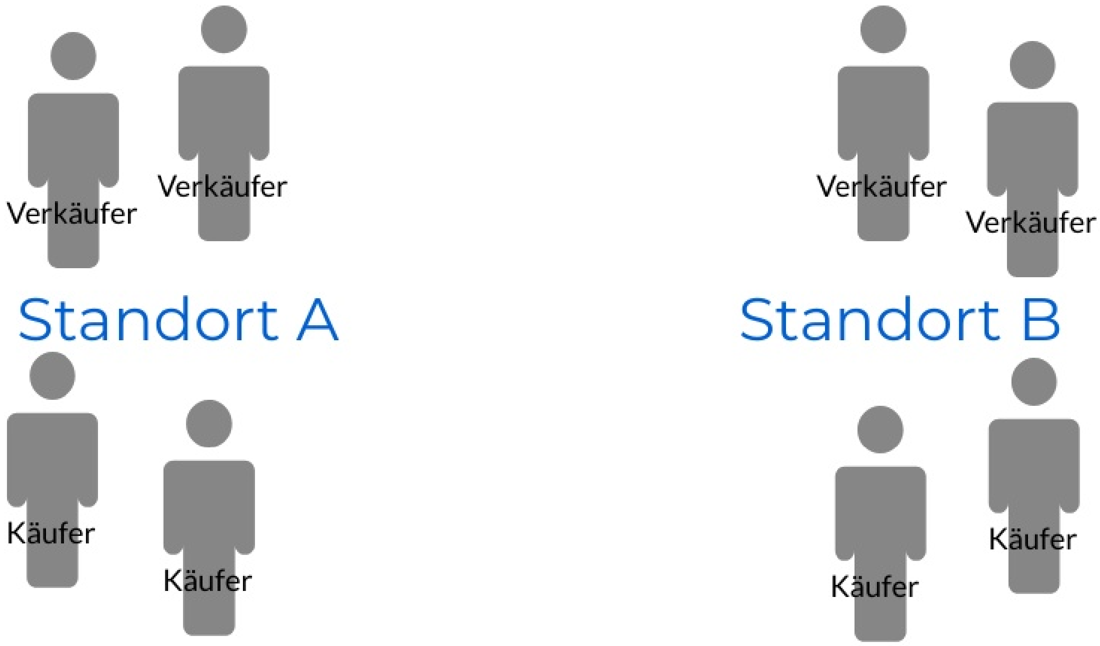
In addition to the amount of the bid, the location from which the bid comes is displayed. This is relevant for the development of potential damage. In addition, you will be informed about the current trade balance between the two locations A and B, and what damage is currently arising from it. Every bid on the market is adjusted so that it already includes the resulting damage or damage avoidance. The amount of this damage or the damage avoidance is displayed next to the bids that have been made. The occurrence of damage is explained in detail below.

If a bid is accepted, the offered / requested unit is transferred at the corresponding price. All bids will then be deleted and new bids can be submitted. Even if your bid is not accepted, you can keep trying to trade and make as much profit as possible.

If the time runs out, the trading period ends. All profits are realized and all units of the good are reset. Then a new round begins. There will be a total of 16 rounds.

Location

There are always 4 buyers and 4 sellers. Each participant is either in location A or in location B. In each location there are 2 buyers and 2 sellers. A trader's location does not change during the experiment.



Damages

At the end of each round you will receive a bonus of 10 EU. However, trade can cause damage if it is unbalanced between different locations.

Damage is only caused by net trade between locations. Trading within a location (e.g. between a buyer and a seller in location A) never leads to damage. Trade between locations (e.g. between a buyer in location A and a seller in location B) can cause damage. However, this only happens when trade in one direction predominates. If a unit is sold from location A to location B and a unit from location B to location A, no damage results.

The amount of the total damage depending on the net trade is shown in a table. You can see an example table here:

Damage Table	
Net-Trade between Locations	Damage (in EU)
0	0
1	10
2	20
3	40
4	70
5	80

Examples:

- Upon end of the round, the following trades have occurred: 4 trades within location A, 3 trades within location B. Since there is no trade between A and B, no damage has occurred.
- If there have been 4 trades from A to B and 2 trades from B to A, the net trade between locations is 2. This results in a damage of 20.
- If there have been 5 trades from B to A and 1 trade from A to B, the net trade between locations is 4. This results in a damage of 70.

Test Questions:

1. Suppose you are a buyer with the following payout table:

Payout Table (Example)	
Unit	Payout
1	80
2	60
3	30
4	20
5	10

   You have acquired a total of 3 units in one round. How high is your total payout (regardless of the prices paid)? (170)

2. You paid a price of 40, 60 and 30 for units 1, 2 and 3. What is your profit this round? Note the commission per purchase. (170-130 + 1.5 = 41.5)

3. Suppose you are a seller with the following cost table:

Cost Table (Example)	
Unit	Costs
1	20
2	30
3	30
4	40
5	60

   You have sold a total of 3 units in one round. What are your total costs (regardless of the prices paid)? (80)

4. You received a price of 40, 60 and 30 for units 1, 2 and 3. What are your profit this round? Note the commission per purchase. (130-80 + 1.5 = 51.5)

5. The following damage table is given.

Damage Table	
Net-Trade between Locations	Damage
0	0
1	10
2	20
3	40
4	70
5	80

   What is the total damage if there are 5 trades from A to B and 3 trades from B to A? (20)

6. At the end of a round there is a damage of 20. What is the portion that is deducted from you?(0)

The Medical and Financial Threat of COVID-19 Deteriorates Efficiency Considerations, Boosts Altruism, but Keeps Time Preferences Constant

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Abstract

This study examines inter-temporal distribution decisions on private payments and donations in times of the coronavirus pandemic. We measured simultaneously individual efficiency concerns, altruism and time preferences in an online experiment conducted among US residents in different stages of the crisis' maturity. Participants were asked to distribute money between different dates and recipients ,that is, today versus in two weeks, private payouts versus donations for the fight against the pandemic. To assess participants' affectedness by COVID-19, we collected data on participants' employment status and financial situation, as well as their individual vulnerability against COVID-19. We demonstrate that having at least one COVID-19 risk-factor and self-reported financial affectedness are negatively correlated with the individual preference for efficiency, whereas the same factors have a positive effect on altruism. Our results point at an equally important role of personal, financial and medical security for the willingness to provide support during the crisis.

JEL classification: D91, H12, I12

Keywords: Experiments, COVID-19, Social Preferences, Intertemporal Choice, Crisis, Prosociality

4.1 Introduction

In 2021, the world is in its second COVID-19 year. The virus keeps our planet in suspense and puts entire societies into states of emergency, generating a rapid growth of people in need. As of June 2021, over 33.5 million people were infected in the US alone (Dong et al., 2020), resulting in adverse effects like 9.9 million fewer jobs compared to February 2020, and 48 percent of all US households reporting job or income loss (Root & Simet, 2021). These findings are not restricted to the US but also affect citizens of low- and middle-income countries (Egger et al., 2021). In response, both immense state aid programs and enormous private donations are provided, although donors themselves are often medically and financially affected by the coronavirus pandemic. Our study gives insight into the extend to which the personal involvement of the donors affects the way they are willing to provide support in these times of crisis. That is, are people more altruistic when personally involved with COVID-19 infections in their personal proximity? Are they less willing to donate when being financially negatively affected by the pandemic? How does the number of COVID-19 cases in the personal environment influence efficiency considerations: do people postpone their donations if giving at later times yields the higher benefit for the recipient? Or do they forgo efficiency gains for the sake of immediate giving when the pandemic rages in their near environment?

Our study is of major importance as the effect of the coronavirus pandemic is not only limited to direct impacts on health and labour provision but can also be detrimental to behavioural patterns such as altruism, efficiency and time preferences. Our study connects different facets of the crisis with these contributors of long-term behavioural aspects. Thus, the results will inform government policy on the one hand how crises affect peoples' preferences for efficient spending, donations and saving behaviour. On the other hand, our findings may help policy makers to develop countermeasures in order to alleviate the specific shape of the behaviour changes. Since this crisis is far from being over yet, and further pandemics are almost certain in the future, this is necessary to channel important drivers such as medical or financial vulnerabilities. Our findings will help us to avoid activism for the sake of doing something and to optimise an efficient crisis management at the economic and political level.

Throughout our study, we apply a two stage analysis strategy: in a first stage, we experimentally measure three key economic preferences among our participants on the individual level. These are: the inter-temporal discount factor for private payments, a time-independent degree of altruism weighting donations relative to own income, and a variable measuring the importance of individual efficiency concerns. In a second stage, we estimate whether the three parameters vary systematically with the degree by which the pandemic influences the individual environment of subjects. For this, we use data of an online experiment with American participants in spring and summer 2020. During that time, the COVID-19 crisis progressed, creating a natural variation of the impact of the pandemic on participants' life.

We structure the personal involvement along three dimensions: first, we analyse whether medical involvement concern (e.g., personal infection risk, the infection of a closed family member, or the number of infected subjects in their state) with regards to the COVID-19 influences individual parameters for altruism, efficiency concerns, the willingness to postpone private consumption, and the willingness to postpone donations. Second, we ask whether financial concerns (e.g., substantial financial losses, unemployment, or the loss of healthcare cover) affects our three parameters. Finally, we investigate to what degree political alignment alters the parameters in a systematic way.

In the long run, economic preferences are assumed to be stable (Krupka & Stephens Jr, 2013; Meier & Sprenger, 2015; Stigler & Becker, 1977). However, a more recent strand of literature finds that experiences can change preferences (Schildberg-Hörisch, 2018). Negative experiences, such as wars or natural disasters are likely to impact preferences (Haushofer & Fehr, 2014), for example individual risk preferences (Dohmen et al., 2016; Eckel et al., 2009; Guiso et al., 2018; Malmendier & Nagel, 2011). Literature on the malleability of time preferences is more scarce: while studies show that systematic changes in the decision environment can influence time preferences (DeSteno et al., 2014; Ifcher & Zarghamee, 2011) and amplify over time (Meier & Sprenger, 2015), the potential effect of naturally occurring extreme events on time preferences is far less clear (Bauer & Kramer, 2016; Cassar et al., 2017). Likewise, there is mixed evidence towards crises having a positive (Voors et al., 2012) or negative (Fisman et al., 2015) effect on altruism. Reciprocity may change in the aftermath of a natural disaster (Cassar et al., 2017; Picozzi et al., 2014), as do public good donations (Whitt & Wilson, 2007). Evidence from earthquake-affected and non-affected Chilean villagers shows that reciprocity to unilateral trust is lower in earthquake-affected areas (Fleming et al., 2014).

Regarding the specific effect of COVID-19, there is a most recent literature testing for instance its influence on health relevant behaviour (Campos-Mercade et al., 2021). Compared to pre-pandemic levels, there is an increase in altruism, cooperation, trust and risk tolerance (Shachat et al., 2020). The positive effect on generosity is also supported by other studies (Branas-Garza et al., 2020). Additionally, increasing the salience of the pandemic positively impacts altruism but also tolerance to inequalities due to luck (Cappelen et al., 2021). With respect to the effects of lockdown measures, there is evidence towards behaviour shifts following the implementation and abolition of social distancing rules (Casoria et al., 2021). Hence, there is already some evidence on the potential effects of COVID-19 on preferences, but the previously mentioned studies miss the – from our perspective – important effect of the pandemic on efficiency concerns. Furthermore, the COVID-19 pandemic differs from other disasters such as earthquakes and tsunamis, which have been the main focus of the disaster literature, in that it is a long-term threat with nearly no regions that are save from being affected.

To provide a complete picture of the potential preference shift, we adapt the state-of-the-art approach(Cohen et al., 2020; Frederick et al., 2002, A number of surveys review the research on time preferences, e.g.,) to measure time preferences, efficiency concerns and altruism simultaneously based on convex time budget decisions (Andreoni et al., 2018; Andreoni &

Sprenger, 2012; Augenblick et al., 2015). In earlier studies, when exploring the interplay between time preferences and altruism (i.e., when dividing money between oneself and another person), subjects are substantially more selfish with immediate in comparison to delayed consequences (Koelle & Wenner, 2018). While giving to others decreases with delay (Buser & Dreber, 2016; Kovarik, 2009), the relation may not be monotonic (i.e., altruism increases between a one and two months time gap of charitable giving (Breman, 2011)).

Our results show that in general individuals react predominantly to efficiency as long as they distribute money between the same recipient (themselves versus themselves or charity versus charity). As soon as money is allocated between different recipients, efficiency plays a minor role suggesting that a moral dilemma overrules efficiency concerns. With respect to the impact of the crisis, all three aspects, that are, having at least one COVID-19 risk-factor, self-reported financial affectedness, or political conservatism are negatively correlated with the individual preference for efficiency concerns. Surprisingly, the same factors have a smaller effect on individual altruism. We find that neither the number of cases per capita nor the 7-day incidence in the state of residence have an impact on behavioural parameters. We also do not find any change in time preferences for any of our explanatory variables. Yet, there is considerable heterogeneity: women display a smaller behavioural changes with respect to their altruism and their efficiency concerns are not influenced by financial affectedness. We find slight differences in how age groups react to the crisis: the negative effect of financial affectedness on efficiency concerns decreases with participants' age.

4.2 Results

In our experiment, participants make a series of distributive decisions. In each decision, they divide wealth in the form of ten experimental tokens – provided by the experimenters – between two options that vary with respect to the recipient, time of payment and efficiency. See figure 4.1 for a design overview and sample decisions. In some decisions participants divided money between themselves and a charity supporting medical facilities in their fight against the coronavirus pandemic, while one time of payment was directly after and the other 14 days past the experiment. In other decisions, participants allocated money only for themselves or only for the charity, with the same difference in payment timing. Finally, we featured two decisions allowing participants to allocate tokens between the participant themselves and the charity, but no time difference between payments. For each combination of recipient and timing, the participants made six decisions. A multiplier varied for the second option altering the efficiency of tokens allocated to the second option. Participants can increase efficiency by allocating more to the option with the higher multiplier, independent of the corresponding recipient. We thus define a raise in efficiency as an increase in total monetary payments. For example, a single decision may be as follows: the participant distributes ten tokens to either receive the token value herself directly after the experiment, or to have 1.4 times the tokens' value donated after 14 days. The corresponding amounts paid out were displayed next to the sliders, to eliminate any need for calculation on the participants' side. In total, there were six blocks of allocation decisions with six efficiency variations each, resulting in 36 decisions per participant. We randomized the order of the block for each participant, to control for any order effects. These decisions indicate participants' preferences for an immediate payment, efficiency, and a donation.

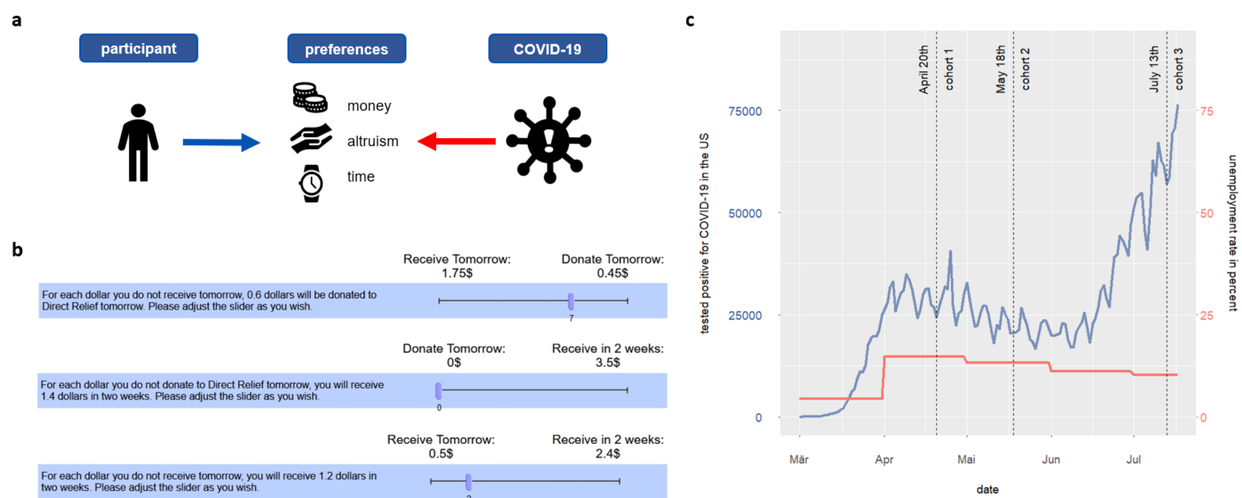

Figure 4.1: Model of Experiment, Examples and Dates. **a**, Schematic model of the experiment. The preferences of our participants are shaped by medical and financial affects of the coronavirus pandemic. **b**, Sample of 3 out of 36 slider tasks solved by the participants. Recipient, efficiency, dates of payments and selected allocation of payments varies for this screenshots. **c**, Dates of data collection, as well as accumulated number of COVID-19 infections in the United States by date in blue and unemployment rate in the US in red. The experiment was executed in three cohorts to cover different maturity of the coronavirus pandemic.

Table 4.1: List of variables

Variables	Description
financial factors	
financially affected	survey question based on how strong participants declare to be financially affected by the pandemic on a scale from 1 (no changes) to 10 (very severe changes)
jobloss	survey question whether participant lost job due to the coronavirus pandemic
stateunempl	unemployment rate of participant's state and cohort (U.S. Bureau of Labor Statistics, n.d.)
medical factors	
casespercap	all confirmed cases of participant's state since start of the pandemic per 100.000 residents (U.S. Census Bureau, n.d.)
insurance	survey question whether participant is health insured at the date of the experiment
knowinfected	survey question whether participant knows someone who has COVID-19 or has had the disease himself/herself
predisposed	survey question whether participant is in at least one of the risk groups (like being immunocompromised and/or diabetic) for severe COVID-19 disease
stringency	stringency index for the participant's state at the time of participation
political alignment	
conservatism	survey question of participant's political orientation on a scale from 1 (liberal) to 10 (conservative)

After the experiment we surveyed the participants to obtain these variables. By providing their state, we could additionally determine *casespercap* and *stringency*. Analysis of positive tested with COVID-19 (COVID Tracking Project at The Atlantic, n.d.) is based on data by CC and is licensed under CC BY-NC 4.0. Our results do not change in a relevant way if we use the 7-day incidence instead of cases by state. The stringency data are based on the stringency index (Hallas et al., n.d.) of the participant's state which summarises the lockdown style closures and containment policies with regard to COVID-19 in one figure.

4.2.1 Experimental Procedures

The experiment was conducted online in three cohorts with a target of 300 participants each through Amazon Mechanical Turk (MTurk). In total, 886 MTurkers participated in April, May and July 2020. The implementation at different points in time and the associated subdivision into cohorts allows different levels of maturity of the pandemic to be analysed: At the time of the first cohort, the medical impact of the pandemic was at a high plateau, while the economic impact peaked. In the following cohort, the economic impact slightly decreased and the medical aspects were at a moderate level. At the time of the third cohort, the medical impact of the coronavirus pandemic had increased sharply respectively achieved a high level while economic impact still decreased somewhat. We aimed at recruiting participants from both weakly and strongly affected states, in order to have enough variation in the impact of COVID-19 between cohorts. At the moment of the data collection of the first cohort, the selected states were of particular interest because of the comparatively low/high number of cases relative to the population of the state.

4.2.2 Empirical Analysis

Table 4.1 presents the details about the financial and medical variables, which we later use in the regression. Additionally. table A.4.1 in the appendix gives an overview over participant and state demographics both for our full sample and for each cohort separately. While most of our demographics remain stable over the cohorts, there is a trend towards more individuals that are/have been infected (positively tested for COVID-19). Nevertheless, the variable *infected* is not suitable for our analysis as there is only very little variation in our sample as only 25 individuals in our sample have been tested positive for COVID-19. However, the variable *knowinfected*, a dummy that is one if the participant knows at least one person that has been tested positive, shows a similar development over time. We thus assume that *knowinfected* captures the individual risk of contracting COVID-19 and thus is a good replacement for being/having been infected. There also is a trend towards more participants reporting to have at least one risk-factor for COVID-19, as captured in the *infectionrisk* variable. This could be due to changes in knowledge about these risk-factors themselves. There also is a clear trend towards leaning more towards conservative politics, as shown by the variable *conservatism*. While it is not the focus of our study, we include the variable as it is correlated with our measured behaviour. Unemployment numbers are actually decreasing over cohorts, which can be explained by the steep 10%-points rise in US unemployment numbers from March to April, before the start of our data collection.

We start with the analysis of the impact of COVID-19 on behavioural preferences. Our study was designed to capture individual behavioural parameters, specifically sensitivity to efficiency, altruism and time preference. We use each participants 36 incentivised choices to estimate the three parameters on the participant level. These parameters capture: how much more an individual gives to a more efficient option, how much more an individual gives to a charitable option, and how much more an individual gives to an option that is paid out at a sooner point in time. These parameters are assumed to be linear and to not interact with each other. We can thus use the following estimation model for each individual:

$$c_{1ij} = \gamma_0 + \mathit{efficiency}_i(1 + r_j) + \mathit{altruism}_i(d_{1j} - d_{0j}) + \mathit{delta}_i(t_{0j} - t_{1j}) + \epsilon_i \tag{4.1}$$

Where i indicates the individual and j the corresponding block of 6 sliders. c_{1ij} is the consumption choice on the right option, which is the option that varies in efficiency between the sliders. In line, $(1 + r)$ is the corresponding multiplicator (0.6, 0.8, 1.0, 1.2, 1.4, 1.6). The dummies d_{0j}, d_{1j} indicate whether the left/right option is a donation or not. Similarly, t_{0j}, t_{1j} indicate that the left/right option is paid out at a later point in time. Table 4.2 in the appendix shows the exact coding of the dummies for each block. The expressions $(d_1 - d_0)$ denotes differences in recipient between options. The corresponding coefficient, *altruism* captures how much more an individual gives to a donation. $(t_0 - t_1)$ indicates a difference in the time of payment, so that the coefficient *delta* shows how much more an individual allocates to an option that pays out sooner. As a result, the estimates *efficiency*, *altruism* and *delta* are centred around 0, with a value of 0 indicating not reacting at all to efficiency, the recipient or the time of payment.

We continue to estimate equation 4.1 by applying a maximum-likelihood Tobit estimator where possible, to account for the censored data, and using linear OLS otherwise. Our estimates average at the following values: *efficiency*$= 1.75$, *altruism*$= -1.18$, *delta*$= 0.18$. These can be interpreted as follows: On average, our participants give 1.75 more tokens out of 10 to an option that pays out 1 additional USD per token allocated. Also, 1.18 less tokens are given to an option that is a donation (when the other option is not a donation). Finally, 0.18 more tokens are allocated to an option that is paid out earlier. Note that although the mean estimates fit well with the literature, we find a significant amount of individuals that are efficiency averse, over-altruistic or show negative present bias, as seen in figure 4.2. This can be explained by the overlapping of motives. An individual that displays high altruism may ignore efficiency when deciding to donate. This is supported by the correlation between our estimates, as also seen in figure 4.2. Our analysis is limited in that our data-set does not have enough individual observations to reliably estimate interactions between motives on an individual level.

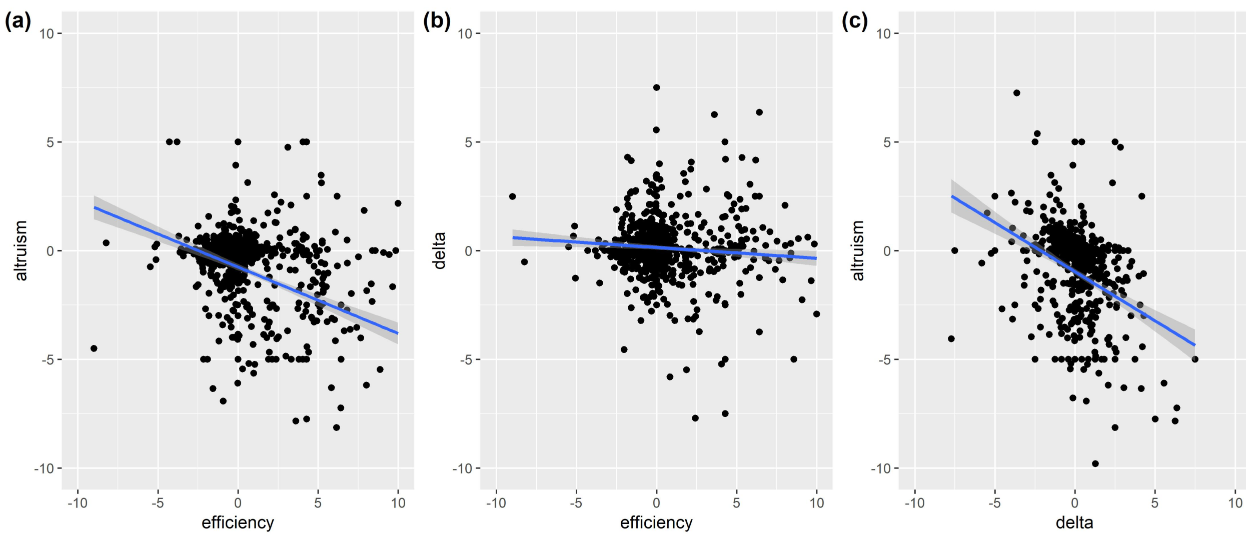

Figure 4.2: Scatterplot for all combinations of our 3 estimated variables. Each point represents a single participant. The line is a linear regression line between the pairs, with the shaded area indicating the confidence interval of the mean.

In a next step, we estimate the impact of the COVID-19 crisis on the previously gathered parameters. We use a linear OLS regression model to determine the exact impact of different indicators of COVID-19 affectedness on coefficients *efficiency*, *altruism* and *delta*. The corresponding explanatory variables have been standardised to allow for easy comparability. Figure 4.3 shows the estimated coefficients with corresponding confidence intervals. The regression includes control variables for: age, gender, state of residence, number of children, type of household, education and cohort dummies.

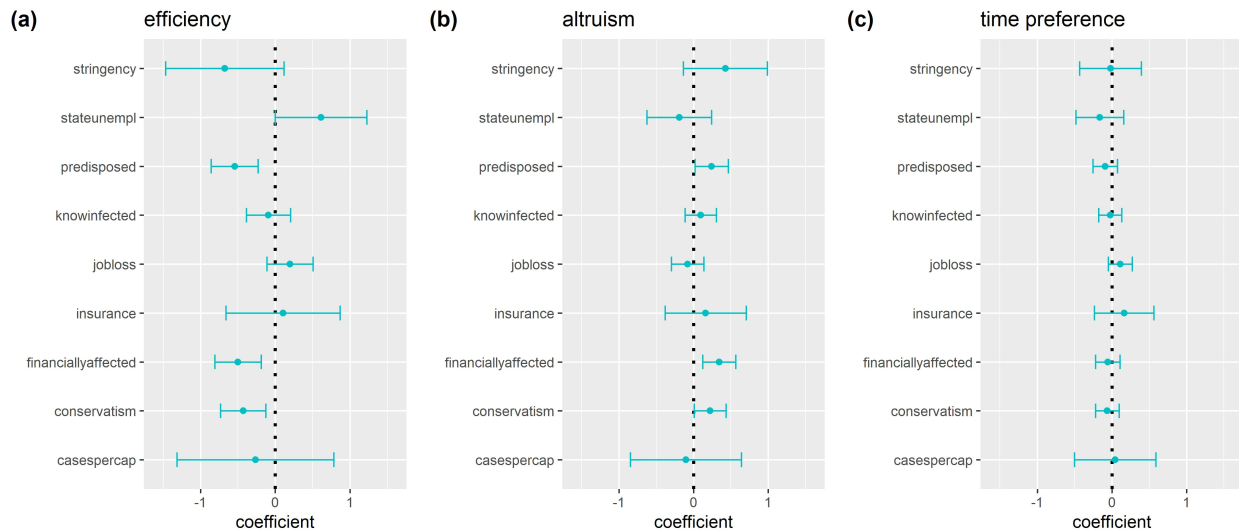

Figure 4.3: Coefficient Magnitudes. Error bars show 95%-confidence intervals.

Our first finding is that sensitivity to efficiency is negatively impacted by: being predisposed (t = -3.393, p = 0.001, 95% CI = -0.858, -0.229), self-assessed financial affectedness (t = -3.136, p = 0.002, 95% CI = -0.812, -0.187) and conservatism (t = -2.786, p = 0.006, 95% CI = -0.733, -0.128). This implicates that both being financially affected and having a high probability of severe health damages when infected have a negative impact on how individuals react to efficiency. In terms of effect sizes, the mentioned coefficients are not significantly different from each other. The effects differ with respect to altruism. The same measures that have a negative impact on efficiency concerns mostly have a positive effect on altruism. Being predisposed (t = 2.131, p = 0.034, 95% CI = 0.019, 0.465), self-assessed financial affectedness (t = 3.043, p = 0.003, 95% CI = 0.122, 0.565) and conservatism (t = 2.015, p = 0.045, 95% CI = 0.006, 0.435) all increase altruism. With respect to time preference, none of our explanatory variables have a significant effect.

We also analyse the data with respect to gender and age heterogeneity. Figure 4.4 shows the coefficients and confidence intervals separated by gender. We find one main difference for males and females: While males' efficiency concerns are negatively impacted by being financially affected, the effect is not distinguishable from zero for females. This difference is significant between genders (t= 3.289, p = 0.002, 95% CI = 0.421, 1.668). The average level of financial affectedness only varies very little between genders, with 5.23 for males and 5.18

for females. This points towards a major difference in how sexes react to the financial impact of COVID-19.

When we look at effect differences for various age groups, figure 4.5 displays the effect sizes for different age brackets. As seen in the top left, the effect of *financialaffected* on efficiency seeking behaviour increases with the age of the participant. In addition, the positive effect of financial affectedness on altruism and time preferences is most pronounced for individuals aged between 41 and 50. This also holds true for the effect of conservatism, with the strongest effects again falling into the 41-50 age group. We also have a look at the effect of the strength of government measures, indicated by the stringency variable. We find that altruism increases with stringency in the oldest age group compared to the other age groups.

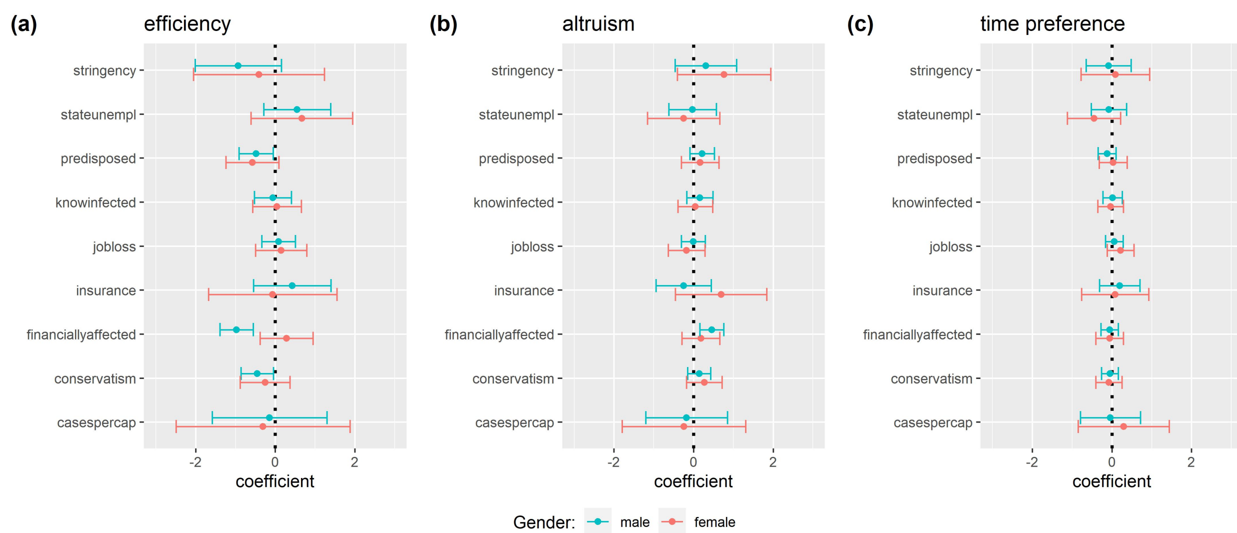

Figure 4.4: Coefficient Magnitudes, separated by gender. Error bars show 95%-confidence intervals.

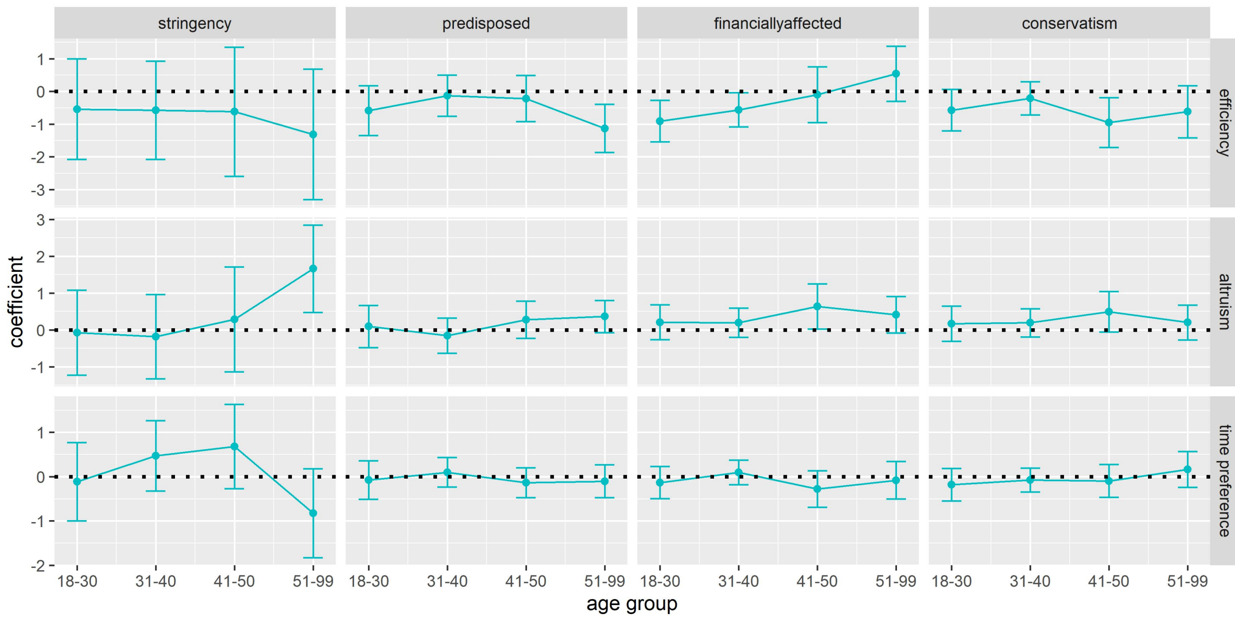

Figure 4.5: Coefficient Magnitudes, separated by age groups. Error bars show 95%-confidence intervals.

Another finding of our experiment is the reaction to efficiency depending on the recipient. We estimate each individuals reaction to the level of efficiency $(1 + r)$ within each decision block. The aim here is to see if there is any difference with respect to how individuals react to efficiency alone, depending on the combination of recipients. Figure 4.6 presents the average coefficient on efficiency within each block. As before we use maximum likelihood Tobit estimator where possible and linear OLS otherwise. The main finding here is that while the reaction to efficiency is strongly pronounced in decision blocks where the recipients are identical (e.g. charity vs. charity or participant vs. participant), but much less so when recipients are mixed (Kruskal-Wallis rank sum test , S_0S_1 & D_0D_1 vs. all other blocks, p-value $<$ 2.2e-16). We see this as major evidence towards individuals ignoring efficiency concerns when weighing private payments against a donation. This is interesting in that it does not fit to the large body of literature showing that the price of giving influences altruistic decision-making. Another interpretation of our results would be that altruism only affects the amount of tokens distributed, not the actual payments that follow from the distribution. We can reject that individuals go for a reference point in private payment or the donation, as there is no significant difference in the reaction to efficiency dependent on which option is affected by the multiplier (paired t-test, S_0D_1 vs. D_0S_1: t = -0.541, p = 0.588, 95% CI:

-0.253, 0.144).

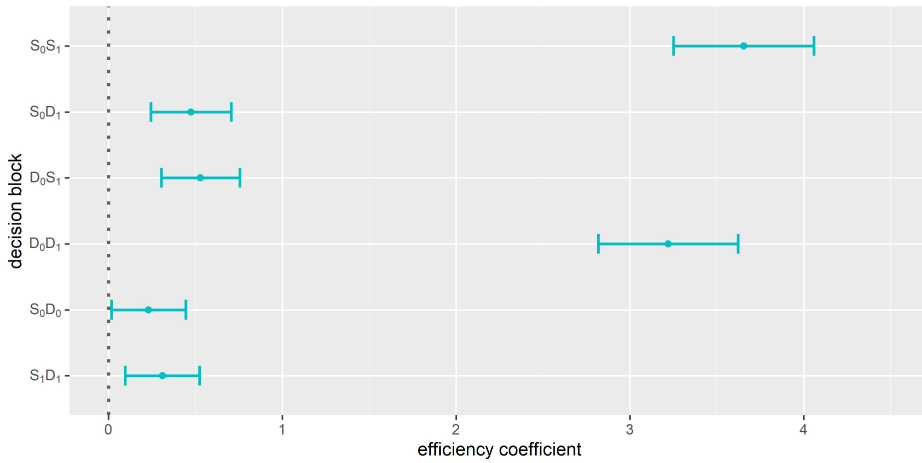

Figure 4.6: Efficiency Coefficient for each decision block. We estimate each participants reaction to the efficiency level within each block, then take averages over individual estimates. Error bars show 95%-confidence intervals. We define the name of the decision blocks to represent the combinations of recipient and timings. For instance, S_0D_1 consists of one option being payout to the participant (S) directly after the experiment (0) and the other option being a donation (D) after two weeks (1). For a more detailed explanation, and how the names map into the dummy variables mentioned earlier, refer to table 4.2

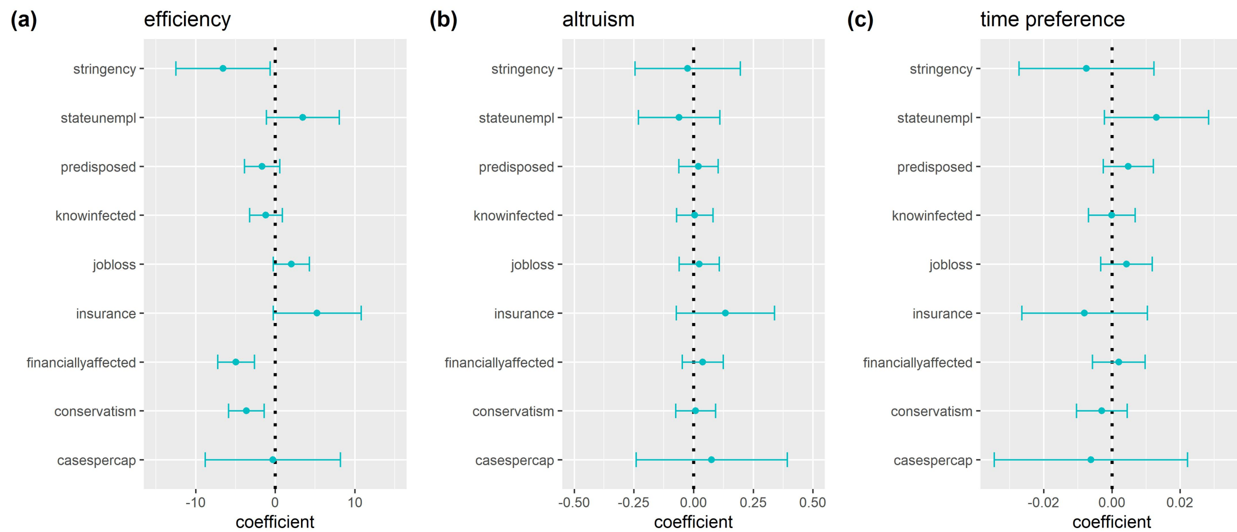

Figure 4.7: Coefficient Magnitudes from the structural estimation. Coefficients shown are based on an assumed 12 USD daily base consumption level. Error bars show 95%-confidence intervals.

We expand our analysis by applying a CARA structural model pioneered by Andreoni & Sprenger (Andreoni & Sprenger, 2012) to our data. A structural estimation approach has the advantage that it can capture economic preferences based on a underlying micro-economic model and deliver more fitting estimates. However, due to the structure of our data we can not use the structural estimation for all our participants. The explanatory variables are identical to the non-structural model. The main difference is the usage of base consumption levels in the dependent variable and the normalisation of the estimates. The effect of time and recipient differences are normalised by the efficiency reaction, which refers to the utility function curvature in the structural model. The structural approach has one main caveat for our data, which are nonsensical estimates of the mentioned utility function curvature which are restricted to values less or equal to one by the utility maximisation process. We restrict our data-set to individuals with rational estimates, which only applies only to about half our participants. See figure 4.7 for an overview over estimated coefficients. The results shown are for an assumed daily base consumption of 12 USD, see the estimation tables in the appendix for robustness over different assumed consumption levels. We find strong support for the effect of financial affectedness and conservatism on efficiency seeking behaviour, and limited evidence towards the effect of being predisposed. In addition, the structural estimation finds that the stringency of government measures has a negative effect on efficiency concerns.

The structural approach fails to find evidence on any effect on altruism. We assume that the differences between the structural and non-structural approach are mainly due to the selective data-set for which the structural model applies. The details of the structural estimation are shown in the methods section, with the corresponding estimation tables in the appendix.

4.3 Discussion

Today's politics is dominated by the fight against COVID-19 and its consequences. Enormous amounts of money are spent by governments on medical equipment and financial support. Some of the spending pay off (e.g., successful vaccine development in the US and Europe), while others turn out to be a failure (e.g., the massive acquisition of malfunctioning face masks in the European Union). The demand for spending may differ substantially with the maturity of the pandemic and the personal threat involved in the pandemic: medically vulnerable people may ask to give more for medical equipment whereas financially affected people may demand for higher subsidies. However, is there evidence under which conditions people turn to activism for the sake of doing something? In other words, when do people support giving despite substantial efficiency losses?

Our study seeks to answer this question by simultaneous estimating individual tastes for time, efficiency and altruism and testing to which degree the tastes – particularly those for efficiency – differ with the severeness of the pandemic. As such, the results of our study will help us to identify conditions that are likely to lead to pure activism and people's demand for actions that imply systematic inefficiencies.

Indeed, we find that both being financially affected and being at high risk of suffering severe symptoms by COVID-19 equally likely reduce individuals' care for efficiency while, at the same time, increases altruism. Yet, it is not a general risk of infection (measured in our study by either state-level case numbers or knowing infected individuals) that have any systematic influence on individual tastes. Thus, it is not the general medical threat of COVID-19 that matters for the systematic shift in preferences. Rather, it is the personal threat situation that coincide with the shift. Importantly, there is substantial heterogeneity: females' preferences react less to the pandemic in general, while there is a trend towards lower impact of being financially affected by COVID-19 for older individuals. Overall, efficiency seems to play a minor role when allocating money between a donation and a private consumption. However, it matters when both payments have the same receiver (i.e., when people allocate their own money between today and later in time, or when people donate today or at a later point). Hence, it is the latter trade-off that is potentially subject to pure activism.

Of course, our study is limited in that we measure preferences at different times, using the maturity of COVID-19 as a natural variation. Therefore, reverse causality may be an issue. That is, our results may suffer from endogeneity if, for example, altruistic individuals have a higher probability of being adversely affected by COVID-19. While we cannot rule those cases out at the individual level, it is worth mentioning that using state-level variables alleviates the problem as it seems unlikely that the entire state of residence reacted to the crisis during our data collection.

Overall, we demonstrate that there are circumstances in which a systematic alternation of the demand for giving coincides with a particular maturity of the crisis. Those circumstances are restricted to personal threats of the pandemic and allocation decisions of money between different points of time, but within the same domain. Yet, they coincide with poor efficiency considerations and intensified altruism. Although those two characteristics need not be problematic per se, it is important for policy makers to acknowledge them. They may shift the public debate towards demanding something for the sake of doing something, and may hinder the efficient management of the current and future crises.

4.4 Methods

Our overall approach can be summarised as follows: We gathered data on incentivised allocation decisions from 886 participants located in the US on an online platform. We also collected measures for affectedness by COVID-19 on both the individual and state level. We then use the allocation decision to estimate individual parameters for efficiency, altruism and time preference. We then estimate the impact of the COVID-19 measures on the previously estimated parameters with a fitting regression model.

4.4.1 Experimental Details

We decided to recruit our participants from the pool of MTurk which is an online labour market for virtually performed tasks. This allows online experiments to be carried out, which is becoming increasingly important for social science and economic research (Kuziemko et al., 2015; Paolacci et al., 2010). In addition, it enabled to circumvent COVID-19 related problems such as presence of participants and to invite participants from different states.

Before execution of the experiment, we decided to cluster our data-set according to geographical orientation of the participants: we divided into south/west with focus on California, Texas, Arizona, New Mexico, Utah, South Carolina and north/east with special interest in Massachusetts, Michigan, New Jersey, New York. At the moment of the data collection of the first cohort, these states were of particular interest because of the particularly low/high number of cases and the population of the state. With the start of the second cohort, we recognised too few participants in Utah and New Mexico. For this reason, we also invited additional participants from Nevada and Colorado.

Due to recruiting limitations, we expanded our selection in cohorts two and three. We chose not to repeatedly measure participants' preferences for two main reasons: Firstly, repeated choices suffer from endogeneity and researcher demand issues. Secondly, the Mturk pool is difficult to maintain in a long-term panel. Thus, participants were allowed to participate in this study only once.

The financial incentives were structured as follows: participants received 1 USD as flat compensation. We randomly selected only one of the 36 questions to be payoff relevant, re-rolling for each cohort. Payments to participants were made through Mturk. The corresponding donation was made to Direct Relief, an international nonprofit, nonpartisan organisation providing essential medical resources to medical staff world wide. A total donation receipt was made available to each participant. Note that this receipt was not tax deductible for our participants. The date of payment and amount of the payout depended on the decision of the respective participant on this allocation decision. Participants had 2.50 USD to allocate for each decision situation. Due to the efficiency rates in favour of the second option, the potential payouts respectively donations varied between 2.00 USD and 4.00 USD. On average participants received 2.56 USD as payout and had 1.20 USD donated.

The online questionnaire was generated using SoSci Survey (Leiner, 2018) and was made available to users via www.soscisurvey.de. The participants' payouts were made via amazon Mturk. The experiment was pre-registered on OSF. There were a total of 2 registrations, one prior to data collection on April 20, 2020, followed up by a registration specifying the analysis made before finishing the data collection, on Mai 15 2020. This registration is currently embargoed until 30th December, 2021.

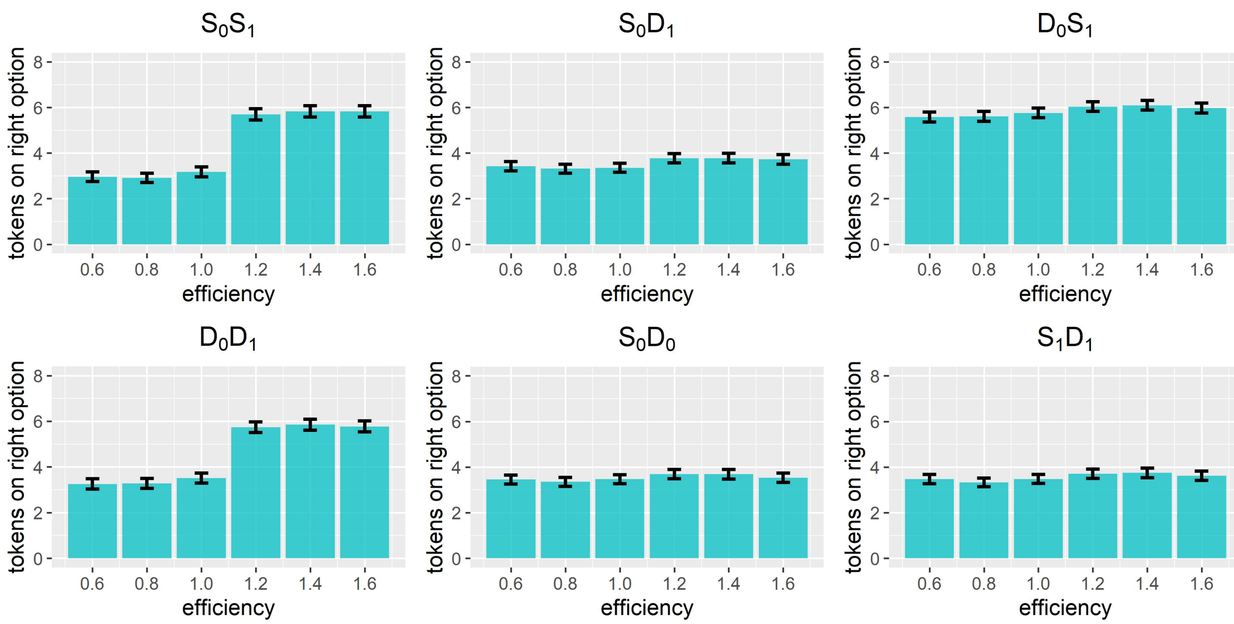

Figure 4.8: Average experimental tokens allocated to the option on the right. Error bars show 95%-confidence intervals. Details on decision block names in table 4.2

Table 4.8 gives an overview over average behaviour within each decision block. It supports the results shown in figure 4.6, displaying and increase in tokens allocated for higher efficiency levels in blocks S_0S_1 and D_0D_1, but no change in the other blocks where individuals distribute between themselves and the charity.

4.4.2 Regressions

Table 4.2 gives more detail about the coding of the dummies for all estimation models. With respect to estimation model 4.1, in order to deal with the inevitable censorship given by the design, we use Tobit maximum-likelihood estimation when possible. We can only estimate via Tobit for 369 participants, as the rest either has too few or too many constrained observations. For the remaining 517 participants, we use simple linear OLS.

Table 4.2: Overview over blocks definitions and dummies

Block	Left Option	Right Option	d_0	t_0	d_1	t_1
S_0S_1	private payout now	private payout in two weeks	0	0	0	1
S_0D_1	private payout now	donation in two weeks	0	0	1	1
D_0S_1	donation now	private payout in two weeks	1	0	0	1
D_0D_1	donation now	donation in two weeks	1	0	1	1
S_0D_0	private payout now	donation now	0	0	1	0
S_1D_1	private payout in two weeks	donation in two weeks	0	1	1	1

Following a common approach from the literature, we also apply a structural model for individual estimates. In order to achieve a high degree of comparability, the structural model is fit to estimate the same parameters as the simple model above (with differences in normalization). We use a fitting CRRA utility function for our structural model:

$$U_{ij}(c_{0ij}, c_{1ij}, w_0, w_1, a_i, \alpha_i, \delta_i) = a_i^{d_{0j}} \delta_i^{kt_{0j}} (c_{0ij} - w_0)^{\alpha_i} + a^{d_{1j}} \delta_i^{kt_{1j}} (c_{1ij} - w_1)^{\alpha_i} \tag{4.2}$$

As before, the subscripts i and j indicate the individual and decision block. Where c_{0ij} and c_{1ij} are the tokens distributed to the left and right option, with the corresponding budget constraint given by the limited amount of tokens available: $(1 + r_j)c_{0ij} + c_{1ij} = m$. δ_i can be interpreted as the daily discount rate . While it is possible to estimate different discount rates for private payments and donations in the strucutral estimation, there is no fitting pendant in the non-strucutural estimation. a_i represents the altruism weight, whereas α_i is the utility function curvature, which can also be interpreted as the individual risk aversion, or in our case as the strength of the reaction to efficiency. A value of 0 implies no reaction to efficiency at all, while positive(negative) values imply higher(lower) consumption with higher efficiency. A value of $\alpha_i = 1$ results in cornersolutions, as utility is maximized by only consuming the more efficient option. The additional parameters w_0 and w_1 can be

interpreted as reference points or base consumption levels. We focus our analysis on the base consumption of $w_0 = w_1 = 10$USD, but also show, that our results are robust over various levels of base consumption.
Maximizing with respect to c_{0ij} and c_{1ij} and using log-linearization on (4.2) leads to the following estimation tangency condition, which can be easily estimated:

$$ln(\frac{c_{0ij} - w_0}{c_{1ij} - w_1}) = \frac{1}{\alpha_i - 1} ln(1 + r_j) + \frac{ln(a_i)}{\alpha_i - 1}(d_{1j} - d_{0j}) + \frac{ln(\delta_i)}{\alpha_i - 1} k[t_{1j} - t_{0j}] \quad (4.3)$$

We estimate the following model:

$$ln(\frac{c_{0ij} - w_0}{c_{1ij} - w_1}) = \beta_{i0} + \beta_{i1} ln(1 + r_j) + \beta_{i2}(d_{1j} - d_{0j}) + \beta_{i3}[t_{1j} - t_{0j}] \quad (4.4)$$

Yielding the following solutions for coefficients:

$$\alpha = \frac{1}{\beta_{i1}} + 1$$

$$a = e^{\frac{\beta_{i2}}{\beta_{i1}}}$$

$$\delta_D = e^{\frac{\beta_{i2}}{k\beta_{i1}}}$$

We estimate equation 4.4 for the complete data-set. Note that the optimisation puts the following restriction on α_i: $\alpha_i \leq 1$. Unfortunately, the estimation produces a relevant amount of α-estimates above this threshold. This is where our study differs most from similar studies. We continue by using only the subset of participants with meaningful estimates. With respect to the estimation method, we again use Tobit where possible and OLS otherwise. However, in the structural approach, the tobit model can generate massive outliers. Thus, we exclude outliers above the 90% quantile, which generates a approximately normal distribution of the individual estimates. Table 4.3 shows the averages over the individual estimates. Note that we cannot estimate a present bias with our experimental design, so the present bias is included in our daily discount factor delta.

The rest of the approach is as before, we obtain individual measures for a_i, α_i and δ_i, to use in the second step estimation. Note that the exact interpretation for these is different from the variables generated by the simple estimation, but the implication of changes is identical.

Table 4.3: Mean over individual estimated preferences

Variable	BC6	BC8	BC10	BC12	BC14
alpha	-3.5730	-4.7657	-4.3706	-5.7796	-6.4325
altruism	0.6463	0.6499	0.6402	0.6498	0.6405
delta	0.9901	0.9903	0.9924	0.9914	0.9923

An increase in a_i still indicates higher altruism, the same holds for α_i and δ_i. The advantage of the structural estimates is a more straightforward interpretation of the coefficients as well as a higher comparability of both the levels of the estimates as well as the coefficients of the second step estimation.

In our second estimation step, which is identical for both the structural and non-structural estimates, we regress different variables connected to the COVID-19 crisis on the three individual characteristics generated from either model in the first step. We separate these variables by the level of the impact, either individual or social. Individual level variables are gathered through our post experiment questionnaire, while social level variables are gathered from government data sources and assigned corresponding to the state of residence. Further separation of the variables can be done via the impact domain, either health-wise or financially, and in the mode of impact, where factors either influence the probability of being affected or the magnitude of damages in case of an infection. Table A.4.1 gives an overview over our choice of explanatory variables. The social-level measures are gathered from openly accessible databases (COVID Tracking Project at The Atlantic, n.d.) and recorded on the state level. While the number of COVID-19 cases and deaths can also be broken down to FIPs-code levels (e.g. from John Hopkins U data), this data is not available for more than half the ZIP codes in our data. We thus only use case and death numbers on the state level. Control variables include: age, gender, number of children, type of household, education and state dummies.

Table B.4.1 shows results of the second step estimation for all three behavioural variables with and without controls. The inclusion of our control variables does not change our results in a relevant way. Tables B.4.2-B.4.4 show results for models including gender interactions, quadratic regressors, and age subgroups. All models include the previous controls. All the coefficients shown in Figure 4.3, 4.4 and 4.5 can be reproduced from these tables. The results of our structural estimation approach can be seen in tables C.4.1-C.4.3. Our data displays some variation with respect to the choice of base consumption levels. Overall, our structural estimation supports our findings with regard to efficiency concerns. However, we can not reproduce the results for altruism. We argue that this is mainly due to the selective sample with reasonable α values in the structural estimation.

4.5 Appendix

4.5.1 Appendix A: Supplementary Tables

Demographics

Table A.4.1: Demographics: Last column shows p-value results from a Kruskal-Wallis test between all cohorts. Note that stateunemployment differs from the values shown in Figure 4.1 as it shows the average over state rates.

Variable	Total	Cohort 1	Cohort 2	Cohort 3	P-val
age	38.51	38.38	39.02	38.13	0.82
gender	0.40	0.40	0.39	0.41	0.92
educ	3.65	3.64	3.65	3.65	0.99
children	1.00	1.03	0.96	1.02	0.29
houeseholdtype	2.42	2.53	2.53	2.21	0.01
incomebefore	3.46	3.47	3.57	3.35	0.20
income	3.35	3.32	3.46	3.27	0.28
financialaffected	6.21	5.99	5.97	6.67	0.00
jobloss	0.16	0.15	0.16	0.18	0.67
stringency	70.33	77.84	70.96	62.26	0.00
savingsaffected	6.29	6.23	6.34	6.32	0.91
insurance	0.82	0.82	0.83	0.81	0.81
predisposed	0.30	0.21	0.32	0.39	0.00
knowinfected	0.08	0.03	0.10	0.12	0.01
infected	0.03	0.01	0.03	0.05	0.01
conservatism	5.20	4.93	5.11	5.57	0.02
casespercap (per 100.000)	732.21	349.93	646.90	1195.52	0.00
7-day incidence	87.23	86.50	52.02	122.46	0.00
stateunemplyoment	0.14	0.15	0.15	0.11	0.00
stateinsurance	91.10	90.97	91.28	91.05	0.67
N	886	296	292	298	

Main Regression Tables

Table B.4.1: OLS, using simple model estimates, models with controls refer to Figure 4.3

	Dependent variable:					
	efficiency	altruism	delta	efficiency	altruism	delta
jobloss	0.202	−0.108	0.123	0.197	−0.080	0.111
	(0.154)	(0.106)	(0.078)	(0.157)	(0.111)	(0.082)
stringency	−0.217	0.015	−0.009	−0.676*	0.426	−0.019
	(0.164)	(0.114)	(0.083)	(0.405)	(0.287)	(0.211)
financiallyaffected	−0.570***	0.389***	−0.033	−0.500***	0.344***	−0.056
	(0.153)	(0.106)	(0.078)	(0.159)	(0.113)	(0.083)
insurance	0.113	0.240	0.072	0.104	0.162	0.164
	(0.377)	(0.261)	(0.191)	(0.391)	(0.277)	(0.204)
predisposed	−0.597***	0.300***	−0.125*	−0.544***	0.242**	−0.089
	(0.149)	(0.103)	(0.076)	(0.160)	(0.114)	(0.083)
knowinfected	−0.121	0.112	−0.020	−0.092	0.094	−0.023
	(0.145)	(0.101)	(0.074)	(0.151)	(0.107)	(0.079)
conservatism	−0.646***	0.266***	−0.050	−0.430***	0.221**	−0.062
	(0.146)	(0.101)	(0.074)	(0.155)	(0.109)	(0.080)
casespercap	−0.021	−0.129	0.051	−0.267	−0.104	0.043
	(0.144)	(0.100)	(0.073)	(0.536)	(0.380)	(0.279)
stateunempl	0.161	−0.030	−0.055	0.612*	−0.193	−0.164
	(0.163)	(0.112)	(0.082)	(0.313)	(0.222)	(0.163)
cohort2				0.349	−0.223	0.124
				(0.553)	(0.392)	(0.288)
cohort3				−0.008	0.384	−0.036
				(1.181)	(0.837)	(0.615)
gender				−0.373	0.099	−0.214
				(0.297)	(0.211)	(0.155)
Controls	No	No	No	Yes	Yes	Yes
Observations	886	886	886	886	886	886
R^2	0.071	0.047	0.008	0.156	0.092	0.050

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors in parentheses, controls include: age, gender, number of children, type of household, education, state-dummies

Table B.4.2: Efficiency Regressions, refers to efficiency coefficients in Figures 4.3,4.4 and 4.5

	Dependent variable:					
	efficiency					
	basic	18-30	31-40	41-50	51-99	gender interaction
jobloss	0.197	0.354	0.259	0.092	−0.096	0.139
	(0.157)	(0.288)	(0.278)	(0.384)	(0.517)	(0.209)
stringency	−0.676*	−0.541	−0.576	−0.621	−1.312	−0.692
	(0.405)	(0.787)	(0.767)	(1.008)	(1.019)	(0.432)
financiallyaffected	−0.500***	−0.907***	−0.562**	−0.101	0.542	−0.931***
	(0.159)	(0.324)	(0.267)	(0.434)	(0.430)	(0.207)
insurance	0.104	0.936	−0.178	0.076	−0.151	0.315
	(0.391)	(0.760)	(0.693)	(0.911)	(1.141)	(0.482)
predisposed	−0.544***	−0.583	−0.129	−0.217	−1.130***	−0.551***
	(0.160)	(0.388)	(0.320)	(0.360)	(0.376)	(0.211)
knowinfected	−0.092	0.152	−0.409	−0.475	0.019	−0.058
	(0.151)	(0.369)	(0.304)	(0.441)	(0.280)	(0.233)
conservatism	−0.430***	−0.571*	−0.209	−0.950**	−0.621	−0.477**
	(0.155)	(0.325)	(0.259)	(0.389)	(0.408)	(0.200)
casespercap	−0.267	0.285	−0.426	−1.765	−1.112	0.140
	(0.536)	(1.050)	(0.925)	(1.253)	(1.469)	(0.560)
stateunempl	0.612*	0.677	0.113	0.852	1.694*	0.790**
	(0.313)	(0.569)	(0.533)	(0.939)	(0.923)	(0.338)
cohort2	0.349	−0.229	0.398	1.322	0.359	0.252
	(0.553)	(1.017)	(1.041)	(1.461)	(1.441)	(0.552)
cohort3	−0.008	−1.140	−0.622	3.590	1.460	−0.351
	(1.181)	(2.144)	(2.207)	(2.987)	(3.137)	(1.179)
gender	−0.373	−0.131	−0.591	−0.124	−0.186	0.037
	(0.297)	(0.606)	(0.527)	(0.806)	(0.779)	(0.715)
gender*jobloss						0.116
						(0.317)
gender*stringency						0.011
						(0.343)
gender*financiallyaffected						1.045***
						(0.318)
gender*insurance						−0.483
						(0.781)
gender*predisposed						0.030
						(0.308)
gender*knowinfected						0.035
						(0.305)
gender*conservatism						0.138
						(0.298)
gender*casespercap						−0.559*
						(0.299)
gender*stateunempl						−0.414
						(0.341)
Observations	886	284	297	144	161	886
R^2	0.156	0.241	0.311	0.285	0.315	0.177

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors in parentheses, controls include: age, gender, number of children, type of household, education, state-dummies

Table B.4.3: Altruism Regressions, refers to altruism coefficients in Figures 4.3,4.4 and 4.5

	Dependent variable:					
	altruism					
	basic	18-30	31-40	41-50	51-99	gender interaction
jobloss	−0.080	0.041	0.070	0.111	−0.340	0.005
	(0.111)	(0.215)	(0.211)	(0.276)	(0.306)	(0.149)
stringency	0.426	−0.073	−0.183	0.288	1.660***	0.436
	(0.287)	(0.588)	(0.582)	(0.724)	(0.603)	(0.309)
financiallyaffected	0.344***	0.208	0.192	0.639**	0.412	0.477***
	(0.113)	(0.242)	(0.202)	(0.312)	(0.254)	(0.148)
insurance	0.162	−0.114	−0.278	−0.021	0.483	−0.065
	(0.277)	(0.568)	(0.526)	(0.654)	(0.675)	(0.345)
predisposed	0.242**	0.094	−0.157	0.278	0.366	0.196
	(0.114)	(0.290)	(0.243)	(0.258)	(0.223)	(0.151)
knowinfected	0.094	−0.077	0.307	0.486	0.072	0.130
	(0.107)	(0.276)	(0.231)	(0.317)	(0.165)	(0.167)
conservatism	0.221**	0.170	0.193	0.494*	0.205	0.163
	(0.109)	(0.243)	(0.197)	(0.280)	(0.241))	(0.143)
casespercap	−0.104	−0.216	−0.428	0.070	0.091	−0.079
	(0.380)	(0.786)	(0.701)	(0.900)	(0.869)	(0.400)
stateunempl	−0.193	−0.392	0.198	0.087	−0.944*	−0.121
	(0.222)	(0.425)	(0.404)	(0.674)	(0.546)	(0.241)
cohort2	−0.223	−0.556	−0.933	0.098	0.062	−0.188
	(0.392)	(0.760)	(0.789)	(1.049)	(0.852)	(0.394)
cohort3	0.384	−0.266	0.612	−0.028	0.987	0.447
	(0.837)	(1.603)	(1.675)	(2.145)	(1.855)	(0.843)
gender	0.099	−0.586	0.243	0.372	0.066	−0.411
	(0.211)	(0.453)	(0.400)	(0.579)	(0.461)	(0.511)
gender*jobloss						−0.171
						(0.227)
gender*stringency						0.026
						(0.245)
gender*financiallyaffected						−0.317
						(0.227)
gender*insurance						0.606
						(0.559)
gender*predisposed						0.061
						(0.221)
gender*knowinfected						−0.076
						(0.218)
gender*conservatism						0.120
						(0.213)
gender*casespercap						−0.050
						(0.214)
gender*stateunempl						−0.202
						(0.244)
Observations	886	284	297	144	161	886
R^2	0.092	0.184	0.186	0.253	0.248	0.099

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors in parentheses, controls include: age, gender, number of children, type of household, education, state-dummies

Table B.4.4: Delta Regressions, refers to time preference coefficients in Figures 4.3,4.4 and 4.5

	Dependent variable:					
	delta					
	basic	18-30	31-40	41-50	51-99	gender interaction
jobloss	0.111	0.046	0.089	0.115	0.121	0.069
	(0.082)	(0.165)	(0.147)	(0.185)	(0.260)	(0.110)
stringency	−0.019	−0.112	0.473	0.682	−0.826	−0.116
	(0.211)	(0.451)	(0.406)	(0.487)	(0.513)	(0.227)
financiallyaffected	−0.056	−0.133	0.094	−0.276	−0.082	−0.089
	(0.083)	(0.185)	(0.141)	(0.210)	(0.216)	(0.109)
insurance	0.164	−0.341	0.782**	0.333	0.190	0.137
	(0.204)	(0.435)	(0.366)	(0.440)	(0.575)	(0.254)
predisposed	−0.089	−0.074	0.099	−0.136	−0.104	−0.170
	(0.083)	(0.222)	(0.169)	(0.174)	(0.190)	(0.111)
knowinfected	−0.023	0.070	−0.139	0.051	−0.104	0.028
	(0.079)	(0.212)	(0.161)	(0.213)	(0.141)	(0.123)
conservatism	−0.062	−0.181	−0.076	−0.095	0.163	−0.062
	(0.080)	(0.186)	(0.137)	(0.188)	(0.205)	(0.105)
casespercap	0.043	0.056	0.018	−0.043	0.186	0.140
	(0.279)	(0.602)	(0.489)	(0.605)	(0.740)	(0.294)
stateunempl	−0.164	−0.293	−0.221	−0.255	0.083	−0.128
	(0.163)	(0.326)	(0.282)	(0.453)	(0.464)	(0.178)
cohort2	0.124	−0.199	0.882	0.537	0.104	0.084
	(0.288)	(0.582)	(0.550)	(0.705)	(0.725)	(0.290)
cohort3	−0.036	−0.485	0.673	1.222	−0.593	−0.122
	(0.615)	(1.228)	(1.167)	(1.442)	(1.579)	(0.620)
gender	−0.214	−0.398	−0.404	−0.297	0.384	−0.297
	(0.155)	(0.347)	(0.279)	(0.389)	(0.392)	(0.376)
gender*jobloss						0.119
						(0.167)
gender*stringency						0.173
						(0.180)
gender*financiallyaffected						0.073
						(0.167)
gender*insurance						0.098
						(0.411)
gender*predisposed						0.185
						(0.162)
gender*knowinfected						−0.067
						(0.160)
gender*conservatism						−0.008
						(0.157)
gender*casespercap						−0.145
						(0.157)
gender*stateunempl						−0.110
						(0.180)
Observations	886	284	297	144	161	886
R^2	0.050	0.101	0.167	0.195	0.191	0.055

Note: *p<0.1; **p<0.05; ***p<0.01

Standard errors in parentheses, controls include: age, gender, number of children, type of household, education, state-dummies

Structural Estimation

Table C.4.1: Structural Estimation Alpha (CRRA, Tobit where possible, OLS otherwise), cleaned data to omit 10 percentiles (double sided for alpha), refers to efficiency coefficients in Table 4.7

	Dependent variable:				
	alpha				
	BC6	BC8	BC10	BC12	BC14
jobloss	0.672	3.439*	0.677	2.128*	1.826
	(1.732)	(1.774)	(0.806)	(1.219)	(1.218)
stringency	−7.565*	−8.770**	−5.898***	−6.646**	−8.070***
	(4.363)	(4.452)	(2.015)	(3.059)	(3.063)
financiallyaffected	−4.091**	−5.815***	−3.113***	−5.080***	−5.238***
	(1.709)	(1.734)	(0.810)	(1.210)	(1.216)
insurance	3.182	1.880	4.492**	5.250*	8.813***
	(4.052)	(4.135)	(1.882)	(2.819)	(2.826)
predisposed	−7.259***	−4.517**	−1.540*	−1.951	−2.938**
	(1.832)	(1.895)	(0.879)	(1.324)	(1.325)
knowinfected	−1.813	−0.555	−1.680*	−1.529	−2.215
	(1.928)	(1.988)	(0.892)	(1.358)	(1.356)
conservatism	−5.988***	−6.305***	−2.185***	−3.730***	−3.735***
	(1.666)	(1.698)	(0.783)	(1.167)	(1.180)
casespercap	6.545	6.779	−1.172	−0.305	−1.015
	(5.839)	(5.954)	(2.780)	(4.206)	(4.215)
stateunempl	−3.316	−2.876	3.919**	3.407	4.403*
	(3.312)	(3.375)	(1.516)	(2.300)	(2.301)
cohort2	−8.741	−6.649	−0.535	−1.359	−2.813
	(5.716)	(5.846)	(2.691)	(4.073)	(4.072)
cohort3	−30.832**	−34.429***	−7.046	−9.405	−10.447
	(12.183)	(12.516)	(5.761)	(8.739)	(8.742)
gender	1.220	−0.072	−0.455	−0.145	−0.175
	(3.258)	(3.309)	(1.508)	(2.259)	(2.278)
Observations	417	413	390	399	394
R^2	0.287	0.249	0.277	0.264	0.267
Note:	*p<0.1; **p<0.05; ***p<0.01				
	Standard errors in parentheses, Controls include: age, gender, number of children, type of household, state-dummies				

Table C.4.2: Structural Estimation Altruism (CRRA, Tobit where possible, OLS otherwise), cleaned data to omit 10 percentiles (double sided for alpha), refers to altruism coefficients in Table 4.7

	Dependent variable:				
	altruism				
	BC6	BC8	BC10	BC12	BC14
jobloss	0.041	0.043	0.033	0.025	0.028
	(0.038)	(0.041)	(0.044)	(0.045)	(0.044)
stringency	0.039	0.008	−0.042	−0.026	−0.037
	(0.096)	(0.102)	(0.110)	(0.114)	(0.110)
financiallyaffected	0.029	0.007	0.012	0.039	0.023
	(0.038)	(0.040)	(0.044)	(0.045)	(0.044)
insurance	0.122	0.139	0.138	0.133	0.118
	(0.090)	(0.095)	(0.103)	(0.105)	(0.102)
predisposed	−0.005	−0.005	0.033	0.024	0.029
	(0.040)	(0.043)	(0.048)	(0.049)	(0.048)
knowinfected	0.047	0.024	0.008	0.006	0.008
	(0.043)	(0.045)	(0.049)	(0.051)	(0.049)
conservatism	0.015	0.001	−0.006	0.009	0.001
	(0.037)	(0.039)	(0.043)	(0.043)	(0.042)
casespercap	−0.054	−0.008	0.034	0.073	0.057
	(0.129)	(0.136)	(0.152)	(0.157)	(0.152)
stateunempl	−0.043	−0.072	−0.052	−0.060	−0.055
	(0.073)	(0.077)	(0.083)	(0.086)	(0.083)
cohort2	0.035	−0.017	−0.083	−0.065	−0.084
	(0.126)	(0.134)	(0.147)	(0.152)	(0.147)
cohort3	0.146	0.033	−0.018	−0.064	−0.054
	(0.269)	(0.286)	(0.314)	(0.325)	(0.315)
gender	−0.054	−0.025	−0.009	−0.059	−0.026
	(0.072)	(0.076)	(0.082)	(0.084)	(0.082)
Observations	417	413	390	399	394
R^2	0.118	0.107	0.100	0.103	0.099

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors in parentheses, Controls include: age, gender, number of children, type of household, state-dummies

Table C.4.3: Structural Estimation Delta (CRRA, Tobit where possible, OLS otherwise), cleaned data to omit 10 percentiles (double sided for alpha), refers to time preference coefficients in Table 4.7

	Dependent variable:				
	delta				
	BC6	BC8	BC10	BC12	BC14
jobloss	0.004	0.003	0.005	0.004	0.005
	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)
stringency	0.003	0.002	−0.008	−0.008	−0.008
	(0.014)	(0.014)	(0.010)	(0.010)	(0.010)
financiallyaffected	−0.010*	−0.009*	0.0001	0.002	0.001
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
insurance	−0.005	−0.005	−0.010	−0.008	−0.009
	(0.013)	(0.013)	(0.009)	(0.009)	(0.009)
predisposed	−0.003	−0.003	0.007*	0.006	0.006
	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)
knowinfected	0.004	0.003	−0.00004	−0.0001	0.0001
	(0.006)	(0.006)	(0.004)	(0.005)	(0.004)
conservatism	−0.009	−0.009*	−0.004	−0.003	−0.004
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
casespercap	−0.009	−0.007	−0.009	−0.006	−0.006
	(0.019)	(0.019)	(0.014)	(0.014)	(0.014)
stateunempl	0.005	0.004	0.013*	0.013*	0.013*
	(0.011)	(0.011)	(0.008)	(0.008)	(0.008)
cohort2	0.006	0.003	−0.009	−0.011	−0.012
	(0.018)	(0.019)	(0.013)	(0.014)	(0.014)
cohort3	0.016	0.010	0.015	0.010	0.010
	(0.039)	(0.040)	(0.029)	(0.029)	(0.029)
gender	−0.013	−0.013	−0.013*	−0.016**	−0.015**
	(0.010)	(0.010)	(0.008)	(0.008)	(0.008)
Observations	417	413	390	399	394
R^2	0.121	0.118	0.094	0.096	0.094
Note:	*p<0.1; **p<0.05; ***p<0.01 Standard errors in parentheses, Controls include: age, gender, number of children, type of household, state-dummies				

Cohort subsets and Interactions

Table D.4.1: OLS, using simple model estimates, Cohort subsets

	Dependent variable:								
	efficiency			altruism			delta		
	Cohort 1	Cohort 2	Cohort 3	Cohort 1	Cohort 2	Cohort 3	Cohort 1	Cohort 2	Cohort 3
jobloss	0.191	0.422	−0.019	−0.117	−0.021	−0.044	0.328*	0.108	−0.109
stringency	−4.106	−0.605	1.158	1.680	0.119	2.335	0.166	−0.073	−3.658
financiallyaffected	−0.394	−0.180	−0.596**	0.366	0.169	0.569***	−0.195	0.045	−0.021
insurance	0.381	−0.137	0.334	−0.287	0.158	0.479	0.298	0.429	−0.178
infectionriskdummy	−0.171	−0.723**	−0.757***	0.366	0.194	0.361*	−0.259	−0.015	−0.070
knowinfected	−0.328	−0.145	0.066	0.308	0.183	−0.071	0.019	−0.060	−0.011
conservatism	−0.438*	−0.627*	−0.195	0.273	0.086	0.169	−0.122	0.048	−0.062
casespercap	−14.311	−21.755	−4.255	9.535	8.134	−5.913	1.886	5.385	9.698***
stateunempl	−2.290	0.347	11.086	1.456	0.745	−28.342	−0.289	−0.832	27.807**
Observations	296	292	298	296	292	298	296	292	298
R^2	0.222	0.157	0.312	0.141	0.112	0.157	0.098	0.102	0.172

Note: *p<0.1; **p<0.05; ***p<0.01

Standard errors in parentheses, controls include: age, gender, number of children, type of household, education, state-dummies

Table D.4.2: OLS, using simple model estimates, Cohort interactions

	Dependent variable:		
	efficiency	altruism	delta
jobloss	0.197	−0.112	0.342**
stringency	0.200	0.116	−0.398
financiallyaffected	−0.516*	0.312	−0.174
insurance	0.280	−0.197	0.269
infectionriskdummy	−0.127	0.355*	−0.304*
knowinfected	−0.304	0.318	−0.047
conservatism	−0.476*	0.307	−0.132
casespercap	−1.022	0.177	0.277
stateunempl	0.651	−0.484	−0.372
cohort2:jobloss	0.156	0.098	−0.243
cohort3:jobloss	−0.132	−0.012	−0.416**
cohort2:stringency	−0.665	0.189	−0.033
cohort3:stringency	−1.449	0.335	0.788
cohort2:financiallyaffected	0.226	−0.123	0.190
cohort3:financiallyaffected	−0.212	0.221	0.186
cohort2:insurance	−0.648	0.334	0.242
cohort3:insurance	0.059	0.621	−0.453
cohort2:infectionriskdummy	−0.678*	−0.233	0.355*
cohort3:infectionriskdummy	−0.477	−0.075	0.275
cohort2:knowinfected	0.157	−0.140	0.017
cohort3:knowinfected	0.362	−0.382	0.039
cohort2:conservatism	−0.108	−0.184	0.183
cohort3:conservatism	0.237	−0.106	0.041
cohort2:casespercap	0.302	−0.135	0.170
cohort3:casespercap	−0.185	−0.094	0.270
cohort2:stateunempl	0.485	0.269	−0.032
cohort3:stateunempl	0.859	0.362	−0.349
Observations	886	886	886
R^2	0.167	0.101	0.069

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors in parentheses, controls include: age, gender, number of children, type of household, education, state-dummies

4.5.2 Appendix B: Experimental Instructions

These are the instructions per screen of the experiment. Please note, screen 2 to 7 are in random order for each participant. After these seven screens, the participants are asked to answer socio-demographic questions as well as questions about risk attitudes.

Screen 1: Welcome and main information about procedure

Welcome to the study and thank you for your participation!

General

You are participating in a scientific experiment by the University of Hamburg, Germany. You will be asked to make a number of decisions. It is important that you read the following instructions carefully. We analyze your anonymized answers only for scientific purposes.

In this study, you will decide 36 times on how to divide money. In some decisions

- you divide an amount of money for yourself over two points in time, or
- you divide an amount of money for yourself or a charity over two points in time, or
- you divide an amount of money for a charity over two points in time.

After your decisions, one of these decisions is randomly selected and implemented with real money. The date and conditions of this decision determine when you and the charity receive the payoffs. The charity is the Direct Relief foundation (further details on the charity will be provided below). During the experiment, money is distributed in points. This means that during the experiment we are talking about points rather than Dollars. At the end of the experiment, the points are converted into Dollars. We explain below, how you can influence the money transfers.

Your job

In each of the 36 decisions we will endow you with 10 points. Sometimes you will split points you receive either tomorrow or in two weeks. Sometimes you will split points a charity receives tomorrow or in two weeks. Sometimes you will split points for a charity tomorrow or for you in two weeks or reverse. The value of the points will differ between the two points in time. An example is best suited to illustrate this:

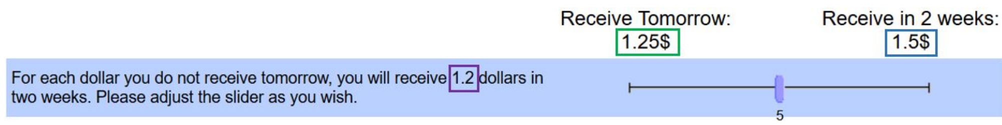

Figure 1

In this example, you split points for you. You always see the earlier point in time to which you can assign points on the left side (marked in green) and the later point in time to which you can assign points on the right side (marked in blue). In this example, you can split the points using the upper slider. You can assign between 0 and 10 points to the later time and the remaining points to the earlier point in time by moving the bar.

The value of the points on the earlier date is always the same (0.25 Dollar per point). The value of the points you split up later may vary. That is, in some decisions, points that are assigned to the later point in time have a higher value than the points that are assigned to the earlier point in time. In other decisions, the points allocated at the later point in time have a lower value than the points allocated at the earlier point in time. In other situations, points allocated earlier or later have the same value.

The value of points are outlined for each decision (e.g., in purple in Figure 1). In the example, each point is worth 0.25$ at the earlier time and 20% more at the later time, i.e. 0.3$. Therefore, the exchange ratio is 1:1.2. If you distribute your 10 points to the earlier time, 10 x 0.25$ = 2.5$ will be paid to you tomorrow. If you distribute your 10 points to the later time, 10 x 0.3$ = 3$ will be paid to you in 2 weeks. If you distribute 5 points to the earlier time and 5 points to the later time, 5 x 0.25$ = 1.25$ are paid to you tomorrow and 5 x 0.3 = 1.5$ are paid to you in two weeks.

Here is another example (Figure 2): you split points for Direct Relief tomorrow and you in two weeks. The points on the left side (marked in green) are donated to Direct Relief tomorrow, the points on the right side (marked in blue) are paid to you in two weeks. The value of the points tomorrow is always 0.25 Dollar per point. In this example, the value of the later points is 40% higher: each point is worth 0.35$ at the later time. The exchange ratio is 1:1.4. If you distribute your 10 points to the earlier time, 10 x 0.25$ = 2.5$ will be paid to Direct Relief tomorrow. If you distribute your 10 points to the later time, 10 x 0.35$ = 3.5$ will be paid to you in 2 weeks. If you distribute 5 points to the earlier time and 5 points to the later time, 5 x 0.25$ = 1.25$ are paid to Direct Relief tomorrow and 5 x 0.35 = 1.75$ are paid to you in two weeks.

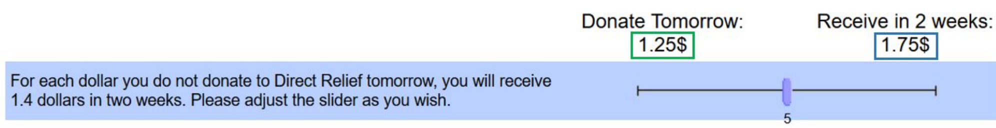

Figure 2

Note: The slider does not appear until you click in the horizontal bar. Please find below an illustration of the initial situation before you clicked in the horizontal bar.

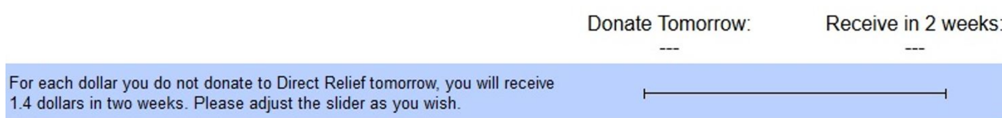

Figure 3

On each screen, you will make six of the decisions described above, one after the other, with varying exchange ratios and varying receivers of the payment: either you or Direct Relief. When you have made your decisions and you are sure, press the “next page” button and your decisions will be saved. Once your decisions are saved, you cannot change them.

Once you have finished all your 36 decisions, we will ask you some general questions. As mentioned above, at the end of the experiment, we randomly select and implement one of the 36 decisions so that each of your decisions can be implemented with the same probability.

The payoffs

You will receive your payment depending on your decision at the designated date via a MTurk bonus payment. You will also receive the information which decision was implemented. In addition, we will display a receipt of the total amount donated here: https://www.wiso.uni-hamburg.de/fachbereich-vwl/professuren/lange/forschung/studie.html

The selected donation will be made by us at the designated date to Direct Relief. This organization provides personal protective equipment and essential medical items to as many health workers as possible, as quickly as possible, for medical facilities across the U.S. responding to coronavirus.

Screen 2: Decision Slider S_0S_1

With the following 6 sliders you can allocate money between receiving an amount **tomorrow**, and a different amount **in 2 weeks**. You can receive a maximum of 2.5$ tomorrow. How much you receive in 2 weeks varies. Each slider has 10 steps.

You will see the exact amounts of money allocated above each slider.

Screen 3: Decision Slider S_0D_1

With the following 6 sliders you can allocate money between receiving an amount **tomorrow**, and donating an amount to Direct Relief **in 2 weeks**. You can receive a maximum of 2.5$ tomorrow. How much is donated in 2 weeks varies. Each slider has 10 steps.

You will see the exact amounts of money allocated above each slider.

Screen 4: Decision Slider D_0S_1

With the following 6 sliders you can allocate money between donating an amount to Direct Relief **tomorrow**, and receiving a different amount yourself **in 2 weeks**. You can donate a maximum of 2.5$ tomorrow. How much you receive in 2 weeks varies. Each slider has 10 steps.

You will see the exact amounts of money allocated above each slider.

Screen 5: Decision Slider D_0D_1

With the following 6 sliders you can allocate money between donating an amount to Direct Relief **tomorrow**, and donating an amount to Direct Relief **in 2 weeks**. You can donate a maximum of 2.5$ tomorrow. How much is donated in 2 weeks varies. Each slider has 10 steps.

You will see the exact amounts of money allocated above each slider.

Screen 6: Decision Slider S_0D_0

With the following 6 sliders you can allocate money between receiving an amount **tomorrow**, and donating an amount to Direct Relief **tomorrow**. You can receive a maximum of 2.5$ tomorrow. How much is donated varies. Each slider has 10 steps.

You will see the exact amounts of money allocated above each slider.

Screen 7: Decision Slider S_1D_1

With the following 6 sliders you can allocate money between you receiving an amount **in two weeks**, and donating an amount to Direct Relief **in two weeks**. You can receive a maximum of 2.5$ tomorrow. How much is donated varies. Each slider has 10 steps.

You will see the exact amounts of money allocated above each slider.

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