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Abstract

The first chapter introduces a theoretical model of inequality aversion which can also be used in an environment with information asymmetries. The model is based on the non-paternalistic approach where, the own utility function incorporates the utility of other people as perceived by a decision maker. Moreover it allows extensions for other motives which may result in pro-social behavior. I extend the model by adding shame aversion as an additional driver for apparently altruistic behavior. Threat of shame is induced by different levels of exposure of either own actions or identity to the third party observers. I also experimentally test predictions of the model using a very simple environment of a dictator's game. The experimental design aims to remove additional confounding behavioral effects present in the previous literature. The results suggest that even a very small exposure results in significantly higher amounts sent to recipients. The analysis also shows that the agents, who believe that they can conceal their own actions in front of the less informed counterpart, exploit this information asymmetry for their monetary benefit.

The second chapter examines endogenous decisions to acquire useful information. My experimental design tries to test predictions of ego-utility theories and other relevant theories about the decision-making process of agents in the environment with costless signals. Only slightly more than half of the subjects acquired an optimal number of the signals for payoff maximization. The results suggest that for the subjects making sub-optimal decisions, aversion to cognitive dissonance is the prevalent channel. Contrary to this, I find much less support for the ego-utility theory and theory of information ignorance in my setting. The availability of information alone does not automatically lead to an improvement in decisions.

The third chapter (co-authored by Peter Katuščák) examines interaction of financial and pro-social motives in public good provision. One prominent mechanism suggested to alleviate problem of free-riding is a fixed-prize lottery with winning probabilities proportional to individual contributions (Morgan, 2000; Morgan and Sefton, 2000). Yet, as extensively documented by economic experiments, subjects often contribute even in the absence of incentives of this kind, suggesting that their contributions are driven social preferences. This raises a question of how the lottery incentive interacts with social preferences. We present an experiment in which we de-couple the contribution effect of own prize seeking from the potential crowding out effect due to the perception that the others contribute because of their prize seeking, rather than to benefit the group. Even though the lottery increases contributions relative to the voluntary contribution case, we find that it also crowds out voluntary contributions that are likely driven by social preferences.

Abstrakt

První kapitola představuje teoretický model averze k nerovnosti, který má využití také v situacích informační asymetrie. Model je založen na nepaternalistickém přístupu, kde funkce vlastního užitku zahrnuje užitek ostatních tak, jak je vnímán tím, kdo rozhoduje. Model navíc umožňuje rozšíření motivů, které můžou vést k pro-sociálnímu chování. Dále je rozšířen přidáním averze ke studu jako další motivace pro navenek altruistické chování. Hrozba studu je vyvolaná různými úrovněmi odhalení buď vlastních rozhodnutí, nebo vlastní identity před nezúčastněnými pozorovateli. Experimentálně také testuji předpovědi modelu, přičemž využívám jednoduchého prostředí hry Diktátor. Cílem tohoto experimentálního designu je odstranění možných zkreslujících behaviorálních efektů, přítomných v jiných hrách. Výsledky ukazují, že i nepatrné vystavení se studu má za následek signifikantně vyšší částky pro příjímatele. Analýza rovněž ukazuje, že lidé, kteří si myslí, že můžou zatajit své rozhodování před míň informovanými protějšky, využívají tuto informační asymetrii pro dosažení vyššího finančního zisku.

Druhá kapitola zkoumá endogenní rozhodnutí k získání užitečné informace. Můj experimentální design testuje predikce "ego-utility" teorií a dalších relevantních teorií o rozhodovacím procesu agentů v prostředí s bezplatnými signály. Jenom mírná většina subjektů získala optimální počet signálů pro maximalizaci výplaty. Výsledky naznačují, že pro subjekty, kteří dělají neoptimální rozhodnutí, je averze ke kognitivní disonanci převládající motivací. Naopak, ve svém výzkumu nacházím mnohem menší podporu teorie "ego-utility"a teorie informační ignorace. Samotná možnost informace automaticky nevede k lepšímu rozhodování.

Třetí kapitola (spoluautor Peter Katuščák) zkoumá interakci mezi finančími a pro-sociálními motivy u poskytování veřejných statků. Jeden z předních mechanismů, navržených pro zmenšení problému free-ridingu, je loterie s fixní cenou, kdy je pravděpodobnost výhry úměrná jednotlivým příspěvkům. Jak bylo značně zdokumentováno ekonomickými experimenty, subjekty často přispívají i bez pobídek tohoto druhu, jelikož jejich příspěvky jsou motivovány sociálními preferencemi. To nám klade otázku, jak finanční motivace loterií interaguje se sociálními preferencemi. Představujeme experiment, kde oddělujeme efekt přispívaní motivován snažením se o vlastní výhru od efektu potenciálního vytěsňování kvůli přesvědčení, že ostatní přispívají, kvůli výhře a ne proto, aby z toho měla užitek celá skupina. I když loterie relativně zvyšuje příspěvky vzhledem k dobrovolnému přispívání, zjistili jsme, že taky potlačuje dobrovolné příspěvky, motivované sociálními preferenciemi.

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Thank you, Lenka.

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Preface

This dissertation examines behavioral aspects of our decision making in a few specific settings using tools of experimental economics. There has been an effort to increase an explanatory power of economics by incorporating psychological insights and taking into account empirical evidence about behavior of people. Now, behavioral economics can successfully model the most distinctive patterns of decision making which was not possible using classical approach.

Newly acquired knowledge helps us to understand motivations and outcomes of people in many situations. Ideally, it should help us to predict the effects or improve design of different policies. However, as we move away from stylized environment some observed behavior is more difficult to explain. Vast amount of accumulated observational and experimental evidence not only answers many questions but creates new questions and puzzles as well.

These difficulties to explain certain behavior may arise because of deviations from basic assumptions about an environment (addressed in the first chapter), more theoretical approaches to a same topic with different predictions (addressed in the second chapter), or presence of two or more determinants of behavior with opposite direction of effects (addressed in the third chapter). I contribute to the existing literature by shedding light on the decision making of agents in environment with information asymmetries, in environment with endogenous selection of information, and in environment when monetary and non-monetary incentives may interact.

1 The Effect of Shame in Dictator Games with Information Asymmetry

1.1 Introduction

Selfishness and altruism in human behavior have been examined by experimental economists using dictator games. In these games, one player (a dictator) decides how to split a certain amount of money between himself/herself and the other player (a recipient). If dictators were maximizing only their monetary payoffs, they would keep everything for themselves and leave recipients empty handed. However, dictators do not usually make such decisions. Holt (2007) provides evidence of experiments in which the average share for the recipient is 31 percent; only fewer than 10% of dictators keep everything for themselves. Similarly, Andreoni and Miller (2003) also found in their experimental study that only around 23 percent of their subjects behave "perfectly selfishly". The described behavior might be sensitive to the design of the experiment and socio-demographic characteristics, but in general the subjects mostly transfer non-zero amounts to recipients. See Camerer (2011) for a detailed overview.

This contradiction to the theory of purely selfish people has been explained by fairness concerns and inequality aversion. Fehr and Schmidt (2006) review many theories of other-regarding preferences based on different assumptions and models. These theories work well in an environment where both parties are fully informed about all aspects of the game. There are, however, many situations when both parties are not symmetrically informed about everything and one party has some information advantage over the other (e.g. principal-agent situations). The behavior in such situations differs from full and can not be explained by current inequality aversion models (Andreoni and Bernheim, 2009, Charness and Dufwenberg, 2006;Mitzkewitz and Nagel, 1993, Rapoport and Sundali, 1996, Straub and Murnighan, 1995).

The first new contribution of this paper to the current state of knowledge is an introduction of the theoretical model which extends inequality aversion principles into the information asymmetry environment. Even though the model imposes only minimal assumptions about the functional forms, it gives important predictions for behavior of people in different information environments. It is based on an (apparently) non-paternalistic[1], pro-social approach of the fully informed party. The crucial feature of the model is the belief of a fully informed party about the beliefs of the less informed party.

[1]Non-paternalistic altruism describes the situation when a decision-maker values the utility of an affected individual as opposed to a paternalistic approach when this decision-maker values consumption or distribution of some goods irrespective of the preferences of the affected individual. See Flores (2002) for more information.

The second contribution is that the model does not restrict the set of motives for the observed pro-social behavior only to the innate altruistic motives. It can easily be extended by other different motives. In this paper I add shame aversion as one of the drivers for "fair" behavior. The model is extended by the shame features based on the psychological literature. Following Tangney (1995) the shame is induced by exposure of the actions and/or identity to other people . If an unfair (from the decision maker's point of view) decision leads to negative emotions (Reuben and Van Winden, 2010), some agents may prefer to avoid such behavior by choosing the action which would not lead to such emotions. A potential change in behavior depends on the strength of the agent's exposure. In the second half of the paper I experimentally test the predictions of the model along two dimensions using a one-shot dictator game.

The first dimension is investigation of the information environment predictions. Here, the decision of an agent is fully or partially disclosed only to his or her counterpart who is financially affected by this decision. In my study, the opportunity to conceal own action for a dictator will come from the random pie size which is always known only for dictators. The second dimension is investigation of the potential shame effect in line with the situation captured by the model. Usually, the exposure is done publicly in front of all subjects in the experimental works when studying "audience effects" (e.g. Andreoni and Bernheim, 2009). So, I experimentally test the effect of shame on decisions of dictators in a one-shot dictator game.

The third contribution of this paper is in the exclusion of many confounding effects which are present in the previous experimental literature (more details in Section 3). Regarding the exposure, I plan to introduce a more realistic, and for the application less costly, environment where each decision making agent is observed by only a limited number of observers (in this case only one observer per agent).

The results observed are mostly in line with the predictions of the model. Even a small level of exposure leads to an up to 8 percentage point increase in monetary transfers. Interestingly, almost all of the change caused by the exposure is driven by a higher share of non-zero contributors rather than by an increase in the average contributions (conditional on contributing). Regarding the information asymmetry predictions, the results show that the mean share sent to recipients depends on the perceived beliefs induced in the less informed party. If agents believe that they can induce beliefs in the less informed party that their behavior is closer to fair behavior than it actually is, then the difference in amounts sent could be up to 18 percentage points lower (depending on the treatment). Given the parametrization and design of the experiment, the results are supposed to be more likely lower bounds of the shame and information asymmetry effects.

1.2 The basic model

The model, introduced in this section, formalizes principles of inequality aversion with non-paternalistic pro-social preferences. Moreover, it analyzes behavior in an environment with different levels of information completeness and decision maker's anonymity. The model preserves the Fehr and Schmidt (1999a) spirit in a way that agents derive utility from monetary earnings and dislike inequality. However, it can be used for explaining exploitation of information asymmetry and different motivations for the observed, apparently pro-social, behavior. I start from the simplest setting of complete information and then I will add more complicated (and usually more realistic) features of the environment.

This model could be used to describe the situations when one side of the contract has a power to conceal, at least partially, information about the surplus to be divided. Wage offers from employers to employees depending on the observability of the firm profits could serve as an example. The extension of the model into the shame dimension could approximate situations like publishing a list of tax debtors or possible changes in decisions between secret vs. non-anonymous voting procedures.

1.2.1 Complete information

Let's start, for simplicity, with the setting of a standard dictator game of dividing an amount of π. Let's suppose that the size of the pie, π, is general knowledge. Agent i makes a decision about how much to transfer to agent j, the transferred amount is denoted by x_j. The rest of the pie, the amount x_i is kept by agent i. The agents in the model dislike disadvantageous inequality only and care about the (perceived) utility of the other player. Disutility from inequality depends on the difference between the monetary outcomes, denoted by the function $h(x_j - x_i)$. I do not assume any specific functional form of the inequality aversion function, but I assume, for simplicity, that it is continuous and twice differentiable for all possible values of the argument and $h(x_j - x_i) = 0$ if $x_i \geq x_j$. I also impose the reasonable assumptions on the shape of this function. The first derivative of this function $h'(x_j - x_i)$ is positive, so the higher the inequality, the higher the disutility. The second assumption is that the h function is convex ($h''(x_j - x_i) > 0$). Both assumptions are also justified by empirical evidence (Loewenstein et al., 1989). So the utility of agent i is the following:

$$U_i = x_i - h(x_j - x_i) + \gamma U_j$$

where parameter γ expresses individual sensitivity to the perceived utility of the other player[2], I assume this parameter is weakly greater than 0 and strictly lower than 1.

[2]The model allows for heterogeneity in the parameter γ but I will omit the individual subscripts for

As I will later extend the model into the situations when the amount kept by the decision maker, x_i, is not known to everybody I express the argument of the h function in the terms of π and x_j ($x_i = \pi - x_j$). It is difficult to tell what is the utility of agent j from the point of view of agent i. If agent i does not have any additional information about agent j I use the straightforward approach by using the own inequality aversion function of agent i even for agent j. So the utility function can be rewritten to:[3]

$$U_i = \pi - (1-\gamma)x_j - h(2x_j - \pi) - \gamma h(\pi - 2x_j)$$

Given the restriction on the γ parameter, it is obvious that agent i never chooses x_j greater than $\pi/2$[4]. Then the utility function shrinks to $U_i = \pi - (1-\gamma)x_j - \gamma h(\pi - 2x_j)$.[5] The solution to the utility maximization problem is trivial in this case and gives the optimal transfer under complete information,x_j^c, being either zero for a corner solution or some positive transfer up to one half of the pie which is given by condition $1 - \gamma = 2\gamma h'(\pi - 2x_j^c)$ (see Appendix for more details on derivation of results). Similarly to the Fehr and Schmidt (1999a) model the utility function also contains aversion to the advantageous inequality. In my model such aversion arises because of non-paternalistic altruism in this model as opposed to their model where it is directly innate in an agent's distributional preferences. It might seem of minor importance how both types of the models come to the same predictions, which are observationally impossible to distinguish. However, the structural analysis of underlying preferences plays an important role when the information asymmetry is introduced.

1.2.2 Information asymmetry

Now suppose that agent j observes only the amount x_j transferred for her and has no exact information about the pie size π. Even if π is not known to agent j, she is not prevented from having either information about the objective distribution of π and from creating her own inferences about the pie size after observing transfer x_j. There could be many ways the agents could create beliefs about the pie size if they can observe only transfer x_j. Therefore,

convenience at this point.

[3]An alternative approach is to include the utility of agent i into the utility of agent j. This recursive process could continue infinitely. Given the range for the γ values, the final result for the utility of agent i is multiplied by $1/(1-\gamma)^2$ and the qualitative predictions of the model would be preserved. Therefore, for the rest of the paper I disregard the possibility of second and higher -order beliefs about the utility function. For the discussion about the depth of reasoning on theoretical and empirical grounds see Binmore (1987), Bacharach (1992), Nagel (1995).

[4]Also supported by the empirical evidence (e.g. Camerer, 2011)

[5]Alternatively, it is possible to keep direct expression for advantageous inequality aversion in the utility function and proceed in that way. However, it would drop when solving for optimal transfer. So, for simplicity I decided to omit it at this step.

I use only a very general function, $m(x_j)$, which describes agent i's beliefs about how recipient maps the observed transfer x_j into the expectation of the pie size (dictator believes that $E_{\text{recipient}}(\pi|x_j) = m(x_j)$). I assume this function to be differentiable and increasing in $x_j (m'(x_{j0}) > 0)$. Now the utility of the agent i for a given transfer x_j is:

$$U_i = \pi - (1-\gamma)x_j - \gamma h(m(x_j) - 2x_j)$$

Again, depending on the values of parameters and functional forms, the solution to the utility maximization could be a corner solution with zero transfer. More interesting is the interior solution satisfying the condition: $1-\gamma = 2\gamma h'(m(x_j^a)-2x_j^a)+m'(x_j^a)(1-\gamma h'(m(x_j^a)-2x_j^a))$, where x_j^a denotes the optimal transfer under information asymmetry.

Comparing the optimality conditions for interior solutions in both information availability environments, there is no clear prediction for the comparison of the transferred amount x_j in both environments. The amount sent depends on the exact form m and h functions and on the parameter γ.

Claim 1. Let x_j^a denote the optimal transfer under information asymmetry and x_j^c denote the optimal transfer in a complete information setting. If there is an optimal transfer x_j^a such that for a given pie size π it satisfies the condition $m(x_j^a) = \pi$, then it holds that:
i) $x_j^a > x_j^c$ if $1 > \gamma h'(m(x_j^a) - 2x_j^a)$
ii) $x_j^a < x_j^c$ if $1 < \gamma h'(m(x_j^a) - 2x_j^a)$
iii) $x_j^a = x_j^c$ if $1 = \gamma h'(m(x_j^a) - 2x_j^a)$

If the dictator assumes that he induces correct beliefs with the optimal transfer x_j^a, then a comparison of outcomes in different information environments depends on the marginal disutility from the induced inequality and on sensitivity to the other player's utility (see appendix for derivation of all claims). Intuitively, each additional unit of the transfer decreases inequality faster in the environment with full information. This is because the additional unit of transfer under information asymmetry also increases the recipient's expectation about the pie size. So in the domain of the inequality aversion function where marginal disutility is really high ($1 < \gamma h'(.)$ and due to the convexity of the h function) it is relatively more utility-harming to increase the transfers in order to decrease inequality in an asymmetry of information setting.

So far the reasoning about the difference between the transfers in those two environments has been based on the assumption that the agent induces correct beliefs about the pie size with each transfer under information asymmetry. This assumption might not necessarily be correct. As the agent does not know the exact mapping function m of her counterpart, she can either use her own mapping function or have some beliefs about the counterpart's function. Both options may lead to inaccuracy in the induced pie size ($m(x_j^a) \neq \pi$). This leads to different predictions about the optimal transfer which depend on the exact shape of the h and m functions, and on theγ parameter.

Claim 2. Let x_j^a denote the optimal transfer under information asymmetry and x_j^cdenote the optimal transfer in a complete information setting. If $m(x_j^a) \neq \pi$ then the comparison between x_j^a and x_j^cdepends on the combination of h and m functions shape, and on the γ parameter as is stated in the following table:

Table 1: Predictions under information asymmetry

	$1 > \gamma h'(m(x_j^a) - 2x_j^a)$	$1 < \gamma h'(m(x_j^a) - 2x_j^a)$	$1 = \gamma h'(m(x_j^a) - 2x_j^a)$
$m(x_j^a) > \pi$	$\hat{x}_j > x_j^c$	ambiguous	$x_j^a > x_j^c$
$m(x_j^a) < \pi$	ambiguous	$x_j^a < x_j^c$	$x_j^a < x_j^c$

Combining *Claims 1* and *2*, there are nine possible resulting predictions for the comparison of different information environments. This ambiguity in the predictions is caused by the fact that I do not impose any specific functional forms of the functions. Another important implication of this model is the fact that an increase in an induced pie size expectation leads to an increase in the optimal transfer (for a given actual pie size). In other words, if the agent believes that she can induce higher beliefs about the pie size and thus induce a higher inequality, a higher transfer is needed to achieve optimal inequality.

If advantageous inequality aversion is acquired into the own utility function through the utility function of the other agent (as opposed to innate advantageous inequality aversion) then the model introduced could explain differences in pro-social behavior under information asymmetry. The crucial feature of this model is that people care about the utility of other people. So far, I have not discussed the motives behind such behavior.

1.3 Beyond non-paternalistic altruism

It would be a hasty conclusion to say that people care about the utility of other people only because of pro-social motives. There have been other explanations of giving positive amounts in dictator games such as guilt, shame, the effort to be considered as a "fair" person, reputation building and other. Some of these motives can be present only under certain conditions. If those motives are not stable, but depend on the environment for the model, it would mean that the parameter γ is a function of the environment. This is the parameter reflecting how people care about the utility of others. The experiments of Reuben and Van Winden (2010) illustrate that unfair actions of the players are correlated with a higher intensity of emotions like shame and guilt. Without ruling out other motives I am going to focus on the shame effects. Following similar reasoning and implications as for shame, the model could also be easily extended using other motives[6].

1.3.1 Shame in literature

In order to examine the possible effects of shame, it is necessary to have definitions and understanding of what it is. Tangney (1995) provides an overview of shame-related studies in the psychological literature. At first, shame was studied together with guilt without a clear distinction. Then a distinction was made in a way that describes guilt as an inner feeling, which we do not need other people to know about our action in order to feel it, while for shame, we need other people to be aware of our actions in order to feel it (Tangney and Dearing, 2003). Later defining the difference between the two included the criteria of the role of the "self" (Lewis, 1971). For the feeling of shame, the evaluation of some action needs to be focused on self, while for feelings of guilt, the evaluation needs to be focused on the action done. So for shame, it is not necessary to be directly observed, it is enough to have a feeling of being observed or evaluated. However, exposure to other people still plays an important role (Tangney, 1995):

> "...shame experiences were more likely to involve a concern with others' evaluations of self, whereas guilt experiences were more likely to involve a concern with one's effect on others" (p. 1136).

In the experimental economics literature, there are studies focusing on behavior which may be attributed to shame effects. Such effects are in general examined by providing an opportunity to conceal own behavior from other participants under experimental conditions. Studies have been performed on different types of games. Fehr and Gächter (1999) and Rege and Telle (2004) study shame in public good games. Both studies vary the level of ex-post anonymity after all decisions are made. Rege and Telle (2004) find a positive effect of the higher exposure on public good contributions, while Fehr and Gächter (1999) find an effect only when anonymity has been removed before the game and combined with the meeting of group members, after the game.

Tadelis (2007) uses a trust game design with varying disclosures of the subject's anonymity, and information about random intervention. Tadelis (2007) also introduces a model with shame aversion in this paper. His results confirm the effect of shame on the behavior of agents. However, here the decision of the possibly shame-affected decision maker comes into effect only after the other player trusts him/her. So, the shame effects are confounded with

[6]For example, it can capture the properties of situations with moral costs in the spirit of List (2007).

reciprocity effects in this paper (unless we impose a restriction of additivity and no interaction of these effects).[7]

There are also studies of ultimatum games which may have a connection to shame effects (Mitzkewitz and Nagel, 1993, Rapoport and Sundali, 1996). Here, the pie size is unknown to the recipients and level of exposure is varied by changing the variance of the pie size (note that according to the psychological literature, only a feeling of being observed or evaluated is enough for shame) which is fully known only to proposers. A higher variance provides more opportunities to "hide behind a small pie" as it makes a proper evaluation from the side of the recipients more difficult. The evidence suggests that for higher variances of the pie size, proposers keep larger shares of it. The question here is which part of the observed behavior is caused by shame and which by the strategic behavior present in ultimatum games and its possible interaction with shame effects.

There has been an experimental study using the dictator game with asymmetric information. Andreoni and Bernheim (2009) developed a theoretical model which is based on the utility coming from the dictator's social image. They completely remove anonymity among the participants. Subjects in their experiment are undergraduate economics students from the same university, so the removal of anonymity may lead also to concerns for future interaction. They argue that this is not a problem for the purpose of their work. However, I will try to filter out this concern or minimize its impact in this study. Exogenous change in exposure levels is governed by different probabilities of nature intervening and deciding about the split at certain default values (what is general knowledge for everybody). They find a significant effect of this exposure on proportions of people sending either half of the pie or nothing (in this case, nature's intervention led to 0 or 1 for the recipient, depending on the treatment). However, different outside options and different natural intervention probabilities may draw the attention of subjects from a pure distributional problem to thinking about different entitlements to the pie and different beliefs about expectations (also the experimenter's expectations). Although the aim of their study is not directed at shame effects it provides some patterns of how shame may affect the behavior of agents.

It is possible to find a possible flavor of shame effects also in studies which are focused on an other possible motivator of the observed pro-social behavior. Their authors call it guilt aversion and it is defined as failing somebody's expectations in these experiments. If people are guilt averse (in the way, how they define it[8]), they have negative utility from not fulfilling

[7]Ong and Lin (2011) show that "kind" behavior evokes reciprocation even in cases when first movers do not know about any possibility of reciprocation by other subjects, and second movers could keep everything without first movers knowing this.

[8]I will stick to the definition of guilt from the psychological literature in this study. Then guilt effects should be the same regardless of the exposure level.

these expectations. In their actions, this would look like non-selfish behavior, if they believe that their counterparts have "non-selfish" expectations. Charness and Dufwenberg (2006) vary expectations in trust games by allowing communication which anchors the expectations of the subjects. Dana, Cain, and Dawes (2006) exclude the expectations in dictator games completely by announcing to recipients that some game has been played only in cases where the dictator decides to send a positive amount. Both of these studies find an effect of not fulfilling somebody's expectations on the decision making of more informed players. Although these effects are strong, they may be confounded with the effects of shame or shame aversion. The decision to send zero to a recipient in the mentioned dictator game does not only exclude any expectations of the recipient but also prevents any feeling of exposure to others and therefore any evaluation of the dictator by the recipient.

Given the psychological literature, the emotion of shame is induced by exposure of own actions to other people. Even if the above mentioned experimental evidence may be confounded by other motives, it creates a strong suspicion about the ability shame to change behavior of the agents. If a selfish (from the decision maker's point of view) decision would lead to negative emotions, some agents may prefer to prevent such emotions by choosing more pro-social action. If the exposure of own decisions to the other people leads to the threat of shame the agent could put more weight on the utility of other agents in order to prevent negative emotions.

1.3.2 Shame in the model

Suppose that the strength of exposure could be expressed by one variable denoted by e. Alternatively, I can break down the exposure level into more variables, each capturing different channels (e.g. shame, loss of anonymity, reputation building, probability of future interaction). However, this is beyond the scope of this paper and for this moment I stick to one variable capturing the strength of exposure which is the main driver of shame intensity. Then the parameter γ can be expressed as a function of e, $\gamma(e)$. To be consistent with literature about the shame I assume $\gamma(e)$ to be increasing in the level of observability. The utility functions then change to:

$$U_i = \pi - (1-\gamma(e))x_j - \gamma(e)h(\pi - 2x_j) \quad \text{or} \quad U_i = \pi - (1-\gamma(e))x_j - \gamma(e)h(m(x_j) - 2x_j)$$

depending on information availability. The value of $\gamma(0)$, i.e. the value with complete anonymity, expresses a true altruistic behavior or true altruistic behavior with motives which do not depend on observability. Given the assumption of increasing $\gamma(e)$, the predictions of the model are very trivial for varying observability in both information settings. For information asymmetry I need to impose two realistic assumptions: for no value of x_j an additional amount of transfer would increase the expectation about the pie size by more than double of this amount, $m'(x_j) < 2$, for all values of x_j which could be rationally expected for a given distribution of π; [9] and there are no strong convexities or concavities in m function (value of $m''(x_j)$ is "very small"). More on the derivation of the following claim can be found in the Appendix.

Claim 3. For any optimal transfer greater than zero (interior solution) in both information situations, an increase in observability leads to a decrease in inequality.

Intuitively, the increased observability of an agent's decisions is connected with a judgment of these decisions. Therefore the feeling of shame should be greater for any "unfair" decision. In order to prevent such negative emotions the agent should shift her decision more towards to an equal split. [10] In this section of the paper I incorporate the influence of shame in the model of inequality aversion with non-paternalistic utility features.

The model described extends the inequality aversion models to an environment with varying information asymmetry or observability of decisions. It analyzes giving behavior with only a few assumptions which are consistent with empirical evidence or psychological theories. Despite its parsimony, the model predicts changes in behavior of the agents within the inequality aversion framework. The agents who are averse to inequality and also care about such inequality aversion for their counterparts may transfer a different amount of money in the situation when this counterpart is fully informed about the pie size or the amount they keep for themselves. Increased observability should lead to higher transfers in order to prevent a possible threat of shame. I test some predictions of the model with an economic experiment which is described in the following section.

1.4 Experimental design

It is not feasible to test all predictions of the model with many different parametrizations. I focus on testing hypotheses about the predictions for the small exogenous variations in the environment. If there is significant change in outcome for small variations in environment, it is very likely that this effect could be amplified using greater variations. For testing the predictions about the information environment I use a randomly drawn pie size while varying the information availability about the pie size for a recipient of the transfer. I use distribution of the pie size, which allows for exploitation of information asymmetry, but the range of the values is still relatively narrow. For exposure predictions I cannot test all the possible channels of its effect (mentioned above) so I concentrate on the effects of shame

[9] So the value of $m(0)$ is at least minimum possible value of π and condition $m'(x_j) < 2$ holds only if x_j is lower than half of the maximum value of the pie size.

[10] Which is in most of the situations assumed to be fair (Andreoni and Bernheim, 2009).

and try to minimize other effects which can in general be called audience effects. Here I use exposure of the dictator's picture and decisions to only one anonymous observer, without any power to interfere. So the aim of my experimental design is in the lower bounds of effects connected with varying information completeness and exposure.

If the game is more complex, the cognitive process of the subject may focus on the very structure of the game, and perceived exposure in the game is considered only to a certain extent. Because the crucial aim of my study is to trigger thoughts about exposure and the consequent possible shame, I use a dictator game which has a very simple setting and does not include strategic concerns or reciprocity concerns.

The novelty of my approach lies in disentangling the information completeness from exposure effects. In dictator game studies done so far, the change in exposure was automatically connected with the exposure to the subjects directly financially influenced by the dictators. There could be a potential interaction between the exposure effects and the fact that a decision maker is exposed to the agent she/he can directly affect in monetary terms. Therefore, I employ third party observers who are not affected by the decisions of the dictators.

1.4.1 The dictator game with a randomly drawn pie size

I use the dictator game with a randomly drawn pie size with asymmetric information about the realized value. In particular, the information advantage is on the side of the dictator (male pronouns further on) who knows the exact realized value of pie size before the splitting decision, while the recipient (female pronouns further on) observes only the amount transferred to her. This allows the dictator to partially conceal full information about his decision and prevents any proper evaluation from the side of recipient (reducing the feeling of exposure). Varying the ex-post disclosure of the actual pie size and the presence of the third party observer allows me to test the predictions of the model.[11]

Subjects are randomly assigned to one of two (or three, depending on the treatment, see the next section) types, labeled Type A or Type B (or Type C). They remain the same type for the rest of the experiment. The pie size is drawn from U[50, 150], where dictators know the exact realization, and recipients know only the ex-ante distribution. This information is explained in the instructions to all subjects. Then, the dictators are instructed to split the pie according to how much they have decided to transfer to a randomly chosen recipient,

[11] Also varying exposure to the experimenter (single-blinded vs double-blinded designs) may be considered as shame effects sticking to the above mentioned definitions. As this is not the main goal of this study and exploring this area is beyond the possibilities of this project, I will not vary the level of exposure to the experimenter. There is also evidence when exposure to the experimenter is not very strong, that the observed behavior does not differ significantly between single- and double-blind settings in the most common games (Barmettler et al., 2012). Moreover, in my experiment, an experimenter can immediately observe only earnings of the subjects, not their decisions, and it is difficult to infer decisions from earnings (details later).

keeping the rest for themselves. Depending on the treatment, the subjects are informed about ex-post disclosure of the pie size to the receivers. The subjects are also informed about a demographic questionnaire at the end of the experiment. All earnings during the experiment are stated in experimental units (EU). Conversion rate, 1 EU= 2 Czech crowns (CZK),[12] is announced to subjects in written instructions at the beginning of the experiment. The instructions are available upon request from the author.

For the dictators' decisions, I use a strategy method (Selten, 1967) with five different pie sizes. The pie sizes are drawn from U[50,70], U[70,90]...U[130,150], respectively. They are displayed sequentially in random order. After all five decisions are made, one of the presented pie sizes and corresponding decision is chosen as payoff-relevant[13]. This way I have five decisions spread across the whole support of the distribution. Also this design allows me to test whether there are some effects of the absolute size of the pie on the share given to receivers. After the dictators make their decisions in this stage of the experiment, I elicit estimates from the recipients about the pie size based on the amounts observed that they receive and also the estimates of dictators about the recipients' estimates (second-order beliefs).[14] If their estimate is correct within range $\pm$ 7 from the true value, they earn an additional bonus. These data allow me to control for an effect of fulfilling somebody's expectations when making decisions (trying to avoid guilt).

In the next stage, I ask the subjects to rate the intensity of ten emotions [15] on a scale from 1 (very low) to 7 (very high). They are chosen in a way that includes a spectrum of positive/negative emotions towards either self or others. Another reason to include more emotions was to dilute the salience of the emotion of core interest (shame). Consequently, they are asked to estimate the intensity of these emotions by their counterpart. If this estimate is at most 1 point from the true value, they earn an additional bonus (40 CZK). The purpose of this elicitation is to control for another channel as to how shame may step into the decision making process. Some subjects may not realize the utility consequences of pie revelation ex ante in a one shot game without previous experience. Therefore the threat of negative emotion may not change their behavior. However it may lead to an increased

[12]The exchange rate at the time of the experiment was approximately 1 USD = 19.2 CZK or 1 EUR=25 CZK.

[13]The evidence on the effects of the strategy method is inconclusive. However, there is no evidence of treatment effects present using the strategy method and not being present using the direct-response method (Brandts and Charness, 2011).

[14]Because this takes some time I need to keep recipients busy with a different task to prevent revealing the type of each player. Recipients are asked to complete a general knowledge test with multiple choices. They are motivated by some small reward for each correct answer. The presence of this test has not been mentioned in the paper instructions and dictators are not informed about this for the duration of the whole experiment in order to prevent possible distributional effects.

[15]Happiness, Disappointment, Envy, Shame, Regret, Guilt, Contempt, Anger, Sympathy, and Gratitude

intensity of some emotions ex post.

Finally, subjects are asked to fill out a questionnaire asking for their basic socio-demographic characteristics, what they consider to be a "fair" split, and the number of people in the lab they knew before the experiment (to control for a potentially different initial level of anonymity they perceived). Female subjects are also asked questions about their menstrual cycle, as different levels of estrogen in different phases of the cycle could significantly influence their behavior; for more details see Chen et al. (2013a). Then subjects are presented with a screen which informs them about their earnings from the experiment with an added show-up fee. In order to prevent inference about the pie size from possibly earned bonuses in some treatments, the feedback consists only of the sum of all earnings.

1.4.2 Treatments

One dimension of this experiment is based on a varying ex-post disclosure of the pie size. In the first alternative, the pie size is not revealed to recipients. In the second alternative, both player types are ex-ante informed that the pie size will be revealed ex-post, after the decision about splitting it is made. In this case the level of the dictator's anonymity in front of the recipient is held constant. This variation is aimed at testing the predictions of the model about the information asymmetry.

The second dimension of the experiment is aimed at the effect of exposure and more specifically at the threat of shame (which is not connected to the financial consequences of someone's decision). Therefore, a third party observer is added (Type C). This observer has no power to influence the outcome of splitting. On the other hand, an observer can always see the camera shot of the dictator's face, together with the pie size and his decisions (no connection to the variation of the pie-size disclosure to the recipients). Each dictator is observed by one observer. Earnings of observers are determined by a random draw from four possible values at the end of the experiment[16]. So, a combination of two possible ex-post pie size disclosure options (pie size not known to the recipient at the end of the experiment - NK, pie size known - K) and two options for the presence of observers (observer present - O, no observer - NO) gives a 2x2 factorial design.

[16] I also needed to keep observers busy at the time dictators are splitting the pie in order not to reveal the role assignment. They are asked to estimate the decisions of the observed dictators and are motivated by a small bonus (40 CZK or 2 USD) or if they are close to the actual decision. This is not announced to the players in the written instructions and only observers learn this from additional on-screen instructions.

1.4.3 Hypotheses to be tested

The described design allows me to test the following hypotheses connected with the effect of exposure on dictators' decisions. All of them are in the form of null hypotheses with alternative hypotheses of dictators sending different amounts in different treatments.

- *Hypothesis 1: Dictators do not send a different share of the pie when their decision is fully revealed to the recipients.*
- *Hypothesis 2: Dictators do not send a different share of the pie when their decision is fully revealed to the financially unaffected observers when their anonymity is partially broken.*

I will test both hypotheses at the level of overall means but also at the extensive or intensive margins, in order to have more information about the source of the possible variation in the overall outcome values.

1.4.4 Procedure

The experiment took place in the Laboratory of Experimental Economics at the University of Economics in Prague at the end of October 2012 and in the first half on November 2012. The experiment was computerized using zTree software (Fischbacher, 2007). The experiment was conducted in English and subjects knew this when they registered for the sessions[17]. There was no written comprehension test because the subjects considered the game simple in the pilot sessions. However, the subjects were encouraged to ask for clarification and help if needed. For O sessions, there were also 6 observers in each session and each observed two dictators. However, subjects were only told that each dictator is observed by one observer in the written instructions.

As the assignment of the roles is random and subjects need to understand this, a photo of each subject was required in the O sessions. This was done when they were entering the lab [18].

[17]There may be slight differences in understanding the meaning of various emotions across languages. So, in order to unify it, there was a brief English explanation of the emotions on the screen and also a Czech translation of these emotions (for the vast majority of the subjects, the Czech language is either their mother tongue or they have perfect command of it).

[18]Before this, the subjects needed to sign a consent form which stated they were informed about the photography issues together with the notice that the photo will be used for research purposes only and will not be shown in any output from the project. No one refused to participate in the experiment after finding out about this procedure.

1.4.5 Subjects

Together, 430 subjects participated in 16 sessions of this experiment (4 for each treatment)[19]. There were 12 dictator - recipient pairs (11 for one NO-K session due to an unexpectedly low turnout of participants) for all treatments in each session. The whole session lasted around 40 minutes for NO sessions and 45 minutes for O sessions. The average experimental payoff was 305 CZK including a show-up fee of 150 CZK. Subjects received their payoff privately in cash at the end of the experiment.

Subjects were mostly students studying at various universities in Prague[20]. The gender ratio was almost balanced (females 47.4%, males 52.6%)[21]. Regarding the country of origin, 69.3% of subjects were from the Czech Republic , 20.2% from Slovakia , 3.5% from Russia or Ukraine , 7% from other countries. For their majors, 76.3% of subjects have economics or business as their major, 8.4% science, engineering or medicine , 5.1% mathematics or statistics , 4.9% other social sciences , 5.3% humanities and other . Subjects also differ in the academic degree they hold: 57.2% of subject do not hold any degree, 35.6% hold a bachelor's degree, 7% a master's degree. The average age of the subjects was 22.3 years ranging from 18 to 38.

1.5 Results

1.5.1 Basic results

As there is absolutely no effect of the pie size on the share given to recipients, I normalize and report the decisions of the dictators in shares given to recipients for most of the analysis. Overall, in 13.2% of decisions, dictators kept the whole pie. The mean value of amount for the recipient across the treatments was 0.263. Offers higher than 0.5 could be observed in 6.4% of the cases with about half of such decisions are only slightly above half of the pie.[22]

Table 2 presents the mean share of the pie transferred to recipients together with the

[19] The ORSEE recruitment software (Greiner 2015) and the LEE database have been used.

[20] This minimizes concerns for future interaction and perception of the game continuation after the experiment. The subjects were asked to report on the number of people in the lab they knew before the experiment. 56% of dictators did not know anyone, 82.2% of dictators knew at most one person, 92.1% of dictators knew at most two people out of 23 (or 29, depending on treatment) subjects in the lab.

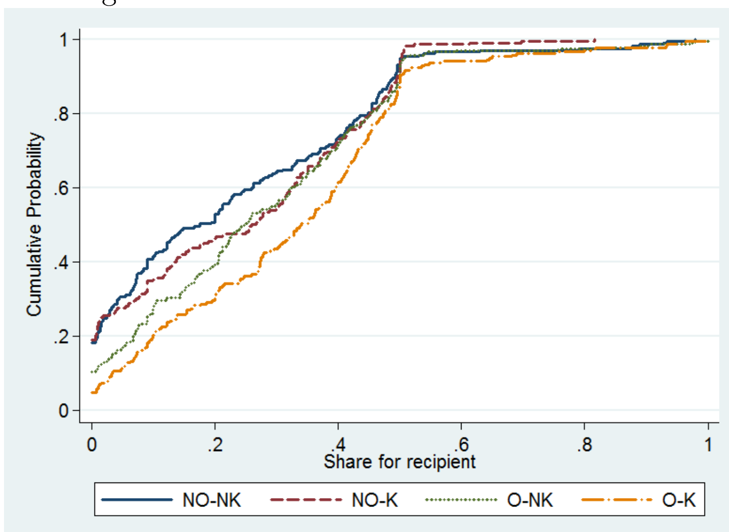
[21] In order to prevent big gender imbalances through the sessions, male and female subjects were recruited separately with the equal proportion of free places for each gender. This specific recruitment procedure was not known to the subjects. However, when the subjects came to the lab I did not insist on exactly balanced gender ratio of participants in order not to trigger thinking about possible experimenter's expectations.

[22] Most unusually high offers are caused by few subjects. This behavior may be caused by a misunderstanding of the instructions as these subjects reported very high levels of regret, envy and disappointment compared to other dictators and their reported fairness perceptions do not differ from other dictators. Omitting these subjects from data analysis does not qualitatively change the main results so I decided to keep them in the data set for further analysis. If their presence will changes the results, I will comment on it.

Table 2: Mean share of the pie transferred to recipients with standard errors

		Disclosure	
		NK	K
Observer	NO	0.225 (0.030)	0.240 (0.027)
	O	0.269 (0.027)	0.319 (0.024)

Figure 1: Cumulative distribution functions

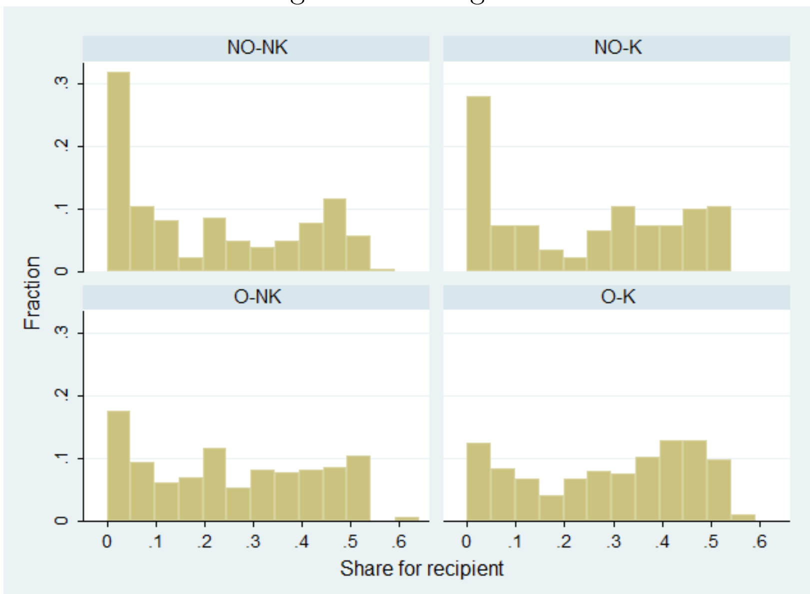


standard errors in parentheses (clustered at the subject level) for each treatment. Means are slightly higher for K and O treatments, what is in line with the predictions of the model. The subjects send the highest share of the pie when an observer is present and a recipient knows the exact size of the pie. A more detailed overview of decisions are in Figures 1 and 2 which present the cumulative distribution functions and histograms of the dictators' decisions in each treatment. From Figure 1 it is obvious that the distribution of the O-K treatment first-order stochastically dominates the distributions of all other treatments. Histograms show a higher share of the lowest offers in the treatments without observer and somewhat lower proportion of decisions sending around half of the pie.

As the decisions of one dictator cannot be considered to be independent, I use two approaches for the statistical analysis. In the first approach, I average the decisions within the subjects and then use the Mann-Whitney ranksum test (AV)[23]. In the second approach, I

[23] I included post hoc power calculations for Mann- Whitney ranksum tests. The power calculation with the GPower program (Faul et al., 2009) shows that the power to identify effects of size 0.5 times the population standard deviation is 76.8% between the two groups at 0.05 significance level, the power to identify a larger, 0.8 population standard deviation; the effect is 98.4%. The power is lower by a few decimal percentage points for identifying treatment effects by comparing the NO-K group due to one missing dictator in this group. The power to identify the effects of lower size is not sufficient (e.g. 24.6% for identifying 0.2 population standard

Figure 2: Histograms



use the Mann-Whitney ranksum test with clustering at the subject level (CL) [24]. In both cases the null-hypothesis is that decisions in two compared treatments are from the same distribution.

Table 3 presents p-values of all relevant comparisons. The effect of disclosure is not significant given that the observer is not present, with the presence of an observer the subjects send higher shares when the pie size is disclosed after the decision, and this effect is marginally significant. A more detailed analysis of disclosure is provided in the separate subsection. Comparing treatments with and without an observer, dictators send higher shares when somebody observes them, but this difference is statistically significant only when a recipient can ex-post observe the pie size. Testing for the joint effect of observer and disclosure (O-K treatment compared to NO-NK treatment), dictators send significantly higher shares when both players, recipient and observer, are fully informed about their decisions.

Imposing distributional restrictions and performing robustness checks with OLS or tobit specifications and share on the pie given to recipients as a dependent variable, the qualitative results are stable across different regression specifications. Changing the set of exogenous variables and clustering on the subject level, the dummy variable for observer presence has p-value at most 0.031 with a positive coefficient; the dummy variable for the presence of disclosure is insignificant at conventional levels regardless of the regression specification. There is only one other variable which is significant across all specifications and it is the gender dummy with a higher given share when a dictator is female. Other variables such as

deviation effect).

[24] For the details of this method see Newson (2002) or Datta and Satten (2005)

Table 3: Testing for the equality of distributions, p-values of the ranksum tests for different comparisons

Compared treatments	Mann- Whitney	M-W with clustering at the subject level
NO-NK vs NO-K	0.599	0.618
O-NK vs O-K	0.098	0.176
NO-NK vs O-NK	0.202	0.194
NO-K vs O-K	0.060	0.056
NO-NK vs O-K	0.009	0.011

age, income, the number of people the subject knows present in the lab, degree held, major or reported fair split are not significant in any used specification[25].

Naturally, one could possibly argue that a change in behavior may be caused by the change in second order beliefs. In that case, dictators just adjust their behavior in order to fulfill different beliefs they have about the recipients' expectations. That would support the guilt aversion approach in the previous literature (Dana et al., 2006, Charness and Dufwenberg, 2006). Comparing beliefs about a recipient's expectations, there is no statistically significant difference between beliefs in all treatment comparisons.[26] This result does not contradict the conclusions of the literature dealing with behavior motivated by fulfilling somebody's expectation but suggests the existence of some other channel causing the observed behavior. The predictions of the model about the exposure to the observer are confirmed by the data even for such small levels of exposure. The results suggest that even a much smaller intensity of exposure can change the behavior of people compared to the previous studies (e.g. Andreoni and Bernheim, 2009).

1.5.2 Information asymmetry

Comparing only the overall results for the disclosure dimension may be misleading, as the model allows different kinds of results depending on the heterogeneity in individual parameters, namely γ, and the functional form of the m function, in the total utility function. It may also be the reason for not obtaining significant differences in the disclosure dimension. Average data do not reflect heterogeneity in the utility functions. Even when there could be a significant difference at the individual level, it can be averaged close to zero in the aggregate data. As the purpose of this project was not calibration of these functions or parameters I can not test all the predictions of this model for the introduction of information asymmetry. However, I can examine the relation between m function value and the transfer in the infor-

[25]The results are available from the author upon request

[26]Either when using t-test (p-values are in the range from 0.282 to 0.943) or when relaxing the distributional assumptions and using the Wilcoxon rank-sum test (p-values from 0.424 to 0.834).

mation asymmetry environment. This examination tests Claim 2, that a higher induced pie size (belief of dictator about induced pie size) leads to a higher transfer, discussed in Section 2.2

The recipients were asked to provide their estimate of the pie size when they were able to observe only the transfer to them. Dictators were asked to provide their estimate of the recipient's estimate. Both estimates were incentivized. I can use the estimate of dictators about the induced pie size as a proxy for m function value in the information asymmetry treatments. Then I can compare this belief about the induced pie size with the actual pie size. Transfers by dictators, according to their beliefs about the induced pie size are presented in Table 4.[27]

Table 4: Transfers by the beliefs about the induced pie size

Mean share transferred (Number of subjects in the group)		
	NO-NK	O-NK
$m(x_j) > \pi$	0.311 (16)	0.384 (17)
$m(x_j) < \pi$	0.185 (30)	0.201 (28)

It is apparent that people with beliefs that they induce higher than actual pie size transfer higher amounts. I test for the statistical difference between the transfers using two approaches; comparing means of two different groups or using difference $m(x_j) - \pi$ as a predictive variable and share sent as a predicted variable.

I compare mean transfer by different groups ($m(x_j) > \pi$ vs. $m(x_j) < \pi$) using either t-test or ranksum test. There is statistically significant difference in transfer for O-NK treatment between two groups. For NO-NK the difference is marginally significant and depends on the test used.[28] I can also explore the relationship between the transfer size and deviation of $m(x_j)$ from the actual pie size, π. Results are qualitatively similar using either OLS or tobit specification. The relationship is statistically significant for O-NK treatments (p-value 0.002 for both specifications) and statistically significant for NO-NK treatment only at a 10% significance level (p-value 0.085 for OLS and 0.098 for tobit). Difference $m(x_j) - \pi$ is uncorrelated with any demographic measure (correlation coefficient at most 0.1)

Even though the data for testing information environment predictions is limited (caused by the focus of the experimental design), the results are in line with the predicted outcome. Higher induced pie size is connected with higher transfers to recipients.

[27] I exclude 2 resp. 3 subjects from NO-NK resp. O-NK treatments with $m(x_j) = \pi$ as there are only few of them for a proper analysis.

[28] p-value for O-NK (NO-NK) treatments is 0.005 (0.059) for two tailed t-test, and p-value for Wilcoxon rank-sum test is 0.003 (0.124)

Table 5: Share of the non-zero amounts given (with the clustered standard errors)

		Disclosure	
		NK	K
Observer	NO	0.817 (0.052)	0.809 (0.054)
	O	0.896 (0.043)	0.95 (0.029)

1.5.3 Intensive margin vs. extensive margin

A detailed inspection of both intensive and extensive margins is needed for a better understanding of the treatment effects. If the mean of the shares for a recipient is higher in one treatment compared to another there are two possible reasons behind it (or a combination of them): first, the increase in mean contribution for dictators giving a positive amount (intensive margin) and second, an increase in the number of dictators giving a positive amount (extensive margin).[29] Also if there is no treatment effect in the overall means, we cannot make conclusions about the effects at the margins.

For the extensive margin, I was comparing the share of decisions giving positive amounts between the treatments (summarized in Table 5). Comparisons between O and NO treatments show a significant difference between the proportions of subjects giving something positive(p-values at most 0.013)[30]. Comparisons between K and NK differ in their results. While in the O dimension there is also a significant difference between the K and NK treatments (p-values from 0.026 to 0.05), there is no such result in the NO dimension (p-values from 0.577 to 0.942). For the intensive margin, comparing the means of the subject who gave something positive, dictators send slightly higher amounts in K treatments compared to NK treatments and also in O treatments compared to NO treatments. However, this difference is not statistically significant.[31]

So, the differences between the amounts sent in different treatments are caused mainly by the different share of decisions keeping the whole pie. However, the sent positive amounts (conditional on sending a positive amount) do not differ statistically between the treatments. The change in proportion of people keeping the whole pie is in line with the results of Andreoni and Bernheim (2009). However, there is no significant increase in the decisions around the

[29] It is straightforward to make a division between no giving at all and giving something positive at the zero contributions. But some subjects may perceive also giving 1 to the recipient as giving "nothing" for some reasons. For some of them, the lowest possible amount in their mental domain of splitting the pie may be 1 or they might have understood the instructions in a way that they need to transfer at least something. In order to see whether the results are sensitive to this division, I performed all the following tests considering either 0 or 1 or 2 as giving nothing.

[30] Using a proportion test

[31] the Wilcoxon rank-sum tests

50-50 division of the pie. This may be due to already mentioned differences between the experimental designs.

1.5.4 Reported emotions

The subjects were also asked to report the intensity of their emotions after a random choice of payoff relevant split and a possible disclosure in this experiment. Also they were asked to guess at the intensity of the emotions of their counterpart. Regarding the emotions of dictators, I have two relevant sets of emotions in the data. The first set comes directly from dictators and the second set comes from recipients when they were asked to estimate the intensity of emotions for dictators (incentivized).

For the first set, reported intensities of emotions are largely concentrated around the lowest values. The modal value is 1 for 9 out 10 emotions (except for happiness) and the median value is at most 2 for 7 out of 10 emotions. For some emotions it could obviously be expected given their essence and purpose of their presence (see Section 3.1.). However, lower intensities are frequently also reported for the emotions of interest (shame, guilt). There is obviously some weak linear relationship for shame depending on the shares, which is stronger when I exclude the six clearly outstanding observations for the subjects giving unusually high shares to recipients (correlation coefficient changes from -0.21 to -0.27).[32] For the second set of reported emotions coming from the estimates of recipients, the same concentration of data around the lowest values and weak correlation with dictators' decisions can be observed.

This may be caused by a few reasons which may be crucial for different subjects reporting their emotions: subjects make decisions in order to avoid a higher intensity of negative emotions, they lack the incentives to report their true emotions, or the experimental setting, in general, does not induce these kinds of emotions for them. Although I cannot rule out the last two reasons completely, there is evidence in previous works that subjects do not report their emotions only at the lowest intensities (e.g. Reuben and Van Winden, 2010). The results of emotion elicitation are in line with the argument that subjects try to prevent negative emotions by changing their actions (and this is expected also by the recipients).

1.6 Discussion

Information asymmetry is very likely to occur in social interactions. It is not possible to explain observed behavior within the framework of current inequality aversion models. The

[32] These subjects report a relatively high intensity of shame together with anger, disappointment or envy. As there is strong suspicion that these subjects did not understand the instructions correctly, it is very likely that their reported emotions are confounded also with emotions coming from this fact (besides the decision itself). This is the only time outliers have an impact on the results, and it is only minor.

first contribution of this paper is in creating a unified theoretical framework for studying wide a range of behavior in an environment with various information asymmetries and exposure levels. I introduce a model which studies inequality aversion also in the environment of information asymmetry. The model can be extended by different motives as to why people care about others. In this paper it incorporates the level of observability from the other people as well. So, in this way it can be used to analyze different forms of shame effects. I test predictions of the model in the economic experiment.

The second contribution of this paper is in the experimental design, which studies the shame effects without the confounds present in the previous studies. This experimental design was aimed at testing the effects of shame coming from exposure in an environment where subjects have the possibility to partially hide their true actions in front of their counterparts. The purpose was to focus the attention of subjects to the exposure and trigger their thinking about it before making their distributional decisions.

The only difference between treatments was variation in the level of exposure which is a sufficient condition to induce shame in psychological literature (Tangney and Dearing, 2003). Another requirement for identifying shame effects was to filter out or at least control for additional confounds. Compared to the previous literature, the experiment was designed in a way which removes any strategic or efficiency concerns (discussed previously). Change in the social distance happens only from the point of view of observers, not dictators; as they cannot identify who observed them. Additionally, in my analysis, I control for beliefs about fair splits, second- order beliefs about the expected transfer, the number of people each subject knows in the lab, and for their reported emotions.

The aggregate results show that exposure, even to a third party observer, has a significant effect on dictators' decisions in dictator games. The result is in line with the results of audience-effects literature (Andreoni and Bernheim, 2009), but the effect is present in an environment with much lower level of observability. Another important theoretical and experimental result is that behavior in the information asymmetry environment is sensitive to the beliefs of the decision maker about the beliefs which his actions would induce in the less informed agent. A more detailed inspection of the data showed that decreased anonymity leads to a lower fraction of dictators keeping the whole pie but does not lead to an increase in average positive transfers. This suggests that policies aimed at the public disclosure of actions or identity are more likely to cause an increase in the share of giving people rather than an increase in given amounts.

The results suggest that the ex-post removal of anonymity or information asymmetry has the power to trigger thinking about consequent exposure and the possible threat of shame. More importantly, this cognitive process is transferred to different actions more likely than to different intensities of emotions. Given the experimental design (only one anonymous observer, anonymity in front of the recipient, no punishment etc.) the observed results are very likely to describe a lower bound of the possible effects. Although there is need for further research regarding the various forms or intensities of exposure, the relatively cheap ex-post disclosure of either actions or of the identity of the agents is able to change their decisions ex-ante.

Acknowledgments

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Appendix 1

Derivation of model predictions

Complete information

For the basic setting with full information each agent faces the following maximization problem:

$$\arg\max_{x_j} \pi - (1-\gamma)x_j - \gamma h(\pi - 2x_j) \qquad \text{s.t. } x_j \geq 0$$

A solution of the problem gives the first order condition of $1-\gamma = 2\gamma h'(\pi - 2x_j)$, equating the marginal loss of utility from own material payoff with marginal decrease in disutility from inequality. This leads to an optimal transfer of x_j^cgiven by:

$$x_j^c = \begin{cases} \frac{1}{2}[\pi - h'^{-1}(\frac{1-\gamma}{2\gamma})] & \text{if } 1-\gamma < 2\gamma h'(\pi) \\ 0 & \text{if } 1-\gamma \geq 2\gamma h'(\pi) \end{cases}$$

Checking for the second order condition and using the convexity of h function, we get $-4\gamma h''(\pi - 2x_j) < 0$. So indeed, the value of the interior optimal transfer maximizes the utility of the agent.

Information asymmetry

In the information asymmetry setting the agent faces the following maximization problem:

$$\arg\max_{x_j} \pi - (1-\gamma)x_j - \gamma h(m(x_j) - 2x_j) \ \text{ s.t. } x_j > 0$$

Solving this optimization problem we get the optimal value of transfer, x_j^a. The first order condition is$1-\gamma = 2\gamma h'(m(x_j) - 2x_j) + m'(x_j)(1 - \gamma h'(m(x_j) - 2x_j))$ which yields either zero transfer if $1-\gamma > 2\gamma h'(m(0)) + m'(0)(1-\gamma h'(m(0))$ or positive value of $x_j = x_j^a$ which satisfies the above mentioned condition.

Comparison of x_j^c and x_j^a

The first order conditions for both information environments differ in one additional term in information asymmetry and in the argument of the h function. Considering assumption of $m'(x_j) > 0$, the outcome of the comparison depends on the sign of the F.O.C.s second term in the information asymmetry setting , $(1 - \gamma h'(m(x_j) - 2x_j^a)$, and on the relation between actual size of the pie and value of the m function. Given three possibilities of that sign and three possibilities for the sign of $\pi - m(x_j)$ expression we get nine possible predictions of the x_j^a value compared to x_j^c. The predictions are all described in the text and their derivations follow trivially from comparison of the first order conditions.

Exposure variable in the model

All utility maximization results remain the same with the replacement of parameter γ by parameter $\gamma(e)$ with its value depending on the information environment.

Comparative statics

Increase in exposure increases the value of parameter γ in this model. Looking at the comparative statics for optimal solution in full information situation we get the straightforward result:

$$\frac{dx_j^c}{d\gamma(e)} = -\frac{1+2h'(\pi-2x_j^c)}{2\gamma(e)h''(\pi-2x_j^c)(-2)} > 0.$$

For the environment with information asymmetry we get the following expression:

$$\frac{dx_j^a}{d\gamma(e)} = -\frac{1+h'(m'(x_j^a)-2x_j^a)[2-m'(x_j^a)]}{m''(x_j^a)[1-\gamma(e)h'(m'(x_j^a)-2x_j^a)]+\gamma(e)h''(m'(x_j^a)-2x_j^a)[2-m'(x_j^a)][m'(x_j^a)-2]}.$$

Using the assumption $m'(x_j) < 2$ we can easily see that the nominator of the fraction is positive. The sign of denominator is negative using the fact that $[2 - m'(x_j^a)][m'(x_j^a) - 2]$ product is negative and using the assumption of $m''(x_j^a)$ being very small and thus keeping the whole denominator in the negative value. So the result of comparative statics is the same as for full information setting:

$$\frac{dx_j^a}{d\gamma(e)} > 0.$$

2 Ego-utility and Endogenous Information Acquisition; An Experimental Study

2.1 Introduction

It is not surprising to claim that people do not always make optimal decisions, at least from financial point of view. Instead of an exhaustive list of such occasions I would like to point out the areas of health care, financial investments, and job safety, where such non-optimal decisions may have serious monetary or even fatal consequences[33]. But regardless of the reasons for such bad decisions, a simple remedy exists. One needs proper information for better decision making.

Additional information is usually enough to steer the decision making process back to optimal choices. Evidence of such an improvement in decisions can be found in many studies (e.g. Grieco and Hogarth, 2009, Juslin et al., 2000, Arkes et al., 1987, Ryvkin et al., 2012). So why do we still observe non-optimal decisions, even in cases where relevant information is widely available (and even for free)?

The problem is that information acquisition and use of this information is based on the endogenous decisions of the agents. Information has been exogenously provided to the agents in the mentioned studies. But the situation is usually different in many day-to-day situations. Information is available to the agents but they make decisions on how much of it to acquire and to what extent it will influence their decisions. Availability of information alone does not automatically lead to an improvement in decision making. There is no agreement in the current literature (discussed in the next section) about how economic agents choose information when the choice is left completely up to them. The main idea of this paper is to examine how agents acquire costless information which might help them to make better decisions.

There are a few theories predicting different information acquisition decisions and their processing by subjects. The classical approach clearly predicts that agents should use each available piece of information and base their decisions on it. However, theories exist which do not predict full information acquisition. I focus mainly on the ego-utility theories (Köszegi, 2006) and their predictions about signal acquisition. They claim that agents may prefer to sacrifice monetary payoffs in order to increase their utility through positive beliefs about their own skills. I also present other theories, such as cognitive dissonance or confirmation

[33] For more evidence and explanation see for example Dunning, Heath, and Suls, 2004, Kines, Andersen, Spangenberg, Mikkelsen, Dyreborg, and Zohar, 2010, Sawacha, Naoum, and Fong, 1999, Odean, 1998

bias, relevant for endogenous signal acquisition. Their basic assumptions and predictions are explained in the next section. Furthermore, it is not always clear how the acquired information is translated into the final decision. Most of the theories mentioned in the next sections assume a Bayesian updating approach for information processing. However, Bénabou and Tirole (2002) explain the confirmation bias which allows the agents to selectively choose which information will be taken into account.

This paper experimentally tests the predictions of the mentioned theories about the decision making process in situations where useful information is available but the decision of how to deal with such availability is left to the agents. Such situations describe many real-life situations with serious monetary or non-monetary consequences to our lives. Therefore it is important to shed light on the "decision making black-box" between making the information available and the actual decision of the agents. If the reason for low information acquisition[34] is the cost of information, the remedy would be to decrease this cost. If the problem is an unwillingness to acquire the information, then mandatory acquisition would improve the decisions. The most cumbersome situation would occur if people did not follow the acquired information. Then some enforced decision might be desirable in situations with the most serious consequences.

The subjects are asked to make a choice between two lotteries in my experiment. The outcomes of the lotteries are based on their performance in the knowledge quiz from the topic of their choice. They can improve their decision by acquiring costless (but noisy) signals. They can acquire up to 10 such signals which are easy to interpret. There are two treatments for the purpose of exploring ego-utility motives. Subjects have an opportunity to create positive beliefs about their performance (and increase their utility derived from these nice beliefs) in the first treatment, while the second treatment switches-off such an opportunity for additional ego-utility.

Only slightly more than one half of the subjects acquire the full amount of signals. Interestingly, most of the subjects with a sub-optimal amount of information acquire either no or only one signal. This paper cannot examine all possible parametrizations and subtle changes in the environment. However, I find support mainly for rational-agent theory (profit maximizing Bayesian updaters) and for cognitive dissonance aversion. On the other hand, I find a little or no evidence for the ego-utility theory and minor support for the theory of self-deception in my setting.

[34] Lower than the socially desirable optimum.

2.2 Related literature

There are several theories predicting the different optimal information acquisition decisions and their processing by people. This paper examines the behavior in the environment with costless information (or "signal") which is noisy but still informative. The reason for this simple framework is to abstract price effects from information and to reflect the essence of most of the day to day decision problems (for example sufficiency of free but sometimes imprecise information from the internet). In this case, the classical approach clearly predicts that the agents should use each available piece of information and base their decisions on it. This claim relies on the fact that information is in a form which does not require cognitively costly processing and therefore the marginal cost (explicit plus processing cost) of each additional signal is virtually zero. In fact, this is not what we observe in reality. I already mentioned examples of such behavior. The obvious conflict of supposedly rational behavior and observed behavior has been explained by several alternative theories.

2.2.1 Ego utility

The overconfidence, or ego-utility, stream of literature predicts a sub-optimal level of signal sampling in some situations (from a payoff maximizing point of view). Dunning et al. (2004) and Merkle and Weber (2011) describe the situations affected by the presence of overconfidence[35]. People tend to derive utility from having positive beliefs about themselves. The essential idea in this stream of literature is that beliefs about one's own ability (or about beauty, health, etc.) directly enter the utility function. If we assume that a signal updates beliefs, then it also influences the utility of the decision maker. Köszegi (2006), Jermias (2006), Carrillo and Mariotti (2000) claim that decisions about information acquisition can be influenced by our beliefs.

Köszegi (2006) introduces a model in which the agent has to decide whether to take more ambitious but risky action or to not take any action. Success in the ambitious action is positively correlated with a level of certain skill. The agent can estimate her skill from the costless noisy signals and can stop receiving signals at any time. The crucial feature of this model is that agents derive "ego-utility" from considering themselves as types with higher skills. So they stop collecting signals once the last signal gives them the feeling that they are skilled (even though it may not be correct because of the noise in the signal). With the described signal sampling process some agents may sacrifice monetary payoffs (by not sampling all possible signals) in order to preserve a positive self-image. If people start with on average correct initial beliefs, they would end up on average with overconfident beliefs.

[35] For more evidence on overconfidence see for example Svenson (1981); Grieco and Hogarth (2009); McKenzie, Liersch, and Yaniv (2008); Karelaia and Hogarth (2010); Odean (1998)

The ego-utility model of Köszegi (2006) predicts aversion to additional information if a person already holds positive beliefs about their own ability. But there are also opposite views in the existing literature. Ko and Huang (2007) introduce their model and conclude that overconfidence leads to higher information acquisition. Mobius, Niederle, Niehaus, and Rosenblat (2011) provide evidence that around 10% of their subjects are averse to new information and underconfidence is more likely to cause aversion to additional information. Eil and Rao (2011) claim that signal acquisition behavior is actually influenced by the direction of the previous signal. A negative signal is likely to lead to information aversion. Although the mentioned studies come to different predictions, they also have common features important for this study.

Simplifying their conclusions, the theories divide belief space into two areas, either with positive or with negative beliefs about one's own ability. One of them is associated with being information averse and the other with information seeking behavior. Once a person is in the information averse part of belief space, there is decreased willingness for signal sampling. While Köszegi (2006) claims that positive signals update the beliefs about one's own skills in the information averse area, studies by Mobius et al. (2011) and Eil and Rao (2011) claim that negative signals would lead to information aversion.

All ego-utility theories assume that some uncertainty is needed in order to create a possibly biased self-image. Once the exact and objective information is present, the ego-utility cannot be based on biased beliefs. So if the information about one's own skill or ability or performance is provided, then ego-utility is prevented[36]. However, there is another notion which can potentially interact with ego-utility motives. As uncertainty creates potential for manipulating beliefs about one's own skills (and theoretically leads to a lower desire for information), it might also give rise to curiosity (increasing the desire for information).

Curiosity could be described as "yearning for information". Although there could be more psychological explanations for curiosity. It could be defined as a kind of impulsive behavior (e.g. Hartig and Kanfer, 1973) or using information-gap theory (e.g. Loewenstein, 1994). I am not going to stick to either one or another definition. Regardless of its psychological underpinning, curiosity might lead to increased information acquisition if the objective feedback about one's own skills is reduced.

2.2.2 Cognitive dissonance

Besides the ego-utility theories, there are also other theories which predict specific behavior either for information acquisition or information processing. The stream of cognitive disso-

[36]This does not hold for agents with really high skills or abilities who are aware of the level of their skills even without feedback.

nance theories (introduced in Festinger, 1962) provides an explanation for signal acquisition below the optimal levels. It is based on discomfort resulting from two (or more) contradictory beliefs at the same time. In order to avoid this discomfort one may not want to get an additional signal if it may be in conflict with the previously acquired signal. Assuming noise in the signals, the second signal may be contradictory to the first one. This might cause the person to stop an information search after the first signal is observed (regardless of its value). Another kind of dissonance follows from the conflict between prior beliefs and the acquired information, if this information does not support the prior beliefs. This might prevent any signal acquisition at all.

> "When dissonance is present, in addition to reduce it, the person will actively avoid situations and information which would likely increase the dissonance" (p. 3, Festinger, 1962)

Harmon-Jones and Mills (1999) provide a more detailed overview of cognitive dissonance theory and its implications. The main implication for my experimental testing is that even in the situations with costless information, signal acquisition may not even happen or may stop immediately after the first signal.

There are many similar studies concluding that the optimal information acquisition for the agents might be below the maximum available amount (e.g. Hirshleifer, 1971, Carrillo and Mariotti, 2000). The essential characteristic of the listed studies was that each piece of information is processed in a rational (Bayesian[37]) way (this holds only partially for Mobius et al. (2011)[38]).

2.2.3 Confirmation bias

Another stream of studies relies, to a certain extent, on the fact that even after the information is acquired, it is processed in a biased way. The agents can control their beliefs to confirm their initial preferences or choices (e.g. Akerlof and Dickens, 1982; Kunda, 1990 explain this approach[39]).

Bénabou and Tirole (2002) incorporate ego-utility into their decision making model. However, improvement in self-image is not achieved through optimal acquisition of the information but through a self-deception process. This process relies on the endogenous selection of information which will be taken into account. Therefore endogenous information acquisition

[37] See Charness et al. (2007) and Charness and Levin (2005) for examples of Bayesian approach experimental testing.

[38] Bayesian processing is present only for the neutral tasks not affecting self-confidence.

[39] More references to similar studies can be found in Jermias (2006).

depends on the parameters of their model. If self-deception is costless, any information acquisition is possible (in the framework of this study) as this does not influence the final beliefs about one's own skills. If the acquired signals are processed in a biased way, the resulting action or choice may not correspond to the optimal one (conditional on the acquired signals).

There are many theoretical and experimental papers studying the effects of exogenous information on decision-making (e.g. Grieco and Hogarth, 2009, Ryvkin et al., 2012). However, such an environment does not resemble many day to day situations. This paper contributes to the literature in the situations where the availability of information is endogenous. There are a few contradictory theoretical predictions, based on the mentioned theories, for such situations (Carrillo and Mariotti, 2000, Köszegi, 2006, Bénabou and Tirole, 2002). The first contribution is to experimentally test these theoretical predictions in a unified framework to find out which theory is prevalent in actual decision-making for information acquisition. There are two experimental papers (Mobius et al., 2011; Eil and Rao, 2011) testing the predictions of particular theories (ego-utility and confirmation bias) about endogenous information acquisition . They conclude that willingness to acquire additional information depends on the direction of the previous signals. However, their conclusions contradict theoretical predictions of ego-utility theory by Köszegi (2006). The second contribution of this paper is to explore whether and how decision making to acquire information is dependent on the previous signal. Mobius et al. (2011) use a combination of exogenous and the possibility of endogenous signals, Eil and Rao (2011) use the BDM mechanism (Becker et al., 1964) in order to elicit willingness to pay for information. I use a very simple environment with only endogenous information acquisition of costless signals.

2.3 Experimental design

There are a few crucial requirements for experimental design in order to distinguish among the possible motivations for information acquisition. First, there must be an initial task allowing for subjective, possibly biased, self-assessment. This is important for the possible effects of cognitive dissonance and ego-utility to take place in the consequent information acquisition process. Second, the information acquisition process must be in the form of acquiring an informative, but noisy, signal in order to allow for confirmation bias and strategic ignorance. Third, the information acquisition must create a dilemma between monetary payoff maximization and cognitive dissonance aversion/ ego-utility preservation. This requirement could disentangle the effect of profit maximization from other motivators. Fourth, in order to isolate the ego-utility motive, there must be variation in treatments changing the opportunity to create ego-utility.

2.3.1 Structure of the experiment

The experiment consists of two decision-making stages, questionnaire and feedback phases. In the first stage of the experiment, the subjects are asked to answer a knowledge quiz with 20 questions (multiple choice, one correct answer) from the topic they prefer. The topic they can choose is one of the following: Science, Sport, Geography, Art, or History. The subjects are advised to choose the topic in which they think they can achieve the best result. The subjects do not have any other information about the consequent stage except the hint that each correct answer increases their earning opportunities in the next stage. The reason for not providing additional information is to prevent strategic behavior which might occur if they know more about the second stage. Grieco and Hogarth (2009) show that this kind of task, even without the choice of the topic, is capable of inducing biased beliefs about one's own performance.

In the second decision-making stage, they are asked to choose one of two lotteries. They are labeled as "Option A" and "Option B"[40]. The first lottery (Option A) consists of known probabilities of winning either 30 CZK or 150 CZK[41]. The second lottery (Option B) offers the same potential prizes but they are informed that the probability of winning 150 CZK is equal to the proportion of the correct answers from the first stage (which is unknown to the subjects at this point). In order to create a dilemma for everybody, the probability for Option A is set in a way that it equals the actual performance in the quiz plus/minus 2 percentage points (+/- sign set randomly). The subjects do not know that the probabilities they observe for Option A are set in this way. So even with beliefs about their own performance close to the true value (or any suspicion that probabilities in Option A are set close to the actual performance), it is unlikely that some additional information about own performance would not be helpful for making the optimal choice.

The subjects have an option to acquire additional information (signal) about their own performance. The additional requirement for the design is that the signal acquisition and processing cost are so low that they do not interfere with other possible motives in the information acquisition process. Therefore, in line with the previous reasoning, the subjects can acquire costless information which are supposed to help them to make a more profitable decision. An advantage of using costless signal is to compare the theories without additional confounds (interaction with signal prices). It also reflects the essence of day to day situations more realistically (information on the internet). The information is in the form of a message stating if their actual performance in the quiz is "Above" or "Below" the known probability of winning 150 CZK in the first lottery (Option A). The signal is true with a probability of

[40] Due to the possible negative connotation of the word "Lottery" in Czech.

[41] 1 EUR was approx. 27.5 CZK and 1 USD was approx. 20 CZK at the time of experiment.

2/3 and false with a probability of 1/3. They can acquire up to 10 such signals. All previous signals remain on the screen. The only cost of signal acquisition is just click on the button and its interpretation should not be problematic. All acquired signals stay displayed on the screen in order to remove forgetting effects and allow easier signal processing[42].

In order to test for the presence of ego-utility I vary the treatments with respect to feedback in the final stage. The subjects receive information about their actual quiz performance in half of the treatments (denoted by F) and do not receive any feedback[43] about their performance in the other treatments (NF). No feedback about the actual performance can lead to the creation of biased beliefs about one's own abilities (Köszegi, 2006). According to ego-utility theory, the true information about your own ability prevents bias in self-assessment while an absence of such information allows for biased self-assessment. If the preservation of ego-utility is the prevailing motivator in the information acquisition process, then we should observe a lower number of signals taken in the NF treatments. The feedback type is known before the lottery choice (without any remark about the alternative feedback type).

After the lottery choice and before the feedback stage the subjects are asked to complete a very brief demographic questionnaire. The questionnaire contains questions about basic demographic characteristics (e.g. age, country of origin, number of siblings, education, etc.) and about the perceived level of risk taking preferences. Then the structured feedback follows. It displays the lottery choice, all possible signals taken and random choice of computer about the earnings. Moreover, for F treatments, it displays the actual performance in the quiz.

Considering the four basic theories for information acquisition, the experiment distinguishes among them in the following way: If the agents are prevailently payoff maximizers, they will acquire 10 signals and follow their values. If ego-utility is important determinant of behavior, there will be higher signal acquisition in the NF treatment and the signals will be followed. Agents with cognitive dissonance aversion will acquire zero or one signal (and will follow it). If confirmation bias is the main determinant of behavior, we will observe any number of acquired signals, but these signals may not be followed.

2.3.2 Logistics

The experiment was conducted in the Laboratory of Experimental Economics at the University of Economics in Prague in January 2014. The subjects were recruited using the ORSEE system (Greiner 2015). Altogether 138 subjects participated in six sessions (3 F treatments, 3 NF treatments) with 63% of the participants being males. The experiment

[42] In order to prevent possible contagion of the results by unintended order effects, the layout of the screen is randomized with respect to the order of options and with respect to the placement of the signal list on the screen.

[43] Not even after the whole experiment is over.

was programmed using the zTree software (Fischbacher, 2007). The average payment was 205 CZK[44] (including a 100 CZK show-up fee) which was paid at the end of the experiment in cash. The subjects were mostly students from different Prague universities. Most of the subjects were Economics or Business students (almost 75%) mostly coming from the Czech Republic or Slovakia. The average age was slightly above 23 years.

2.4 Results

In the first stage of the experiment all 5 quiz topics were chosen by some participants. The average number of correct answers was 12.3 (s.d. 3.2) and the number of the correct answers ranged from 6 to 20, with only two people achieving the best performance of 20 correct answers. Even assuming the possibility of strong bias in the beliefs about the correct answer, more than 91% of the subjects were at least 4 correct answers from the possible maximum performance. So it can be supposed that for the vast majority of the subjects the signal should be helpful even for the strong bias in their beliefs about their own performance.

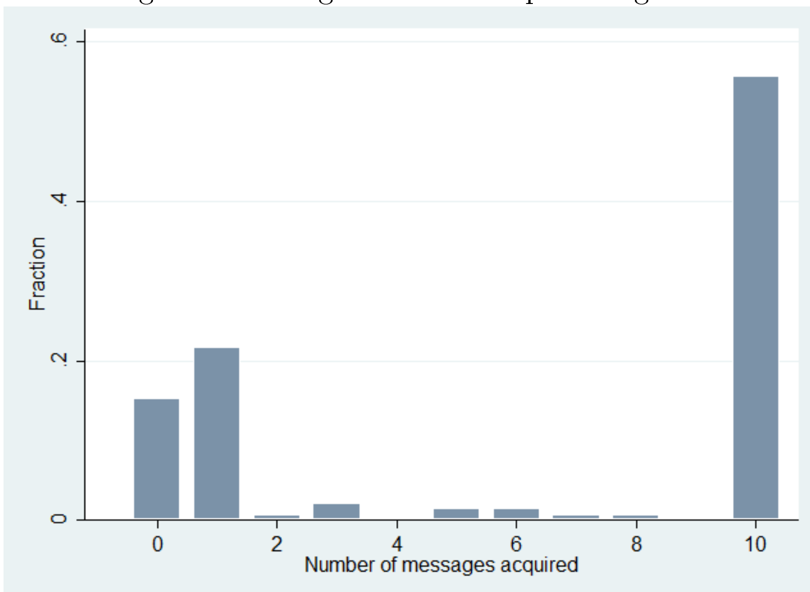
2.4.1 Overall signal acquisition

The average number of acquired signal is 6.14 which is apparently below the predicted optimal level. One could argue that in order to make the optimal decision the subjects could also stop signal acquisition before the maximum amount. The reason could be that the already acquired signals have updated the actual beliefs about the performance in such a way that even if all the remaining possible signals were in the opposite direction, it could not lead to a change in the lottery decision. With heterogeneity in beliefs and randomness in the signals, this would most likely lead to signal acquisition counts spread over the whole domain of possible number of signals. However, the data exhibit a different pattern. Closer inspection of the data (Figure 3) does not support the described reasoning for not taking the full set of signals for most of the subjects.

The subjects fall into four main groups. Only ten subjects (7.25%) took more than two and less than nine signals. For most of those 10 subjects it was the case that one signal was strongly prevalent and they made their decision following the prevalent signal. The rest of the subjects took either the full set of 10 signals (55.8%), or one signal (21.74%) or no signal at all (15.21%). There is mixed support for such results in the discussed theories. Signal acquisition was neither correlated with the achieved score in the quiz ($\rho = 0.06$) nor with the quiz topic (highest value of ρ is 0.12). After splitting the subjects into two groups based on their number of correct answers being above/below the median, there is no statistical

[44] approx. 7.5 EUR or 5 USD at the time of experiment.

Figure 3: Histogram of the acquired signals



difference between signal acquisition of these two groups[45]. There was also no correlation of signal acquisition with other social or demographic characteristics like age, country of origin, field of studies, highest earned degree, monthly spending, and number of siblings. There was a higher mean number of acquired signals for a group of subjects who self-report averse attitude towards risk[46] compared to the group reporting risk-seeking attitudes[47] (5.5 vs. 6.8 of acquired signals) which consist of almost 40% of the subjects. However, this difference is not statistically significant (with a p-value of 0.113). Male subjects acquired more signals but the difference is again not statistically significant.

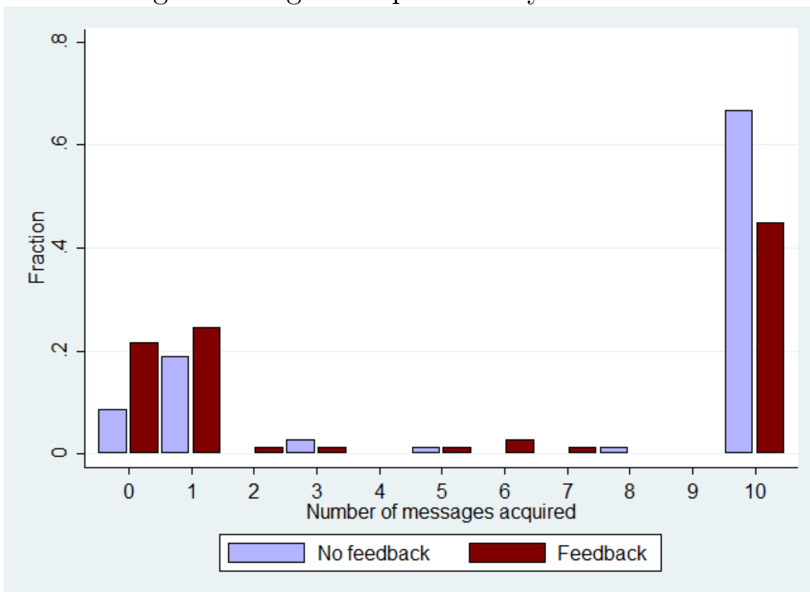
In the following section of this article I relate my results to the existing literature. The full set of 10 signals is consistent with the standard monetary earnings maximization approach. For this approach, the prediction is relatively clear and no other number of signals than 10 would be rational. But this is not case for 44.2% of the subjects. So employing the standard approach only slightly more than half of the observed behavior could be explained. However, the full set of signals does not exclude cases of signal acquisition predicted by the ego-utility theory (Köszegi, 2006). Here, the subjects could simply be still in process of "fishing" for the right set of signals, giving them upward bias in self assessment. In fact, I found the exact opposite effects compared to the ones predicted by this theory.

[45] Using the Wilcoxon ranksum test.

[46] "very averse" or "quite averse" or "somewhat averse" response in the questionnaire.

[47] "quite risk-seeking" or "somewhat risk-seeking" response in the questionnaire.

Figure 4: Signal acquisition by treatment



2.4.2 Ego-utility

The theory of ego-utility (Köszegi, 2006) has no clear predictions about the number of signals acquired. It suggests only that the subjects will stop information acquisition if their beliefs are enough to create a positive self-image. A necessary condition for enjoying the "ego-utility" is the absence of objective information about true performance otherwise positive self-image cannot be created. Therefore only NF treatments allow creation of a positive bias in self-image. Figure 4 presents a histogram of signal acquisition by treatments.

The number of signals is surprisingly greater in NF treatments. The difference between shares of subjects choosing 0 or 10 signals in different treatments is statistically significant at the 5% level[48], the difference for the shares of subjects acquiring 1 signal in different treatments is not statistically significant. Contrasting only the ego-utility stream of literature with the classical approach, there would not be any reason against going for 10 signal in the Feedback treatment. However, all the mentioned ego-utility theories predict that depending on your beliefs about your own performance, your willingness to acquire the signals is not stable. So one can argue that a higher number of signals might be caused by the subject having such beliefs which make them seek the signals. But that argument is not consistent with almost no signals between 2 and 9 because due to the random character of the signal, some subjects would have become averse to the signals during the acquisition process.

Different signal acquisition across the treatments could be explained by curiosity. Van Dijk and Zeelenberg (2007) show that curiosity may be a sufficiently strong motivator and it can

[48]p-values of 0.033 and 0.010 respectively using two tailed t-test.

override other widely accepted motivators (like regret in their case). So the willingness to know one's own performance can be a stronger motivator than building up biased beliefs for some subjects. This is the case in this experiment. But I cannot disentangle ego-utility from curiosity within the scope of this design in order to quantify their amplitudes.

For ego-utility based theories, I can test their different predictions of signal acquisition depending on the direction of the previous signal [49]. But given the distribution of the signal acquisition data, only very basic comparison is possible. Looking at the data pooled over the treatments, 78.8% of the subjects who received a negative first signal continued to sample more signals. While out of those subjects who received a positive signal, 70.8% continued in the signal sampling. But the difference is not statistically significant (p-value of 0.320). More relevant comparison would be for NF feedback subjects only as they have an opportunity to create biased beliefs. For NF treatment, 83.3% of the subjects continued sampling after the negative first signal and 76.9% of the subjects continued sampling after the positive first signal. More subjects sampling the signals after the negative first signal would favor Köszegi (2006) theory over Mobius et al. (2011) and Eil and Rao (2011). But again, this difference is not statistically significant (p-value of 0.542). Due to an insufficient data structure and low number of subjects I cannot perform more tests in an effort to distinguish between the predictions of the different mentioned ego-utility theories, so I will leave this for future studies.

This whole part commenting on the ego-utility theories is based on the assumption that the created environment triggers overconfidence. Even though I did not elicit the beliefs of the subjects, I can infer, to a certain extent, the level of overconfidence from the choices of the subjects. If some person exhibits overconfidence (after the signal acquisition) in this experiment, she will choose Option B. An under-confident person would choose Option A. This distinction is not completely clear as a person with unbiased beliefs may chose both options. However, I use it as a proxy to the direction of the bias as a sufficiently under-confident person never chooses Option B and an overconfident person never chooses Option A. Looking at the pooled data, there is no statistical difference from the 50-50 split of the chosen options. However, having biased beliefs which survive more than a few moments is possible in the NF treatment and not in the F treatment. The subjects have chosen Option B in the NF treatment in 60.9% of the situations, while in the F treatment in 46.4% of the situations[50]. The choice of the Option B is also negatively (significantly) correlated with the number of correct answers, which is in line with the previous overconfidence literature (e.g. Ryvkin et al., 2012). This result may be simply driven by a smaller opportunity for being

[49]Described in detail in the second section of this paper.

[50]The difference is significant at the 10% confidence level, using a two-sided t-test.

overconfident for high performers. But given the range of correct answers mentioned earlier, this would be the case only for extremely biased beliefs about performance, which would be possible only for a few top-scoring subjects.

The evidence suggests the presence of overconfidence in the choices of the subjects in the NF treatment. But there is no clear support for any of the ego-utility based theories regarding the signal acquisition conditional on the direction of previous signals due to insufficient variation in decisions.

2.4.3 Cognitive dissonance

The theory which could explain such a clear distinction in the number of acquired signals is based on the aversion to cognitive dissonance. Festinger (1962) suggests that people may avoid acquiring signals if they expect some conflict between available information. A similar action could also be observed if the possible information were in conflict with their beliefs. As mentioned earlier, following that reasoning, if decision making is mainly driven by cognitive dissonance motives, the subjects would acquire either 0 or 1 signal. This holds for almost 37% of the subjects (27.54% for NF treatment, 46,38% for the F treatment). Even though there is a higher share of the subjects taking one signal than taking no signal, the difference between them is not statistically significant[51].

The theory of cognitive dissonance is the only one among the mentioned alternative theories which predicts such a clear cut between taking at most one signal and more than one signal. The other theories could also lead to either no or one signal. But those theories do not exclude acquisition of two or more signals.

2.4.4 Confirmation bias

Using only confirmation bias (Bénabou and Tirole, 2002) to explain the overall patterns of signal acquisition would not be very successful. If one is waiting for a signal which would be in favor of the initial beliefs, it would be enough to stop after the first occurrence of such a signal. This could potentially hold for those who acquired only one signal. But there was no subject with 10 signals who sampled only one signal of a certain value. This theory deals more with following the signal than with signal acquisition. Due to distribution of the signal acquisition it makes sense to examine the behavior for the subjects taking either one or ten signals.

For the subjects acquiring only one signal the analysis is straightforward. Overall, 30 subjects took one signal. Consequently, 25 out of those 30 subjects (83.33%) followed this

[51] Using proportion t-test (p-value is 0.16).

signal. So the subjects with only one signal mostly follow this signal when making their decision. This could be in line with the information ignorance approach only in a very unlikely case that those solo signals were confirming the initial belief of the subjects about their performance.

Out of 77 subjects who took the full set of ten signals, eight of them observed equal division of signals. For the remaining 69 subjects, one signal (ABOVE or BELOW) was dominant. Moreover, everybody acquiring ten signals received at least two signals of each kind. So the first appearance of the "expected" signal did not stop the signal acquisition. But this fact, by itself, does not contradict the confirmation bias theory as any number of unfavorable signals might be ignored.

The sets of ten signals were followed in 59 cases (85.51%). So, for both groups of subjects (taking either one or ten signals) the signal(s) were not followed only in approximately 15% of cases. There was no statistical difference in the share of subjects following the signal[52] between the subjects acquiring 1 and 10 signals. Moreover, such behavior was optimal for the given randomly drawn signals. Following the signal led to 3.2 times higher probability of optimal decision in the "one signal" group and to 7.6 times higher probability of optimal decision in the "ten signals" group.

I can identify only those subjects who made a decision not in the line with the observed signals. I cannot identify the agents with initial beliefs in line with the signal. But given the random nature of the signals, I cannot conclude that there is strong support for the confirmation bias theory. Additional experimental design features would be needed in order to make deeper examination of confirmation bias theory predictions. As the testing of confirmation bias theory was not the main focus of this study, I refrain myself from making additional claims about the reflection of this theory in the decision making process at this point.

2.5 Conclusion

The first goal of this paper was to experimentally test the theoretical predictions for endogenous signal acquisition. I find a prevalence of monetary maximization or cognitive dissonance theory (Festinger, 1962). For a small group of the subjects, there is the suggestion of confirmation bias (Bénabou and Tirole, 2002) being present. I find no support for endogenous signal acquisition being driven by ego-utility (Köszegi, 2006).

The second goal was to explore the effect of previous signals on further information acquisition. However, the sharp cut data structure is insufficient to make any strong conclusions. I find slight support for Köszegi's (2006) theory over Mobius et al. (2011) and Eil and Rao

[52]Using a proportion t-test (p-value is 0.665).

(2011) but differences are statistically not significant, so this question is left for future research.

The initial knowledge quiz created an opportunity for biased beliefs about one's own performance in one treatment. The consequent lottery choice could be improved with the help of at most ten costless signals. The results show that only slightly more than half of the subjects acquired an optimal amount of signals (following the payoff maximization objective). There were almost no choices between 2 and 9 signals. The remaining subjects acquired either one or no signal.

This paper cannot examine all possible parametrizations and subtle changes in the environment, but some general conclusions can be reached. From the perspective of the mentioned theories, these results suggest that the aversion to cognitive dissonance is the prevalent motivator for those who did not choose the full set of signals. Comparing no-feedback and feedback sessions, the difference between the number of acquired signals is statistically significant but in the opposite direction than predicted by the theory of ego-utility (Köszegi, 2006). One possible explanation for this non-intuitive result could be the curiosity of the subjects about their own performance (See Van Dijk and Zeelenberg, 2007). The mentioned division of the subjects into three groups (0 or 1 or 10 signals) did not allow a proper examination of other ego-utility theory predictions. Namely, those connected with signal acquisition depending on the direction of previous signals. Another important result is that once the signals are acquired, they are mostly followed (in 85% of the cases) regardless of the number of acquired signals. This finding is not massively supportive for the theory of Benabou and Tirole (2002). However, around 15% of the subjects make decisions against the acquired signals what might be economically significant group in some situations.

Summing it up, almost half of the subjects did not acquire the optimal number of signals which, if followed, led to an increase in the monetary payoff. So it seems that only making useful information available does not necessarily lead to an improvement in decisions. If the negative consequences of a non-optimal decision are serious, some paternalistic or behavioral policies aimed at information acquisition support may improve the choices of the agents.

3 Do Fixed-Prize Lotteries Crowd Out Public Good Contributions Driven by Social Preferences?

co-authored by Peter Katuščák

3.1 Introduction

There is extensive theoretical and experimental research on designing mechanisms that overcome under-provision of pure public goods under the voluntary contributions mechanism (VCM).[53] One particular line of research, starting with Cornes and Sandler (1984, 1994) and Andreoni (1990), proposes bundling public goods together with private goods. Morgan (2000) considers an environment in which the private good component consists of a lottery (raffle) ticket that gives the owner a chance to win a prize financed by a portion of collected contributions. Each contributor receives the number of lottery tickets that is proportional to his or her contribution (e.g., one ticket for each contributed Euro). At the end, one lottery ticket is drawn at random and the winner receives the prize. Therefore, the probability of winning the prize is equal to the share of a given individual's contributions to the total sum of all contributions. Morgan (2000) shows theoretically that fixed-prize lotteries with large prizes can induce equilibrium contributions that, after subtracting the prize, generate amounts of the public good arbitrarily close to the social optimum. The underlying idea is that one could move toward the efficient level of public good contributions if the positive externality inherent in contributions could be counterbalanced by an artificially designed negative externality. Under a fixed lottery prize, holding other contributions fixed, whenever an individual contributes an additional Euro, his or her expected winnings rise, at the expense of the expected winnings of the other contributors. This is the negative externality artificially introduced by the lottery.[54] Morgan and Sefton (2000) conduct an experimental test of this theory and indeed find that contributions increase with the size of the lottery prize.[55]

[53] This literature is surveyed by, among others, Ledyard et al. (1997), Chen (2008) and Chaudhuri (2011).

[54] On the other hand, Morgan shows that pari-mutuel lotteries, in which the prize is equal to a fixed share of the collected contributions, do not alleviate the free-rider problem. The reason is that the negative effect of a larger contribution on the expected winnings of the others is fully offset by the increasing size of the prize.

[55] Alternative contribution-boosting mechanisms are based on the use of (all-pay) auctions. Goeree et al. (2005) theoretically compare performance of various types of mechanisms. Even though experimental results (Orzen et al., 2008; Schram and Onderstal, 2009) are not always consistent with theoretical predictions, the findings confirm the basic theoretical conclusion that the prize-based mechanisms (both lotteries and auctions) raise more contributions than the VCM. Further empirical literature focuses on different parameters of the

Contrary to the theoretical prediction of complete free riding, there is a large experimental literature (Chen, 2008) documenting that subjects contribute positive amounts to the public good under the VCM. Importantly, this happens even in one-shot settings and in repeated play under the "stranger" protocol, in which contribution groups are randomly rematched in every round and, hence, repeated interaction effects do not play a significant role. Virtually all systematic explanations of this finding appeal to subjects having social preferences of some kind. Subjects could be altruistic (Becker, 1974, Andreoni, 1989, 1990), or they could be social welfare maximizing (Laffont, 1975), or they could act out of reciprocity (Dufwenberg and Kirchsteiger, 2004, Falk and Fischbacher, 2006, Rabin, 1993, Sugden, 1984) to positive expected contributions of the others. In addition, subjects could also be driven by inequality aversion (Fehr and Schmidt, 1999b, Bolton and Ockenfels, 2000) given positive expected contributions of the others, but such an argument relies specifically on *advantageous* inequality aversion. Assuming utility is linear in the public good, altruism and social welfare maximization predict that one's contribution is independent or decreasing in the average expected contribution of the others (depending on whether utility is linear or concave in the well-being of the others/the group, respectively). On the other hand, reciprocity and inequality aversion predict an increasing pattern. Fischbacher et al. (2001) implement a direct contribution elicitation tool based on the strategy method, with subjects deciding how much to contribute conditional on the average contribution of the other group members. They find that about half of the subjects can be classified as "conditional cooperators" in that their conditional contribution increases with the average contribution of the others, another third are "conditional free-riders," with the remaining one sixth displaying other patterns of conditional contributions.[56] Among the theories mentioned earlier, these findings unambiguously favor the reciprocity/inequality aversion explanation.[57] Croson (2007) comes to the same conclusion analyzing experimental data on unconditional contributions and beliefs about the contributions of others, and also dynamic contribution responses in a repeated linear public goods game.

If positive contributions in the VCM are driven by reciprocity to positive expected contributions of the others, then introduction of a lottery may (partially) crowd out this motivation

prize-based mechanisms, such as the effect of multiple prizes (Faravelli and Stanca, 2007; Lange et al., 2007a) or an asymmetry in valuations of public goods (Lange et al., 2007a).

[56]The empirical result that one's own conditional contribution on average increases with the contributions of the other group members was obtained even earlier by Weimann (1994) and Bardsley (2000). However, they only consider two realizations of the contributions by the other group members (a low one and a high one).

[57]In principle, conditional cooperation might be an artifact of subject desire for conformity, rather than reciprocity or inequality aversion. Bardsley and Sausgruber (2005) find that about one third of conditional cooperation is indeed driven by preferences for conformity. However, that still leaves an important role for reciprocity and inequality aversion.

to contribute. The reason is that whereas positive contributions under the VCM are clearly interpretable as an attempt to benefit the group, this is no longer the case under the lottery. In the latter case, contributions are likely to be at least partially driven by a private motive to win the lottery prize. As a result, contributing out of reciprocity becomes at least partially crowded out. Indeed, there is evidence from many domains that introducing monetary incentives crowds-out pro-social behavior. For example, crowding our has been identified in contract design (Fehr and Gächter, 2000, Falk and Kosfeld, 2006), volunteering (Frey, Goette, et al., 1999, Gneezy and Rustichini, 2000), charitable giving (Meier, 2007), adherence to civic duties (Frey and Oberholzer-Gee, 1997), and trust relationship (Bohnet, Frey, and Huck, 2001, Fehr and List, 2004).[58] In fact, for the case of lottery financing of public goods, Morgan (2000) himself points out that:

> "One possible drawback of employing lotteries in financing public goods is that the linkage between private gain from a lottery and public goods provision may actually reduce a taste for altruism or "warm glow" that individuals obtain through giving behavior. Depending on the magnitude of this effect, it would certainly narrow (or possibly reverse) the predicted gap between the provision of public goods through voluntary means and that obtained through lotteries."

If positive contributions in the VCM are instead driven by inequality aversion (IA) in combination with positive expectations of other's contributions, less obvious the effect of introducing a lottery on this motivation to contribute is. As we argued before, the basic argument for giving a positive amount under the VCM relies on *advantageous* IA. As argued by Fehr and Schmidt (1999b), it is reasonable to assume that *disadvantageous* IA is at least as strong as advantageous IA. It is therefore reasonable to assume that both types of IA are present. Starting from the VCM, consider what impact introduction of the lottery has on other's contributions and payoffs. It is likely that others' contributions are higher due to prize seeking. Holding one's contribution fixed, this increases one's own payoff relative to the payoffs of non-winners. On the other hand, one's own payoff is likely to decrease relative to the payoff of the lottery winner, if among the others. Since it is not clear whom one takes as a reference point for the evaluation of inequality, it is impossible to determine what the impact of introducing the lottery is on the original motivation to contribute due to IA.

Regardless of what specific type of social preferences drive positive contributions in the VCM, little is known about the presence or magnitude of potential crowding-out effects of various contribution-boosting mechanisms. The aim of this study is to contribute to filling this gap by shedding light on the extent to which lottery financing of public goods crowds-out

[58] Frey and Jegen (2001) provide a more detailed overview of crowding-out effects in various domains. Benabou and Tirole (2003) outline a possible theoretical underpinning for the crowding-out effect.

pro-social giving. Our study thus contributes to two streams of literature. First, it informs the literature on the design of fundraising campaigns. Second, it adds to the broad literature on crowding-out of intrinsic motivation.

We find the presence of a crowding-out effect robust across various parametrizations. Looking at results from the pooled sample, crowding-out of intrinsic motivation decreases the effect of the additional monetary incentive by roughly one third. Moreover, for conditional cooperators, as defined by Fischbacher et al. (2001), the analogous figure reaches more than 60% under a high lottery prize. We thus document that although the lottery increases contributions over all (which replicates the result of Morgan and Sefton (2000)), such gain comes at a significant cost in terms of crowding out giving driven by pro-social intrinsic motivation. Moreover, from the point of view of fundraising design, our results suggest that the ability of a self-financing lottery to increase net fundraising is sensitive to the social preference profile of the population targeted by the fundraising campaign.

The rest of the paper is structured as follows. Section 3.2 describes our experimental design. Section 3.3 presents experimental results. Finally, Section 3.4 concludes and discusses interpretation of the results.

3.2 Design

As documented by Morgan and Sefton (2000), introduction of a lottery is likely to increase individual contributions. Such an overall effect combines the effect of the one's own prize-seeking incentive to contribute, with the potential crowding-out effect due to others contributing due to prize-seeking rather than to benefit the group. The key to our experimental design is therefore to separate the two effects. We achieve this by introducing an *intermediate* treatment in which one group member cannot win in the lottery, so his or her contribution is affected by the potential crowding-out effect, but not by the prize-seeking effect. A comparison of the intermediate treatment with the VCM treatment then identifies the crowding-out effect. On the other hand, a comparison of the intermediate treatment with the lottery treatment then identifies the pure effect of prize-seeking.

In order to be able to identify the crowding-out effect at the individual level, we utilize a within-subject design with the three treatments mentioned above. For all three treatments, we use modifications of the voluntary contribution mechanism (VCM) frequently employed in public goods experiments in the related literature. Each contribution group consists of 4 subjects.[59] Each subject is endowed with 10 tokens, which he or she can allocate between a

[59]This is the group size used by Morgan and Sefton (2000) in their Iowa experiment and by Lange et al. (2007b) and Orzen et al. (2008). In addition, this is also the group size used by Fischbacher et al. (2001) and Herrmann and Thöni (2009).

private account and a “group project”. For future reference, the number of tokens a subject allocates to the group project will be called his or her “contribution”. A token allocated to the private account generates a payoff of 1 experimental point for the given subject and 0 for anyone else. A token allocated to the group project generates a payoff (marginal per capita return, or MPCR) of 0.75 experimental point to each group member.[60] Our choice of the MPCR is motivated by several previous experiments in the same laboratory showing that lower levels of MPCR are insufficient to generate a significant incidence of positive contributions in the VCM treatment and therefore space for potential crowding-out effects.

Each treatment is further augmented by the presence of an account of R tokens that is provided from outside the subjects' endowments. In the lottery treatment, this account is used to finance the lottery prize. We use an external account rather than a part of subjects' contributions to finance the prize in order to make sure that the prize can be paid out irrespective of the level of contributions (which could be insufficient to finance the prize). The addition of this external account to the lottery treatment introduces a wealth effect, however. In order to neutralize this effect across the three treatments, we also add the same external account to the other two treatments. The following subsections detail how we use the external account in the other two treatments. We use two different parametrizations for R: $R = 8$ and $R = 12$. This choice is motivated by the divisibility of R by 4 (important in the VCM and the intermediate treatment, see below) and by the resulting Nash equilibrium contribution levels in the lottery treatment being in the interior of the contribution choice space. As shown in the Appendix, the only values of R that satisfy these requirements are $R = 4$, $R = 8$ and $R = 12$. We do not use the smallest of the three possible lottery prizes because it arguably generates the smallest difference across the three treatments.[61] We implement the variation in lottery prize in a between-subjects design.

Using this setup, in each treatment we first elicit unconditional contributions. Using the instrument of Fischbacher et al. (2001), we then also elicit contributions conditional on various possible average unconditional contributions (rounded to the nearest integer in the set $\{0, 1, 2, .., 10\}$) of the other three group members. We label this instrument for subjects as a “contributions table”. A contribution, unconditional or conditional, can be any integer from the set $\{0, 1, 2, .., 10\}$.

There is no feedback on one's payoffs or on others' contributions, or on payoffs from the previous decisions until the very end of the experiment. We implement this in order to avoid subjects affecting one another's decisions throughout the course of the experiment. As a result, each subject can be treated as an independent unit in a statistical analysis.

[60] This is the same MPCR as that used by Morgan and Sefton (2000) in their Iowa experiment.
[61] Our choices of R are analogous to those used by Morgan and Sefton (2000).

To avoid potential wealth and hedging effects, we pay for only one elicited contribution situation. This random choice has three dimensions. First, we pay for one randomly selected treatment. All the subjects within the same contribution group in that treatment are paid for the same treatment. Second, within that treatment, the payoffs are determined using the contributions table of one randomly selected group member and the unconditional contributions of the other three group members. Third, if the payoff-relevant treatment is the intermediate treatment, then one randomly selected group member is a lottery non-participant, while the other three are lottery participants. The three dimensions of randomness are independent of one another. Further details of how we implement these random draws are provided below.

The following subsections describe the three treatments in detail. The next subsection then describes the logistics of the experiments, the subject pool and the sample size.

3.2.1 Voluntary Contribution Mechanism (VCM)

This is one of our two baseline treatments. The only modification in comparison with the standard way VCM is usually implemented in laboratory experiments is the addition of an external account of R tokens. This account is evenly split among the four group members and added to their private accounts. Note that this transfer cannot be used to increase one's contribution beyond the initial constraint of 10 tokens. Subjects are informed of the transfer before they make their contribution decision.

3.2.2 Lottery (LOT)

This is the other of our two baseline treatments. It introduces a fixed-prize lottery on top of the VCM, closely following the design of Morgan and Sefton (2000). Relative to the VCM, each token contributed automatically buys one lottery ticket. After the four group members decide on their contributions, one lottery ticket is drawn at random, and the winner receives the prize of R. That is, each of the four group members has a probability of winning the prize equal to the proportion of his or her contribution in the total group contribution. In case all contributions are zero, the prize is randomly allocated to one of the group members, with each group member having an equal probability to win the prize.

3.2.3 Intermediate Treatment (IM)

This is the crucial treatment in between VCM and LOT aimed at disentangling a potential crowding-out effect of lottery introduction from the effect of own prize seeking. This treatment is analogous to LOT with one modification: one group member is excluded from the possibility to win the lottery prize. The probabilities of winning the prize for the other three group members are analogous to LOT. This exclusion creates a wealth effect, however. Given the four contributions, the excluded group member is poorer in expectation relative to VCM or LOT, while the opposite is true for the other three group members. In order to counterbalance this wealth effect, the lottery non-participant receives a fixed transfer of $0.25R$ to his private account. As in VCM, this transfer cannot be used to increase the subject's contribution beyond the budget constraint of 10 tokens. The lottery prize that the other three group members compete for is then given by $0.75R$.

The idea behind this treatment is that the non participants' material incentives to contribute are the same as in VCM. However, his or her contribution may be different in reaction to the fact that the other three group members now have a stronger private material motive to contribute. Hence a comparison of the contribution of this subject in IM and in VCM identifies the crowding-out effect. On the other hand, a comparison of the contribution if this subject in LOT and in IM identifies the contribution effect of his own lottery prize seeking.

We elicit each subject's contribution in two situations (sub-treatments): in the position of a lottery participant, a sub-treatment we label *intermediate-lottery* (IM-LOT), and in the position of a lottery non-participant, a sub-treatment we label *intermediate-fixed* (IM-FIX). In order to minimize any order effects, we exactly balance the order of the two sub-treatments within each contribution group. As in the other two treatments, we first elicit unconditional contributions, followed by conditional contributions. At the end of the experiment, conditional on the IM treatment being chosen to be payoff-relevant, one of the four group members is randomly chosen to be in IM-FIX, while the other three are assigned to IM-LOT.

3.2.4 Logistics, subject pool and sample size

The experiment consists of 8 sessions of 24 subjects, giving 192 subjects in total[62]. One half of the sessions (96 subjects) is implemented with $R = 8$, the other half with $R = 12$. All sessions were conducted at the *Laboratory of Experimental Economics* (LEE) at the University of Economics in Prague, in October 2013. The experiment was conducted using a computerized interface programmed in zTree (Fischbacher, 2007). Subjects were recruited using the Online Recruitment System for Economic Experiments (**?**) from a subject database of the lab. Our subjects are students from various universities in Prague, most from the University of Economics. Almost 70% of the subjects report "Economics or Business" as their field of study, with the remaining subjects reporting other fields. Of the 192 subjects,

[62]This sample size allows for statistical testing with sufficiently high power. A power calculation with the GPower program ((Faul et al., 2009)) indicates that total sample of 96 subjects detects potential small treatment effects (0.25 times the standard deviation) with power 78.4% (using matched pair t-test and 0.05 significance level). Effects with a size of 0.4 times the standard deviation are identified with 99.98% power (same test and significance level).

103 are female and 89 are male.

A session begins with an introductory stage and proceeds with general instructions, three treatment stages (labeled as "decision" stages for subjects), a demographic questionnaire, a feedback stage and a cash payment stage. The general instructions describe the outline of the experiment and the exchange rate used for cash payments. The subjects are informed that they will receive stage-specific instructions at the beginning of each treatment stage. They are told that they are anonymously matched to three different other subjects in each stage. The subjects are further told that they will not be receiving any feedback on other subjects' decisions or on anyone's payoffs until the feedback stage. Finally, they are told that only one of the three treatments is chosen at the end of the experiment to be payoff-relevant.

Each treatment stage starts with printed instructions specific to that stage. The instructions first describe the basic game and the resulting payoff structure. They then describe how the unconditional contribution and the contributions table will be elicited. The subjects are informed that if the given stage is selected to be payoff-relevant at the end of the experiment, then the payoff or a group member randomly chosen at the end of the experiment is determined using his contributions table, while the payoffs of the other three group members are determined using their unconditional contributions. In IM, the instructions also mention that if that stage is selected to be payoff-relevant, then a group member randomly chosen at the end of the experiment is assigned to the role of lottery non-participant, while the other three group members are assigned to the role of lottery participants. Finally, the subjects are provided with two examples of payoff computation.

The instructions are followed by a quiz to check understanding. An experimenter checks the answers of each subject. In case of an incorrect answer, a subject is given an explanation and asked to submit a new answer. The experiment continues only after each subject answers all the quiz questions correctly.[63] Afterwards, subjects submit their unconditional contributions, followed by their conditional contributions (contributions table). There is no time limit to submit the decisions, but if some subjects are very slow, we gently prompt them to submit the response by mentioning that there are only few remaining subjects who have not submitted their responses. During the treatment stages, the subjects have access to a Windows calculator.

To minimize a potential impact of order effects, in each session we exactly balance all six permutations of the three treatments. That is, each of the six permutations is used for exactly 4 subjects. The text of the treatment-specific instructions is identical across all six permutations. However, in the second and the third chronological treatment, separately for

[63]No subject had to be excluded from the experiment due to not being able to successfully answer the quiz questions, potentially after some corrections.

each permutation, we highlight differences compared to the previous treatment.[64]

After all subjects are finished with their choices, we administer a demographic questionnaire. We elicit gender, age, country of origin, number of siblings, academic major, the highest achieved academic degree so far, estimate of monthly spending budget and the number of other subjects in the lab a subject knew before coming to the lab. In addition, for female subjects, we administer an additional questionnaire eliciting menstrual cycle information.[65]

After the demographic questionnaire, three volunteer subjects are asked to draw a token with a number from a non-transparent bag. The first token determines the number of the payoff-relevant treatment stage (1, 2 or 3, in the chronological order). As a result, one third of the subjects in any session are paid according to each of the three treatments (VCM, LOT, IM). The second token determines whose payoffs are determined by the contributions table, whereas the payoffs of the others are determined using their unconditional contributions. The third token determines the identity of the lottery non-participant for IM. For the purpose of the second and the third draw, each subject is assigned an order number (1, .., 4) within his or her group and each of the two draws chooses a token from the set $\{1, .., 4\}$.

The experimental point payoffs are converted into cash payments at the exchange rate of 1 experimental point for 10 Czech koruna (CZK).[66] The average cash payoff, including a 100 CZK show-up fee, is 332 CZK[67] for about 2 hours of participation.[68]

3.3 Results

In this section, we discuss our results. Subsection 3.3.1 focuses on unconditional contributions, while subsection 3.3.2 discusses conditional contributions.

3.3.1 Unconditional contributions

Table 7 presents means of unconditional contributions in all treatments, separately for the two values of R, with IM separated into IM-FIX and IM-LOT. Standard deviations are presented in the parentheses. Of the two IM sub-treatments, only decisions in IM-FIX are relevant for the purpose of the analysis. Therefore, we do not report any results based on contributions

[64] We implemented the highlighting based on pilot experiments in which subjects expressed frustration over having to repeatedly read a lot of the same information.

[65] The purpose of collecting this information is to continue in the line of research started by one of the coauthors in Chen et al. (2013b).

[66] 1 EUR was equal worth around 25.7 CZK and 1 USD was worth around 18.8 CZK at the time of the experiment

[67] This was approximately 12.9 EUR or 17.7 USD at the time of the experiment.

[68] For a comparison, the hourly wage that students could earn at the time of the experiment in research assistant or manual jobs typically ranged from 75 to 100 CZK.

Table 6: Treatments

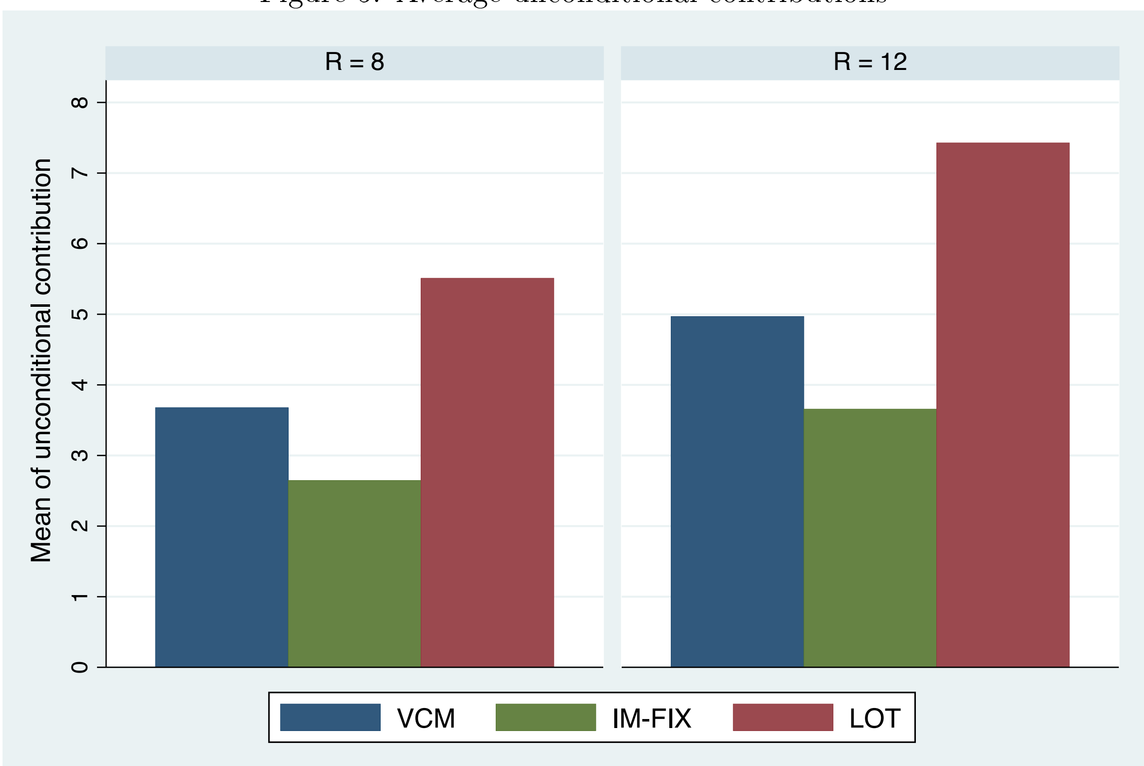
For all treatments: 4 subjects in the group Endowment: 10 tokens MPCR=0.75		**Between-subject treatments** **R=8**	**R=12**
	VCM	+ 2 tokens to private account	+ 3 tokens to private account
	LOT	lottery prize 8 tokens all 4 subjects participate	lottery prize 12 tokens all 4 subjects participate
Within-subject treatments	**IM- FIX**	+ 2 tokens to private account no lottery participation other 3 subjects participate	+ 3 tokens to private account no lottery participation other 3 subjects participate
	IM- LOT	lottery prize 6 tokens 3 potential participants	lottery prize 9 tokens 3 potential participants

Table 7: Average unconditional contributions

	Treatment			
Lottery prize	VCM	LOT	IM-FIX	IM-LOT
R=8	3.677	5.510	2.646	5.760
	(3.594)	(3.476)	(3.458)	(3.396)
R=12	4.969	7.427	3.656	7.312
	(4.011)	(2.983)	(3.778)	(3.048)

in IM-LOT in the rest of the paper. The means are also presented in the Figure 5 for a better illustration.

Figure 5: Average unconditional contributions



Consistent with the previous literature, we find sizable positive contributions in VCM. Moreover, consistent with Morgan and Sefton (2000) and Orzen et al. (2008), we find even higher positive contributions in LOT. More importantly for the purpose of this paper, however, we observe a sizable drop in average contribution in IM-FIX relative to VCM. For $R = 8$, the average contribution drops from 3.677 to 2.646. For $R = 12$, the average contribution drops from 4.969 to 3.656. The treatment differences are statistically significant at any conventional level. Table 8 presents results of the corresponding t-tests.[69]. This drop in

[69] All tests presented in the paper are two-sided tests.

the average contribution indicates the presence of a significant, lottery-induced, crowding-out effect of pro-social behavior under VCM. The size of the effect is 1.031, or by approximately 18% of the average VCM contribution, under $R = 8$ and 1.313, or by around 16% of the average VCM contribution, under $R = 12$.

Table 8: Treatment effects on average unconditional contribution

	Lottery prize:	
	$R = 8$	$R = 12$
LOT - VCM	1.83***	2.46***
	(0.33)	(0.35)
IM-FIX - VCM	-1.03***	-1.31***
	(0.32)	(0.40)
LOT - IM-FIX	2.86***	3.77***
	(0.38)	(0.41)

Notes:

[1] Standard errors are presented in parentheses.

[2] *** denotes significance at 1% level.

Even though introduction of the lottery results in an approximately one-half increase in the average contribution (from 3.677 to 5.510 under $R = 8$ and from 4.969 to 7.427 under $R = 12$), the pure effect of the material incentives introduced by the lottery is even higher. This effect is identified by comparing LOT with IM-FIX. Under $R = 8$, the average contribution increases by 2.864 from 2.646 under IM-FIX to 5.51 under LOT. Under $R = 12$, it increases by 3.771 from and from 3.656 to 7.427. In both cases, this constitutes slightly more than a two-fold increase in the average contribution. As a result, the crowding-out effect reduces the pure effect of the material incentives by approximately 35% (1.031/2.864 under $R = 8$ and 1.313/3.771 under $R = 12$).

These calculations indicate that approximately one third of the intended lottery-driven material incentive on contributions is crowded-out by elimination of purely socially-driven giving. The crowding-out effect poses an important fundraising challenge in the context of our experiment. If the introduction of the lottery had only a pure material incentive effect on contributions as identified by LOT minus IM-FIX, the aggregate increase in contributions would be $4 \times (5.51 - 2.646) = 11.456$ under $R = 8$ and $4 \times (7.427 - 3.656) = 15.084$ under $R = 12$. Hence in both cases the additionally generated contributions would exceed the lottery prize by about quarter to a third. This suggests that if the prize were to be self-financing, as is the case in vast majority of field applications, introduction of the lottery would more than pay for itself, hence increasing the net fundraised amount.[70] On the other hand, although LOT does increase contributions relative to VCM, the aggregate increase falls short of the lottery prize. The observed aggregate increase is $4 \times (5.51 - 3.677) = 7.332$ under $R = 8$ and $4 \times (7.427 - 4.969) = 9.832$ under $R = 12$. Therefore, within the context of our experiment, the crowding-out effect makes the difference between the lottery being able to increase the net amount fundraised and not being able to.

The presence of the sizable crowding-out effect in the pooled sample raises a question of how large the effect is in different sub-populations of subjects. In particular, some contributors may be strongly driven by pro-social incentives, while others might be mostly driven by a participant's own material incentives. We would expect a stronger crowding-out effect in the former group relative to the latter group. However, it is hard to judge a subject's pro-sociality based on his or her unconditional contribution. A low unconditional contribution might be interpreted as a lack of pro-social motivation, but it might also be interpreted as the reaction of someone with strong pro-social incentives to low beliefs about expected contributions of the others. To avoid this problem, we classify subjects using their *conditional* contributions in VCM. To do so, we use the methodology of Fischbacher et al. (2001). First, subjects who have a profile of conditional contributions that is (weakly) increasing in the average contribution of the others, with a Spearman correlation between the two positive and significant at 1%, are called *conditional cooperators* (CCs). Second, subjects whose every conditional contributions is zero are called *free-riders* (FRs). Third, all conditional contribution profiles not fitting the first two categories are lumped into the category called *others*. This group includes various conditional contribution profiles such as full contributions, a hump-shaped profile, and a set of not easily classifiable profiles. This classification results in 93 CCs (48.4%), 66 FRs (34.4%) and 33 others (17.2%). The type distribution is very similar to those identified by Fischbacher et al. (2001) and Herrmann and Thöni (2009), although they used the MPCR of 0.4.

Using this categorization, we examine the crowding-out effect in unconditional contributions separately for each category. It might be puzzling that subjects categorized as free-riders have positive unconditional contributions on average in VCM. This follows from the fact that the categorization is determined by conditional contributions, which might all be zero even if the unconditional contribution is positive. Overall, 18 of the 66 subjects categorized as free-riders have a positive unconditional contribution in VCM. Such a discrepancy might be accounted for by, for example, noise in the submission of subjects' decisions.[71]

[70] We use the term "suggests", since the behavior of subjects under a self-financing prize design might be different than in our setting, even though we believe that any such difference would be small.

[71] Fischbacher et al. (2001) and Fischbacher and Gächter (2006) identify analogous discrepancies in the behavior of free-riders.

Figure 6: Unconditional contributions by conditional cooperation type

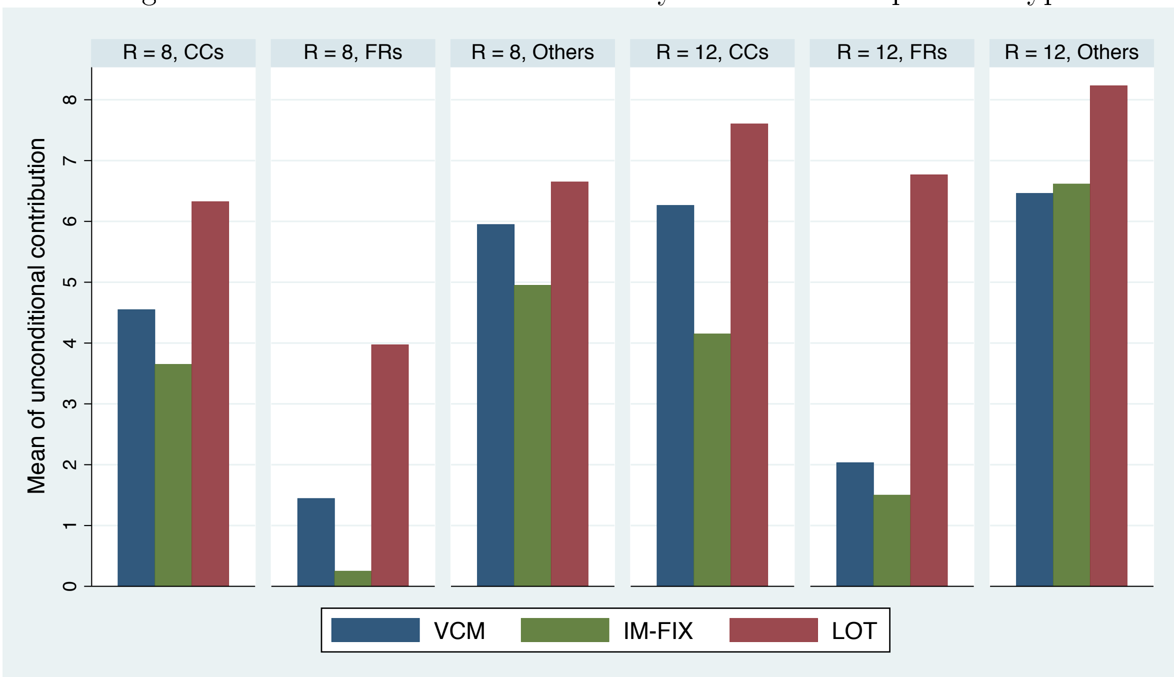


Figure 6 displays the average unconditional contribution for each conditional cooperation type, separately by the two lottery prizes. The crowding-out effect is identifiable in each of the six plots with the exception of the "others" group under $R = 12$. Based on these averages, Table 9 presents pair-wise treatment effects by type and lottery prize, together with t-tests for their statistical significance. The first block of results (IM-FIX - VCM) is the most important for our purpose, it measures the size of the crowding-out effect. For CCs, there is a robust crowding-out effect for both prize sizes, although the effect is only marginally statistically significant for $R = 8$. For FRs, there is a statistically significant crowding-out effect for $R = 8$, but not for $R = 12$. For the others, there is no statistically significant treatment effect for either of the two prize sizes. The last line of the table present the difference between the size of the crowding-out effect for CCs and the FRs, separately for each prize size. The size of the effect is not significantly different for $R = 8$, but it is marginally statistically significant for $R = 12$, with CCs having a stronger crowding-out effect than FRs. The latter finding is consistent with the hypothesis presented above: subjects with stronger pro-social motivations are more strongly affected by crowding-out in comparison to subjects more strongly driven by their own material incentives.[72]

Regarding the pure material incentive (LOT - IM-FIX), the second block of Table 9 shows that the effect is strongly present for both CCs and FRs for both prize sizes, with the

[72]We do not test for differences in the size of the crowding-out effect relative to the others, since we do not know how to interpret the findings of such a test.

effect being stronger for FRs, statistically significantly so for $R = 12$. Again, this finding is consistent with the hypothesis that FRs are more strongly driven by own material incentives to win the prize in comparison to CCs. The pure material incentive effect is also present for the others, but only with marginal statistical significance.

Finally, the last block of Table 9 presents the overall effect of introducing the lottery (LOT - VCM). Both CCs and FRs display a strong positive increase in the average contribution, with the effect being stronger for FRs, statistically significantly so for $R = 12$, reflecting the analogous finding from the previous block of the table. The lottery introduction also increases the average contribution of the others, but statistically only marginally significantly so, and only for $R = 12$. Overall, the results for the others indicate a combination of a smaller sample size (33 subjects) and a larger amount of noise in their decisions.

Replicating the calculation presented earlier for the pooled sample, we observe that, for CCs, the crowding-out effect reduces the pure material incentive effect by around 34% (0.90/2.68) for $R = 8$ and 61% (2.11/3.45) for $R = 12$. In comparison, the corresponding figures for FRs are 32% (1.19/3.72) for $R = 8$ and 10% (0.53/5.27) for $R = 12$. The proportional crowding out is barely different across the two types for $R = 8$. On the other hand, it is 6 times larger for CCs in comparison to FRs for $R = 12$. Hence the hypothesis that subjects with stronger pro-social motivations are more strongly affected by crowding-out in comparison to subjects more strongly driven by their own material incentives is not supported for the lower prize size, but it is strongly supported for the higher prize size.

These findings allow us to obtain a deeper insight into the net fundraising challenge of insufficient additional contribution generation in LOT vs. VCM in the pooled sample. The finding that if the lottery had only a pure material incentive effect on contributions, as identified by LOT minus IM-FIX, there would be an aggregate increase in contributions in excess of the lottery prize, holds true in any population comprised of CCs and FRs (but not of the others) for both prize sizes. However, the findings differ when it comes to the actual increase in aggregate contributions from VCM to LOT. In a population comprised only of CCs, the increase in aggregate contributions falls short of the lottery prize under both prize sizes. On the contrary, in a population comprised only of FRs, the increase in aggregate contributions exceeds the lottery prize under both prize sizes.[73] Therefore, within the context of our experiment, these results suggest that a self-financing lottery increases net fundraising in a population dominated by FRs, but *decreases* net fundraising in a population dominated by CCs.

[73]The calculations underlying these claims can be easily carried out based on the effects reported in Table 9, analogously to how we carried out the calculations for the pooled sample (see above).

Table 9: Treatment effects on average unconditional contribution by conditional cooperation type

	IM-FIX - VCM		LOT - IM-FIX		LOT - VCM	
	$R = 8$	$R = 12$	$R = 8$	$R = 12$	$R = 8$	$R = 12$
CCs	-0.90*	-2.11***	2.68***	3.45***	1.78***	1.34***
	(0.53)	(0.55)	(0.57)	(0.56)	(0.50)	(0.31)
FRs	-1.19**	-0.53	3.72***	5.27***	2.53***	4.73***
	(0.50)	(0.77)	(0.62)	(0.71)	(0.58)	(0.74)
Others	-1.00	0.15	1.7*	1.62*	0.70	1.77*
	(0.64)	(0.59)	(0.85)	(0.75)	(0.63)	(0.91)
CCs - FRs	0.29	-1.58*	-1.05	-1.81**	-0.75	-3.39***
	(0.73)	(0.94)	(0.84)	(0.90)	(0.76)	(0.80)

Notes:
[1] Standard errors are presented in parentheses.
[2] * , ** and *** denote significance at 10%, 5% and 1% level, respectively.

3.3.2 Conditional contributions

In this subsection, we analyze treatment effects on conditional contributions. Figure 7 displays average conditional contribution profiles by treatment and prize size. As in the unconditional contribution data, we observe a consistent increase in contributions in LOT relative to VCM under both prize sizes. However, the crowding-out effect is absent under $R = 8$.[74] Under $R = 12$, it is present, but only for higher values of the conditioning variable (CV). In particular, the treatment effect IM-FIX minus VCM is numerically small (less than 0.1 in absolute value) for $CV \leq 2$ and statistically insignificant. For $CV = 3$ and $CV = 4$, the difference is -0.24 and -0.43, respectively, and it is statistically insignificant at conventional levels. For values of CV between 5 and 10, the difference ranges from -0.74 for $CV = 5$ to -1.19 for $CV = 10$, with the t-test p-value ranging from 0.006 to 0.015.[75] In comparison to the average effect of the pure material incentive (LOT minus IM-FIX) of approximately 3.55 on average, which is quite stable across all levels of CV[76], this constitutes roughly a one-fifth to one-third crowding out effect for values of CV between 5 and 10.

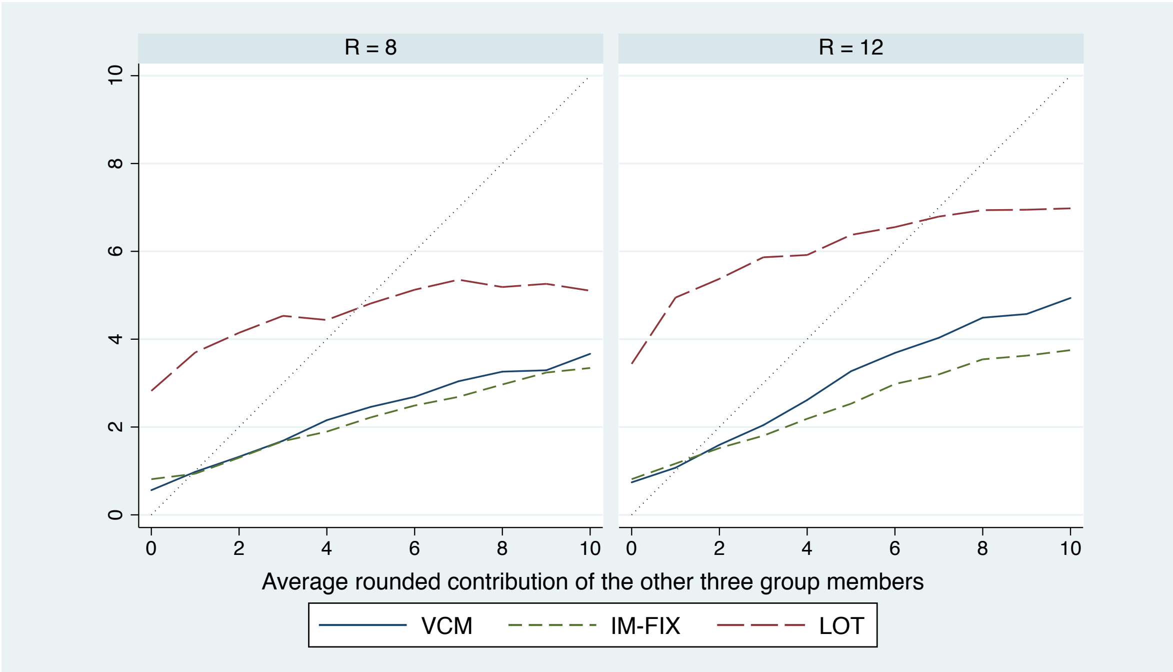
Differentiating subjects by their conditional cooperation type as in the previous subsection, Figure 8 displays average conditional contribution profiles by treatment, type and prize

[74] The difference IM-FIX minus VCM is numerically small and statistically insignificant for any value of the conditioning variable. The results are available from the authors upon request.

[75] Detailed results are available from the authors upon request.

[76] In a regression of this treatment effect on CV, the slope coefficient (and its standard error) is -0.012(0.057).

Figure 7: Average conditional contributions



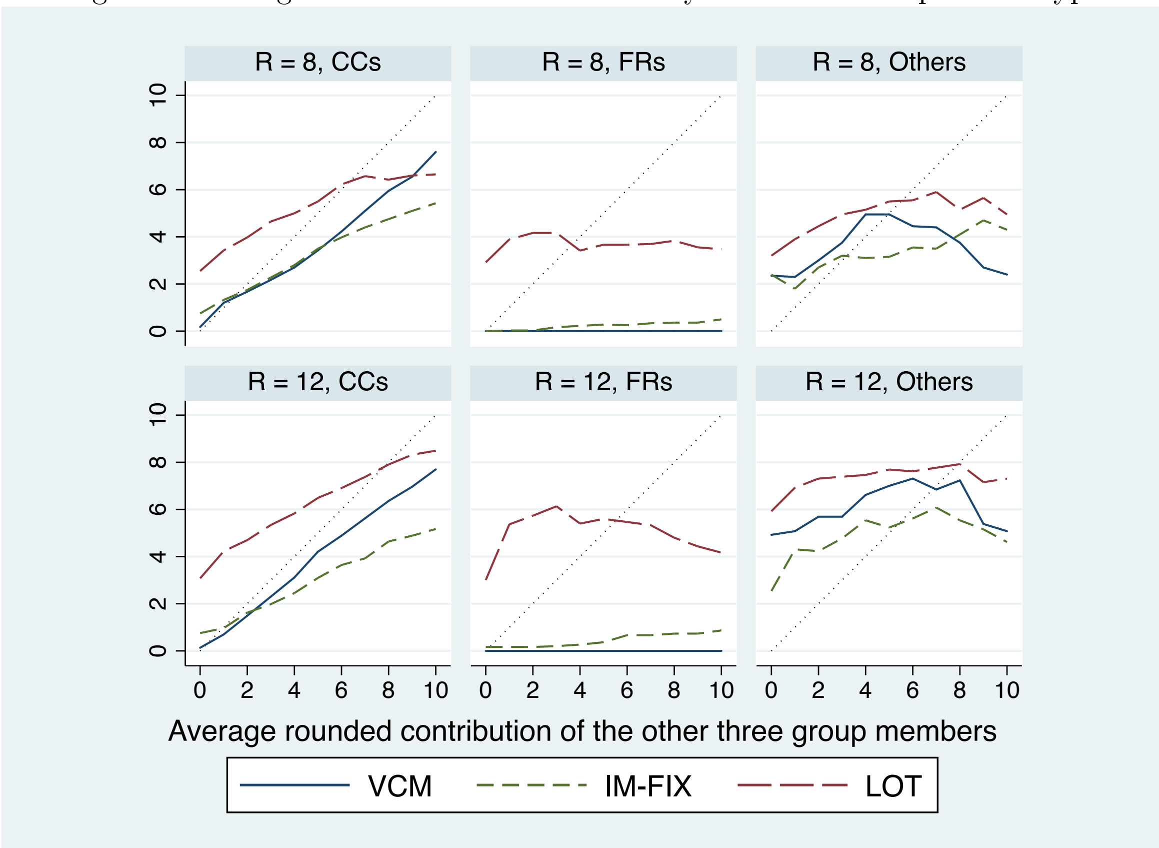
size. By construction, there cannot be any crowding-out effect for FRs. In comparison to the results from the pooled sample, the crowding-out effect is stronger for CCs.[77] In particular, it is now statistically significantly present for values of CV in the range $\{8, 9, 10\}$ under $R = 8$ and in the range $\{5, .., 10\}$ under $R = 12$. In these ranges, this constitutes approximately a one-third to 80% (for $R = 12$), a sometimes even more than 100% (for $R = 8$) crowding-out effect of the pure material incentive.

3.4 Conclusion and discussion

We investigate a possible crowding-out of pro-social incentives in fundraising for public goods by provision of explicit monetary incentives in the form of a fixed-prize lottery. Our paper extends the theoretical findings of Morgan (2000) and the empirical findings of Morgan and Sefton (2000), Orzen et al. (2008) and Schram and Onderstal (2009), showing that introduction of a lottery increases contributions (on average). To identify the crowding-out effect, we introduce a new treatment in addition to the VCM and lottery treatments considered in the previous literature. In this treatment, three contribution group members are in a position analogous to the lottery treatment, competing for a lottery prize of three quarters the size of the prize in the standard lottery treatment. The remaining group member does not compete for the prize, but receives a compensatory transfer in the amount of one

[77] The data for the others is too noisy and the sample size is too small to draw any reliable conclusions.

Figure 8: Average conditional contributions by conditional cooperation type



quarter of the prize in the standard lottery treatment. This subject is therefore faced with the same mapping of her own and others' contributions into her own payoff as in the VCM, with her own material incentive to seek the prize being switched off. Therefore the change in contribution between this treatment and the VCM identifies the crowding-out effect of pro-social contributions stemming from others having incentives to contribute driven at least partially by their own material gain.

We find a strong crowding-out effect on unconditional contributions in a pooled sample. This effect reduces the overall prize-seeking effect on contributions by about one third. Moreover, the effect is robust over a range of lottery prizes consistent with non-maximal Nash equilibrium contributions in the lottery game. We then separate the sample into three distinct groups by the pattern of conditional cooperation in VCM as defined by Fischbacher et al. (2001): conditional cooperators (CCs), free-riders (FRs) and others. Identifying conditional cooperation with pro-sociality, we hypothesize that the crowding-out effect identified in the pooled sample is stronger for CCs than for FRs. We do not find support for this hypothesis in case of lower prize size. The size of the crowding-out effect is approximately one third of the prize-seeking effect, as in the pooled sample. On the other hand, we find support for the hypothesis in case of the higher prize size. The size of the crowding-out effect for FRs is around 10% of the prize-seeking effect, while it is approximately 60% of the prize-seeking effect for CCs.

Regarding conditional contributions, in the pooled sample, there is no crowding-out effect for the smaller lottery prize, while there is a statistically significant crowding out effect for the larger lottery prize in the upper half of the conditioning domain. In proportion to the prize-seeking effect, the size of the crowding-out effect in this sub-domain ranges from one fifth to one third. When separating subjects into conditional cooperation types, we find, analogously to unconditional contributions, a stronger than average level of the crowding-out effect. Among CCs, the effect is present for both prize sizes, but again only in the upper part of the conditioning domain. In proportion to the prize-seeking effect, the size of the crowding-out effect in this sub-domain ranges from one third to more than 100%.

Our findings extend the evidence on the presence of crowding-out effects of pro-social motives by monetary incentivization into an important and empirically relevant mechanism of lottery fundraising for public goods. The results also suggest that the strength of the aggregate crowding-out effect is sensitive to the distribution of pro-social preferences in the population. In particular, for relatively high prizes, the crowding-out effect is likely to be stronger in populations with a majority of pro-social types in comparison to populations with a majority of self-regarding types. These findings have an important implication for fundraising design: the effectiveness of lottery incentivization is likely to be a function of social preference distribution in the target population. Lotteries, as opposed to pure contribution campaigns, are likely to be more effective in populations dominated by self-regarding individuals than in populations dominated by more pro-social types. Moreover, our results suggest that the difference might be between being able and being unable to increase net aggregate contributions when introducing a self-financing lottery.

Our study also has its limitations. Most importantly, although we believe that the identified crowding-out effect is predominantly driven by reciprocity to (expected) contributions of the others, we cannot rule out other theoretical explanations, such as inequality aversion. This complicates precise theoretical extrapolation from our results. More research is necessary to disentangle the two theories as explanations of giving in the VCM. Also, although we manage to consider a range of lottery prizes in our environment, due to budgetary and logistical constraints, we do not vary other parameters such as group size and MPCR. It would therefore be interesting to examine the results we have obtained vis-a-vis results obtained for other parameterizations. Finally, in order to provide a more direct examination of circumstances and population preference profiles under which a self-financing lottery prize increases net fundraising, it would be desirable to run an experiment with the lottery being self-financing. However, as mentioned earlier, this poses a challenge of how to finance the prize in cases of insufficient contributions. One solution would be to cap the prize by the amount of collected contributions. Although a such solution might be experimentally desirable for a study more focused on fundraising design, the uncertainty in the prize size it introduces makes it undesirable for a clean study of contribution incentives, as we pursue in in this study.

Appendix 2: Risk-Neutral Nash Equilibrium in the Lottery Treatment

Let $n \geq 2$ be the size of a contribution group, $w > 0$ be each player's initial endowment, $\alpha \in (0,1)$ be the MPCR, $R \geq 0$ be the externally-financed lottery prize, g_i be the contribution of player i and $\bar{g}_{-i}$ be the average contribution of the other three group members to the group project. Then the expected monetary payoff of player i is given by

$$E(\pi_i) = w - g_i + \alpha[g_i + (n-1)\bar{g}_{-i}] + \frac{g_i}{g_i + (n-1)\bar{g}_{-i}} R$$

if at least one of the contributions is strictly positive and

$$E(\pi_i) = w + \frac{R}{n}$$

otherwise (that is, in case of all contributions being 0, the prize is allocated randomly with equal probabilities). Note that if $R = 0$, this setup corresponds to the standard VCM in which each player's strictly dominant strategy is to contribute zero. If $R > 0$, then it is always preferable to contribute a positive amount rather than 0 if everybody else contributes zero, but the best response is not well-defined. Otherwise, if $R > 0$ and $\bar{g}_{-i} > 0$, note that the expected payoff is strictly concave in g_i. Hence the best response can be derived by considering the sign of the first derivative. In particular, since for this case we have that

$$\frac{\partial E(\pi_i)}{\partial g_i} = -(1-\alpha) + \frac{(n-1)\bar{g}_{-i}}{[g_i + (n-1)\bar{g}_{-i}]^2} R,$$

the best response is given by

$$g_i(\bar{g}_{-i}) = \begin{cases} \min\left\{ \sqrt{\frac{(n-1)R\bar{g}_{-i}}{1-\alpha}} - (n-1)\bar{g}_{-i},\, w \right\} & \text{if } 0 < \bar{g}_{-i} < \min\left\{ \frac{R}{(1-\alpha)(n-1)},\, w \right\} \\ 0 & \text{if } \frac{R}{(1-\alpha)(n-1)} \leq \bar{g}_{-i} \leq w \end{cases}$$

This statement also includes of the case $R = 0$, but, for the reasons stated earlier, it excludes the case $R > 0$ and $\bar{g}_{-i} = 0$. Note that the best response function has a limit point at the origin, is continuous, concave on the part of the domain on which it is positive, and it has an infinite slope at 0. As a result, for any admissible combination of parameter values, there is a unique Nash equilibrium that is symmetric with

$$g_i^* = g^* \equiv \begin{cases} 0 & \text{if } R = 0 \\ \frac{n-1}{n^2(1-\alpha)}R & \text{if } 0 < R < \frac{n^2(1-\alpha)w}{n-1} \\ w & \text{if } \frac{n^2(1-\alpha)w}{n-1} \leq R \end{cases}$$

Under the parametrization $n = 4$, $w = 10$ and $\alpha = 0.75$ that we use in the experiment, it follows that

$$g^* = \begin{cases} 0 & \text{if } R = 0 \\ \frac{3}{4}R & \text{if } 0 < R < 13\frac{1}{3} \\ 10 & \text{if } 13\frac{1}{3} \leq R \end{cases}$$

As a result, the only values of R that are divisible by 4 and that generate Nash equilibrium contribution levels strictly within $(0, 10)$ are $R = 4$, $R = 8$ and $R = 12$. Also, note that the optimal conditional contribution is given by

$$g_i(\bar{g}_{-i}) = \begin{cases} \min\left\{\sqrt{12R\bar{g}_{-i}} - 3\bar{g}_{-i},\ 10\right\} & \text{if } 0 < \bar{g}_{-i} < \min\{\frac{4R}{3},\ 10\} \\ 0 & \text{if } \frac{4R}{3} \leq \bar{g}_{-i} \leq 10 \end{cases}$$

For the values $R = 8$ and $R = 12$ that we use in the experiment, the optimal conditional contribution is inverse U-shaped in $\bar{g}_{-i}$ and always positive. Moreover, for $R = 8$, it reaches its maximum of 8 at $\bar{g}_{-i} = 2\frac{2}{3}$ (or $\bar{g}_{-i} \in \{2, 3\}$ for the provided rounded values). For $R = 12$, there is a flat maximum of 10 on the interval $[4 \pm \sqrt{96}/3]$ (or $\bar{g}_{-i} \in \{1, .., 7\}$ for the provided rounded values).

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