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Abstract

Personal information is shared extensively every day, partly in exchange for benefits or as a reaction to other people's information sharing. In this paper, we experimentally investigate these two factors by analyzing the interaction of peer comparison and incentives to disclose potentially privacy-sensitive information. We find that information sharing is higher under incentives, and further increases under peer comparison. This effect is driven by those initially disclosing less, who additionally report to feel more compelled to reveal information. Our results provide an explanation for the current information sharing trend while pointing to a potentially neglected side-effect.

Keywords: Personal information disclosure, Peer comparison, Incentives, Experiment

JEL Classification: C92, D30, D82

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"Most hiring requires a LinkedIn profile now so although we use this narrative of choice [...] they substantively don't really have a choice because in the modern workforce you have to use social media, and you have to use the internet. [...] When people have to use these platforms [...] to get a job, they will still use it, and so we are sort of coercing and compelling people to hand over a lot of information [...]."

– Christopher Wylie, Cambridge Analytica - May 16, 2018

1 Introduction

Extensive sharing of personal information has become a stylized fact and one of the major societal changes of the 21st century. Every day people post billions of personal information online. While this personal information sharing may be partly driven by a direct preference for information sharing, there might also be a strategic aspect in it.[1] Revealing personal information might create rewards, for example, soft benefits like social appreciation or direct monetary profits. For example, people compete for the beneficial attention of overnight guests or recruiters on Airbnb and LinkedIn, respectively, for that of followers on Instagram or Youtube, and for the attention of lenders on microfinance or crowdfunding platforms like Kickstarter or Prosper.com, via endogenous personal information disclosure. Kickstarter even recommends borrowers to include soft, personal information into their requests, and in fact, such information seems to affect outcomes beneficially (Böhme and Pötzsch 2010; Michels 2012; Pope and Sydnor 2011). While competition in information revelation may always have existed, for example, in the housing market, in which potential tenants bring a folder of documents to stand out, nowadays its impact is exacerbated by online markets.

Another aspect of information sharing behavior is the influence of peers. The more others share, the more likely one adapts to their behavior (Acquisti et al. 2012; Böhme and Pötzsch 2011; Chang et al. 2016). This effect might be especially pronounced in situations in which peers compete for benefits as described above. Under peer competition, extensive revelation in form of more and more provision of personal information might result. Even if such forms of information disclosure provided helpful insights to the other side of the market, extensive revelation might also distort to what one pays attention to, thereby eliminating correct inference about quality.[2] Under such conditions, the classical unraveling result that good types always

[1] See Farrell (2012) who discusses the properties of privacy as an economic good.

[2] See Bartoš et al. (2016) for research on how limited attention can influence the selection of candidates.

reveal information and can thereby be identified might not hold (Milgrom 1981) what transforms information sharing into a rent seeking contest (Lazear and Rosen 1981; Tullock 1988).

Apart from the potentially limited forecasting power of personal information, extensive sharing by peers under competition for benefits might be problematic if it conflicts with intrinsic privacy concerns and thereby causes costs. Abstaining from the information sharing economy might harm a person even if she is of similar capability as her competitors, and becomes worse the more people participate. In the end, she might reveal her personal data in spite of strong privacy concerns since non-disclosure is too costly. Compared to a situation without excessive information sharing due to benefits and peer dynamics, such a person may be worse off. In that sense, new information markets might cause neglected side-effects.

This paper analyzes the interaction of competition for benefits and observing peers' sharing as a channel explaining extensive personal information disclosure. In particular, we investigate whether incentives to reveal personal information lead to more information sharing, and how one adapts one's initial choice in reaction to peer comparison. Regarding potential costs of extensive information disclosure, we additionally explore whether and how the interplay of these two factors is associated with subjectively perceived pressure to reveal information.

We investigate these questions in a laboratory experiment, which enables us to provide causal evidence on competition via personal information disclosure, and to disentangle via a two-by-two design how peer comparison and disclosure competition interact. Two participants compete for distribution power in an impunity game.[3] In the main treatments, a third participant selects who determines the allocation. In order to be selected, candidates striving for distribution power can endogenously reveal answers from a potentially privacy-sensitive questionnaire, thereby making information sharing strategic. In the control treatments, distribution power is randomly assigned so information sharing has no strategic aspect. As a second dimension, we inform participants in half of the treatments (without prior announcement) about their competitor's disclosure choice, and give them the opportunity to adapt their own. Thereby, we can test for the effect of peer comparison on disclosure behavior with and without competition involved. Afterwards, we measure perceived pressure to disclose information, the probability to be selected based on the amount of shared information, and generosity of offers in the impunity game.

We find that information disclosure doubles under strategic incentives compared to the control condition with random assignment of allocation power. Moreover, subsequent peer

[3]The impunity game by Bolton and Zwick (1995) is a version of the ultimatum game in which a rejection by the responder has no payoff consequences for the proposer.

comparison boosts information disclosure in the strategic but not in the random setting. This effect is driven by subjects who are initially relatively unwilling to disclose much, but reveal more information when learning to lack behind. In line with the idea of reluctant adaptations of the less disclosure-willing candidates, these participants report feeling more compelled to disclose information afterwards. Regarding outcomes, disclosing more information indeed raises the probability to be selected as the distributor and thereby pays off for the disclosure-willing market-side, but does not translate into more generous behavior, i.e., does not serve as a good screening device for the one who selects the distributor.

Our paper primarily builds on two strands of literature: the value of personal information provision and the impact of peer comparison.[4] We contribute to the first literature by endogenizing the information sharing decision, and to the second by providing evidence on the existence and consequences of peer comparison in the new and highly relevant context of personal information sharing. We combine both literatures by investigating the interaction of peer pressure and strategic incentives for information sharing, and try to explore potentially neglected costs. Examining this interplay and its consequences while endogenizing the sharing of personal information, our experiment substantially extends a design by Brandts et al. (2006).[5]

Several studies show a positive value of personal information sharing in line with our results. For example, subjects in distribution games give more if personal information like name, major, hobbies, and home city of the recipient are revealed (Bohnet and Frey 1999; Charness and Gneezy 2008)[6]. Hermstrüwer and Dickert (2017) report higher contributions when participants previously consented to reveal their name together with their contribution afterwards.[7] Remarkably, participants even seem willing to pay for seeing the partner's photo in trust games (Eckel and Petrie 2011). With regard to the *endogenous* provision of personal information, a

[4]We also touch several other strands of literature. Our experimental design consists of elements from the partner selection and proposer competition literature. Regarding partner selection, a couple of studies shows that partner selection can help to overcome coordination failures (Coricelli et al. 2004; Page et al. 2005; Riedl et al. 2016; Wang et al. 2012). Proposer competition prevails to affect the distribution of money in favor of the responder (Roth et al. 1991). Moreover, by topic, our project is related to the economics of privacy. Several papers document a rather low willingness to pay for the protection of personal data (Benndorf and Normann 2018; Beresford et al. 2012; Jentzsch et al. 2012; Tsai et al. 2011). See Acquisti et al. (2016) and Tucker (2015) for comprehensive surveys on this topic.

[5]Brandts et al. (2006) utilize a personality questionnaire to determine allocation power in a distribution task either randomly or based on this questionnaire. Since information is exogenously provided in Brandts et al. (2006), their focus lies on how being actively selected affects distributional behavior, while we are interested in the amount of information endogenously provided.

[6]However, Charness and Gneezy (2008) cannot confirm this result in the ultimatum game.

[7]In the opposite setting in Holm and Samahita (2018), participants are more likely to subsequently hide their picture if they behaved less generously, but Hermstrüwer and Dickert (2017) do not find such an effect for names.

study close to ours is Gaudeul and Giannetti (2017) who find higher contributions in public good games when group formation is based on endogenously provided names. Observational data from online microfinance platforms mostly support the idea that personal information sharing is valuable. Michels (2012) reports lower interest rates for loan requests containing a photo, and inversely, chances to get a loan decrease without it (Pope and Sydnor 2011). The latter's analysis reveals that even given observable financial indicators, the provision of a picture matters for receiving funding. Böhme and Pötzsch (2010) find evidence for such a relationship for commercial but not for private borrowers.

Theoretically, the positive value of information sharing is predicted by unraveling theory (Milgrom 1981). Under market competition, good types share their private information while non-sharing correctly evokes suspicion about quality. However, laboratory tests can confirm unraveling only partially (Jin et al. 2017), especially when adding a more privacy-sensitive framing (Benndorf et al. 2015). We go one step further by using not only exogenously assigned information but real-world privacy types which are less quality-predictive and more personal. This renders full unraveling even less likely in our experiment. Evidence for how well voluntarily provided personal information can predict types is mixed. Duarte et al. (2012) observe a positive relationship between the appearance of trustworthiness in pictures and actual trustworthiness in microfinance. While creditors in Pope and Sydnor's (2011) study seem to make use of voluntarily provided personal information, they fail to fully infer all relevant hints on creditworthiness. Relatedly, Iyer et al. (2016) only find a significant effect of insightful inference from voluntarily provided personal information for low credit categories. We add causal evidence on the willingness and value of voluntary personal information sharing to this literature under varying provision incentives .

As a second dimension, our project is related to several aspects of the literature on peer effects, predominantly peer pressure driven by conformity seeking (Asch 1951; Bernheim 1994) and social comparison (Clark and Oswald 1998; Festinger 1954; Frey and Meier 2004). A variety of empirical papers documents that peers have a strong impact on how we behave.[8] Given the diverse range of settings in which peer effects seem to be at work, peer comparison also likely affects personal information disclosure. However, evidence analyzing peer effects in endogenous information revelation is rare. First related results point into the direction that the amount of information others reveal influences one's own disclosure behavior. Findings by

[8]For example, people show more effort in the workplace (Falk and Ichino 2006; Mas and Moretti 2009), vote in elections (Bond et al. 2012; DellaVigna et al. 2017; Funk 2010), adapt their investment behavior (Bursztyn et al. 2014) or donate more (Alpizar et al. 2008; DellaVigna et al. 2012; Frey and Meier 2004; Meer 2011) due to peers. See Bursztyn and Jensen (2017) for a review on peer pressure.

Acquisti et al. (2012) and Chang et al. (2016) indicate that people are more willing to answer sensitive questions or disclose sensitive pictures, respectively, when knowing that others did so. On online mircrofinance platforms, Böhme and Pötzsch (2011) find that borrowers adapt their loan request to the most recent requests listed on the top of the starting page regarding how much to write, whether to add a photo, what personal information to disclose, and how identifiable to present oneself. Results regarding adaptations within the same loan category further suggest positive peer effects, but are less conclusive.

Both the influence of peers and the importance of personal information sharing may however cause unintended and non-negligible side effects. Research by DellaVigna et al. (2012, 2017) shows that actions meant to increase welfare can even have negative welfare effects if social pressure is accounted for.[9] Moreover, there is evidence that peer comparison harms happiness (Reyniers and Bhalla 2013), and that peer pressure in form of competition decreases well-being without creating any gains (Brandts et al. 2009). Exploratory studies surveying or interviewing Facebook users confirm peer pressure in the online world. Wang et al. (2011) report that the desire to appear favorable to one's peers induces people to post what they regret afterwards. With regard to social comparison, Lee (2014) finds a positive correlation between comparison seeking frequency on Facebook and negative feelings from comparison. We contribute to this literature by providing indicative evidence for hidden costs under peer comparison in the personal information sharing domain.[10]

The contributions of our paper are threefold. Firstly, we allow for endogenous sharing of personal information, and thereby show how such information can be strategically employed to compete. Experimental research regarding the value of personal information has mainly been based on exogenous provision of personal information so far (Bohnet and Frey 1999; Brandts et al. 2006; Charness and Gneezy 2008; Eckel and Petrie 2011), and thereby misses how such information can be used strategically to attract attention. While Benndorf et al. (2015) motivate their study with strategic privacy-sensitive information disclosure, they only use exogenously assigned, impersonal information without an intrinsic privacy value for participants. Our study goes one step further in understanding privacy concerns more deeply by using information with an intrinsic private value for participants. Furthermore, we complement observational studies

[9]Similarly, Funk (2010) observes a policy intervention aimed at increasing voter turnout which has the opposite than the expected consequence because of not taking the role of social pressure into account.

[10]Recent theoretical models in economics try to combine peer effects with information disclosure and privacy. Daughety and Reinganum (2010) build a model with different privacy scenarios in which marginal types in a regime in which it is possible to waive privacy are in equilibrium pressured to reveal their type because they care about how they are perceived by others. Ali and Bénabou's (2016) model emphasizes that in fast changing societies with variability in norms, extensive personal information sharing based on image concerns hinders the correct aggregation of information by a policy maker to infer society's true aggregated preferences.

on endogenous personal information sharing (Böhme and Pötzsch 2010; Michels 2012; Pope and Sydnor 2011) with causal evidence, and show how it varies under different incentives. Doing so enables us to detect a new form of competition in society.

Secondly, we provide novel evidence on the dynamics created by the interplay of strategic incentives and peer comparison, leading to more and more personal information disclosure. The combination of these two factors may explain the recent boom in extensive personal information sharing, a stylized fact of the digital age, whose dynamics have mainly been neglected so far. Thirdly, we provide suggestive evidence that such extensive but not directly relevant information sharing might not necessarily generate improvements for all involved parties. If disclosure-unwilling individuals feel compelled to disclose more than they intrinsically would like to but are hindered by high privacy cost to fully catch up with others' information sharing, they may incur unnecessary cost without being able to affect outcomes beneficially.

The remainder of this paper is structured as follows. We present our experimental design and corresponding hypotheses in section 2. Sections 3 reports and discusses the results. The last section concludes.

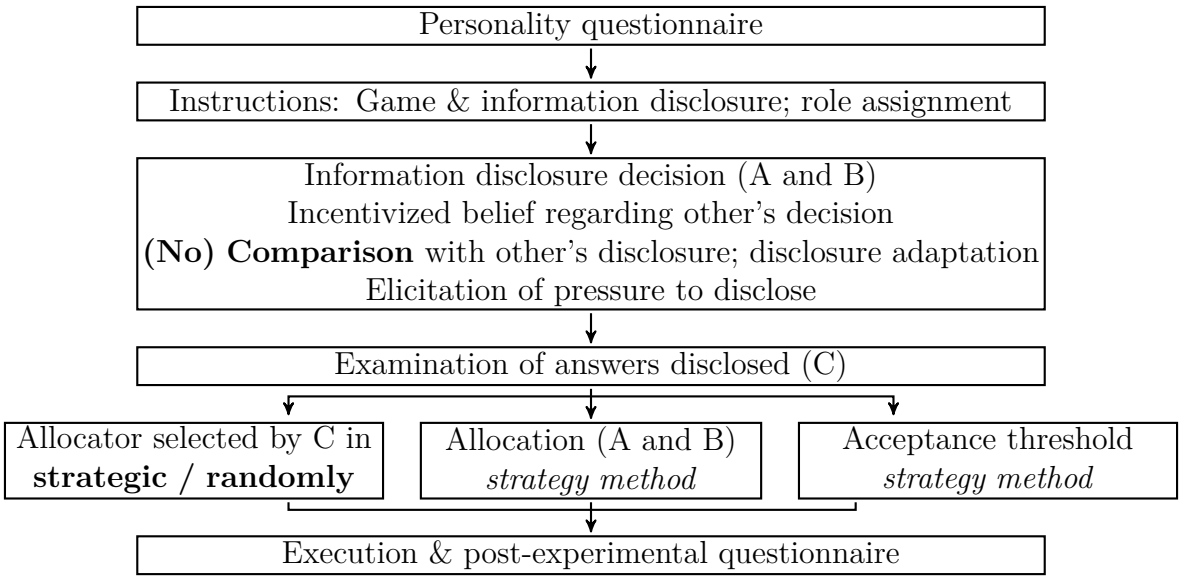
2 Experimental Design

Our experimental design, depicted in Figure 1, consists of the following parts: First of all, information is generated. Second, participants endogenously decide which pieces of information to reveal. Third, they can revise their decision after peer comparison. Fourth, information is taken into account for role selection. Fifth, the allocation and the resulting payoffs are determined. Steps one, four, and five are adapted from Brandts et al. (2006). In order to guarantee understanding, participants had to answer several comprehension questions correctly before being allowed to make decisions. In what follows, the different parts, procedures, and treatments are described in detail. We first focus on the personal information we elicit, continue with the game, the endogenous information revelation process, and additional measures we use, and finish with describing the treatments and their corresponding hypotheses. The experimental instructions can be found in Appendix A.

Personal information

While a first-best approach to study personal information disclosure might be to access real-world data, for example, from social media, such data also come with shortcomings. First, they

Figure 1: Structure of the experiment



Notes: Overview of the experimental steps. A unit of observation in the experiment consists of three participants A, B, and C. Treatment differences are marked in bold letters.

are complex and what information people have already accessed or what they infer from it is out of experimental control, what likely impairs causal inference. Secondly, studying the interaction of strategic incentives and peer comparison, our channel of interest, with field data seems hardly possible on a experimentally sound level. Instead, we follow a second-best approach, and generate potentially sensitive but anonymous and controllable personal information as Frik and Gaudeul (2016).

We use a 12-item questionnaire to elicit opinions and personality traits measured on a 7-point scale, shown in Table 1. Some questions refer to characteristics potentially related to experimental game behavior, while others ask for rather unrelated, subjective opinions or attitudes regarding controversial or sensitive issues. For example, we elicit how participants perceive inequality, whether money is their only reason to participate in experiments, how they assess payment for organ donation, or whether they feel impairment when failing an exam.[11] Participants receive 3 Euro for answering the questionnaire, being aware that all information they provide can affect their payments in the experiment, but without knowing yet what will

[11]The information we elicit are mainly subjective statements and can, by the nature of this kind of information, hardly be verified. Although some authors argue that the use of information which cannot be verified might be problematic in contexts related to pricing privacy (Benndorf and Normann 2018; Schudy and Utikal 2017), alternatives like pictures or names used in previous studies (Benndorf and Normann 2018; Bohnet and Frey 1999; Charness and Gneezy 2008; Eckel and Petrie 2011; Gaudeul and Giannetti 2017; Hermstrüwer and Dickert 2017; Holm and Samahita 2018) create problems of identifiability instead. Using information which cannot be verified but contain no inherent right or wrong can overcome this issue (Frik and Gaudeul 2016), and is adapted in this work. We are interested in endogenous information revelation as a reaction to different treatment manipulations, and there is no reason to assume that answering the questionnaire initially varies between our treatments.

Table 1: Questionnaire

Question 1	Do you make decision mainly in a way that you benefit yourself?
Question 2	Do you consider inequality in society, which is based on different performances, as something negative?
Question 3	Are there reasons which justify to read emails or messages of friends?
Question 4	Would you accept a well-paid job if you know it hurts others?
Question 5	Do you only participate in laboratory experiments because of money?
Question 6	Is it in some situations acceptable to lie?
Question 7	Should people who voluntarily donate an organ receive payment for it?
Question 8	Is winning important to you?
Question 9	Is it okay to read one's text messages on the cellphone while driving?
Question 10	Does it affect you a lot if you fail an exam or would fail one in the future?
Question 11	Is it okay to drive a car after drinking one glass of beer (0.5 liters) or one glass of wine (0.2 liters)?
Question 12	Is it important to you what others think about you?

Notes: Scale: 1 = not at all, 7 = definitely. Order randomized.

follow in the second part.[12] We emphasize voluntariness of participation and the right to leave the experiment at any time.

The questions are designed such that there is no general right or wrong. Consequently, a particular answer may not signal a good or bad type, but rather requires a subjective assessment which leaves room for interpretation. We use this kind of questions for four reasons. Firstly, it creates variation in answers which might affect revelation and selection behavior. Secondly, having no clear right or wrong renders lying unreasonable. Thirdly, in everyday life, one often has to decide which information to disclose to others without knowing how that information will be perceived and interpreted. Fourthly, such questions preserve anonymity. We randomize the order of questions to avoid any order effects.[13]

[12]Eliciting information in the first part for the second part, in which they might be payoff-relevant, without prior knowledge of this connection might be considered as problematic since we only inform participants gradually about the course of the experiment. However, such an approach becomes necessary in experimental economics if more elaborate research questions require more flexible designs. See, for example, Brandts et al. (2006) and Khalmetski et al. (2015) for other research which requires non-standard techniques. Since the purpose of our experiment is to investigate how economic and social pressure affect the willingness to disclose potentially sensitive information, telling participants in advance what will follow would distort their initial reports. In fact, asking participants for their acceptance of subsequently using their answers does not indicate any resentment. On a 1-7 point scale with 7 being the full approval of subsequent information usage, the lowest treatment average is 5.47.

[13]In particular, we display the questions on two separate screens with six questions each, and randomize the screens' order as well as the position of questions within screens to avoid order effects. Acquisti et al. (2012) find order effects in the willingness to answer intrusive questions. We use ten different random orderings of questions, and control for these orderings in the regression analyses.

Distribution game

After answering the questionnaire, participants receive the second part of the instructions explaining the experimental game and the preceding possibility to reveal information. The impunity game (Bolton and Zwick 1995) is played one-shot in randomly assigned groups of three players. One player, the proposer, distributes a pie of 17 Euro between herself and the other two group members. The other two players are responders who can only accept or reject their own share. They only learn their own proposed shares and decide independently of each other. Unlike in the ultimatum game, a rejection in the impunity game does not imply that all players earn zero. Instead, only the rejecting player receives zero while the proposer's payoff remains unaffected, as does that of the other responder. However, the proposer is informed about the responder's rejection as a form of *voice*.[14]

We utilize the strategy method when eliciting proposer and responder choices, i.e., participants make decisions for all situations they could face. In the role of the responder, we elicit acceptance thresholds which are then implemented conditionally on the first stage offer. A special feature of our game is that not all three group members can become the proposer. In particular, at the beginning of the experiment we randomly match three players into a group and assign them to one of the three roles A, B, and C[15], which remain constant during the whole interaction. Participants in role A and B compete for becoming the proposer of the impunity game. We refer to this role as the *allocator* from now on. The participant in role C cannot become *allocator* and always takes the role of a responder, but selects the *allocator*. Before doing so, she can access the information revealed by players A and B. The next section explains this endogenous disclosure procedure.

Endogenous information disclosure

After reading the instructions of the second part of the experiment, participants are aware of the allocation task and the opportunity to reveal information in this setting. Participants in role A and B can decide which answers from the questionnaire they want to reveal to player C. For each information revealed, subjects have to pay a small fee of 10 Cents which is subtracted from their lump-sum payoff of 3 Euro from the first part. Keeping information secret is possible at no

[14] In order to render voice meaningful, the proposer has to offer at least 1 Euro to every participant including herself. As a consequence, a rejection inevitably causes a loss for the rejecting responder. We refrain from payoff-relevant punishment to avoid that varying beliefs regarding responder behavior between the different treatments drive proposers' choices. A simple dictator game would also not fit our purpose since it may risk to neglect possible reactions to *allocator* proposals by players in the different roles of the experiment.

[15] In order to avoid ordinal ordering inherent in the letters A, B and C, we use the colors red, blue and green during the experiment.

cost. The small fee mimics transaction costs of personal information disclosure.[16] For example, extending one's online profile requires a small amount of time and effort, which increases the more features you fill in. Methodologically, it limits experimenter demand concerns of asking for information provision in such a setting. Finding information revelation under such conditions would therefore strengthen our results. The information disclosure decision is our main variable of interest in this paper. However, we refrain from stating explicit hypotheses regarding which particular answers are revealed conditional on which score, influence *allocator* selection, or affect game playing, respectively. Instead, we focus on the total number of disclosures independent of content, stating corresponding hypotheses in the end of this section.

Additional measures

Due to our interest in potential side effects of information disclosure, we elicit participants' perceived pressure to disclosure information right after they made their final disclosure choice. Particularly, we ask them "Did you feel compelled to reveal more information than you initially wanted to?", measured on a 7-point scale.[17] Moreover, considerable heterogeneity in disclosure behavior may exist in such a setting and may impact game behavior. This heterogeneity is likely to stem from differences in privacy concerns, which we measure post-experimentally based on Westin's privacy index as in Harris Interactive (2001), and based on social media activity measures, the latter taken from Frik and Gaudeul (2016). Since the decision to disclose might also hinge on the perceived relevance of the answer to predict behavior in the subsequent allocation task, we elicit this factor on a 7-point scale in the post-experimental questionnaire for each question. Moreover, since our experiment involves peer comparison, we use a 7-item version of the INCOM social comparison index (Schneider and Schupp 2011) in order to control for heterogeneity in the habit of comparing oneself with others. On top of that, we elicit beliefs regarding the competitor's answer score and disclosure decisions in an incentive-compatible way. In particular, subjects receive a bonus of 3.50€ and 0.50€, respectively, at the end of the experiment if they correctly guessed the other candidate's answer and disclosure decision.[18]

[16] Revelation costs are also used in Benndorf et al. (2015).

[17] While such survey measures rely on self-reported perceptions different from behavioral decision data, psychologists suggest that self-reports are the best way to measure subjective emotions (Robinson and Clore 2002). This approach has also been adopted by economists. See, for example, Alesina et al. (2004), Blanchflower and Oswald (2004), Brandts et al. (2009), Charness and Grosskopf (2001), and Reyniers and Bhalla (2013).

[18] Given the different chances of a correct answer guess on a 7-point scale and a correct disclosure guess on a 2-point scale, i.e., disclose or non-disclose, we determine bonuses to be equal in expectation, setting them to 3.50 Euro for a correct answer guess and 1 Euro for a correct disclosure guess. One guess is randomly chosen and evaluated for payoff at the end.

Treatments

Table 2: Treatments: Two-by-two factorial design

		Allocator choice	
		random	strategic (by C)
Peer comparison	No	RA	SA
	Yes	RAC	SAC

The experimental design consists of four treatments based on a two-by-two factorial design, which vary in how information is revealed and how the *allocator* is selected. The first dimension distinguishes how the *allocator* is determined and is adapted from Brandts et al. (2006). In *random* treatments, one of the subjects in role A or B is randomly chosen with equal probability to become *allocator*. In *strategic* treatments, C decides whether A or B becomes *allocator*. Obviously, the two conditions differ in their incentives to provide information to C. In *random*, there should be no reason to disclose any information beyond one's genuine preference for information sharing. In contrast, information sharing can serve a strategic purpose in *strategic* because it may raise one's chance to become the payoff-determining *allocator*, creating a situation of proposer competition (Roth et al. 1991).[19] Consequently, varying the selection procedure allows to distinguish non-strategic information disclosure, i.e., one's baseline sharing preference, from strategic information sharing which is triggered by the monetary incentive.

The second dimension of our two-by-two factorial design varies whether there is a social comparison stage or not before information is reveal to C. This allows to investigate how peer pressure affects the willingness to disclose information. In the comparison stage, participants learn which answers the other player competing for *allocator* power disclosed, but not the exact score of the answers. Players A and B can adjust their revelation choice, or simply reconfirm their previous one. The previous choice is preselected as the default on screen so that for maintaining the previous choice participants just have to click on "proceed".[20] If a subject wants to adjust her previous choice, she can do so by changing the preselected disclosure decisions from "no" to "yes" or vice versa. As in the initial disclosure stage, the change in revelation can be made for each question separately and costs 10 Cents per disclosure.[21]

[19] If there is an intrinsic value of decision rights as in Bartling et al. (2014), this effect may also be captured in the *strategic* term.

[20] In order to ensure comparability between treatments, participants in the treatments without comparison also see another screen but with only their own choices displayed. Here, they just have to click on "proceed" to continue. In principle, they can also adjust their choices, but there should be no straightforward reason to do so except that the belief elicitation tasks in between resulted in some deeper thoughts about how much to disclose.

[21] Note that the 10 Cents transaction costs are not reimbursed if a subject decides to hide an answer she disclosed before.

We denote the four treatments resulting from our two-by-two design by *random* (RA), *random-comparison* (RAC), *strategic* (SA), and *strategic-comparison* (SAC). In what follows, we discuss how the different levels of strategic and social impact inherent in these treatments affect information disclosure and game behavior. We refer to the initial disclosure choice before the peer comparison stage as "ex ante" disclosure, and to the subsequent one as "ex post" disclosure, respectively.

Data were collected in the Cologne Laboratory for Economic Research in November and December 2017 using zTree (Fischbacher 2007) for programming and ORSEE (Greiner 2015) for participant recruitment. The experiment lasted approximately 50 minutes and participants earned on average 13€ including a show-up fee of 4€. In total, 294 people participated in 10 experimental sessions. We oversampled the strategic treatments due to our interest in active *allocator* selection by C-participants.

Hypotheses

Allocator selection by C in the *strategic* treatments SA and SAC likely incentivizes individuals to disclose ex ante more information than in the *random* treatments RA and RAC. While *random* allocation elicits one's intrinsic preference for information revelation without additional incentives, the prospect of gaining *allocator* power might seem worth to sacrifice some privacy. This corresponds to incurring a cost, for example, in form of privacy or transaction costs, worth to be paid in exchange for the strategically beneficial position.

Hypothesis 1 (Strategic disclosure): The amount of information revealed ex ante is higher in *strategic* treatments than in *random* treatments.

Subsequent peer comparison likely initiates adaptation to the disclosure behavior of the competitor. Changes in RAC can be fully attributed to a classical peer effect, while changes in SAC are further triggered by competition in gaining the attention of player C via revealing more. Therefore, we expect more disclosure changes in SAC, and in particular more upward changes due to its *strategic* aspect.[22]

Hypothesis 2 (Social comparison): Peer comparison leads to more ex post disclosure changes under *strategic* incentives than without.

[22]The fact that comparison in SAC inherently provides information regarding how much disclosure may be necessary to capture distributional benefits may even emphasize this reaction. Although downward corrections are also possible, e.g., after initially overestimating the other's disclosure, we do not expect that peer comparison initiates much hiding of information in SAC.

Reactions to peer comparison under *strategic* incentives are likely driven by one's own ex ante disclosure choices relative to that of the competitor, and thereby may be heterogeneous. In particular, we expect that those who learn that they revealed fewer answers than their competitor under *strategic* benefits adapt their initial disclosure choice and disclose more.[23]

Hypothesis 3 (Heterogeneous effects): Those ex ante disclosing less in SAC react to peer comparison and adapt their disclosure decision.

So far, we have focused on the effect of incentives and social comparison on information disclosure. If subjects change their initial level of disclosure in SAC after peer *comparison*, this can be driven both by an updated belief about the right amount of information to disclose or by social pressure.[24] In order to investigate the aspect of social pressure, we asked participants "Did you feel compelled to reveal more information than you initially wanted to?" right after they made their ex post revelation decision. Perceived pressure should play a role in *strategic* treatments due to their competitive nature, and should be especially strong in SAC due to peer *comparison*. Regarding heterogeneity, we expect the increase in pressure in SAC to be driven by those who learn that they lack behind in revelation competition.

Hypothesis 4 (Pressure to disclose): Perceived pressure to disclose information increases a) in *strategic* compared to *random* treatments, b) even more so in combination with social comparison in SAC, and in this case c) driven by those learning to be the one disclosing less.

Besides this potential cost, personal information disclosure might also create benefits. Articles analyzing personal information disclosure on microfinance platforms provide evidence that adding not directly related "cheap talk" information about oneself can beneficially influence credit market outcomes (Böhme and Pötzsch 2010; Michels 2012; Pope and Sydnor 2011). If information overbidding was actually a way to compete for attention on such platforms, the extent of personal information sharing should also affect *allocator* selection in the *strategic* treatments of our experiment. Particularly, those individuals who disclose more information should be more likely selected as *allocators*.

Hypothesis 5 (Beneficial information overbidding): People who reveal more information in *strategic* treatments are more likely selected as *allocators*.

[23] Reyniers and Bhalla (2013) find such an effect in the context of charitable donations, i.e., under peer comparison those who attempt to donate less revise their choice upwards. Such a reaction is even more likely to occur in our setting since the incentive to adapt is not only driven by soft factors like image concerns but also by expected monetary benefits in SAC.

[24] See the last section in Results for a discussion.

We refrain from stating explicit hypotheses regarding the influence of sharing on caring, i.e., from information disclosure on generosity in impunity play since the evidence for such a relationship is mixed (Duarte et al. 2012; Iyer et al. 2016; Pope and Sydnor 2011), and it is not the focus of our project.

3 Results

3.1 Descriptive statistics

Table 3: Descriptive statistics: Sample characteristics

	Total	RA	RAC	SA	SAC	p-value
Female	**55.8%**	48.3%	65.0%	51.7%	58.6%	0.232
Age	**24.3**	25.9	23.5	24.3	23.8	0.045
Westin Fundamentalist	**51.0%**	43.3%	53.3%	54.0%	51.7%	0.606
Westin Pragmatist	**47.6%**	56.7%	45.0%	43.7%	47.1%	0.456
Westin Unconcerned	**1.4%**	0.0%	1.7%	2.3%	1.1%	0.911
Profile public	**15.0%**	15.0%	16.7%	16.1%	12.6%	0.905
Profile identifiable	**69.4%**	61.7%	73.3%	67.8%	73.6%	0.415
Ability compare	**4.4**	4.3	4.5	4.5	4.3	0.394
Opinion compare	**4.8**	4.6	5.0	5.0	4.7	0.209
N	**294**	60	60	87	87	

Notes: p-values in last column show accuracy of randomization into treatments based on individual characteristics, and stem from Kruskal-Wallis-tests for age, ability compare, and opinion compare, and from Fisher's exact tests otherwise.

Table 3 shows descriptive statistics of our sample. Participants are 55.8% female and on average 24.3 years old. With reference to the version of Westin's privacy index we use[25], our sample is roughly split in two halves, privacy "pragmatists" and "fundamentalists". Hardly anyone is classified as "unconcerned".[26] Except for a small difference in age, statistical tests do not reveal any differences between treatment groups in terms of demographics, privacy preferences, and social media behavior. Descriptive statistics regarding outcome variables for

[25]In line with the 7-point scale we use for all other ordinal ratings, we also use a 7-point instead of a 4-point scale for the three questions determining the Westin privacy index. These questions stem from the 2001 version of Westin's privacy classification as published in Harris Interactive (2001). See Kumaraguru and Cranor (2005) for a review of Westin's privacy indexes. We classify scores from 1 to 3 as "disagree" and 5 to 7 as "agree", and follow Westin's definition of the three privacy types *Unconcerned, Fundamentalist*, and *Pragmatists* based on those definitions.

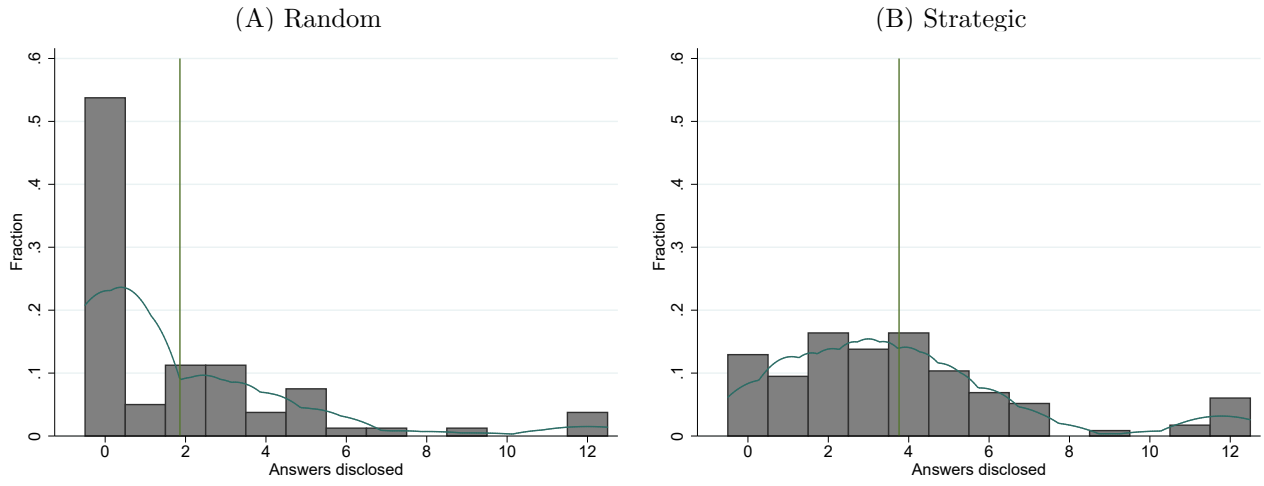
[26]Therefore, we pool pragmatists and unconcerned subjects in the subsequent analyses, and only use a dummy for fundamentalists.

the restricted sample of *allocator* candidates (roles A and B) are summarized in Table 7 in the Appendix.

3.2 Answers ex ante disclosed

First, we analyze the aggregated amount of information disclosed ex ante under the different selection conditions, i.e., before social comparison. Hypothesis 1 predicts more disclosure in *strategic* treatments. Indeed, participants react to the *strategic* setting with more information revelation. Compared to *random*, information revelation doubles from 1.9 to 3.8 answers on average in the *strategic* context. Figure 2 depicts the distribution of the number of answers disclosed. In *random*, more than half of the participants disclose nothing, while only 12.9% do so in *strategic*. Instead, the majority of 46.6% of observations falls in the range between two and four revelations. A Wilcoxon ranksum-test confirms that the two distributions are statistically different form each other ($p < 0.001$). In the Appendix, we provide histograms of which particular answers are disclosed how often in Figure 8, and Table 8 reports probit regression results on factors affecting disclosure on question level. While the answers, that participants give, themselves are of course meaningful for disclosure, the focus of our analysis is not which particular information participants are willing to disclose, but how incentives and social comparison affect information disclosure in general. Therefore, the analysis on answer level is left to the interested reader in the Appendix.

Figure 2: Histograms of answers ex ante disclosed



Notes: Vertical lines represent means. Curved lines represent Kernel density.

Table 4: Effect of strategic incentives on ex ante disclosure

	Answers ex ante disclosed			
	(1)	(2)	(3)	(4)
strategic	1.913***	1.927***	1.974***	2.106***
	(0.432)	(0.663)	(0.652)	(0.670)
comparison		0.475	0.669	0.700
		(0.633)	(0.672)	(0.669)
strategic # comparison		-0.027	-0.203	-0.244
		(0.864)	(0.843)	(0.858)
constant	1.862***	1.625***	2.071	0.971
	(0.317)	(0.501)	(1.457)	(1.740)
basic controls	No	No	Yes	Yes
preference controls	No	No	No	Yes
N	196	196	196	196
R2	0.091	0.096	0.171	0.185

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table reports OLS regression coefficients with standard errors in parentheses clustered on group level. Basic controls include gender, age, and dummies for the ten different randomizations of questions used. Preference controls include dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM.

As a general empirical strategy in this paper, we estimate the effect of our treatment dimension, *strategic* incentives, social *comparison*, and their interaction, on different outcomes y_i, i.e.,

$$y_i = \beta_0 + \beta_1 strategic_i + \beta_2 comparison_i + \beta_3 strategic_i * comparison_i + \beta^{'} X_i + \epsilon_i \qquad (1)$$

in which X_i is a vector of individual characteristics of individual i, and ϵ_i denotes an error term clustered on group level. We are interested in β_1, β_1, and β_3 capturing the effect of *strategic* considerations, social *comparison*, and the differential effect of social *comparison* in *strategic* settings, respectively.

Regarding the number of answers ex ante disclosed by *allocator* candidates, Table 4 reports the corresponding OLS regression results. The effect of the *strategic* incentive to reveal more information is statistically significant at the 1% level as already suggested by the descriptive analysis. Participants in *strategic* disclose on average 1.9 answers more. At this stage, peer *comparison* has not yet taken place so insignificant effects of the *comparison* coefficient and its interaction with *strategic* in column (2) confirm that there are no initial differences between groups with and without subsequent feedback on their competitor's choice.

As controls, age and gender as well as nine dummy variables for the ten random orders of questions are added in column (3). In general, women disclose significantly less than men, quantitatively about one answer less on average. Moreover, to capture attitudes relevant for our setting, column (4) adds control variables for privacy concerns via a dummy for Westin's privacy fundamentalists, the two dimensions ability and opinion compare of the INCOM social comparison index, and two dummy variables capturing identifiability of the participant's social media profile and strangers' access to it. All specifications confirm that strategic incentives enhance information disclosure and thus Hypothesis 1. In the Appendix, we show that controlling for the many zero disclosures, which occur particularly in *random* treatments, by a tobit model even strengthens our results. Moreover, results are robust to a 90% winsorization on treatment level.

Result 1: More information is revealed in *strategic* than in *random* treatments.

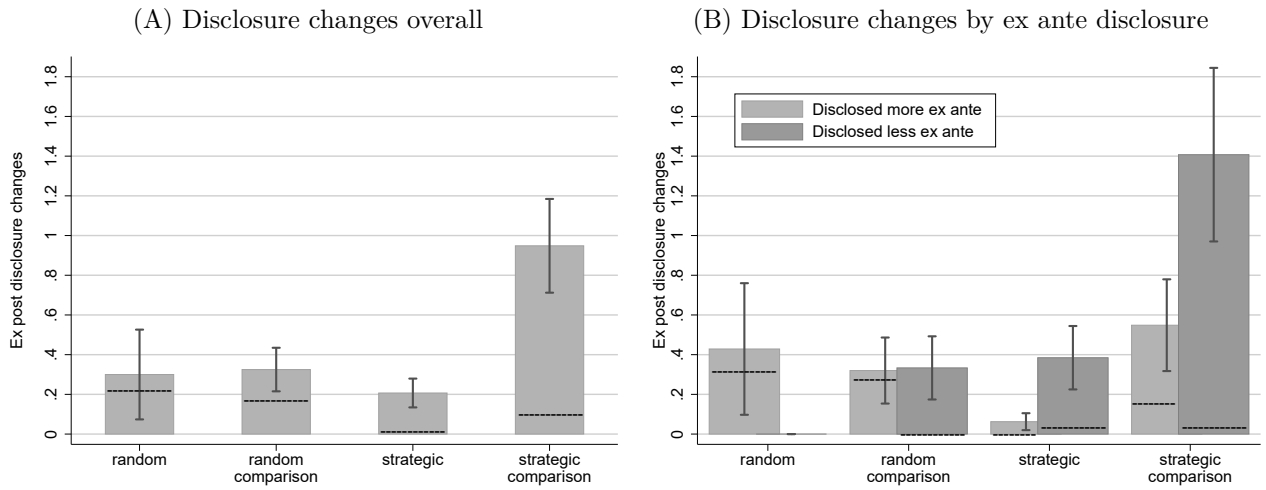
3.3 Ex post disclosure changes

We now investigate how social comparison affects disclosure behavior. After the initial disclosure stage and a belief elicitation task, subjects can revise their disclosure choice. Without prior announcement, participants in *comparison* treatments learn the disclosure choice of the other *allocator* candidate. Particularly, they learn which answers their competitor disclosed, but not the content of answers, and can revise their choices. In order to maintain comparability between treatments with and without *comparison*, subjects can also revise their disclosure choice when not receiving feedback on their competitor's behavior. As the dependent variable, we focus on the absolute amount of disclosure changes independent of their direction. A disclosure change is measured as a different disclosure choice ex post than ex ante, i.e., $x^{ex\,ante} \neq x^{ex\,post}$. We sum up these single disclosure changes for all twelve answers to derive our outcome variable of interest, $\sum_{n=1}^{12} |x_n^{ex\,ante} - x_n^{ex\,post}|_i$, which can range from 0 to 12.[27] Hypothesis 2 predicts that social *comparison* has a stronger effect under *strategic* incentives in SAC than in RAC without.

Panel A of Figure 3 depicts ex post disclosure changes by treatment as a coefficient plot based on OLS regression. The horizontal line separating the bars in an upper and a lower part distinguishes the direction of the changes. The fraction below the line are disclosure reductions, while extensions are depicted above. We observe a small number of ex post disclosure changes in treatments without peer comparisons, probably as a reaction to intermediate belief elicitation.

[27] One would miss important changes when only measuring the amount of information disclosed ex ante and ex post: It would overlook inverse changes like "subsequently disclose answer x, but hide answer y" which would be reported as zero but might indicate adaptation to the other's disclosure.

Figure 3: Coefficient plots of ex post disclosure changes by treatment



Notes: Vertical lines represent standard errors clustered on group level. Horizontal lines divide ex post disclosure changes into upward and downward changes depicted above and below the line, respectively.

Compared to the baseline level of changes in RA, there are not more ex post changes in RAC after social *comparison*. However, significant changes occur when combining social *comparison* with *strategic* incentives to disclose.

Table 5 reports the corresponding regression results, as specified in Equation (1), with ex post disclosure changes as the dependent variable. The *strategic-comparison* interaction effect in column (1) is statistically significantly positive at the 5% level. This means that participants, who face strategic benefits, react to peer comparison. The interaction effect of *strategic-comparison* equals at least 0.72 disclosure changes, and remains significant independent of the control variables included in columns (2)-(4). By adding the three coefficients of interest, a stable effect size of 0.65 disclosure changes emerges for treatment SAC in addition to the 0.3 baseline level of changes in RA. In total, this equals nearly one absolute disclosure change in SAC on average. In contrast, the *comparison* variable is weak and insignificant, and implies that social *comparison* per se does not overcome one's intrinsic preference for privacy, including potential reluctance to disclose personal details.

Controlling for privacy- and social comparison-related factors in column (3) increases the size of the *strategic-comparison* interaction effect. Interestingly, participants who score higher on the ability dimension of the INCOM social comparison index, i.e., those who often compare their own ability with others, make significantly more disclosure changes ($p = 0.028$). In column (4), we additionally control for one's own ex ante disclosure, i.e., the absolute disclosure level, and for the disclosure difference to the competitor, i.e., the relative disclosure. Both factors do not significantly affect adaptation behavior, and leave our results unchanged. The same holds

Table 5: Ex post disclosure changes by treatment

	Ex post disclosure changes					
	(1)	(2)	(3)	(4)	(5) high	(6) low
strategic	-0.093	-0.105	-0.132	-0.284	-0.351	0.143
	(0.236)	(0.219)	(0.220)	(0.278)	(0.294)	(0.361)
comparison	0.025	0.019	-0.045	-0.086	-0.089	-0.004
	(0.250)	(0.230)	(0.237)	(0.247)	(0.340)	(0.414)
strategic # comparison	0.716**	0.718**	0.824**	0.824**	0.610	1.258*
	(0.350)	(0.344)	(0.362)	(0.358)	(0.431)	(0.728)
own ex ante disclosure				0.082		
				(0.062)		
own - other's ex ante disclosure				-0.061		
				(0.047)		
constant	0.300	0.676	-0.106	-0.332	-0.348	-0.526
	(0.225)	(0.530)	(0.477)	(0.475)	(0.573)	(0.938)
basic controls	No	Yes	Yes	Yes	Yes	Yes
preference controls	No	No	Yes	Yes	Yes	Yes
N	196	196	196	196	116	80
R2	0.058	0.081	0.131	0.150	0.205	0.262

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table reports OLS regression coefficients with standard errors in parentheses clustered on group level. Basic controls include gender, age, and dummies for the ten different randomizations of questions used. Preference controls include dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM. The lower candidate is the one who ex ante disclosed strictly fewer answers than her competitor.

when performing a 90% winsorization on treatment level as a robustness check, which can be found in Table 10 of the Appendix. Consequently, the interplay between incentives and social comparison seems crucial for adapting one's personal information disclosure. This confirms hypothesis 2.

Result 2: Peer *comparison* induces significantly more ex post disclosure changes under *strategic* incentives than without.

In order to better understand ex post disclosure changes, we also investigate the direction of disclosure changes, which can be inferred from Figure 3 by looking at the horizontal division lines of the bars. Moreover, we analyse whether a change mimics the disclosure decision of the competitor. The *strategic-comparison* interaction effect is significant for disclosure extensions and for adaptations to the disclosure choice of the other. In particular, in SAC 89.1% of all changes are upward changes and 85.5% are adaptations. For a detailed analysis and

corresponding regression results, see Table 11 in the Appendix. Two important aspects prevail: First, we follow peers in what we disclose, what can be regarded as an intensive margin, and fits to conformity seeking (Asch 1951; Bernheim 1994). One wants to avoid deviating from the disclosure choice of the other, and therefore adapts to her revelation behavior. Second, the primary direction of change with both peer *comparison* and *strategic* incentives is upwards, what resembles an extensive margin. Therefore, the interplay of benefits and observing how much our peers reveal might explain why we see more and more personal information sharing nowadays.

Regrading heterogeneity in ex post disclosure changes, Hypothesis 3 predicts that the observed changes in SAC are driven by those learning to lack behind. This turns out to be true when splitting our sample into two subgroups based on the criterion whether an individual is the one who ex ante discloses more or strictly less information than the other.[28]. Looking at the corresponding coefficient plots for these subsamples depicted in Panel B of Figure 3 shows that the bar for the lower candidate in the SAC treatment is by far the highest. In this condition, subjects make on average 1.4 ex post disclosure changes, of which 97.4% are extensions and 84.2% adaptation mimicking the disclosure behavior of the competitor. In columns (5) and (6) of Table 5, we run the previous disclosure change regressions separately for the two subgroups. The *strategic-comparison* interaction effect turns out to be significant for lower candidates. Those who realize that they disclose more information ex ante do not see the need to react to peer comparison, while those learning that they lack behind revise their ex ante disclosure choice. This support the idea that with peer comparison and competition in information disclosure, those generally unwilling to disclose adapt their behavior to their environment. However, by splitting the sample, one loses statistical power in regression analysis resulting in significance only at the 10% level to substantiate Hypothesis 3.

Result 3: Subjects who disclose less ex ante under *strategic* incentives and peer *comparison* alter their disclosure choice.

One interesting, additional observation when looking at Panel B in Figure 3 is that there are some changes going on under peer *comparison* even without *strategic* incentives to disclose. These changes are bi-directional. Some subjects, who learn that they disclosed more in RAC, reveal less information, what can be inferred from most ex post disclosure changes of higher

[28]Since we call a group member the "lower" candidate only if she ex ante reveals strictly less information, and therefore assign a value of zero to the dummy if both candidates in a pair ex ante disclose the same amount of information, we have more "high" than "low" candidates.

candidates lying below the horizontal line in Panel B in Figure 3. In contrast, lower candidates expand their disclosure so both groups converge to each other. On the contrary, in SAC, even if one is already ahead, one more likely reacts by disclosing more rather than less. This supports the idea that incentives for personal information sharing push the extensive margin of disclosure up.

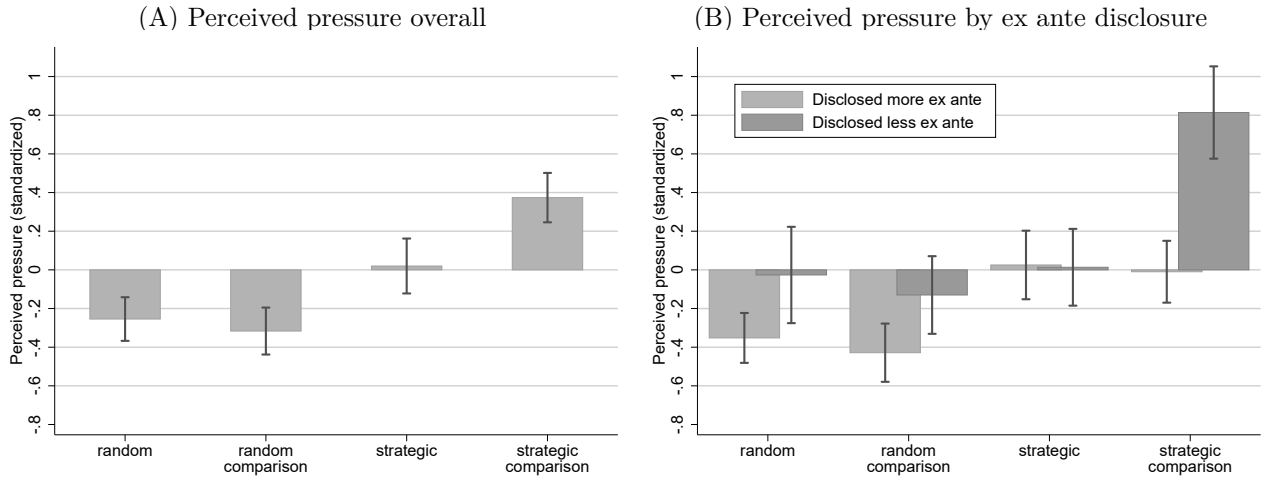
3.4 Hidden costs of information disclosure

Are there potential side effects of extensive personal information disclosure, in particular pressure to disclose? In SAC, particularly those who ex ante reveal less widen their disclosure due to peer comparison. Therefore, we explore whether peer comparison results from peer pressure by analyzing answers to the question "Did you feel compelled to reveal more information than you initially wanted to?", elicited right after participants' ex post disclosure choice. Panel A in Figure 4 shows the coefficient plot of the level of perceived pressure measured in standard deviations for the four treatments. In *random* treatments, the level of pressure is similarly low with and without *comparison*, and lies between -0.32 and -0.25 standard deviations. The corresponding *comparison* regression coefficient, distinguishing the pure effect of social comparison, is insignificant and small in magnitude in all regression specifications displayed in columns (1)-(3) of Table 6. Thus, social comparison per se does not seem to trigger pressure to share information.

However, the combination of peer comparison and incentives seems to render information sharing compelling. We find that the interaction of *strategic* incentives and peer *comparison* increases perceived pressure by 0.42 to 0.48 standard deviations, depending on the specification. Although it is statistically significant only if controlling for other factors, its magnitude is large compared to the coefficient of *strategic* incentives only. If we winsorize the data by 90% on treatment level, shown in Table 15 of the Appendix, this finding is robust and becomes significant at the 10% level already without any controls. Observing significance of the interaction effect but not of the *strategic* coefficient supports Hypothesis 4b but not Hypothesis 4a. The level of pressure in the SA treatment equals the average level in our sample, and is not significantly higher than in the RA treatment. ($p = 0.131$).

We further look at heterogeneity in perceived pressure when being the one ex ante disclosing less. Hypothesis 5 postulates that perceived pressure is comparably high for those individuals. Panel B in Figure 4 shows the corresponding coefficient plot split by who in the pair discloses less ex ante. In *random* treatments, those who disclose more and do so without incentives feel

Figure 4: Coefficient plot of perceived pressure to disclose by treatment



Notes: Vertical lines represent standard errors clustered on group level.

the least compelled. Their information disclosure decision seems to be intrinsically motivated and free from pressure. Similarly to the effect for ex post disclosure changes in Panel B of Figure 3, participants realizing in SAC to have disclosed less ex ante feel most pressured. The effect size is with 0.81 standard deviations large in magnitude compared to the standardized average of zero. Running separate regressions for candidates with ex ante higher or lower disclosure in a pair, the *strategic-comparison* interaction effect in the low candidate subsample in column (5) of Table 6 is significant at the 5% level. Therefore, we infer from our results that learning to lack behind in personal information revelation under competition is perceived as more compelling. This supports Hypothesis 4c. Note that the *strategic* coefficient turns significant in the restricted sample of candidates being ahead in column (4).

Result 4: Perceived pressure to disclose information increases under peer *comparison* in combination with *strategic* incentives, especially when learning to have disclosed less ex ante, but not under *strategic* incentives in general.

3.5 Benefits of information disclosure

Allocator **selection based on information disclosure**

In this section, we analyze how personal information disclosure affects the probability to become *allocator*. *Allocator* candidates seem to assume that C takes personal information into account for *allocator* selection since they disclose more information in *strategic* treatments.

Table 6: Perceived pressure to disclose information by treatment

	Perceived pressure				
	(1)	(2)	(3)	(4) high	(5) low
strategic	0.274	0.250	0.254	0.379**	-0.142
	(0.180)	(0.183)	(0.175)	(0.191)	(0.345)
comparison	-0.062	-0.078	-0.089	-0.066	-0.201
	(0.165)	(0.171)	(0.169)	(0.205)	(0.342)
strategic # comparison	0.416	0.433*	0.475*	0.132	1.117**
	(0.252)	(0.252)	(0.246)	(0.292)	(0.468)
constant	-0.254**	0.105	-0.373	-0.696	0.355
	(0.112)	(0.330)	(0.472)	(0.714)	(0.797)
basic controls	No	Yes	Yes	Yes	Yes
preference controls	No	No	Yes	Yes	Yes
N	196	196	196	116	80
R2	0.076	0.081	0.113	0.202	0.202

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table reports OLS regression coefficients with standard errors in parentheses clustered on group level. Basic controls include gender and age. Preference controls include dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM.

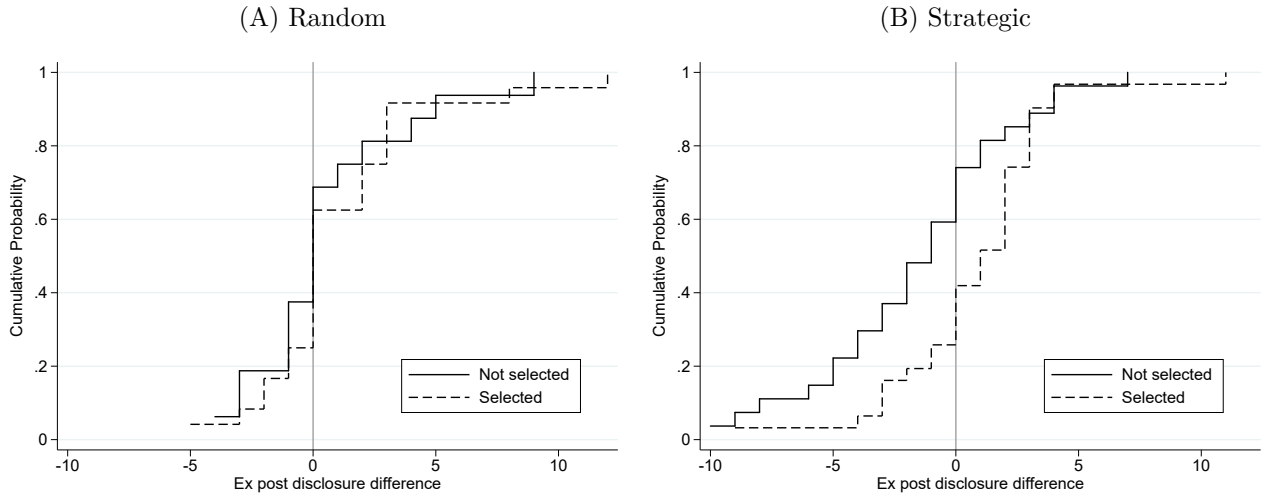
Indeed, participants in role C look at the information provided. On average, they investigate 10.5 out of 12 ex post disclosed answers, and in 79.6% of the cases all answers.[29]

Figure 5 illustrates that disclosing more information than the other candidate indeed increases the likelihood to become *allocator* in *strategic* treatments.[30] The curves present the cumulative probability distribution to be selected conditional on the ex post difference in answers disclosed relative to one's competitor. In Panel B, the line for non-selected candidates is shifted to the left meaning that their probability of *not* being selected is higher the more they lack behind. Over a large range of the abscissa, there is first order stochastic dominance between the two lines. While 25.5% of *allocators* stem from the group of participants disclosing less, suggesting that content also matters for selection, disclosing more seems highly decisive to become *allocator*. In fact, a two-sample Kolmogorov-Smirnov test rejects the hypothesis that the difference in ex post disclosures between selected and non-selected *allocators* is the same ($p = 0.004$). Of course, one should not and does not find such a difference in *random* treatments displayed in Panel A of Figure 5 ($p = 0.988$) where both lines overlap and resemble

[29]In a ranksum-test, the number of answers inspected by C is with 9.9 clicks insignificantly smaller in *random* than in *strategic* treatments with 10.8 clicks ($p = 0.274$).

[30]Since C is not informed about the comparison stage, we can ignore the *comparison* dimension and pool our observations when investigating C's behavior.

Figure 5: Probability to become *allocator* by difference in information disclosure



Notes: Cumulative probability functions of becoming *allocator*.

the density of a normal distribution function. This altogether suggests that the pure amount of personal information sharing can impact how much attention we receive from others. Even if the signaling value of information for allocation behavior is not straightforward in our experiment, competition via information overbidding seems to pay off. The corresponding probit regression analysis, left to the interested reader in Table 12 of the Appendix, confirms these findings and substantiates Hypothesis 5.

Result 5: Disclosing more information significantly increases the probability to be selected as *allocator*.

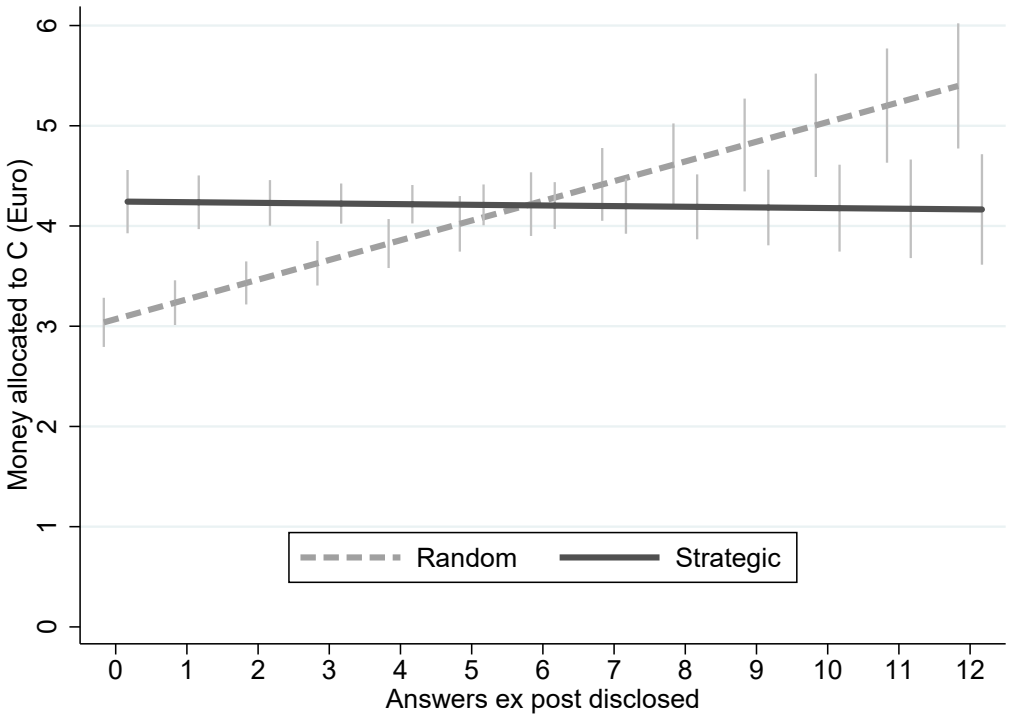
As a corollary to this finding, it is worth pointing out that participants, who ex ante disclose less, most often also disclose less ex post, and are therefore less likely to become *allocator* in SAC. In spite of the opportunity to catch up, they fail to become *allocator* in 74.1% of the cases, which is statistically different from a 50% chance in a two-sided binomial test ($p = 0.019$) and indistinguishable from the corresponding chance without *comparison* in SA in a ranksum-test ($p = 0.698$). Thus, for this group ex post disclosure changes do not pay off.

Allocation behavior

The last section showed that information revelation under competition for being selected seems to pay off for disclosure-willing individuals, but is it also beneficial for the other market side, i.e., players C selecting the *allocator*? In order to explore this, we look at allocation behavior conditional on personal information sharing. If there was a relationship between

information disclosure and allocation behavior, endogenous personal information sharing could indeed help to screen prosocial types in our setting; if not, extensive sharing and its peer dynamics might not be expedient. We measure prosocial behavior by the amount one gives to C. We summarize the main effects here while leaving the more detailed analysis including all regression specifications, discussed in Table 13 of the Appendix, to the interested reader.

Figure 6: Coefficient plot of money allocated to C by information disclosure and selection



Notes: Vertical lines represent standard errors clustered on group level.

First of all and in line with Brandts et al. (2006)[31], participants in *strategic* treatments give significantly more to C, and thereby reciprocate the favor of being selected. This represents the level effect in the coefficient plot in Figure 6. Second, there is no *direct* effect of disclosure behavior on prosocial behavior. Subjects disclosing more personal information in *strategic* treatments do not offer more to player C. This can be inferred from the slope in Figure 6. If anything, revelation competition in *strategic* treatments wipes out a positive relationship between intrinsic disclosure-willingness and generosity in *random* treatments.[32] In the corresponding OLS regression specification in Table 13, this means that the significant effects of the amount ex post disclosed and its interaction with the *strategic* coefficient cancel out. Third, there is no *indirect* effect of being chosen as the *allocator*, based on one's disclosure, on prosocial

[31]Brandts et al. (2006) call the effect that selected participants give more to the selecting party than randomly chosen participants "I-want-you" effect. We confirm its existence in a modified setting.

[32]However, we do not want to emphasize this result too much since the number ob observations with many disclosures in *random* treatments is limited.

behavior. Those who become *allocator* in *strategic* treatments do not offer significantly more to player C than non-selected candidates. Thus, participants in role C do not suffice in picking the "right", i.e., more generous, candidates, and do not benefit from selecting candidates based on endogenously disclosed personal information. Statistical support based on OLS regression results for all findings just discussed can be found in Table 13 of the Appendix. Note that social *comparison* has no effect on allocation outcomes neither in RAC nor in SAC.

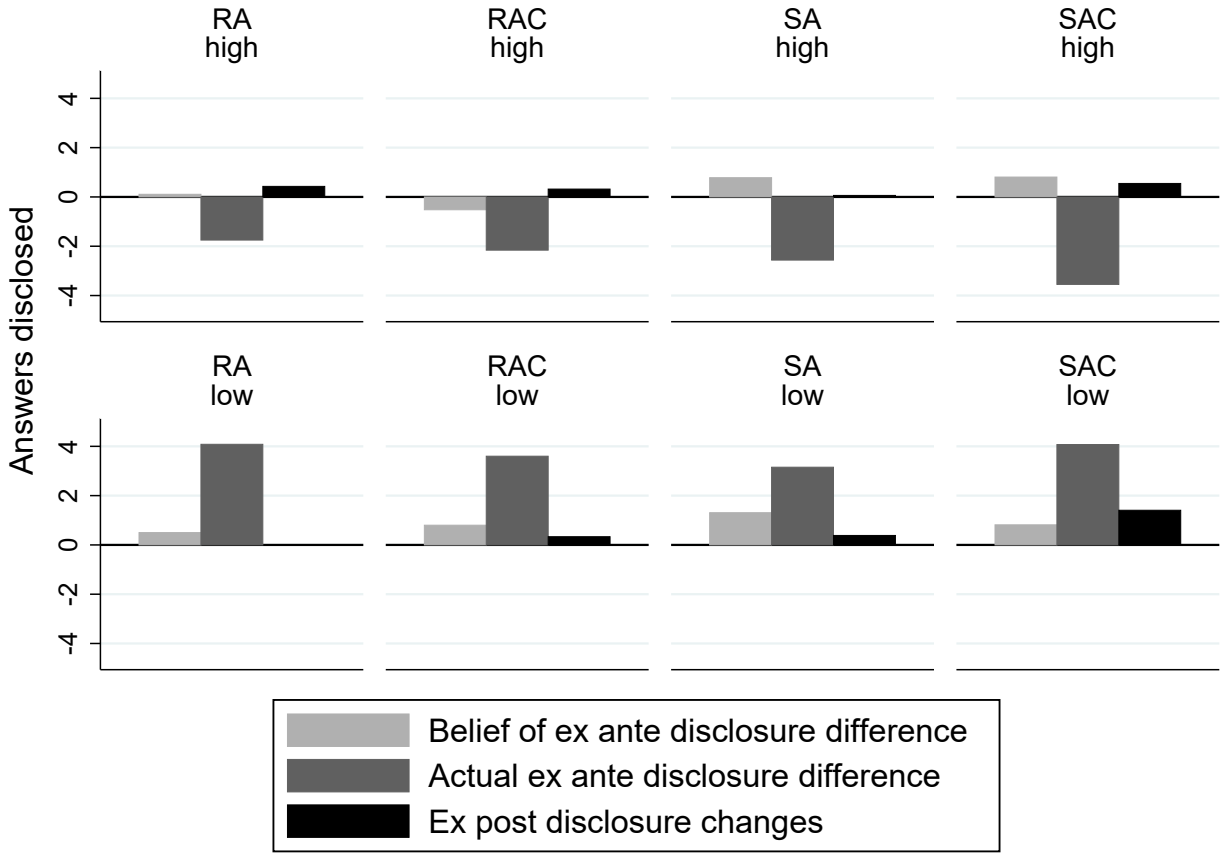
The Appendix also reports acceptance thresholds from the impunity game as a form of "choice and voice". While social *comparison* might decrease acceptance, we do not find robust treatment effects regarding acceptance thresholds. However, acceptance thresholds are significantly higher than predicted by game theory so subjects are willing to forgo some money in our experiment when being offered to little. Even though altruistic sanctioning in monetary terms is excluded, respondents often engage in non-monetary altruistic accusation via choosing positive acceptance threshold.

Discussion of results and behavioral patterns

Our analysis of disclosure behavior systematically disentangles that the combination of benefits for information sharing and observing peers' information sharing increases disclosure. In this section, we try to explore more deeply why this is the case in the SAC treatment. One explanation already discussed and supported by our results is peer pressure stemming from peer comparison. Another conflicting explanation is the effect of information provision per se. Providing important information about the other candidate's information sharing behavior in SAC could trigger changes in disclosure behavior due to less uncertainty about how much and which information one has to reveal to increase one's chances to become *allocator*. However, we argue in the following why a pure information provision argument cannot fully explain our results.

Subjects, likely updating their incorrect beliefs about how much more information disclosure is needed for becoming *allocator*, seem to account for 22.2% of those candidates in our SAC sample, who ex ante lack behind. In these cases, subjects fully catch up or even overbid their competitor in terms of ex post information disclosure after peer *comparison*. Such behavior may resemble imitation learning (Huck et al. 1999; Vega-Redondo 1997). In contrast, 29.6% adapt partly by one to three disclosure extensions, but still disclose less ex post. This group seems to trade off privacy concerns with reducing the distance to the competitor, likely due to conformity seeking (Asch 1951; Bernheim 1994). 44.4% of participants do not react at all when

Figure 7: Disclosure behavior and beliefs by treatment and ex ante disclosure



Notes: The lower candidate is the one who ex ante disclosed strictly fewer answers than her competitor.

learning about the disclosure choice of their peer. This type does not want to trade off privacy against potential benefits of disclosing more.

Since our detailed dataset contains beliefs about how much a subject expects her competitor to disclose, we can examine the information provision explanation in more detail. If the belief about the competitor's amount of disclosure was systematically too low, the additional information provided in *comparison* treatments should initiate more disclosures in order to outbid one's competitor. However, the beliefs of those disclosing less, depicted in Figure 7, are correct: They expect their competitor to disclose more information than themselves as the first bars of "SA low" and "SAC low" show.[33] If participants had expected to disclose less, they should have behaved according to their correct belief by already disclosing more ex ante. Recall here that participants are unaware that they will be able to revise their choice when making their ex ante disclosure decision, and should consequently act as if it is was the final choice. Therefore, in

[33]Remarkably, apart from those who actually disclose more than the other group member in *random* treatments, everybody believes to be revealing less. Thus, in *strategic* treatments, those who actually disclose more assume to disclose less as well.

our view it seems more likely that privacy costs hinder those who lag behind to catch up with those having lower privacy costs. Consequently, additional information under social comparison confirms that one lags behind rather than providing new insights.

Moreover, if wrong beliefs were decisive for disclosing less, one should see a strong reaction when learning how much more revelation is needed to outbid the competitor. Rather than observing this, there is only a minor increase in disclosures after the *comparison* stage in SAC (compare the third bar in category "SAC low" of Figure 7 to the second bar). Thus, the majority of disclosure changes seem more likely to occur due to social pressure and conformity seeking rather than to outbid the competitor. The candidate lacking behind reveals somewhat more, but more often than not refrains from trying to jump ahead in disclosure.

These features highlight peer pressure in information disclosure as the most likely driver of the personal information disclosure dynamics we observe. While we cannot completely rule out that the high perceived pressure in SAC stems from receiving information in a competitive setting per se and is not related to the privacy component of our data, significant pressure seems to exist under such conditions at least if personal information is at work. If anything, general applicability of our results to competitive settings with peer comparison would increase the relevance of our results even further.

4 Conclusion

In this paper, we investigate personal information sharing under competition for benefits. Particularly, we examine the interaction of strategic incentives and peer comparison to disclose personal information as a channel leading to more and more information sharing as observed in the field. Moreover, we provide indicative evidence on a potentially neglected side effect in such a context. Our setting fits best to modern markets, for example, social media platforms like Instagram and Youtube, Airbnb, LinkedIn, or microfinance or crowdfunding platforms like Kickstarter or Prosper.com, in which one market side strives for another's beneficial attention by providing personal information, which have to be subjectively assessed. It also applies to offline markets, for example, the housing market in which prospective tenants try to stand out from the crowd of applicants by bringing a well-designed folder with additional but often rather irrelevant documents, but is exacerbated by online markets.

In our lab experiment, participants can endogenously reveal potentially sensitive answers from a personality and opinion questionnaire in order to be selected to determine the allocation decision in an impunity game. We vary in how far information sharing can serve a strategic

purpose, and analyze how it is influenced by peer comparison. Results show that strategic incentives double disclosure, and that this effect is fostered by subsequent peer comparison. This dynamic response is primarily driven by those participants who learn from social comparison that they a priori revealed less than their competitor. It goes along with an increase in perceived pressure to have to disclose information. While endogenously providing more information in our experiment may or may not signal distribution behavior, we find that more disclosure-willing individuals are more frequently picked, but do not behave more generously.

Which implications can be drawn from our results? First, it is unlikely that all information sharing we observe online nowadays is based on a pure preference for revelation. Rather, modern markets of the 21st century trade personal information as a medium of exchange for benefits, and people respond to this incentive by revealing more. Second, peer pressure exist in personal information disclosure. Observing others who freely share personal information for benefits triggers intrinsically reluctant individuals to adapt their behavior. This adaptation process, driven by the interplay of benefits and observing peers' sharing, sheds light on the channel underlying the present, seemingly unstoppable trend of more and more voluntary information disclosure. Third, the high level of pressure, which participants in our experiment report after being influenced by a more disclosure-willing peer, provides indicative evidence of a potential and so far neglected side-effect of markets with information revelation competition. Those, who freely share information in exchange for benefits and incur low privacy costs, exert social pressure on the more disclosure-unwilling to adapt. The more others share, the harder it becomes to abstain. In effect, disclosure-unwilling individuals may partly catch up, incurring high privacy cost without meaningfully affecting outcomes. They would have been better off in a state with less overall disclosure driven by strategic incentives and peer comparison.

Our results are in line with evidence by Brandts et al. (2009), DellaVigna et al. (2012, 2017), and Reyniers and Bhalla (2013) illustrating that competition or social pressure can reduce well-being or welfare. However, we refrain from a welfare-analysis since the personal data we use may be less predictive for real-world behavior than personal data exchanged in the field. Thus, we would underestimate potential welfare-gains for the selecting market-side. Rather, we focus on understanding the disclosure side, emphasize the power of peer dynamics in markets with gains from personal information sharing, and point out that a reluctant group might be hurt. The effects we find in our setup with anonymous personal information are likely even stronger in the field with non-anonymous and more privacy-sensitive personal data.

Moreover, our finding that competition via personal but not directly relevant information sharing is beneficial for the disclosure-willing market side but does not serve as a good heuristic

for the selecting one is in line with previous literature (Iyer et al. 2016; Michels 2012; Pope and Sydnor 2011). In our setting, incentives might even wipe out the slightly positive relationship between the number of disclosures and generosity towards others. Since a lot of personal information sharing occurs in settings which incentivize people to reveal personal details, for example, on Airbnb to attract guests, on LinkedIn to attract recruiters, or on microfinance and crowdfunding platforms to attract investors, competition via personal information revelation might lead to extensive information sharing in order to catch attention rather than to highlight the qualitatively most suitable options. The recent introduction of "superhosts"[34] on Airbnb might be a result of such information overbidding, and questions the usefulness of extensive endogenous disclosure. As a consequence, personal information sharing may not be caring.

Although our study provides helpful insights into the channels underlying recent extensive (online) information sharing, it also has shortcomings. It relies on rather subjective opinions and attitudes as a source of personal information in a laboratory environment which might be less sensitive than identifiable information like names or photos in the real world. Further research might narrow the gap to field settings to show how peer comparison and strategic benefits partly jointly and partly isolatedly affect endogenous personal information disclosure, but under less experimental control. A more detailed analysis of adaptation patterns of initially disclosure-unwilling individuals and their perceived pressure seems to be another promising perspective for further research.

[34] "Superhost" is a rating of excellence on Airbnb which might have become necessary because with the mass of information already provided by hosts, screening based on this information is no longer useful.

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A Appendix: Instructions

Translated from German. Instructions taken from *strategic* treatments; variations in *random* treatments displayed in [square brackets].

Instructions: Part 1

Welcome, and thank you very much for your participation in this experiment. Please read the following instructions carefully. If you have any questions, feel free to raise your hand at any time. One of the experimenters will approach you to answer your questions. Please do no longer ask questions loudly, and do not communicate with other participants in the experiment. If you break this rule, we have to dismiss you from the experiment and the associated payoff. No participant receives any information about the identity and payoffs of other participants during or after the experiment.

The experiment consists of two parts. You receive the instructions for the second part at the beginning of the second part.

Each participant receives 4 Euros for participating in this experiment. Moreover, your additional payment depends on the statements and on the decisions you and your interaction partners make, i.e., your decisions impact your own payoff as well as that of other participants.

The first part of the experiment begins with a brief questionnaire. Please answer all questions carefully. For filling in the questionnaire, you receive 3 Euros. The questionnaire has to be filled in full. If you do not agree to this practice, you have now or at any time during the experiment the possibility to leave the experiment without further consequences and without losing your guaranteed show-up fee of 4 Euros.

Instructions: Part 2

In this experiment, you interact in a group with two other players. For better distinction, the colors Red, Blue, and Green are assigned to the three participants, and represent their roles in within the group. Groups and the roles Red, Blue, and Green are randomly assigned during the experiment, and then remain fixed for the whole experiment. The allocation decision, which will be explained in what follows, takes place exactly once.

Allocation decision

In this experiment, one participants is to decide about the allocation of 17 Euro between all three group members. We call the player, who makes this decision, the *allocator* in what follows. Only Red and Blue can take the role of the *allocator*. [With a probability of 50% each, chance] Green decides whether Red or Blue can determine the allocation of the 17 Euro in the role of the *allocator*. Green cannot be the *allocator*.

Before the *allocator* is determined [randomly] by Green and the allocation decision is made, group members in the role Red and Blue can disclose information about themselves to the green participant. Whether you provide information about yourself to Green, and if yes, which, is completely optional for you. Particularly, you decide for each answer of the questionnaire whether the green participant is allowed to learn this information. For each answer disclosed, we subtract 10 Cents from your budget of 3 Euros from the first part of the experiment. Green can look at the disclosed information about the other two group members from the questionnaire before [chance] Green decides whether Red or Blue takes the role of the *allocator*.

The allocator can distribute the 17 Euro as integer, positive amounts between himself and the other two group members. The amount has to be distributed in full, and each member has to receive at least 1 Euro. Hence, the allocator can give each group member including himself 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14 or 15 Euro, but the total amount must not exceed 17 Euro.

Each of the other two group members can decide which minimal amount of money he requires to receive from the *allocator* to accept his offer, or reject it otherwise. In case the *allocator's* offer is smaller than the minimum acceptable amount, one rejects his offer and receives 0 Euro. In case the offered amount is higher, one accepts the offer and receives the offered amount, i.e.,

at least 1 Euro. The two participants make this decision independently of each other. This means that your decision whether to accept or reject the offer affects only your own payoff, but does not affect the payoffs of the other two group members. In particular, the payoff of the *allocator* remains unaffected, independent of whether the other two group members accept or reject his offer, and always equals the amount the *allocator* kept for himself. However, the *allocator* learns whether his chosen monetary amounts are accepted or not.

In role Red or Blue, you will be asked to make one decision in case you become *allocator* and one in case you do not become *allocator*. Afterwards, [chance] Green decides who becomes *allocator*. At the end of the experiment, all group members will be informed about the decisions relevant for them, and their resulting payoffs.

Guesses of answers and information disclosed

During the experiment, we will ask you to guess how the other candidate for the role of the *allocator* (Red or Blue) answered the questionnaire, i.e., which answer (with seven response options) he chose for each of the questions. In addition, for each answer you will be asked to guess the other candidate's decision to disclose his response (yes or no). More precisely, this means that Red guesses the answers and corresponding disclosure decisions of Blue, and Blue guesses the answers and corresponding disclosure decisions of Red. Whether Green guesses the answers and disclosure decision of Red or Blue is determined by chance. At the end of the experiment, one of your guesses will be randomly selected for bonus payment. In case an answer guess is selected, you receive a bonus of 3.50 Euro if your guess is correct. In case a disclosure guess is selected, you receive a bonus of 1 Euro if your guess is correct. If your guess is not correct, you do not receive a bonus. Please note that *only one* of your guesses will be paid, i.e., *either* an answer guess *or* a disclosure guess, but not both. For this payoff mechanism, you fare best if you always state the value which equals your true guess.

B Appendix: Additional tables and results

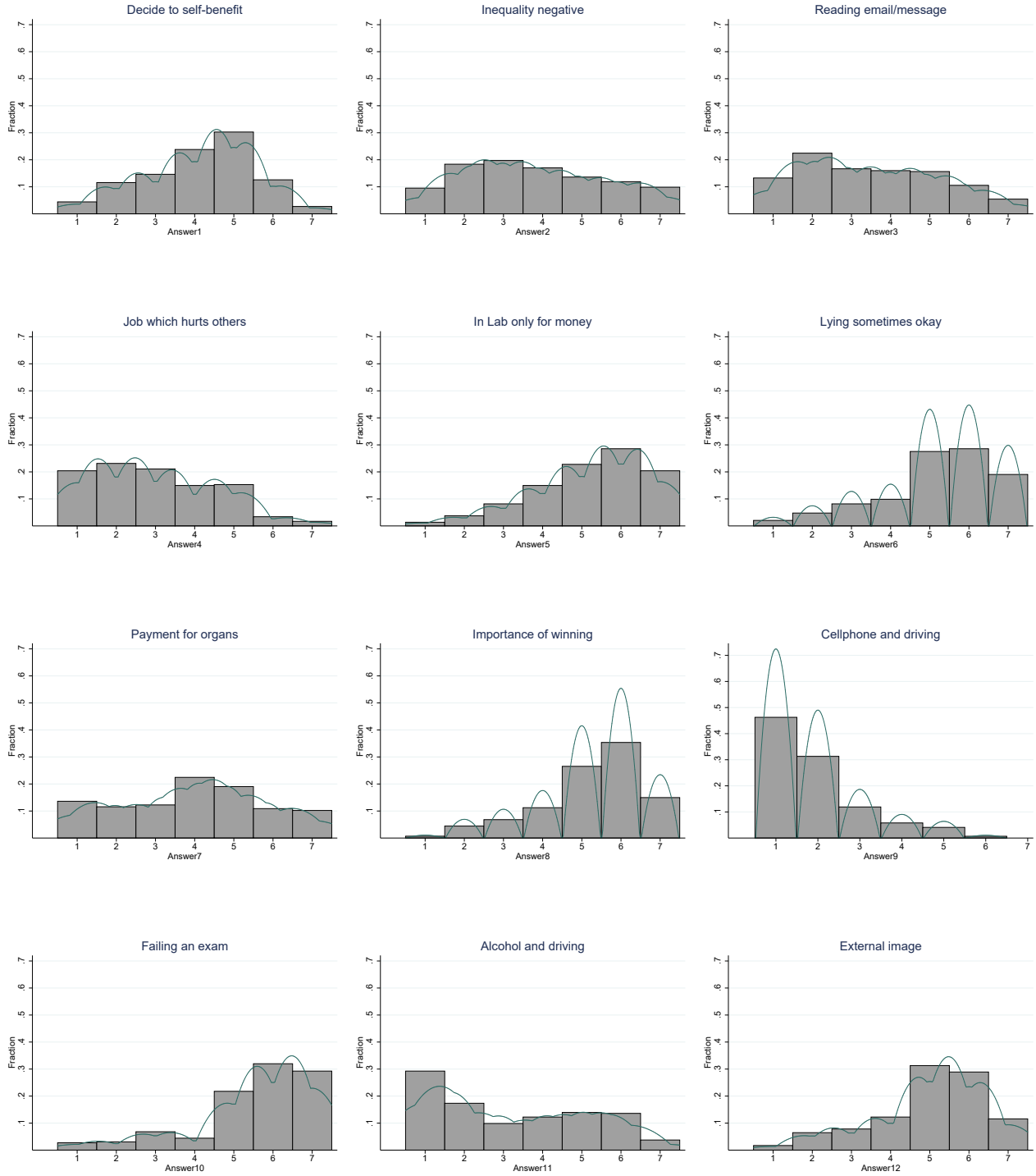
B.1 Descriptive statistics of answers and outcomes

Table 7: Descriptive statistics: Outcome variables

	random	random comparison	strategic	strategic comparison
Answers ex ante disclosed	1.63	2.10	3.55	4.00
	(3.078)	(2.610)	(2.957)	(3.217)
Ex post disclosure changes	0.30	0.33	0.21	0.95
	(1.454)	(0.730)	(0.585)	(1.820)
Perceived pressure (standardized)	-0.25	-0.32	0.02	0.37
	(0.786)	(0.748)	(1.024)	(1.144)
Own payoff (€)	10.70	9.95	9.64	8.98
	(3.818)	(3.493)	(3.764)	(3.706)
C's payoff (€)	3.15	3.63	4.03	4.40
	(1.902)	(1.835)	(2.060)	(2.094)
Acceptance threshold	1.98	2.45	1.83	2.50
	(1.510)	(1.853)	(1.488)	(1.719)
N	40	40	58	58

Notes: Standard deviations in parentheses. Only role A and B considered.

Figure 8: Histograms of answers



Notes: Lines represent Kernel density.

B.2 Further results: Ex post and ex ante disclosure behavior

Factors affecting disclosure on answer level

As reported in probit regressions in Table 8, several factors seem to affect the probability to reveal a particular answer from the questionnaire. Of course, the answer one gave significantly affects disclosure for most questions. Perceived relevance for predicting subsequent allocation behavior increases the probability to disclose the answer. In contrast, a feeling of unpleasantness to reveal a particular answer decreases it, but only for questions one and five with statistical significance at the 5% level. The *strategic* coefficient reports by disclosing which particular answers participants respond to the disclosure incentive. All answers are disclosed significantly more often in *strategic* treatments except answers three, five, six, and ten.

Table 8: Probit regressions - Disclosure-affecting factors on question level

	Answer (x) disclosed ex ante					
	(1)	(2)	(3)	(4)	(5)	(6)
unpleasant	-0.039***	-0.012	-0.023	-0.022*	-0.040***	-0.015
	(0.014)	(0.014)	(0.014)	(0.012)	(0.015)	(0.013)
relevant	0.034**	0.044***	0.015	0.032***	-0.002	-0.004
	(0.016)	(0.012)	(0.015)	(0.012)	(0.013)	(0.013)
answer	-0.121***	0.054***	-0.039**	-0.132***	-0.052***	-0.025
	(0.019)	(0.014)	(0.017)	(0.015)	(0.017)	(0.017)
strategic	0.207***	0.179***	0.091	0.228***	0.062	0.032
	(0.058)	(0.057)	(0.056)	(0.048)	(0.057)	(0.054)
baseline probability	0.348	0.251	0.199	0.381	0.215	0.169
N	196	196	196	196	196	196
Pseudo R2	0.255	0.219	0.098	0.344	0.139	0.081

	(7)	(8)	(9)	(10)	(11)	(12)
unpleasant	-0.000	-0.025	0.017	-0.035*	-0.025	0.008
	(0.017)	(0.016)	(0.017)	(0.019)	(0.018)	(0.016)
relevant	-0.009	0.015	0.049***	0.046***	0.036**	0.023
	(0.014)	(0.016)	(0.014)	(0.015)	(0.015)	(0.017)
answer	0.033**	-0.028	-0.113***	0.015	-0.068***	0.053**
	(0.013)	(0.021)	(0.030)	(0.016)	(0.015)	(0.023)
strategic	0.201***	0.113**	0.177***	0.092	0.135**	0.244***
	(0.062)	(0.052)	(0.053)	(0.057)	(0.055)	(0.052)
baseline probability	0.220	0.214	0.302	0.204	0.252	0.267
N	196	196	196	180	196	196
Pseudo R2	0.146	0.077	0.172	0.109	0.190	0.184

Notes: Marginal effects displayed, representing changes in the probability to disclose a certain answer, with disclosure decision of answer(x) as 0-1 (no/yes) outcome variable. Standard errors in parentheses clustered on group level. Control dummies for the ten different randomizations of questions used included.

Table 9: Tobit regressions - Effect of strategic incentives on ex ante disclosure

	Answers ex ante disclosed			
	(1)	(2)	(3)	(4)
strategic	3.264***	3.581***	3.509***	3.666***
	(0.658)	(1.024)	(0.989)	(1.000)
comparison		1.240	1.429	1.466
		(1.093)	(1.097)	(1.086)
strategic # comparison		-0.592	-0.748	-0.826
		(1.289)	(1.231)	(1.239)
constant	0.236	-0.407	0.765	-0.087
	(0.544)	(0.856)	(1.929)	(2.309)
sigma	3.898***	3.883***	3.714***	3.686***
	(0.314)	(0.320)	(0.300)	(0.285)
basic controls	No	No	Yes	Yes
preference controls	No	No	No	Yes
N	196	196	196	196
Pseudo R2	0.032	0.035	0.051	0.053

Notes: Standard errors in parentheses clustered on group level. Basic controls include gender, age, and dummies for the ten different randomizations of questions used. Preference controls include dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM.

Directions of ex post disclosure changes and adaptation behavior

We look at three other outcome variables of ex post disclosure behavior, namely the direction of changes, i.e., the number of ex post upward and downward changes, respectively, and whether the change made mimics the competitors' revelation. The direction of changes can be inferred from Figure 3 in the main analysis, and Table 11 shows the corresponding regression results. The *strategic-comparison* interaction effect is significant for the number of adaptations to the other's ex ante disclosure and for the number of upward changes, i.e., ex post disclosure of answers not disclosed ex ante. None of our explanatory variables prevails significant for downward changes, i.e., answers ex ante disclosed but hidden ex post.

Two main messages follow from this analysis. First, looking at columns (1) and (2) of Table 11, ex post disclosure changes are adaptations to disclosures of one's competitor. This alludes to conformity seeking (Asch 1951; Bernheim 1994). In fact, the correlation between the number of adaptations and the number of ex post disclosure changes is with 0.84 very high, and the interaction effect in column (1) of Table 11 for adaptations similar in magnitude to that in column (1) of Table 5 for ex post disclosure changes. Testing for similarity of the

Table 10: Robustness to winsorization - Ex ante disclosures and ex post disclosure changes

	Answers ex ante disclosed				Ex post disclosure changes		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
strategic	2.026***	2.002***	2.015***	2.160***	0.090	0.060	0.016
	(0.409)	(0.633)	(0.620)	(0.638)	(0.083)	(0.096)	(0.115)
comparison		0.400	0.573	0.614	0.200*	0.185	0.120
		(0.567)	(0.595)	(0.593)	(0.112)	(0.119)	(0.125)
strategic # comparison		0.048	-0.122	-0.175	0.507**	0.514**	0.620**
		(0.817)	(0.790)	(0.805)	(0.250)	(0.252)	(0.271)
constant	1.750***	1.550***	2.250	1.305	0.100*	0.469	-0.270
	(0.284)	(0.461)	(1.370)	(1.572)	(0.055)	(0.426)	(0.423)
basic controls	No	No	Yes	Yes	No	Yes	Yes
preference controls	No	No	No	Yes	No	No	Yes
N	196	196	196	196	196	196	196
R2	0.109	0.114	0.192	0.207	0.099	0.119	0.170

Notes: Table reports OLS regression coefficients with standard errors in parentheses clustered on group level. Basic controls include gender, age, and dummies for the ten different randomizations of questions used. Preference controls include dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM. Results winsorized by 10% on treatment level.

Table 11: Adaptations to competitor and directions of ex post disclosure changes

	Adaptations		Upward changes		Downward changes	
	(1)	(2)	(3)	(4)	(5)	(6)
strategic	-0.122	-0.151	0.115	0.041	0.208	0.173
	(0.227)	(0.209)	(0.086)	(0.112)	(0.223)	(0.178)
comparison	-0.000	-0.050	0.075	0.011	0.050	0.056
	(0.240)	(0.221)	(0.090)	(0.111)	(0.242)	(0.207)
strategic # comparison	0.707**	0.773**	0.580**	0.681**	-0.136	-0.143
	(0.321)	(0.332)	(0.255)	(0.275)	(0.248)	(0.234)
constant	0.225	0.170	0.075	0.006	-0.225	0.112
	(0.222)	(0.415)	(0.054)	(0.384)	(0.222)	(0.247)
controls	No	Yes	No	Yes	No	Yes
N	196	196	196	196	196	196
R2	0.061	0.122	0.091	0.146	0.011	0.111

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table reports OLS regression coefficients with standard errors in parentheses clustered on group level. Basic controls include gender, age, and dummies for the ten different randomizations of questions used. Preference controls include dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM.

interaction effects across the two regressions with adaptations and ex post disclosure changes as outcome variables yields a p-value of 0.924 so there is no indication to reject similarity of the

two coefficients. When competing with peers, one seems to disclose that kind of information that peers also disclose, what can be regarded as an *intensive margin*.

Secondly, reported in columns (3)-(6) of Table 11, the combination of social and economic incentives to disclose captured by the *strategic-comparison* interaction effect explains disclosure extensions but not disclosure reductions. This alludes to an *extensive margin* because one reveals more information if others do so, given one can benefit from revelation. The finding that the ex ante disclosure changes of interest are mainly disclosure extensions shows that peer comparison in a world with benefits seems to affect disclosure behavior in only one direction, namely to reveal more.

B.3 Further results: Costs and benefits

Allocator selection

Table 12: Effect of difference in information disclosure on probability to become *allocator*

	Allocator			
	(1)	(2)	(3)	(4)
own - other's ex post disclosures	0.042***	0.030*		
	(0.016)	(0.018)		
own - other's relevant ex post disclosures			0.077***	0.074**
			(0.026)	(0.030)
blue displayed first		-0.060		-0.101
		(0.111)		(0.104)
red		0.033		0.012
		(0.139)		(0.125)
baseline probability	0.534	0.537	0.534	0.539
randomization controls	No	Yes	No	Yes
N	58	58	58	58
Pseudo R2	0.077	0.202	0.091	0.249

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table reports marginal effects from probit regressions with robust standard errors in parentheses. One *allocator* candidate is randomly chosen per group to calculate disclosure difference. Relevant disclosures only take disclosures into account which player C marks as relevant predictor of game behavior. "Red" and "blue displayed first" are dummies if candidate's color is red and whether player blue is displayed above red on the screen for *allocator* selection. "Red" corresponds to role A and "blue" to role B in instructions. Randomization controls include dummies for the ten different randomizations of questions used. Only *strategic* treatments considered.

Table 12 shows the results of probit regressions with the probability to be selected as *allocator* as dependent variable. Since active *allocator* selection only takes place in the *strategic* treatments, we only consider this subsample in our analysis. Moreover, we randomly draw one of the two *allocator* candidates in each group since otherwise each difference would be counted twice in the analysis. Columns (1) and (2) investigate how the difference in ex post disclosed answers affects the probability to be selected. Each additionally disclosed answer increases the chance to become *allocator* by 4.2 percentage points in column (1). Since we assigned participants the colors red (A) and blue (B) in our experiment for better identification, we add dummies equal to one if the color assigned is red, and if the blue player is randomly chosen to be displayed first on the choice screen of player C, respectively. Moreover, we add dummies for the order in which the questions are displayed. Doing so decreases effect size and significance in column (2), but still confirms the relevance of disclosing relatively more than the competitor, i.e., a

three percentage points higher probability to be selected for each additional answer disclosed.[35] The same analysis is repeated in columns (3) and (4) for a slightly modified outcome variable, which only considers those answers for the calculation of the disclosure difference which player C marks as relevant indicators for impunity game behavior. Results reveal a qualitatively similar pattern but a bigger effect size, namely a 7.4% to 7.7% higher probability to be selected for each relevant answer one discloses more than the competitor. Consequently, disclosing more answers is indeed beneficial to be selected according to our experimental data, supporting Hypothesis 5.

Allocation behavior

In this section, we look at differences in allocation behavior between treatments and conditional on personal information sharing. Table 13 presents how different characteristics affect prosocial behavior measured by the amount one keeps for oneself as the *allocator* in Panel A, and by the amount one gives to C in Panel B.[36] A lower coefficient in Panel A represents less egoistic behavior, while a higher coefficient in Panel B represents more generosity.

Pooling the data with and without peer comparison in column (1) confirms the "I-want-you" effect (Brandts et al. 2006): Selected *allocators* give more to the selector compared to a situation with random *allocator* assignment, i.e., *allocators* reciprocate the favor of their selection by offering more to C. When investigating all four treatments separately in column (2), the point estimate of the *strategic* coefficient does not change much, and stays in the range of 80 to 90 cents which C on average earns more. This means that previous social *comparison* does not affect subsequent distribution behavior, and seems not to be detrimental for prosociality in our setting. Therefore, we stick with data pooled over *comparison* when investigating allocation behavior in more detail.

Are players selected as *allocators* actually those who act more generously? We can answer this question with our strategy data. Since participants who reveal more information are more likely selected as *allocators*, we investigate whether C benefits from this selection strategy. Although the interaction effect of the *strategic* and the *allocator* variable in columns (3) of Table 13 point in the direction of more prosociality, none is significant, i.e., selected *allocators* do not behave systematically more generously.[37] Therefore, we cannot reject the hypothesis that

[35] We limit the set of control variables to features visible to C when choosing the *allocator* since she does not know other characteristics about the participant.

[36] Since the pie of 17€ is fixed, it is redundant to also report the amount given to the competitor.

[37] Notably, the interaction effect weakens the *strategic* coefficient, which is insignificant in column (3) and smaller than in all other specifications. Thus, subjects in *strategic* treatments may not generally behave more nicely, and some valuable screening of personal information seems to take place before C's *allocator* selection. However, we do not find support for beneficial screening based on endogenously provided personal information on a statistically reliable level ($p = 0.189$).

Table 13: Payoff allocations by treatment and disclosure behavior

	Own payoff				
	(1)	(2)	(3)	(4)	(5)
strategic	-1.015*	-1.062	-0.329	-1.967**	-1.718**
	(0.551)	(0.823)	(0.772)	(0.756)	(0.817)
comparison		-0.750			
		(0.851)			
strategic # comparison		0.095			
		(1.098)			
allocator			-0.250		
			(0.784)		
strategic # allocator			-1.371		
			(1.027)		
answers ex post disclosed				-0.379***	-0.398***
				(0.118)	(0.129)
strategic # answers ex post disclosed				0.445***	0.421**
				(0.164)	(0.173)
constant	10.325***	10.700***	10.450***	10.997***	11.332***
	(0.427)	(0.676)	(0.562)	(0.467)	(1.799)
controls	No	No	No	No	Yes
N	196	196	196	196	196
R2	0.018	0.027	0.047	0.048	0.078

	C's payoff				
	(1)	(2)	(3)	(4)	(5)
strategic	0.828***	0.884**	0.460	1.205***	1.058**
	(0.288)	(0.413)	(0.421)	(0.394)	(0.430)
comparison		0.475			
		(0.430)			
strategic # comparison		-0.113			
		(0.573)			
allocator			0.075		
			(0.406)		
strategic # allocator			0.735		
			(0.556)		
answers ex post disclosed				0.197***	0.209***
				(0.058)	(0.063)
strategic # answers ex post disclosed				-0.203**	-0.192**
				(0.086)	(0.091)
constant	3.387***	3.150***	3.350***	3.039***	2.766***
	(0.217)	(0.336)	(0.292)	(0.240)	(0.946)
controls	No	No	No	No	Yes
N	196	196	196	196	196
R2	0.040	0.051	0.064	0.066	0.095

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table reports OLS regression coefficients with standard errors in parentheses clustered on group level. Controls include gender, age, dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM.

a random *allocator* draw works as well as selection based on endogenously provided personal information, whose content has to be subjectively classified. In Table 16, we report further results on question level regarding which answers predict allocation behavior, and in how far these answers are taken into account for *allocator* selection. These results also point into the direction of inefficient selection.

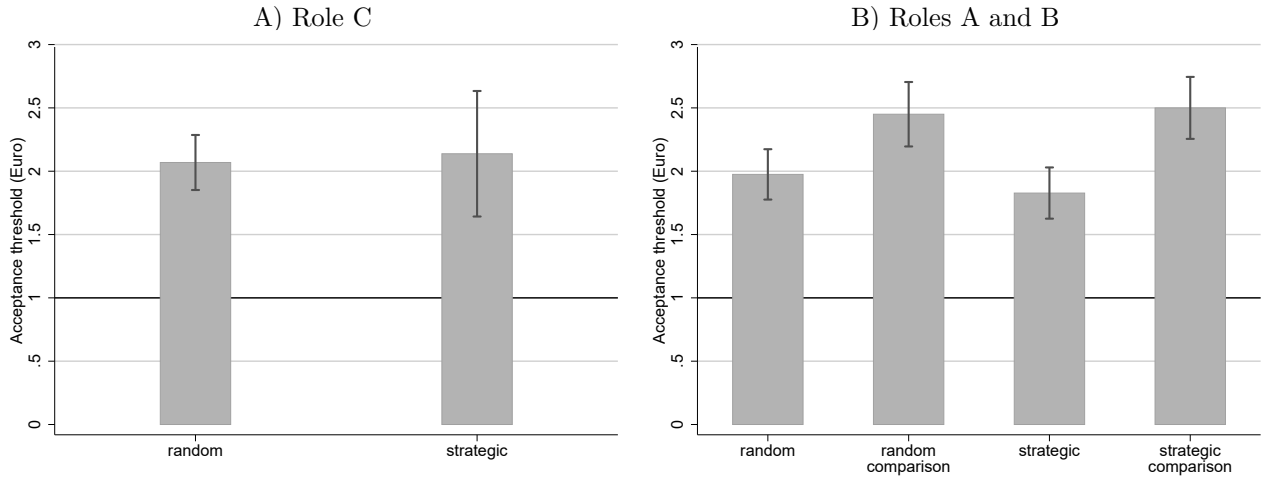
Column (4) of Table 13 shows the direct effect of information disclosure on allocation behavior. There is a significant positive effect of the *strategic* coefficient on the amount allocated to C as in the initial specifications, which can be attributed to 1.97€ of forgone own earnings of the *allocator*. Moreover, we observe highly significant effects of the number of answer ex post disclosed alone and in interaction with the *strategic* coefficient on prosociality. The former effect captures the influence of more information sharing on prosocial behavior in *random* treatments. Interestingly, people intrinsically motivated to share personal information seem to keep less for themselves and give more to others. Quantitatively, for each answer they disclose, they give approximately 18 and 20 cents more to the other candidate and player C, respectively.

The positive relationship between more personal information disclosure and generosity vanishes with incentives. The ex post disclosure coefficient and its interaction with the *strategic* coefficient almost entirely cancel out. This means that more information sharing does not correspond to more prosociality in case of *strategic* incentives for information disclosure. While the level effect of more prosociality in *strategic* treatments remains strong in magnitude, revelation competition seems to destroy the predictive power of endogenous information disclosure for prosocial behavior. Figure 6 shows the corresponding coefficient plot. While without incentives to share information only the intrinsically motivated types disclose information, with incentives the non-intrinsic, opportunistic types also start to disclose, thereby diluting the original relationship. However, since the number of participants in *random* treatments who disclose many answers is limited, we rather interpret this finding with caution. Nonetheless, the positive relationship between intrinsic information sharing and generosity remains when winsorizing ex post disclosures on the 90% level in columns (3)-(6) of Table 15, providing support for its validity.

Acceptance thresholds

This section presents acceptance thresholds in the impunity game elicited for all three players of a group by using the strategy method. Figure 9 plots the acceptance thresholds in all four treatments of subjects in roles A and B in Panel B. We depict those of C separately in Panel A because C is in another info set when stating her acceptance threshold because she already knows who becomes *allocator* at that point in time. Moreover, Panel A consists of only two bars

Figure 9: Coefficient plot of acceptance thresholds by role and treatment



Notes: Vertical lines represent standard errors clustered on group level. The line at level one depicts the minimum payoff when not rejecting.

since C is not informed about the different social *comparison* levels. Since each player receives a payoff of 1€ for sure if she accepts the *allocator's* offer, setting an acceptance threshold higher than 1€ might cause a payoff loss, and is weakly dominated for subjects interested only in their own payoff. Nonetheless, all bars display significantly higher acceptance thresholds (all $p < 0.001$), ranging from 1.83€ to 2.50€. This means that people are willing to forgo some money in our experiment when being offered too little.

Table 14: Acceptance thresholds by role and treatment

	Acceptance threshold					
	(1) C	(2) A and B	(3) A and B	(4) A and B	(5) A and B	(6) A and B
strategic	0.069	-0.049			-0.147	-0.136
	(0.321)	(0.231)			(0.282)	(0.300)
comparison			0.592**	0.600**	0.475	0.454
			(0.227)	(0.242)	(0.321)	(0.341)
strategic # comparison					0.197	0.246
					(0.450)	(0.473)
controls	No	No	No	Yes	No	Yes
N	98	196	196	196	196	196
R2	0.000	0.000	0.032	0.049	0.033	0.051

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table reports OLS regression coefficients with standard errors in parentheses, in columns (2)-(6) clustered on group level.

We try to disentangle what drives the high acceptance thresholds in simple OLS regressions displayed in Table 14, but find no significant differences between treatments and roles except for *comparison*. The *strategic* coefficient is neither significant in column (1) for role C nor in

column (2) for roles A and B, and provide zero explanatory power ($R^2 = 0.000$). In contrast, social *comparison* turns out to push acceptance threshold upwards. While this effect prevails to be significant in columns (3) and (4) when investigated pooled over the *strategic* dimension, it is not if this dimension is additionally taken into account. As a consequence, we refrain from statements regarding acceptance behavior in the impunity game.

Table 15: Robustness to winsorization - Perceived pressure and allocation behavior

	Pressure		Own paoff		C's payoff	
	(1)	(2)	(3)	(4)	(5)	(6)
strategic	0.264	0.242	-2.153***	-1.933**	1.293***	1.161***
	(0.178)	(0.171)	(0.770)	(0.831)	(0.402)	(0.438)
comparison	-0.062	-0.089				
	(0.165)	(0.169)				
strategic # comparison	0.416*	0.474*				
	(0.249)	(0.243)				
answers ex post disclosed			-0.540***	-0.572***	0.274***	0.294***
			(0.147)	(0.149)	(0.075)	(0.076)
strategic # answers ex post disclosed			0.603***	0.594***	-0.280***	-0.277***
			(0.186)	(0.190)	(0.099)	(0.101)
constant	-0.254**	-0.328	11.195***	11.775***	2.945***	2.546***
	(0.112)	(0.454)	(0.481)	(1.788)	(0.248)	(0.943)
controls	No	Yes	No	Yes	No	Yes
N	196	196	196	196	196	196
R2	0.076	0.113	0.057	0.088	0.073	0.103

Notes: Table reports OLS regression coefficients with standard errors in parentheses clustered on group level. Controls include gender, age, dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM. Results winsorized by 10% on treatment level. Pressure reported in standard deviations, payoffs in €.

Selection and allocation outcomes by answers

Table 16 reports how answers translate into outcomes. Regarding allocation behavior in *strategic* treatments, answers 1, 9, 10, and indicatively also answer 2 prevail to be predictive for the amount one allocates to player C, but these answers are only insufficiently taken into account for *allocator* selection in column (2). A probit models in columns (2) finds significant effects of answers 1 and 2 on the probability to be selected as *allocator* only at the 10% level, and no effect for the other answers predictive for behavior. Note that answers taken into account for *allocator* selection are limited to answers which are actually disclosed since player C can only take these answers into account for selection. When considering content of the disclosed answers and disclosure per se separately in column (3), the pattern just described fades. With

Table 16: Effects of answers and disclosures on selection and allocation behavior

	Allocation to C	Allocator	
	(1)	(2)	(3)
Answer1	-0.360**	0.052*	-0.062
Answer2	0.184*	0.030*	0.052
Answer3	0.002	-0.047	-0.074**
Answer4	-0.121	0.000	-0.055
Answer5	0.002	-0.001	0.063*
Answer6	-0.063	0.002	0.050
Answer7	-0.009	0.027	0.067*
Answer8	-0.198	0.024	-0.242***
Answer9	-0.394**	-0.009	-0.030
Answer10	0.243**	-0.003	-0.080
Answer11	-0.059	-0.049	0.043
Answer12	-0.061	0.013	0.167***
Ex post disclosure question1			0.515***
Ex post disclosure question2			-0.316
Ex post disclosure question3			0.168
Ex post disclosure question4			0.203
Ex post disclosure question5			-0.353**
Ex post disclosure question6			0.001
Ex post disclosure question7			-0.002
Ex post disclosure question8			1.253***
Ex post disclosure question9			0.005
Ex post disclosure question10			0.567
Ex post disclosure question11			-0.228*
Ex post disclosure question12			-0.868***
constant	6.736***		
baseline probability		0.498	0.504
randomization controls	Yes	Yes	Yes
N	116	116	116
R2 / Pseudo R2	0.395	0.085	0.343

Notes: Marginal effects of probit model displayed in columns (2) and (3), representing changes in probability to disclose a certain answer with disclosure decision of answer(x) as 0-1 (no/yes) outcome variable. Answers are on a 7-item scale (1 worst, 7 best). Answers in columns (2) and (3) are restricted to those which are disclosed. Column (1) represents OLS regression results with the monetary amount allocated to player C as the outcome variable. Only strategic treatments considered. Standard errors clustered on group level not displayed for the sake of readability. Randomization controls include dummies for the ten different randomizations of questions used. R^2 reported in column (1), Pseudo R^2 in columns (2) and (3).

reference to question 1, its pure disclosure seems to matter more than its content. In addition, disclosure of questions 5, 8, and 12 appears to be important for C's *allocator* selection decision, and content of questions 3, 8, and 12. Remarkably, the questions which predict allocation behavior are *not* those whose content matters for *allocator* selection. The only question which persistently prevails to matter both for selection and for behavior is question 1. This means that C conditions her selection insufficiently on the information available, suggesting inefficient screening in our personal information disclosure context.