Personal Information Disclosure under Competition for Benefits: Is Sharing Caring?

Viola Ackfeld[a], Werner Güth[b]

[a] *University of Cologne, Albertus-Magnus-Platz, 50923 Cologne, Germany*
[b] *Max Planck Institute for Research on Collective Goods*

Abstract

Personal information is shared extensively every day, particularly when competing for others' attention on online platforms. In this paper, we experimentally investigate the interaction of peer comparison and incentives as drivers 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 feeling more compelled to reveal information. Our results shed light on additional drivers for the current information-sharing trend, while pointing to neglected social pressure to disclose personal information in competitive environments.

Keywords: Personal information disclosure, Incentives, Social pressure, Privacy, Experiment

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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 [...]."*

1. Introduction

Extensive sharing of personal information has become a stylized fact and one of the major societal changes of the 21st century. Getting access to many new services or exchange platforms nowadays often implicitly requires sharing one's personal information. Hence, in many situations in life, sharing extensive personal information may be driven by motives which go beyond a direct preference for information sharing.

First, there may be *strategic competition* in personal information disclosure if people strive for others' beneficial attention by providing personal information. For example, people compete via extensive personal information disclosure for the attention of overnight guests or recruiters on Airbnb and LinkedIn. The microfinance platform Kickstarter even recommends

Email addresses: ackfeld@wiso.uni-koeln.de (Viola Ackfeld), gueth@coll.mpg.de (Werner Güth)

that borrowers include soft, personal information in their requests. Without being willing to share information publicly, some people may be excluded from such new marketplaces. In the LinkedIn example, a match with a suitable job candidate who prefers not to share information publicly cannot materialize. While providing information on such platforms can create more and qualitatively better matches, the extent of information provided is often decisive for getting attention.[1] For instance, adding a picture or more keywords to one's profile or posting any form of content may enhance one's visibility on the platform and the received attention, but does not necessarily increase work quality.

Second, one may react to the information disclosure behavior of one's *peers*, i.e., 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 competitive settings. Abstaining from the information-sharing economy may become more and more impossible the more competing peers engage in disclosure. This may lead to situations in which some people, who have similar characteristics, but higher privacy valuations than others, feel pressured to reveal information about themselves and incur high privacy costs. Hence, under information-disclosure competition against peers, they may be worse off compared to a world without such information-sharing dynamics. In effect, credit-worthy types in microfinance or potential candidates on LinkedIn may withdraw from such markets to avoid personal information-sharing competition.

This paper analyzes different motives and potential hidden costs of extensive personal information sharing when people compete for the beneficial attention of another market side. First, we study the interaction of competition for benefits and observing peers' sharing as a new channel which may drive 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. Second, we explore whether and how the interplay of these two factors contributes to subjectively perceived pressure to reveal more, or more unpleasant, 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.[2] In the main treatments, a third participant selects who determines the allocation. In order to be selected, candidates striving for distribution power can reveal answers from a potentially privacy-sensitive questionnaire. These answers may attract the attention of the selecting market side and may be unpleasant to disclose, but can only partly and imprecisely serve as indicators of behavior. Being able to attract

[1] Note the difference to classical applications with a CV. Here, information is not shared publicly, and there is no algorithm involved influencing which profiles are displayed. Hence, all candidates have the same chance of receiving attention.

[2] 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. See Section 2, Distribution game, for a detailed description of the game and why we choose it.

attention or possibly creating trust renders personal information sharing strategic in the main treatments. In the control treatments, distribution power is randomly assigned, so information sharing has no strategic aspect. While the distribution game is limited in mimicking how mutual benefits in online market interactions are distributed, it generates an incentive for personal information disclosure in strategic treatments, similar to striving for the beneficial attention of another market side in modern online markets. As a second dimension of our experiment, we inform participants in half of the treatments (without prior announcement and without telling the selecting player) about their competitor's disclosure choice, and give them the opportunity to adapt their own. In this way, we can test for the effect of peer comparison on disclosure behavior with and without competition involved. Afterwards, we measure the participants' perceived pressure to disclose information, as well as game-related outcomes.

We find that information disclosure doubles under strategic incentives compared to the control condition with random assignment of allocation power. Moreover, subsequent peer comparison boosts information disclosure in the strategic, but not in the random setting. This effect is driven by subjects who have similar characteristics, but are initially relatively unwilling to disclose much, and only reveal more information when learning that they lag 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. We do not find such an effect for strategic incentives in isolation. While revealing more information has a positive effect on being selected, it does not translate into higher generosity under strategic incentives. With abundant information available in strategic treatments, screening for game-relevant personal information becomes necessary to benefit from information disclosure.

Investigating motives and hidden costs of personal information sharing, the contributions of our paper are threefold. Firstly, we study *strategic*, purposeful sharing of personal information and show how such information can be used to compete. Previous experimental research with soft personal information (Benndorf and Normann 2018; Bohnet and Frey 1999; Brandts et al. 2006; Charness and Gneezy 2008; Eckel and Petrie 2011; Schudy and Utikal 2017) has predefined which and how much personal information will be provided, and hence does not take into account how such information can be strategically used to attract attention.[3] In contrast, research studying the effect of competition on information disclosure relies on experimentally induced information without a personal meaning for participants and without a need for interpretation (Benndorf 2018; Benndorf et al. 2015; Forsythe et al. 1989; Jin et al. 2021; King and Wallin 1991; Penczynski and Zhang 2018; Sheth 2021).[4] Our study combines personal

[3]See Hermstrüwer and Dickert (2017) and Holm and Samahita (2018) for experimental research on how the presence of personal information affects prosocial behavior in light of maintaining a social image, and Gaudeul and Giannetti (2017) on group formation and contribution behavior based on the provision of personal information. See Bartoš et al. (2016) for research on how limited attention can influence the selection of candidates.

[4]Benndorf et al. (2015) compare a neutral and a health framing of experimentally assigned game types to study unraveling of information privacy. Like Jin et al. (2021) and Sheth (2021), they find less unraveling than predicted by unraveling theory, and even less unraveling in the health framing. According to unraveling theory (Milgrom 1981), under market competition, good types will share their private information, while non-sharing correctly evokes suspicion about quality. In our setting, where personal information is not a clear indication of

information – characterized by individually different privacy cost and painfulness to disclose – with strategic information sharing meant to attract the beneficial attention of another market side.[5] Observing that personal information in such a setting has only limited power to indicate actual behavior, we show that abundant personal information disclosure under competition can bias attention without helping to select more suitable interaction partners.[6]

Secondly, we shed light on potential channels driving the personal information-sharing trend. In particular, we provide novel evidence on the dynamics created by the interaction of strategic incentives and peer comparison. Acquisti et al. (2012), Böhme and Pötzsch (2011), and Chang et al. (2016) show that peer comparison in isolation spurs one's personal information-disclosure behavior. We also investigate peer effects, but focus mainly on adaptation behavior in response to feedback on the information sharing of others. Moreover, by pairing peer effects with strategic incentives for information disclosure, as a main contribution, we show that peer effects are especially pronounced under competition for benefits and in this case also affect those who are initially unwilling to disclose information. The combination of these two motives may be one explanation for the recent boom in extensive personal information sharing, a stylized fact of the digital age, whose underlying dynamics have received only limited attention so far.

Thirdly, we uncover that extensive information sharing might not necessarily generate improvements for all involved parties. Lee (2014) and Wang et al. (2011) find correlational evidence for hidden psychological costs of information sharing in the online world.[7] To the best of our knowledge, our paper is the first to provide causal, empirical evidence for hidden psychological cost of personal information sharing: People who have similar characteristics but a higher individual-specific and characteristics-unrelated valuation for privacy, may feel pressured to disclose more, or more unpleasant, information than they would like to. In a world with extensive information-sharing dynamics due to incentives and observing peer's sharing, they may be worse off than without. While previous economic research has investigated the hidden cost of social pressure (DellaVigna et al. 2012, 2017; Reyniers and Bhalla 2013) and

characteristics, abundant information sharing rather serves as a means to attract attention. This mimics that nowadays all kinds of personal information - not only relevant information - are used to evaluate people. Hence, classical unraveling predictions may not hold.

[5]Several papers try to measure the economic value of privacy, but find ambiguous results (Acquisti et al. 2013; Benndorf and Normann 2018; Beresford et al. 2012; Jentzsch et al. 2012; Schudy and Utikal 2017; Tsai et al. 2011). See Farrell (2012) for a discussion regarding the economic properties of privacy and Acquisti et al. (2016) and Tucker (2015) for comprehensive surveys on this topic.

[6]Böhme and Pötzsch (2010), Ge et al. (2017), Michels (2012), and Pope and Sydnor (2011) provide evidence that adding soft, personal information can beneficially influence outcomes on microfinance platforms. However, evidence how well such voluntarily provided personal information can predict types is mixed. While Duarte et al. (2012) and Ge et al. (2017) observe a positive relationship between personal infomation and preferable online behavior, Iyer et al. (2016) and Pope and Sydnor (2011) only find ambiguous results.

[7]Wang et al. (2011) report that the desire to appear favorable to one's peers on Facebook induces people to post something they regret afterwards. Lee (2014) finds a positive correlation between comparison-seeking frequency on Facebook and negative feelings from comparison.

competition (Brandts et al. 2009) in isolation, we investigate potential hidden costs based on a combination of these motives in the highly relevant context of personal information sharing.[8][9]

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

2. Experimental design

Our experimental design, depicted in Figure 1, consists of the following stages: First of all, subjects answer a personality questionnaire without knowing what will follow in the experiment. Second, they receive instructions informing them about the distribution game and the possibility of revealing information from the questionnaire. After role assignment, subjects decide which pieces of information to reveal. If they are in a corresponding treatment, subjects learn their peer's disclosure and can revise their disclosure decision. Then, another participant can investigate the information and may take it into account for allocator selection. At the end, the allocation and the resulting payoffs are determined. In order to guarantee understanding, participants have to answer several comprehension questions correctly before being allowed to make decisions regarding our outcomes of interest. In what follows, the different parts, procedures, and treatments are described in detail. The experimental instructions can be found in Appendix A.

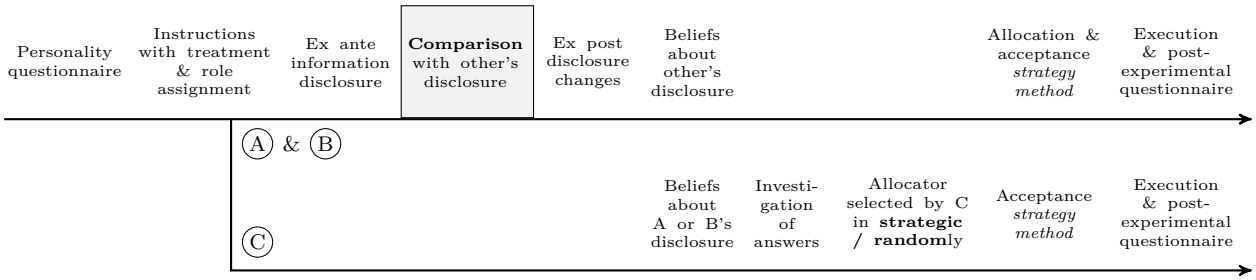
Personal information

While a first-best approach to study personal information disclosure might be to access real-world data, for example from social media, such data have shortcomings. First, they are complex and what information people have already accessed or what they infer from it is out of experimental control, which is likely to impair causal inference. Secondly, studying the dynamic interaction of strategic incentives and peer comparison, our channel of interest, and at the same time eliciting measures for potential hidden psychological costs, hardly seems possible with field data on an experimentally sound level. Instead, we follow a second-best approach and generate potentially sensitive, but anonymous and controllable personal information, similar to Frik and Gaudeul (2016).

[8]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 (2020) 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.

[9]Note that this paper focuses on potentially overseen cost on the disclosing market side, but refrains from a welfare analysis. Since non-anonymous personal information in real-world markets may be more predictive for behavior than the anonymous data used in this experiment, we would otherwise risk underestimating welfare gains. Rather, our approach attempts to reveal a subjective and usually unobservable type of information-disclosure costs. Finding such hidden costs already in a setting with anonymous information likely implies more pronounced effects in the field with identifiable data. Hence, the effects we find may constitute a lower bound for costs, independently of gains.

Figure 1: Timeline of the experiment



Notes: Overview of the experimental steps. One independent observation in the experiment involves three participants: A, B, and C. Separate lines depict the different experimental steps after role assignment. Treatment differences are marked in bold letters. An additional stage in *comparison* treatments is marked in gray. Subjects who are not in this stage do not learn about it at any stage. Strategy method in italics indicates that whoever becomes *allocator* and the offered amounts are not yet communicated, so choices are made for all potential scenarios. Accounting for a different distribution setting, the first two and last four steps are adapted from Brandts et al. (2006).

Table 1: Questionnaire

Question 1	Do you make decisions mainly in such a way that you benefit yourself?
Question 2	Do you consider inequality in society, which is based on different performance levels, as something negative?
Question 3	Are there reasons which justify reading emails or messages of friends?
Question 4	Would you accept a well-paid job if you knew it hurts others?
Question 5	Do you only participate in laboratory experiments because of money?
Question 6	Is it acceptable to lie in some situations?
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 failed 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.

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 one might consider as related to experimental game behavior; 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.[10] 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 follow in the second part.[11] We let participants explicitly consent to this non-standard approach by stating that "additional payment depends on the statements [...] you and your interaction partners make" before the information-revelation stage. Moreover, we emphasize voluntariness of participation and the right to leave the experiment at any time.

The questions are designed such that the answers correlate little with strategic disclosure decisions and generosity.[12] We use this kind of questions for three reasons. Firstly, the answers may attract attention or create trust, but do not necessarily serve as good indicators for partner selection. Secondly, in everyday life, one often has to decide which information to disclose to others without knowing how that information will be perceived and interpreted. Thirdly, such questions preserve anonymity, thereby guaranteeing high experimental standards. If we find

[10]The information we elicit consists mainly of 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 contains no straightforward indication of type, can overcome this issue. We are interested in 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.

[11]Eliciting information in the first part for the second part and introducing the *comparison* stage during the course of the experiment might be considered as problematic, since we only inform participants gradually about parts 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. A recent survey by Charness et al. (2022) supports that many experimental economists perceive such methods as appropriate if their benefits exceed their costs and there is no alternative way to collect data. In our case, one could alternatively use a Conditional Information Lottery (CIL) (Bardsley 2000), i.e., eliciting information in one part as a fictional scenario and informing people about the existence of fictional scenarios and one real scenario in advance. However, given our complex design, adding further complexity by distinguishing real and fictional scenarios may not outweigh its benefits. Also, as Bardsley (2000) mentions, CIL designs risk reduced motivation of participants to complete the tasks when they are likely fictional. Given that subjects answer and can disclose answers to 12 questions, the risk of unmotivated "clicking through" may be especially pronounced in our case. A further alternative would be to tell participants in advance that they will be able to change the disclosure of some information later on. However, fixing some disclosures would decrease the room for disclosure changes and the chance to observe a statistically significant treatment effect. To generate scientifically valid evidence, one would probably have to double the number of questions and disclosure choices. This would again blow our experimental design out of portion and risk subjects losing interest. Hence, we opted for gradual revelation.

[12]See the discussion section in Appendix B.4, particularly Tables B.15 and B.16, for evidence that there is no difference in answer content between those who disclose more or less in a pair.

hidden costs of information disclosure even in case of our comparably less privacy-sensitive information, perceived pressure in the field with identifiable information is likely even stronger. We randomize the order of questions to avoid any order effects.[13]

Strategic information disclosure

From reading the second part of the instructions, participants learn that there will be a distribution game and how they can reveal information to increase their chance to decide about the distribution. We randomly match three players into a group and assign them to one of the three roles A, B, and C.[14] Participants in roles A and B can decide which answers from the questionnaire they want to reveal to player C. They make this decision for each piece of information separately.

For each piece of 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.[15] Keeping information secret is possible at no cost. The small fee mimics transaction costs of personal information disclosure. For example, extending one's online profile requires a small amount of time and effort, which increases with more features filled in. The fee also captures efficiency losses from information sharing. Methodologically, it limits experimenter demand concerns of asking for information provision in such a setting. Finding different information-revelation patterns under disclosure costs would therefore strengthen our results. The information-disclosure decision is our main variable of interest in this paper. In particular, we focus on the total number of disclosures, stating corresponding hypotheses at the end of this section.

Distribution game

The distribution game is the impunity game (Bolton and Zwick 1995), 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 share and decide independently of each other. We use the strategy method when eliciting proposer and responder choices, i.e., participants make decisions for all situations they could face. This allows us to compare the allocation behavior of those who become proposers and those who do not. For responders, we elicit acceptance thresholds which are then implemented conditionally on the first-stage offer.[16]

Besides generating experimental payoffs, we use the distribution game mainly to incentivize strategic information disclosure via the prospect of distributing monetary benefits. While

[13] In particular, we display the questions on two separate screens with six questions each, and randomize the screen order, as well as the position of questions within the 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.

[14] 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.

[15] Revelation costs are adopted from Benndorf et al. (2015).

[16] After choosing one's acceptance threshold, we ask subjects which offer they expect.

people who, for example, receive more attention on LinkedIn may be able to negotiate higher wages and hence take a larger share of the overall pie for themselves, there are many aspects of online platforms which our game does not capture. In particular, online platforms do not require explicit distribution decisions as in our experiments.

A special feature of our game is that not all three group members can become the proposer. Only participants in role A and B compete to become 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 may select the *allocator* in our main treatments (see Treatments). Before doing so, she can access the information revealed by the *allocator* candidates A and B.

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.[17] However, the proposer is informed about the responder's rejection as a limited way to express her disapproval. The payoff-irrelevant punishment in the impunity game allows us to observe reactions of responders, while not weakening the incentive effect that the prospect of gaining *allocator* power may have.[18]

Additional measures

Due to our interest in the potential hidden psychological cost of information disclosure, we elicit participants' perceived pressure to disclose information right after they have 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.[19] Moreover, considerable heterogeneity in disclosure behavior may exist in such a setting and may impact 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),[20] 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 a question to predict behavior in the subsequent allocation task and the associated discomfort, we elicit

[17] In order to render a rejection meaningful, the proposer has to offer at least 1 Euro to every participant including herself.

[18] As a further reason, we refrain from payoff-relevant punishment as in an ultimatum game to avoid that heterogeneity in beliefs regarding responder behavior in the different treatments influences proposer behavior.

[19] While such survey measures rely on self-reported perceptions that may be 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).

[20] In line with the 7-point scales 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: *Fundamentalists* agree that consumers have lost control over personal information, do not believe that companies handle their data in an appropriate way, and question existing privacy laws. The *Unconcerned* make the opposite statements, while *Pragmatists* hold mixed opinions.

these factors for each question on a 7-point scale in the post-experimental questionnaire. In the main specifications, we consider a piece of information as relevant or unpleasant, respectively, if the participant selects at least a value of 5 on the 7-point scale. The sum of answers whose disclosure is perceived as unpleasant serves as a second indicator of potential hidden costs of information disclose. 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 answers and disclosure decisions in an incentive-compatible way. In particular, subjects receive a bonus of 3.50€ and 1€, respectively, at the end of the experiment if they correctly guessed the other candidate's answer and disclosure decision.[21]

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 of becoming the payoff-determining *allocator*, similar to proposer competition (Roth et al. 1991).[22] Consequently, varying the selection procedure allows us 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 revealed to C. A and B are not informed about this stage beforehand, since doing so would bias initial disclosure choices and hence question our

[21] 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€ for a correct answer guess and 1€ for a correct disclosure guess. One guess is randomly chosen and evaluated for payoff at the end. The payoff from the belief-elicitation part is communicated after the game payoffs and before subjects fill in the post-experimental questionnaire.

[22] 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.

results.[23] The *comparison* stage allows us 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. A and B can adapt their revelation choice, or simply reconfirm their previous one. The previous choice is preselected as the default on the screen, so that for maintaining the previous choice participants just have to click on "proceed".[24] 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.[25]

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 may affect information disclosure and perceived pressure to disclose. 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 collection

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.[26] We oversampled the *strategic* treatments due to our interest in active *allocator* selection by C-participants, resulting in 120 subjects in *random* and 174 subjects in *strategic* treatments.

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* selection of the *allocator* elicits one's intrinsic preference for information revelation without additional incentives, the prospect of gaining *allocator* power might seem worthwhile

[23] See footnote 11 for a discussion. C is not informed about the *comparison* stage to ensure comparability over treatments with and without *comparison*.

[24] 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.

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

[26] Sessions took place within 18 days. There is no indication that participants in later sessions learned from participants in earlier sessions about the experiment and the role of the personality questionnaire in the experiment. Regressing the content of each answer on a chronological session indicator yields no significant time effect (all $p > 0.129$).

to sacrifice some privacy. This corresponds to incurring a cost, likely in form of privacy costs, worth being 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.[27]

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

Reactions to peer *comparison* under *strategic* incentives are likely driven by one's own ex ante disclosure choices relative to those of the competitor, and may thus 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. Reyniers and Bhalla (2013) find such an effect in the context of charitable donations, i.e., under peer comparison, those who initially attempted 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.

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

So far, we have focused on the effect of strategic 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.[28] 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 (Brandts et al. 2009), and should be especially strong when paired with peer *comparison* in SAC (DellaVigna et al. 2012, 2017). Regarding heterogeneity, we expect the increase in pressure in SAC to be driven by the initially disclosure-unwilling, who learn that they lag behind in revelation competition.

[27]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.

[28]See Appendix B.4 for a discussion.

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. Previous research both in experimental settings (Bohnet and Frey 1999; Brandts et al. 2006; Charness and Gneezy 2008; Eckel and Petrie 2011; Gaudeul and Giannetti 2017; Hermstrüwer and Dickert 2017; Holm and Samahita 2018), as well as on microfinance platforms (Böhme and Pötzsch 2010; Ge et al. 2017; Michels 2012; Pope and Sydnor 2011), provides evidence that adding soft, personal information can beneficially influence outcomes. If information overbidding is a way to compete for attention, the extent of personal information sharing likely affects *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): Participants who reveal more information in *strategic* treatments are more likely selected as *allocators*.

While the disclosure of particular information and its content certainly influences *allocator* selection, we refrain from studying individual disclosures and their content due to the large multiplicity of twelve answers with seven outcomes each. Furthermore, we refrain from stating explicit hypotheses regarding the influence of sharing on caring, i.e., from information disclosure on generosity in impunity play, since field evidence for such a relationship is mixed (Duarte et al. 2012; Iyer et al. 2016; Pope and Sydnor 2011).

3. Results

3.1. Descriptive statistics

Table B.1 in the Appendix shows descriptive statistics of our sample. Participants are 55.8% female and on average 24.3 years old. With reference to Westin's privacy index, our sample is roughly split into two halves, privacy "pragmatists" and "fundamentalists". Hardly anyone is classified as "unconcerned".[29] Except for age, statistical tests[30] do not reveal any differences between treatment groups in terms of demographics, privacy preferences, and social-media behavior. Descriptive statistics regarding outcome variables for the restricted sample of *allocator* candidates (role A and B) are summarized in Table B.2 in the Appendix.

3.2. Answers ex ante disclosed

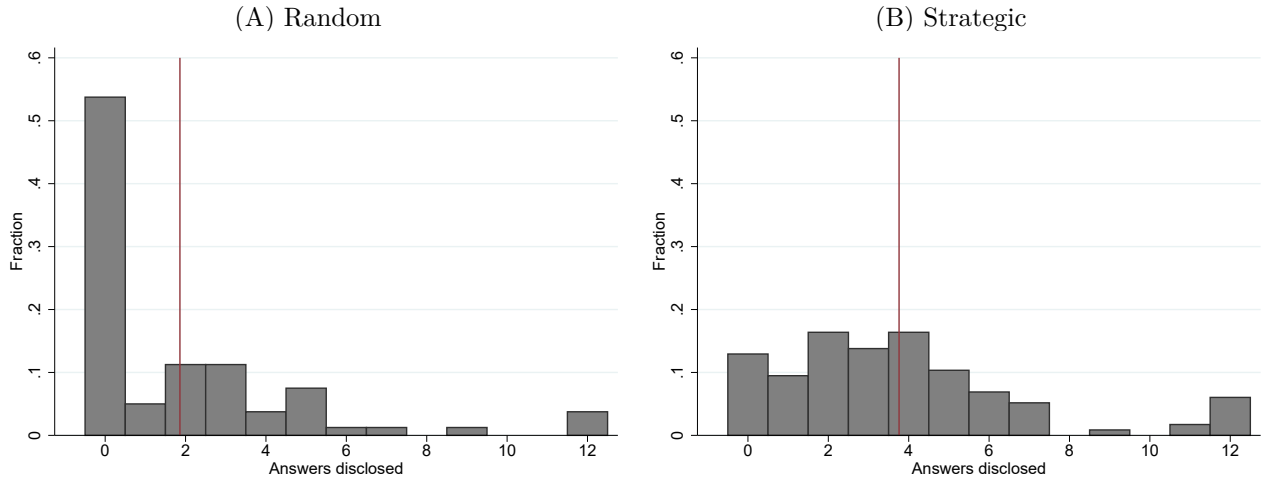
First, we analyze the aggregated amount of information disclosed ex ante, i.e., before social *comparison*. Hypothesis 1 predicts more disclosure in *strategic* treatments. Indeed, participants

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

[30]All statistical tests in this paper are two-sided.

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, in the latter treatment, the majority of 46.6% of observations falls in the range between two and four revelations. A Wilcoxon rank-sum test confirms that the two distributions are statistically different form each other ($p < 0.001$). While the answers that participants give are themselves obviously 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. However, note that there is no difference in answer content between *allocator* candidates who disclose more or less ex ante.[31]

Figure 2: Histograms of answers ex ante disclosed



Notes: Vertical lines represent means.

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. We are interested in β_1, β_1, and β_3 capturing the effect of *strategic* incentives, social *comparison*, and the differential effect of social *comparison* in *strategic* settings, respectively.

Table 3 reports corresponding OLS regression results with reference to the number of answers ex ante disclosed by *allocator* candidates. 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

[31]See Appendix Table B.15. In the Appendix, we also provide histograms of the content of answers in Figure B.1, and Table B.3 reports probit regression results on factors affecting disclosure on question level.

Table 3: 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***
	(0.428)	(0.622)	(0.623)	(0.626)
comparison		0.475	0.669	0.700
		(0.636)	(0.677)	(0.669)
strategic # comparison		-0.027	-0.203	-0.244
		(0.858)	(0.850)	(0.853)
constant	1.862***	1.625***	2.071	0.971
	(0.318)	(0.485)	(1.412)	(1.762)
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 robust standard errors in parentheses. 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.

its interaction with *strategic* in column (2) confirm that there are no initial differences between groups with and without subsequent feedback.

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 that are 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.[32] 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 ex ante in *strategic* than in *random* treatments.

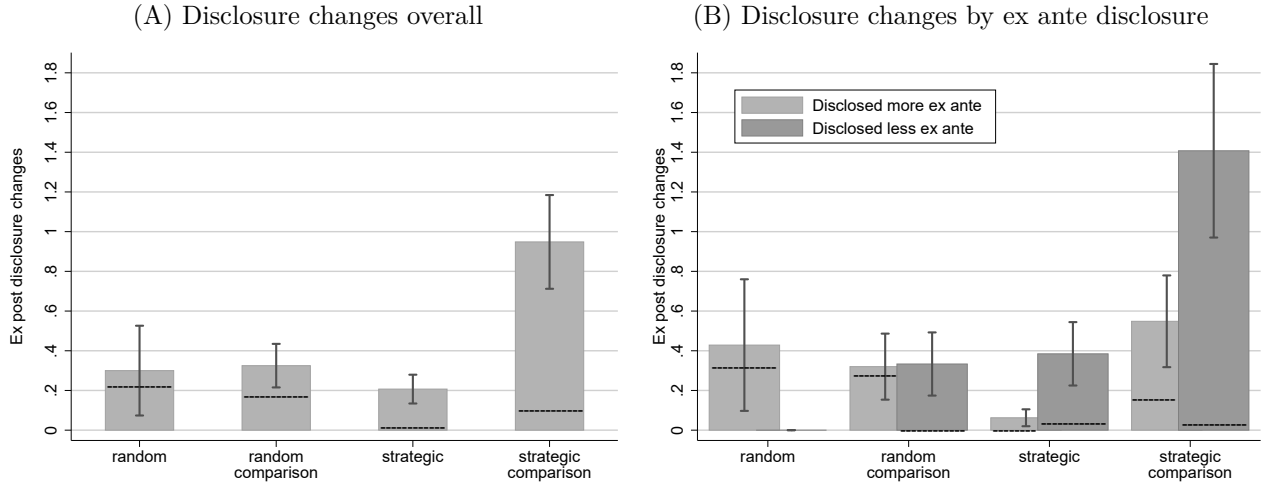
3.3. Ex post disclosure changes

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

[32]Note that we do not observe differential disclosure behavior between Westin privacy types.

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 independently 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. Hypothesis 2 predicts that social *comparison* has a stronger effect under *strategic* incentives in SAC than in RAC without.

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



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

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 fractions 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. Compared to the baseline level of changes in RA, there are not more ex post changes in RAC after social *comparison*. However, substantial changes occur when combining social *comparison* with *strategic* incentives to disclose. Panel B shows that this effect is driven by those initially disclosing less. They make on average 1.4 disclosure changes compared to 0.55 changes of their competitors and to 0.33 changes of candidates lagging behind in *random-comparison* treatments. Overall, in SAC, this results in 5.8 ex post disclosures by ex ante leading candidates compared to 4.7 ex post disclosures of those ex ante lagging behind, a significant difference in a rank-sum test ($p < 0.001$).

Table 4 reports 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 strate-

Table 4: Ex post disclosure changes by treatment

	Ex post disclosure changes					
	(1)	(2)	(3)	(4)	(5) ex ante higher	(6) ex ante lower
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 at 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 ex ante higher / lower candidate is the one who disclosed ex ante at least as many / strictly fewer answers than her competitor.

gic benefits react to peer comparison. The interaction effect of *strategic-comparison* equals at least 0.72 disclosure changes, and even increases in size when including demographic, preference, as well as ex-ante-disclosure control variables in columns (2)-(4).[33] The same holds when performing a 90% Winsorization on treatment level as a robustness check, which can be found in Table B.5 of the Appendix. By adding up 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 and confirms Hypothesis 2. 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 a potential reluctance to disclose personal details.

Separate regressions for subjects ex ante leading or lagging behind in disclosure in columns (5) and (6) further confirm that those subjects adapt their behavior to their environment who

[33]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 ($p = 0.028$). One's own ex ante disclosure, i.e., the absolute disclosure level, and the disclosure difference to the competitor, i.e., the relative disclosure, in column (4) do not significantly affect adaptation behavior.

are initially rather unwilling to disclose information.[34] The *strategic-comparison* interaction effect is significant for lower candidates. Those who learn that they disclose more information ex ante do not see the need to react to peer comparison, while those who realize that they lag behind revise their ex ante disclosure choice in light of peer comparison and competition.[35] This provides evidence for Hypothesis 3.

Result 2: Peer *comparison* induces significantly more ex post disclosure changes under *strategic* incentives than without. This effect is driven by those who disclose less ex ante.

In order to understand ex post disclosure changes better, we also investigate the relevance and direction of ex post disclosure changes. First, is the shared information relevant, i.e., perceived as meaningful for *allocator* selection and allocation behavior? In columns (1)-(4) of Table B.6 in the Appendix, we only consider ex post disclosure changes of answers which player C considers as relevant indicators for game behavior. Results reveal a similar picture to that in the main analysis. The *strategic-comparison* interaction term is significant, independently of how relevance is defined, suggesting that peer comparison indeed fosters the disclosure of more relevant answers.

Second, we examine the direction of disclosure changes, which can be inferred from Figure 3 by looking at the horizontal division lines of the bars. Moreover, we analyze 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 SAC, in particular, 89.1% of all changes are upward changes, and 85.5% are adaptations. For those lagging behind, even 97.4% of changes are extensions. For a detailed analysis and corresponding regression results, see Table B.7 in the Appendix. Two important aspects prevail: First, we follow peers in what we disclose, which 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.

3.4. Hidden cost of information disclosure

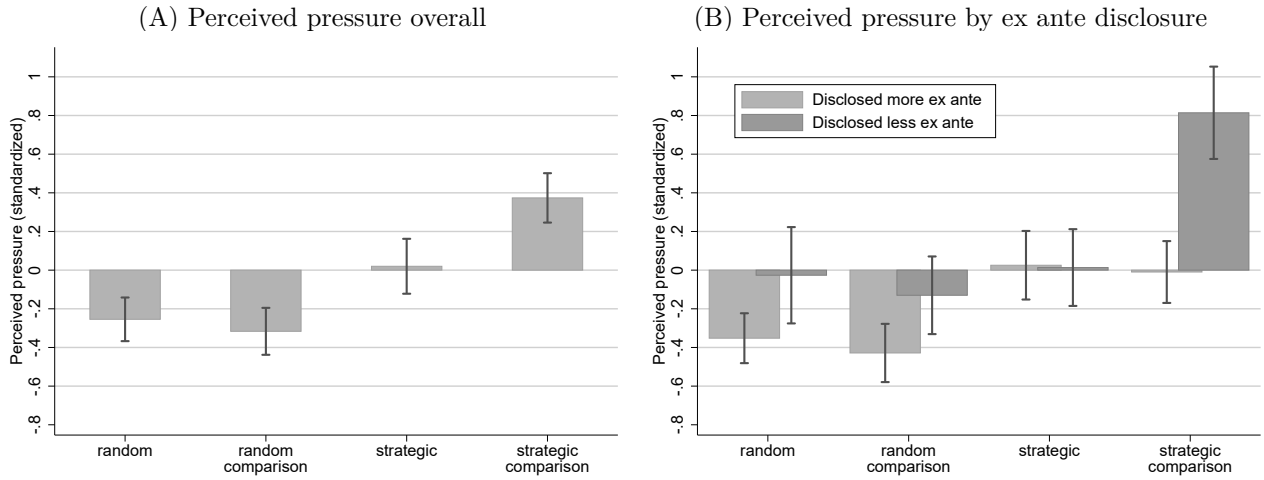
Are there potential hidden costs of extensive personal information disclosure, in particular pressure to disclose? In SAC, particularly those who reveal less ex ante widen their disclosure

[34]Since our two-by-two design already requires an interaction regression when analyzing the full sample, a further interaction for heterogeneous groups would result in triple interaction. In order to maintain interpretability of results, we use sample splits instead. By splitting the sample, one loses statistical power in the regression analysis, resulting in significance only at the 10% level.

[35]Note that there are also some changes in *comparison* even without *strategic* incentives to disclose. These changes are bi-directional. Some subjects in RAC, who learn that they disclosed more, reveal less information, which can be inferred from most ex post disclosure changes of higher candidates lying below the horizontal line in Panel B of Figure 3. In contrast, lower candidates expand their disclosure, so both groups converge towards 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.

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 with and without *comparison* is similarly low and lies between -0.32 and -0.25 standard deviations. The corresponding *comparison* regression coefficient, distinguishing the pure peer effect, is insignificant and small in magnitude in all regression specifications displayed in columns (1)-(3) of Table 5. Thus, social comparison per se does not seem to trigger pressure to share information.

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



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

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, it is large in magnitude. If we winsorize the data by 90% on treatment level, shown in Table B.13 of the Appendix, this finding is robust and becomes significant at the 10% level already without any controls. This provides directional evidence in line with Hypothesis 4b. However, we do not observe a statistically significant increase in pressure from *strategic* incentives alone. 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 on conventional levels ($p = 0.131$). Unlike Brandts et al. (2009), we cannot confirm that competition per se has detrimental effects, and cannot confirm Hypothesis 4a.

As for ex post disclosure changes, it is subjects realizing in SAC to have disclosed less ex ante who feel most pressured. The effect size is large, at 0.81 standard deviations, compared to the standardized average of zero, as Panel B of Figure 4 displays. Separate regressions for candidates with ex ante higher or lower disclosure show a significant *strategic-comparison* interaction effect in the lower candidate subsample in column (5) of Table 5. Therefore, we infer from our results that observing to lag behind in personal information revelation under

Table 5: Perceived pressure to disclose information by treatment

	Perceived pressure				
	(1)	(2)	(3)	(4) ex ante higher	(5) ex ante lower
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 at 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. The ex ante higher / lower candidate is the one who disclosed ex ante at least as many / strictly fewer answers than her competitor.

competition is perceived as more compelling. This supports Hypothesis 4c.

Result 3: 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.

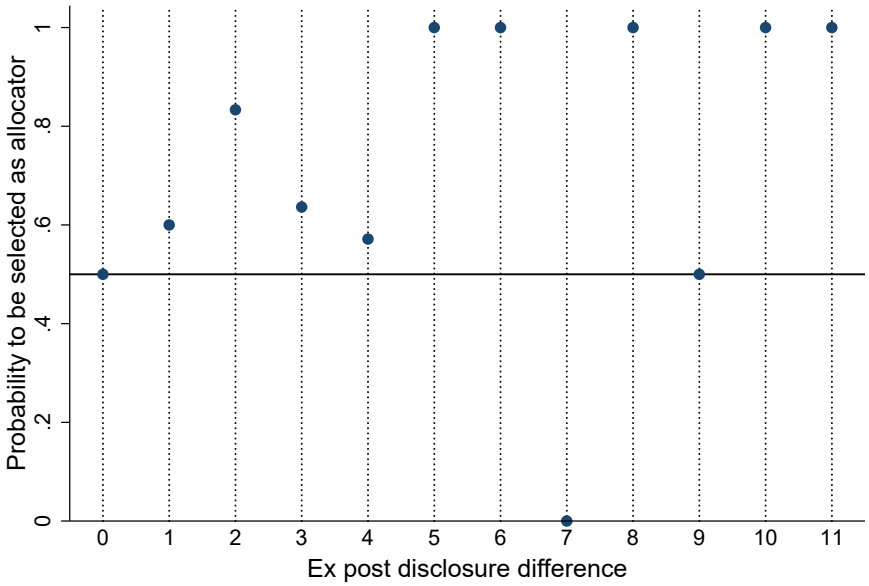
As a second indicator for hidden cost of information disclosure, we additionally focus on the disclosure of information, whose revelation participants consider as unpleasant. Their correlation with all ex post disclosure changes, as well as with relevant disclosure changes, is quite high, at 0.663 and 0.560, respectively. In accordance, regression results in columns (5)-(8) of Table B.6 in the Appendix uncover that the combination of strategic incentives and peer comparison causes unpleasant disclosure changes. The *strategic-comparison* interaction effect is significant at the 10% and 5% level when not including or including controls, respectively. This means that peer comparison and strategic incentives trigger the revelation of information which participants do not like to disclose. Hence, while disclosure changes may have a beneficial effect on outcomes via making more relevant information available, they also come along at the cost of causing discomfort.

3.5. Effects of information disclosure on outcomes

***Allocator* selection based on information disclosure**

In this section, we briefly analyze how personal information disclosure affects the probability of becoming the *allocator*. *Allocator* candidates seem to assume that C takes personal information into account for *allocator* selection, since they disclose more information in *strategic* treatments. Indeed, participants in role C look at the information provided. On average, they investigate 10.5 out of 12 answers disclosed ex post, and in 79.6% of the cases all answers.[36]

Figure 5: Probability of becoming *allocator* by difference in information disclosure



Notes: Probability of becoming *allocator* for each ex post disclosure difference. Only candidates disclosing ex post at least as many answers as their competitors are displayed.

Figure 5 illustrates that disclosing more information than the other candidate indeed increases the likelihood of becoming *allocator* in *strategic* treatments.[37] The dots represent the probability to be selected as *allocator*, conditional on the ex post difference in answers disclosed relative to one's competitor. It prevails that the probability to be selected is systematically above 50% if one is ahead in information disclosure. While 25.9% of *allocators* stem from the group of participants disclosing less ex post, suggesting that what has been disclosed 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 ($p = 0.004$). There is no such difference in treatments with *random allocator* selection ($p = 0.988$). In sum, this suggests that the pure amount of personal information sharing can impact how much attention one

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

[37] Since C is not informed about the comparison stage, we can ignore the *comparison* dimension and pool observations.

receives from others, so engaging in competition via information overbidding seems to pay off for disclosure-willing individuals.

When restricting our analysis to information which C considers as relevant indicators for allocation behavior, the effect that more sharing raises the likelihood of being selected increases. The corresponding probit regression analysis in Table B.8 of the Appendix confirms these findings and substantiates Hypothesis 5.[38]

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

Allocation behavior

While revealing more information is beneficial for becoming the *allocator*, is this role also beneficial in terms of payoff? OLS regressions in Table B.9 in the Appendix confirm that this is true. When dividing the 17€ pie themselves, subjects earn almost three times as much as they would receive from their matched competitor. Instead of the average payoff of 3.40€ from their competitor, they keep 9.73€ for themselves. Hence, becoming an *allocator* is highly beneficial in terms of game payoff.

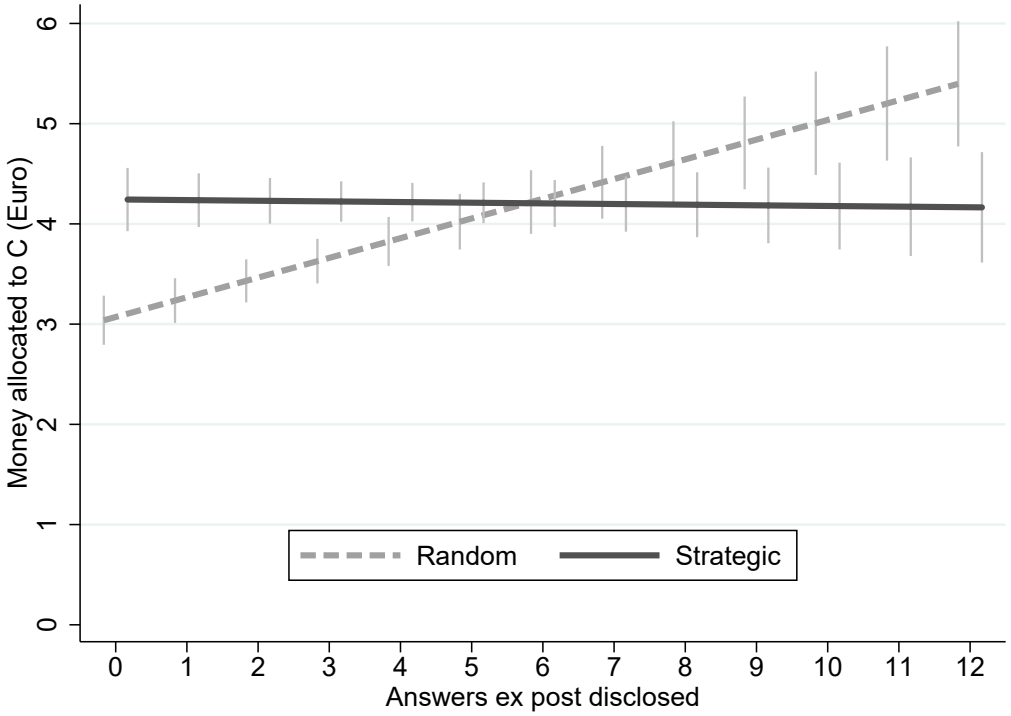
Is *allocator* selection based on information disclosure competition also beneficial for the other market side, i.e., players C selecting the *allocator*? In order to explore this, we look at allocation behavior conditional on personal information sharing. We measure prosocial behavior by the amount one gives to C. We summarize the main effects here and refer to Table B.10 in the Appendix for a more detailed analysis.

First of all, and in line with Brandts et al. (2006),[39] 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 seems to be a positive relationship between intrinsic disclosure willingness and generosity in *random* treatments. This can be inferred from the slope in Figure 6. However, this positive relationship is distorted by revelation competition in *strategic* treatments. Subjects disclosing more personal information in *strategic* treatments do not offer more to player C. Hence, there is no *direct* effect of disclosure behavior on prosocial behavior in treatments with *allocator* selection. In the corresponding OLS regression specification in Table B.10 in the Appendix, this means that the significant effects of the amount disclosed ex post and its interaction with the *strategic* coefficient cancel out. Third, there is no *indirect* effect on prosocial behavior from being chosen as an *allocator* as a result of one's disclosure. Those who become *allocators* in *strategic* treatments do not offer significantly more

[38] As a corollary to this finding, it is worth pointing out that participants who disclose less ex ante also most often 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 rank-sum test ($p = 0.698$).

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

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



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

to player C than non-selected candidates. Thus, participants in role C do not suffice in picking the more generous candidates based on disclosed personal information. Statistical support based on OLS regression results for all the findings discussed above can be found in Table B.10 in the Appendix. Note that we also find that subjects disclosing more in *strategic* treatments do not behave more generously when investigating the interaction of *strategic* incentives with ex ante instead of ex post disclosures. Hence, the loss of information value we observe stems from strategic incentives and, if anything, is only fostered by the feedback and revision possibilities. We do not find a significant impact of social *comparison*.

While there is no positive effect of the total amount of information disclosed ex post, the sum of disclosures becomes meaningful if we focus only on those answers which C rates as relevant for allocation behavior. Regressions in Table B.11 in the Appendix repeat the previous analysis only with the disclosure of relevant answers. Here, more disclosure indeed implies more generosity, i.e., *allocators* disclosing more relevant answers distribute more money to player C and less money to themselves. Taking together the findings on overall and relevant ex post disclosure, there appears to be a positive relationship between disclosing more and being more generous, but sharing irrelevant information under *strategic* incentives obfuscates this relationship. Thus, competition for benefits might lead to more information sharing, and hence to more meaningful information diffusion, but also to less straightforward signaling due to information abundance.

The Appendix also reports acceptance thresholds from the impunity game. 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 too little. Even though altruistic sanctioning in monetary terms is excluded, respondents often engage in non-monetary altruistic accusation via choosing a positive acceptance threshold.

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 new channel leading to more and more information sharing as observed in the field. Moreover, we provide evidence on a potentially neglected hidden cost in such a context, i.e., an increase in perceived pressure to disclose for those who are intrinsically less willing to share information about themselves. Our setting resembles modern markets, for example online portals like Airbnb and LinkedIn or microfinance or crowdfunding platforms like Kickstarter, in which one market side strives for another's beneficial attention by providing personal information. 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 abundant documents, but is exacerbated by online markets.

In our lab experiment, participants can 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 the extent to which information sharing can serve a strategic purpose and analyze how it is influenced by peer comparison. The 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 revealed less, a priori, than their competitors. It goes along with an increase in perceived pressure to have to disclose information and with the disclosure of more unpleasant information. We find that more disclosure-willing individuals are more frequently picked, but that the abundance of information shared under incentives obfuscates the positive relationship between sharing more relevant information and desirable behavior.

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 may 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, may shed light on a new 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, and the sharing of more information, which is perceived as unpleasant, provide indicative evidence of a potential hidden cost 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 adapt, 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 extensive personal information sharing is beneficial for the disclosure-willing market side, but provides only limited insights for the selecting one, is in line with evidence from the field (Iyer et al. 2016; Michels 2012; Pope and Sydnor 2011). In our setting, incentives to share more personal information obfuscate the positive relationship between the number of relevant 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 highlighting the qualitatively most suitable options. The recent introduction of "superhosts"[40] on Airbnb might be a result of such information overbidding and questions the usefulness of extensive information disclosure. As a consequence, personal information sharing may not be caring.

Although our study provides helpful insights into a new channel explaining 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, in part jointly and partly in isolation, affect 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.

[40] "Superhost" is a rating of excellence on Airbnb which has been subsequently introduced and highlights hosts who fulfill certain quality standards. They visually stand out and can be searched explicitly. Such an additional rating 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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Appendix A. 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 not ask any more questions loudly, and do not communicate with other participants in the experiment. If you break this rule, we will 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 the 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 completely. If you do not agree with 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 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 participant should decide on the distribution of 17 Euro between all three group members. In what follows, we call the player who makes this decision the *allocator*. 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 roles 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 to 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, independently 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.

Appendix B. Additional tables and results

Appendix B.1. Descriptive statistics of answers and outcomes

Table B.1: 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	

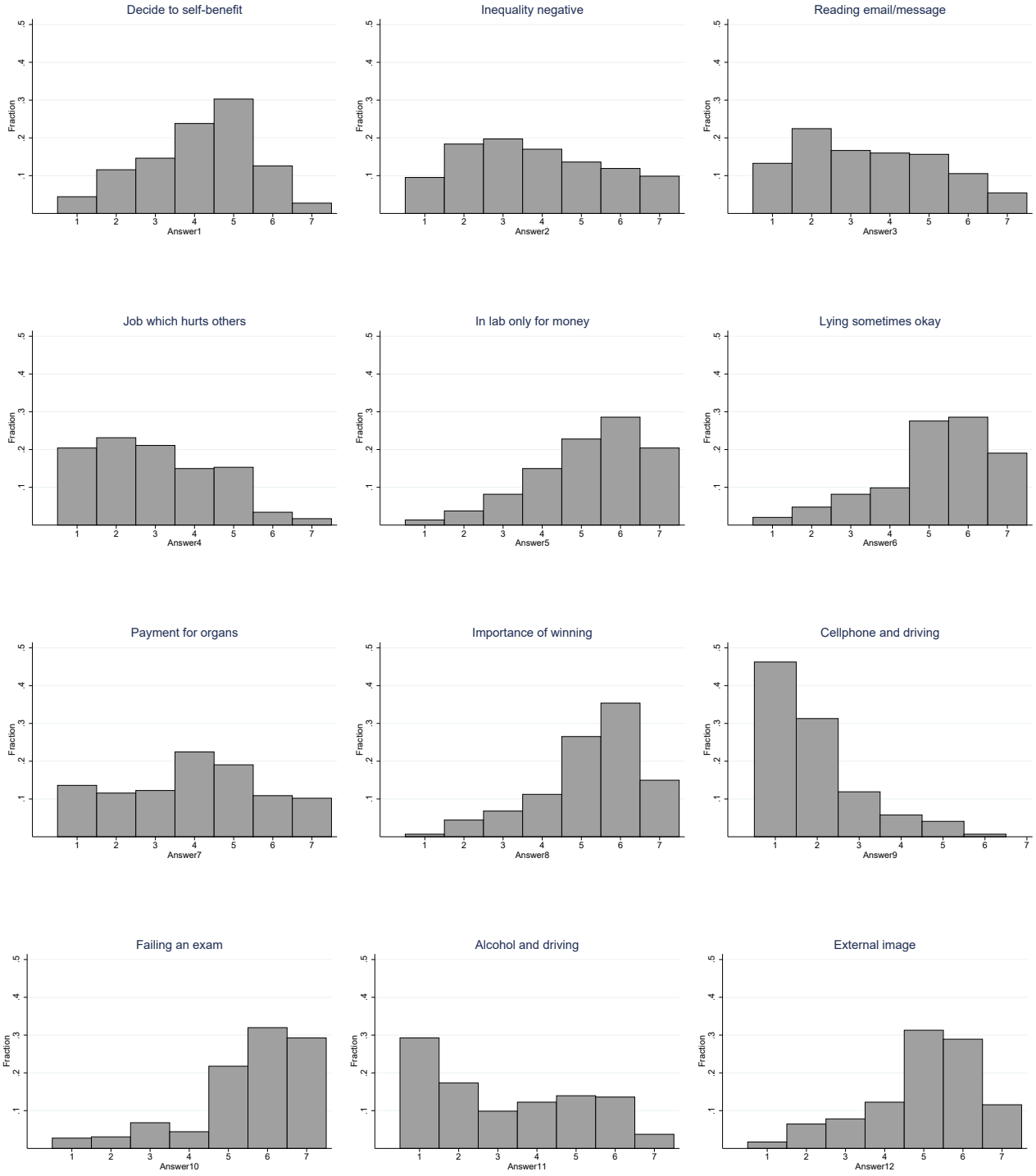
Notes: p-values in last column show accuracy of randomization into treatments based on individual characteristics, and stem from Kruskal-Wallis tests for age, ability compare, and opinion compare, and from Fisher's exact tests otherwise. "Westin Fundamentalist", "Westin Pragmatist", and "Westin Unconcerned" correspond to privacy preference types according to Westin's privacy index. "Profile public" and "profile identifiable" are dummies representing public access and identifiability of subjects' profiles in social media. "Ability compare" and "opinion compare" are dimensions of the INCOM social comparison index.

Table B.2: 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)
Relevant ex post disclosure changes	0.20	0.05	0.12	0.53
	(0.853)	(0.221)	(0.422)	(1.168)
Unpleasant ex post disclosure changes	0.10	0.30	0.26	0.52
	(0.632)	(0.158)	(0.256)	(0.522)
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. "Answers ex ante disclosed" corresponds to the sum of revelations before the *comparison* stage, "ex post disclosure changes" to the absolute number of changes made after the *comparison* stage. "Unpleasant disclosure changes" sums up changes rated at least 5 on a 7-point scale (7 = very unpleasant) by the individual, "relevant ex post disclosure changes" sums up changes the selecting party perceives as relevant for game behavior. "Own payoff" and "C's payoff" are the shares of the 17€pie the *allocator* would give to herself and the selecting party in the impunity game. "Acceptance threshold" corresponds to the minimal offer which a subject does not reject.

Figure B.1: Histograms of answers



Notes: Answers on 7-point scale (1 worst, 7 best).

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

Factors affecting disclosure at answer level

As reported in probit regressions in Table B.3, several factors seem to affect the probability of revealing a particular answer from the questionnaire. Of course, the answer one has given significantly affects disclosure for most questions. Moreover, the perceived relevance for predicting subsequent allocation behavior increases the probability of disclosing the answer. In contrast, a feeling of discomfort to reveal a particular answer decreases it. The *strategic* coefficient captures which answers participants more likely disclose under disclosure incentive. All answers are disclosed significantly more often in *strategic* treatments, except answers three, five, six, and ten.

Table B.3: Probit regressions - Disclosure-affecting factors at 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 at group level. Control dummies for the ten different randomizations of questions used included.

Relevance and discomfort of ex post disclosure changes

Table B.4: 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 at 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.

Table B.5: 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 at 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 B.6 reports OLS regression results regarding the perceived relevance and discomfort of ex post disclosure changes. Patterns are similar to the main results. The *strategic-comparison* interaction effect is significant in all specifications with and without controls, both with respect to relevant and unpleasant disclosure changes, and independent of the definition of relevant and unpleasant changes used. Hence, while subsequent peer comparison under strategic incentives leads to the disclosure of more relevant information, it also makes participants reveal information that they perceive as particularly unpleasant and costly.

Table B.6: Relevance and discomfort of ex post disclosure changes by treatment

	Relevant Changes				Unpleasant Changes			
	(1) >=5	(2) >=5	(3) >=4	(4) >=6	(5) >=5	(6) >=5	(7) >=4	(8) >=6
strategic	-0.079	-0.067	-0.041	-0.072	-0.031	-0.075	-0.060	-0.059
	(0.142)	(0.136)	(0.144)	(0.136)	(0.104)	(0.093)	(0.108)	(0.046)
comparison	-0.150	-0.171	-0.132	-0.164	-0.075	-0.109	-0.105	-0.040
	(0.135)	(0.136)	(0.156)	(0.136)	(0.102)	(0.096)	(0.108)	(0.051)
strategic # comparison	0.564***	0.601***	0.537**	0.458**	0.213*	0.261**	0.291*	0.140*
	(0.211)	(0.224)	(0.243)	(0.214)	(0.126)	(0.131)	(0.159)	(0.074)
constant	0.200	-0.098	-0.135	-0.153	0.100	0.004	-0.120	0.119
	(0.131)	(0.341)	(0.346)	(0.323)	(0.099)	(0.186)	(0.272)	(0.110)
controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
N	196	196	196	196	196	196	196	196
R2	0.059	0.133	0.118	0.115	0.025	0.103	0.108	0.135

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table reports OLS regression coefficients with standard errors in parentheses clustered at group level. Controls include gender and age, as well as dummies for "Westin fundamentalist", publicly accessible and identifiable social-media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM. Greater or equal signs indicate which definition is used for a disclosure change to be considered as relevant or unpleasant on a 7-point scale (7 = highest).

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 mimics the competitors' revelation. The direction of changes can be inferred from Figure 3 in the main analysis, and Table B.7 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 disclosed ex ante, but hidden ex post.

Two main messages follow from this analysis. First, looking at columns (1) and (2) of Table B.7, 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 very high at 0.84, and the interaction effect in column (1) of Table B.7 for adaptations is similar in magnitude

Table B.7: 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 at 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.

to that in column (1) of Table 4 for ex post disclosure changes. Testing for similarity of the 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, which can be regarded as an *intensive margin*.

Secondly, reported in columns (3)-(6) of Table B.7, 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 that 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.

Appendix B.3. Further results: Allocator *selection and distribution outcomes*

Allocator selection

Table B.8: 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 predictors of game behavior. "Red" and "blue displayed first" are dummies if the candidate's color is red and if 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 B.8 shows the results of probit regressions with the probability of being selected as the *allocator* as the dependent variable. Since active *allocator* selection only takes place in *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 answers disclosed ex post affects the probability of being 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 3.0 percentage points higher probability of being selected for each additional answer disclosed.[41] 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

[41] 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.

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 of being selected for each relevant answer one discloses more than the competitor. Focusing on disclosures considered as relevant for game behavior indicates that C indeed tries to select the most generous candidate. Overall, disclosing more answers seems beneficial for being selected according to our experimental data, supporting Hypothesis 5, especially if the question is considered as relevant.

Allocation behavior

In order to check whether becoming *allocator* is beneficial in terms of payoff, Table B.9 compares payoffs if subjects become *allocators* or not. Since we elicit payoff allocations before revealing who becomes *allocator* via the strategy method, for each subject we can calculate a payoff for the case of becoming *allocator*, and one if not. The latter is the payoff allocated to her by her matched competitor if the competitor becomes *allocator* instead. We compare these two possible payoffs based on whether a subject decides herself about the payoff distribution. Column (1) shows a large and highly significant effect from determining the allocation oneself. Compared to a 3.40€ payoff from the competitor, subjects receive 6.33€ more if they determine the allocation themselves. This result is robust if controls are included in column (2). If we additionally include a dummy variable for *strategic* treatments and interact it with whether the payoff is self-determined or not in columns (3) and (4), the self-determination effect even increases in magnitude. This means that becoming *allocator* is highly beneficial in terms of payoff.

Table B.10 presents results on how information disclosure in different treatments carries over to 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.[42] 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, staying in the range of 80 to 90 cents which C earns more on average. This means that previous social *comparison* does not affect subsequent distribution behavior, and seems not to be detrimental to prosociality in our setting. Therefore, we stick with data pooled over *comparison* when investigating allocation behavior in more detail.

Are the 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 to be selected as *allocators*, we investigate whether C benefits from this selection

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

Table B.9: Payoff if determined by oneself or allocated by competitor

	Own payoff			
	(1)	(2)	(3	(4)
self-determined	6.327***	6.327***	7.038***	7.038***
	(0.407)	(0.411)	(0.642)	(0.648)
strategic			0.187	0.217
			(0.278)	(0.284)
self-determined # strategic			-1.201	-1.201
			(0.825)	(0.833)
constant	3.398***	3.190***	3.288***	3.157***
	(0.136)	(0.839)	(0.215)	(0.846)
controls	No	Yes	No	Yes
N	392	392	392	392
R2	0.537	0.548	0.544	0.554

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table reports OLS regression coefficients with standard errors in parentheses clustered at group level. Each subject is included once as if determining the allocation decision herself and once as if receiving the payoff from her matched competitor. 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.

strategy. Although the interaction effect of the *strategic* and the *allocator* variable in columns (3) of Table B.10 point in the direction of more prosociality, it is not significant ($p = 0.189$), i.e., selected *allocators* do not systematically behave more generously. In Table B.14, we report further results on the question level regarding which answers predict allocation behavior, and to which extent these answers are taken into account for *allocator* selection.

Column (4) of Table B.10 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 on prosociality in opposite directions a) with respect to the number of answers disclosed ex post alone and b) with respect to the number of answers' interaction with the *strategic* coefficient. 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 voluntary information disclosure for proso-

Table B.10: Payoff allocations by treatment and disclosure behavior

	Own payoff					
	(1) ex post	(2) ex post	(3) ex post	(4) ex post	(5) ex post	(6) ex ante
strategic	-1.015*	-1.062	-0.329	-1.967**	-1.718**	-1.829**
	(0.551)	(0.823)	(0.772)	(0.756)	(0.817)	(0.757)
comparison		-0.750				
		(0.851)				
strategic # comparison		0.095				
		(1.098)				
allocator			-0.250			
			(0.784)			
strategic # allocator			-1.371			
			(1.027)			
answers disclosed				-0.379***	-0.398***	-0.224
				(0.118)	(0.129)	(0.139)
strategic # answers disclosed				0.445***	0.421**	0.329*
				(0.164)	(0.173)	(0.187)
constant	10.325***	10.700***	10.450***	10.997***	11.332***	10.743***
	(0.427)	(0.676)	(0.562)	(0.467)	(1.799)	(0.494)
controls	No	No	No	No	Yes	No
N	196	196	196	196	196	196
R2	0.018	0.027	0.047	0.048	0.078	0.034

	C's payoff					
	(1) ex post	(2) ex post	(3) ex post	(4) ex post	(5) ex post	(6) ex ante
strategic	0.828***	0.884**	0.460	1.205***	1.058**	1.215***
	(0.288)	(0.413)	(0.421)	(0.394)	(0.430)	(0.397)
comparison		0.475				
		(0.430)				
strategic # comparison		-0.113				
		(0.573)				
allocator			0.075			
			(0.406)			
strategic # allocator			0.735			
			(0.556)			
answers disclosed				0.197***	0.209***	0.126*
				(0.058)	(0.063)	(0.073)
strategic # answers disclosed				-0.203**	-0.192**	-0.166*
				(0.086)	(0.091)	(0.098)
constant	3.387***	3.150***	3.350***	3.039***	2.766***	3.154***
	(0.217)	(0.336)	(0.292)	(0.240)	(0.946)	(0.255)
controls	No	No	No	No	Yes	No
N	196	196	196	196	196	196
R2	0.040	0.051	0.064	0.066	0.095	0.055

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table reports OLS regression coefficients with standard errors in parentheses clustered at group level. Columns (1)-(5) refer to ex post disclosures, (6) to ex ante disclosures. 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.

Table B.11: Payoff allocations by treatment and relevant disclosure behavior

	Own payoff			
	(1) >=5	(2) >=5	(3) >=4	(4) >=6
strategic	-0.566	-0.338	-0.506	-0.400
	(0.578)	(0.622)	(0.619)	(0.620)
relevant answers ex post disclosed	-0.294*	-0.367**	-0.204	-0.410**
	(0.165)	(0.166)	(0.153)	(0.179)
strategic # relevant answers ex post disclosed	-0.076	-0.074	-0.199	-0.114
	(0.310)	(0.305)	(0.319)	(0.295)
constant	10.601***	10.459***	10.529***	10.551***
	(0.454)	(1.764)	(1.771)	(1.766)
controls	No	Yes	Yes	Yes
N	196	196	196	196
R2	0.038	0.077	0.061	0.075

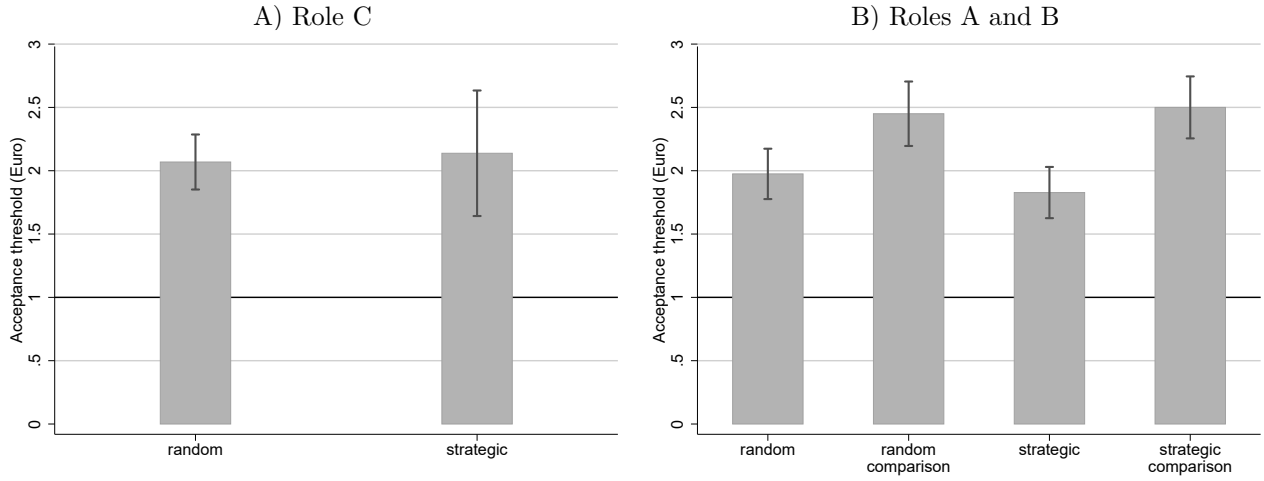
	C's payoff			
	(1) >=5	(2) >=5	(3) >=4	(4) >=6
strategic	0.466	0.325	0.409	0.367
	(0.297)	(0.319)	(0.318)	(0.316)
relevant answers ex post disclosed	0.197**	0.235***	0.147*	0.268***
	(0.084)	(0.085)	(0.077)	(0.090)
strategic # relevant answers ex post disclosed	0.239	0.243	0.300	0.284
	(0.182)	(0.193)	(0.208)	(0.200)
constant	3.203***	3.257***	3.216***	3.207***
	(0.231)	(0.909)	(0.913)	(0.909)
controls	No	Yes	Yes	Yes
N	196	196	196	196
R2	0.083	0.120	0.103	0.119

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table reports OLS regression coefficients with standard errors in parentheses clustered at 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. Greater or equal signs indicate which definition is used for a disclosure change to be considered as relevant on a 7-point scale (7 = highest).

cial behavior. Figure 6 in the main text shows the corresponding coefficient plot. The pattern holds with controls included in column (5) of Table B.10 and also if we look at the interaction of *strategic* incentives with ex ante disclosures in column (6), i.e., before social *comparison*. 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. The positive relationship between intrinsic information sharing and generosity remains when winsorizing ex post disclosures at the 90%-level in columns (3)-(6) of Table B.13. We conclude that strategic incentives enhance information disclosure, but question the quality of what is revealed, thereby creating a loss of information value.

When instead looking at the relationship between generosity and those disclosed answers, which C considers as relevant indicators of game behavior in Table B.11, the amount of disclosure becomes meaningful. As in the previous analysis, there is a significantly positive (negative) effect on the amount the *allocator* gives to C (herself). In contrast, there is no longer a significantly negative interaction between the amount of disclosure and the *strategic* coefficient. This means that *strategic* incentives do not distort the positive relationship between disclosing more and being more generous when it comes to relevant information disclosure. Taken together, this implies that information has to be screened more carefully regarding its relevance if incentives for disclosure are involved.

Figure B.2: Coefficient plot of acceptance thresholds by role and treatment



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

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 B.2 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 information set when stating her acceptance threshold, as she already knows who becomes *allocator* at that point in time. Moreover, Panel A consists of only two bars, 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 subjects are willing to forgo some money in our experiment when being offered too little.

Table B.12: 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 at group level.

We try to disentangle what drives the high acceptance thresholds in simple OLS regressions displayed in Table B.12, 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 provides zero explanatory power ($R^2 = 0.000$). In contrast, social *comparison* turns out to push the acceptance threshold upwards. While this effect turns out 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 conclusions regarding acceptance behavior in the impunity game.

Selection and allocation outcomes by answers

Table B.14 reports how answers translate into outcomes. Regarding allocation behavior in *strategic* treatments, answers 1, 9, 10, and indicatively also answer 2, turn out 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 model in columns (2) finds significant effects of answers 1 and 2 on the probability of being 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 reference to question 1, its pure disclosure seems to matter more than its content.

Table B.13: Robustness to Winsorization - Perceived pressure and allocation behavior

	Pressure		Own payoff		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 at 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% at treatment level. Pressure reported in standard deviations, payoffs in €.

In addition, at the 5% significance level, disclosure of questions 5, 8, and 12 appears to be important for C's *allocator* selection decision, as well as the content of questions 3, 8, and 12.

Appendix B.4. Discussion

Our analysis of disclosure behavior highlights 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 of becoming the *allocator*. However, we argue in the following why a pure information-provision argument cannot fully explain our results.[43] Moreover, we provide evidence that there are no systematic differences with respect to content of the given answers between those who disclose more or less information, respectively.

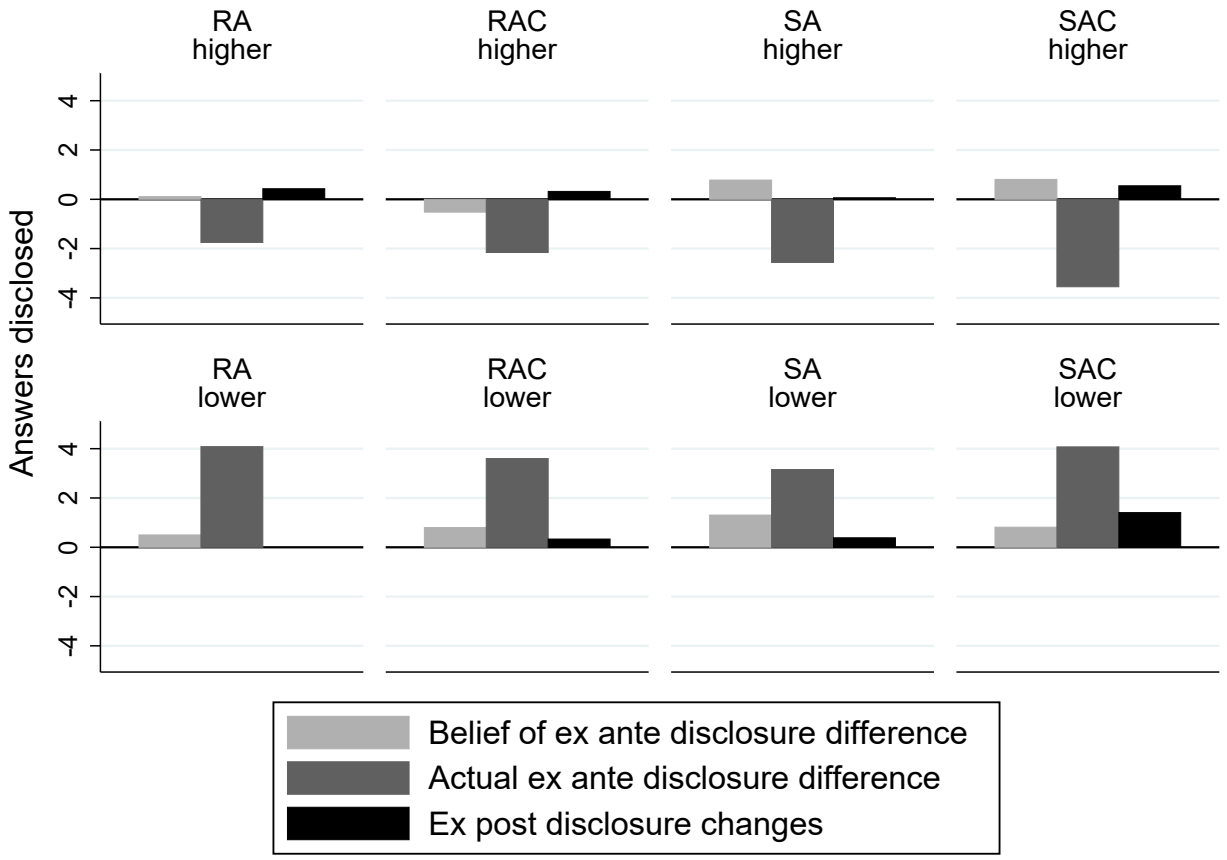
[43]While we cannot completely rule out that the high perceived pressure in SAC stems from receiving information in a competitive setting per se and is not related to the privacy component of our data, significant pressure seems to exist under such conditions, at least if personal information is at work.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

Table B.14: 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 of becoming *allocator* based on question-level disclosures and answers. 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 at 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).

Figure B.3: Disclosure behavior and beliefs by treatment and ex ante disclosure



Notes: The ex ante higher / lower candidate is the one who disclosed ex ante at least as many / strictly fewer answers than her competitor.

Ex post disclosure changes could be driven by wrong beliefs about the competitor's disclosure and by social pressure. 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 lag behind ex ante. 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 by reducing the distance to the competitor. 44.4% of participants do not react at all when learning about the disclosure choice of their peer. This type likely 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 B.3, are correct: They expect their competitor to disclose more information than they do themselves,

as the first bars of "SA lower" and "SAC lower" show. 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 the final choice. Thus, wrong beliefs about how much disclosure is necessary to become *allocator* seem an unlikely explanation for discrepancies in disclosure. Rather, privacy costs may hinder those who lag behind from catching up with those having lower privacy costs. In effect, additional information under social comparison rather 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 lower" of Figure B.3 to the second bar). Thus, the majority of disclosure changes are unlikely to occur in order to outbid the competitor. The candidate lagging behind reveals somewhat more, but more often than not refrains from jumping ahead in disclosure. This highlights peer pressure in information disclosure competition as a more likely driver of the personal information disclosure dynamics we observe.

Overall, our results indicate that there seem to be substantial restrictions that make subjects refuse to share their personal information despite being aware of lagging behind in information-disclosure competition. On the one hand, for some subjects, this might be the content of the information. Subjects not only need to consider the number of questions that they reveal, but also their answers to these questions. If they expect their personal data to send a bad signal, they may prefer to refrain from disclosing their information. On the other hand, some subjects may experience a general unwillingness to share their data, independently of how good or bad their information is, as a form of privacy cost. In order to distinguish these two explanations, we check for systematic differences between the content of answers given by those *allocator* candidates who disclose ex ante more or less information, respectively. Table B.15 in the Appendix reports results from corresponding OLS regressions at question level. Neither for the full sample nor for the restricted sample of subjects in *strategic* treatments are there any significant differences in answer content between subjects who disclose more or less ex ante, except for question 7.[44] Hence, for 11 out of 12 answers, there are no systematic differences in terms of content of the answers one could reveal. We infer from these results that different content and thus different signals are not driving disclosure behavior.

[44]Question 7 reads "Should people who voluntarily donate an organ receive payment for it?" and is not related to generosity. While this answer has a marginally significant impact on whether a subject becomes the *allocator* in the regression with both disclosure and answer content included in Table B.14 in the Appendix, it is not significant without controlling for disclosure and is not predictive for allocating more money to player C in the distribution stage.

Table B.15: Answer scores by more / less initial disclosure on question level

	Answer to question (x)					
	(1)	(2)	(3)	(4)	(5)	(6)
lower candidate	0.180	-0.112	0.167	0.087	-0.339	0.003
	(0.196)	(0.245)	(0.282)	(0.236)	(0.233)	(0.222)
constant	4.618***	4.056***	3.541***	2.956***	4.878***	5.207***
	(0.296)	(0.273)	(0.417)	(0.340)	(0.390)	(0.313)
N	196	196	196	196	196	196
R2	0.057	0.053	0.083	0.080	0.055	0.045
	(7)	(8)	(9)	(10)	(11)	(12)
lower candidate	-0.869***	-0.040	0.151	0.057	0.082	0.111
	(0.293)	(0.212)	(0.156)	(0.254)	(0.307)	(0.202)
constant	4.268***	5.145***	1.966***	5.097***	3.459***	4.986***
	(0.469)	(0.282)	(0.261)	(0.311)	(0.475)	(0.285)
N	196	196	196	196	196	196
R2	0.080	0.036	0.036	0.028	0.030	0.019
	Answer to question (x) - Strategic treatments only					
	(1)	(2)	(3)	(4)	(5)	(6)
lower candidate	0.302	0.079	0.366	0.111	-0.078	0.076
	(0.254)	(0.332)	(0.378)	(0.319)	(0.315)	(0.282)
constant	4.661***	4.023***	3.380***	2.632***	4.289***	4.900***
	(0.359)	(0.302)	(0.544)	(0.260)	(0.426)	(0.433)
N	116	116	116	116	116	116
R2	0.077	0.077	0.086	0.051	0.124	0.102
	(7)	(8)	(9)	(10)	(11)	(12)
lower candidate	-1.116***	0.103	0.263	-0.064	0.400	-0.140
	(0.386)	(0.267)	(0.192)	(0.345)	(0.406)	(0.270)
constant	4.183***	4.949***	1.869***	5.157***	3.112***	5.132***
	(0.588)	(0.330)	(0.313)	(0.443)	(0.682)	(0.336)
N	116	116	116	116	116	116
R2	0.168	0.047	0.087	0.054	0.077	0.026

Notes: Content of answer(x) on 7-point scale as outcome variable. The lower candidate is the one who disclosed ex ante strictly fewer answers than her competitor, with the latter serving as a baseline. Standard errors in parentheses clustered at group level. Control dummies for the ten different randomizations of questions used included.

On top of finding no systematic differences in actual content between those disclosing more or less, there are no differences either with regard to beliefs regarding content. For each answer, we compare a subject's own answer with her guess regarding which answer her opponent has given. Regressing each of these differences again on an indicator for being the one disclosing fewer answers in Table B.16 returns no statistically significant effects. Hence, subjects who disclose fewer answers do not believe that the answers they have given are systematically different from the answers of their competitor. This conclusion always holds, regardless of whether we look at the full sample or only at subjects in *strategic* treatments. Consequently, differences in beliefs regarding content cannot drive our results. Rather, some people seem to feel a general reluctance to share personal details, independently of whether the content is good or bad.

To assess a monetary value of such a reluctance to share personal information, we simulate the psychological cost of an additional disclosure in terms of foregone earnings. We use subjects' beliefs with regard to how much one would get if one's competitor became the *allocator*.[45] The privacy cost of an additional disclosure is calculated as the amount the subject would allocate to herself as *allocator*, $m_{a=i}$, minus the expected earnings when not becoming *allocator* $E(m_{a=j})$ and 10 Cent revelation cost per additional disclosure. For the sake of simplicity, we abstract from answer content and assume that the selecting party simply chooses the candidates with more disclosures. We simulate a scenario in which one is ceteris paribus chosen as *allocator* if one is ahead by at least one answer. Hence, the average minimal privacy cost per additional disclosure $\bar{c}$ is calculated as

$$\bar{c} = \frac{m_{a=i} - E(m_{a=j}) - 0.1 * (|\sum D_j - \sum D_i|) + 1}{|\sum D_j - \sum D_i| + 1} \tag{B.1}$$

where i is the corresponding subject, j her competitor, and D stands for disclosure. We run this simulation for i) all ex post disclosures, ii) only for disclosures which C considers as relevant for *allocator* selection, and iii) for disclosures the subject considers as unpleasant.

According to Figure B.4, privacy costs of candidates lagging behind in the SAC treatment must exceed zero substantially to compensate the foregone earning when not becoming *allocator*. For all disclosures, each additional disclosure must cause privacy costs of at least 3.13€ on average. For relevant disclosures, this value equals 2.93€ and is, at 4.22€ the highest for unpleasant disclosures.[46] Finding valuations substantially higher than zero and the highest minimal value for unpleasant disclosures supports our hypothesis that privacy costs prevent participants lagging behind from catching up with their competitor.

Altogether, we conclude that wrong beliefs about the benefits of information sharing, as well as different content of the information to share, cannot fully explain our findings. On the

[45] This belief elicitation has not been incentivized.

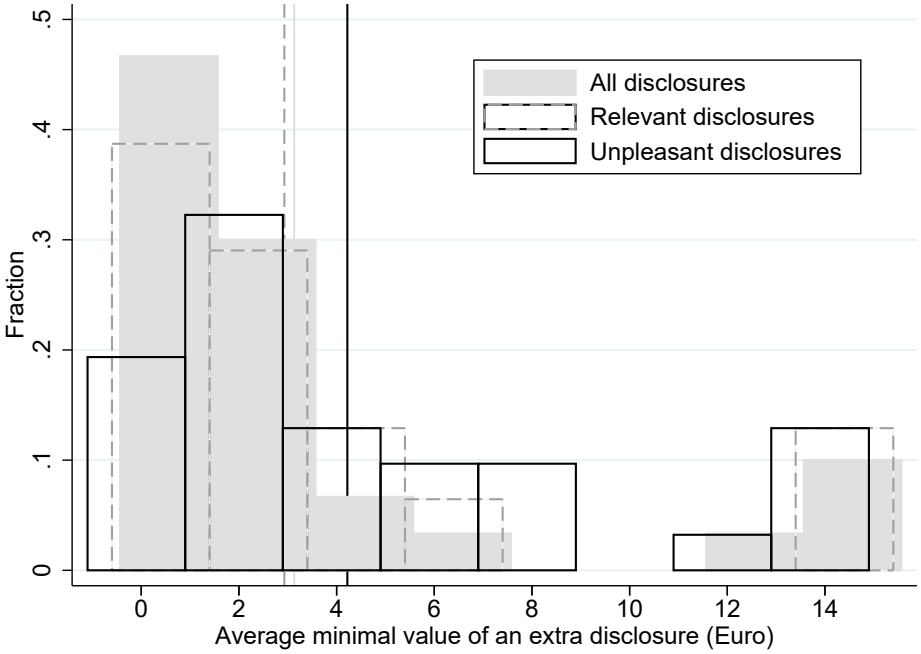
[46] As an additional check, we assume that both candidates disclose all answers ex post and one of them is randomly chosen as *allocator* with equal probability. Expecting the higher self-assigned and the lower competitor-assigned payoff, respectively, half of the cases while paying the maximum amount of revelation costs yields an average privacy valuation of 0.74€ per additional ex post disclosure for those lagging behind in the SAC treatment.

Table B.16: Perceived answer score differences by more / less initial disclosure on question level

	Difference in answer (x)					
	(1)	(2)	(3)	(4)	(5)	(6)
lower candidate	0.023	-0.030	0.134	0.077	0.003	0.113
	(0.201)	(0.222)	(0.211)	(0.269)	(0.231)	(0.292)
constant	0.447*	0.265	-0.025	0.170	0.623*	-0.348
	(0.227)	(0.351)	(0.269)	(0.400)	(0.355)	(0.211)
N	196	196	196	196	196	196
R2	0.067	0.116	0.138	0.026	0.141	0.065
	(7)	(8)	(9)	(10)	(11)	(12)
lower candidate	0.344	-0.350	-0.107	-0.069	0.347	-0.048
	(0.285)	(0.298)	(0.265)	(0.261)	(0.242)	(0.322)
constant	0.203	0.550*	0.512	0.243	-0.424	-0.018
	(0.485)	(0.291)	(0.366)	(0.274)	(0.466)	(0.313)
N	196	196	196	196	196	196
R2	0.079	0.099	0.045	0.066	0.105	0.050
	Difference in answer (x) - Strategic treatments only					
	(1)	(2)	(3)	(4)	(5)	(6)
lower candidate	0.202	0.074	0.089	0.122	-0.354	0.282
	(0.288)	(0.291)	(0.312)	(0.350)	(0.255)	(0.380)
constant	0.274	0.026	-0.545**	0.252	0.927**	-0.516*
	(0.290)	(0.503)	(0.228)	(0.569)	(0.428)	(0.291)
N	116	116	116	116	116	116
R2	0.066	0.094	0.170	0.046	0.172	0.105
	(7)	(8)	(9)	(10)	(11)	(12)
lower candidate	0.459	-0.513	-0.258	-0.018	0.142	0.175
	(0.382)	(0.408)	(0.368)	(0.328)	(0.314)	(0.428)
constant	0.395	0.694*	0.379	0.071	-0.446	-0.275
	(0.604)	(0.383)	(0.460)	(0.389)	(0.679)	(0.421)
N	116	116	116	116	116	116
R2	0.101	0.161	0.079	0.069	0.103	0.063

Notes: Difference between guess of the other's answer and one's own answer as outcome variable for answer(x). The lower candidate is the one who disclosed ex ante strictly fewer answers than her competitor, with the latter serving as a baseline. Standard errors in parentheses clustered at group level. Control dummies for the ten different randomizations of questions used included.

Figure B.4: Histogram of simulated average minimal privacy cost per disclosure



Notes: The figure shows the distribution of minimal privacy valuations per additional disclosure of subjects with fewer ex ante disclosures in the SAC treatment as specified in equation B.1. Vertical lines represent means.

contrary, heterogeneous and substantial privacy costs in a world with information-disclosure competition and conformity seeking can explain large parts of the observed patterns in disclosure behavior. In combination with the perceived pressure that disclosure-unwilling individuals report, our results therefore highlight important new patterns in personal information sharing, which have so far been overseen.