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An Experiment on Group
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Abstract: In a laboratory experiment, I test whether guilt aversion, i.e., a preference to fulfill the expectations of others, plays out more strongly if agents share an induced social identity. Participants play a dictator game in which they can condition their amount sent on recipients' beliefs. Dictators either play with a recipient from their own group (ingroup treatment) or from the other group (outgroup treatment). I find that the positive influence of second-order beliefs on how much a dictator sends is stronger in the ingroup treatment. However, the way dictators react to very high expectations does not differ significantly between treatments. In contrast to previous work I do not find that amounts sent are an inversely u-shaped function of recipients' expectations. Rather, and independently of the treatment, participants tend to ignore very high expectations.

JEL-classification: A13, C72, C91, D64, D84

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I. Motivation

Firms invest heavily in creating a shared identity among their employees because shared identity increases cooperation and trust among co-workers (Eckel & Grossman, 2005). For explaining and predicting generous or trustworthy behavior, guilt aversion is one of the prominent models in economics. It claims a preference to choose actions conforming with another person's expectation (Dufwenberg & Gneezy, 2000; Charness & Dufwenberg, 2006; Battigalli & Dufwenberg, 2007). But can guilt aversion really be boosted by increasing shared identity or does shared identity increase cooperation through other channels? And if beliefs about peer-expectations are at the core of motivating cooperative behavior, are exaggerated or overambitious expectations harmful to a cooperative environment? If they are, can this threat to cooperation be mitigated by ever more social integration?

This paper tests whether closer social distance implemented as a shared social identity can boost guilt aversion, whether exaggerated beliefs reduce the impact of second order beliefs on action, and whether shared identity can compensate any reduction of this sort.

What factors determine the strength of the aversion of guilt is not very well understood. The theory goes back to research from psychologists who treat the sensitivity of people to others' expectations as inherently context-dependent. This context is shaped by relationships. Charness & Dufwenberg (2006) as well as Battigalli & Dufwenberg (2007) motivate their research on guilt by citing articles by psychologists (Baumeister, Stilwell, & Heatherton, 1994; Tangney, 1995), who postulate a "communal relationship" as opposed to a mere exchange relationship as precondition to experiencing guilt vis-à-vis a person. I operationalize the relationship between participants as shared identity and assess the impact of shared identity on guilt aversion in a dictator game. I vary exogenously whether participants share or do not share a common group identity. If even group identity, induced in the lab, changes

how second-order beliefs (i.e., the dictator's expectations over the recipient's expectations) induce actions, the effect of friendship, co-workership, or family ties in the field is likely to be stronger by magnitudes.

In each session, I segregate participants into two minimal groups according to their stated preference for one of two modern painters (Tajfel, Billig, Bundy, & Flament, 1971). I further reinforce their respective group identity by letting the two groups compete for a prize in a real-effort task and subsequently measure to what extent participants identify with their group. Then one half of the dictators in a session are randomly matched with a recipient from their own group and the other half with a recipient from the other group.

From recipients I elicit what amount they think is most likely to be sent to a recipient in their situation (from the same group or from the other group, respectively). Their answers depend exclusively on their belief distribution over the action space of dictators, so that I implicitly elicit recipients' first-order beliefs. Then, I let dictators condition their decision what share of the pie to send on the 11 possible expectations of their matched recipient. I implicitly let them condition their decision on 11 possible first-order belief distributions of their matched recipient. In this way, for every dictator I induce 11 different distributions of second-order beliefs. As each of the 11 different hypothetical recipient expectations corresponds to a specific distribution of second-order beliefs of the respective dictator, and guilt aversion predicts an effect of second-order beliefs on action, I will use the terms "recipient's expectations" and "dictator's second-order beliefs" interchangeably when I analyze my experiment.

I find that social closeness, implemented as shared group identity, indeed boosts guilt aversion controlling for second-order beliefs. Independently of a shared group identity, if recipients expect the dictator to send more than half of the pie, the effect of second-order beliefs on action is attenuated to such a degree that no guilt aversion can be shown for these high recipient beliefs. I do not find a pure level effect of shared group identity on the amounts sent.

My results nuance an explanation of ingroup favoritism. Güth, Ploner, & Regner (2009) as well as Ockenfels & Werner (2014) explain ingroup favoritism by theorizing that participants favor fellow group members because dictators know that recipients expect more in ingroup interactions than in outgroup interactions. They claim that shared group identity shifts the distribution of second-order beliefs to the right. Here, in contrast, I make the claim that social integration changes the degree to which second-order beliefs – independently of their distribution – matter for participants' utility. This is claiming an influence of group identity on the weight that second-order beliefs have in the utility function. My experiment allows me to vary group identity independently of second-order beliefs. If its results confirm my hypothesis, explanations along the lines of Güth et al. (2009) and Ockenfels & Werner (2014) should take into account that second-order beliefs also have a stronger influence in interactions within groups than they do in interactions across groups. At the limit, this would mean ingroup favoritism could arise even if recipients' expectations are held constant. The only prerequisite would be that the difference in shared identity between ingroup and outgroup interactions is strong enough.

Khalmetski et al. (2015) have found that guilt aversion concerns people to very different degrees. Therefore it seems warranted to investigate the factors boosting or attenuating the relevance of guilt aversion. Promises (Ederer & Stremitzer, 2017) and communication (Balafoutas and Sutter, 2017) have been shown to strengthen guilt aversion. But both promises and communication are complex interactions, changing a host of things at once. Testing the effect of social integration – a relatively precisely defined variable which communication can affect, too – can provide the basis for a better understanding of what determines the strength of guilt aversion.

Finally the boosting effect of social closeness on guilt aversion may provide an additional explanation why some experiments have not succeeded in finding guilt aversion in anonymous experiments (Ellingsen, Johannesson, Totta, & Torsvik, 2010; Kawagoe & Narita, 2014).

In the following, I will briefly review the related literature (II.), then introduce the experimental design (III.) and make some behavioral predictions (IV.), before reporting the results (V.), which I discuss in a concluding section (VI.)

II. Related Literature

Guilt aversion has been tested in many experiments. However, many of these experiments, most of which find evidence in favor of guilt aversion, merely test for correlations between self-reported second-order beliefs and actions (Attanasi et al., 2016; Bacharach et al., 2007; Bellemare et al., 2011,¹ Bracht & Regner 2013; Chang et al., 2011; Charness & Dufwenberg, 2006; Dufwenberg & Gneezy, 2000; Guerra & Zizzo, 2004). These studies do not provide conclusive evidence of a causal effect of second-order beliefs on action. They cannot exclude that the inclination to act causes second-order beliefs (false consensus effect; see Ellingsen et al. for a discussion and Khalmetski et al. 2015 for clean experimental evidence for the false consensus effect in a guilt aversion experiment). More recent research has therefore varied second-order beliefs exogenously, allowing causal interpretation. Here, evidence for guilt aversion is mixed. While Reuben et al. (2009), Bellemare et al. (2017a & 2017b), and Khalmetski (2016) find evidence in favor of guilt aversion, Vanberg (2008), Ellingsen et al. (2010), and Kawagoe & Narits (2014) do not find evidence in favor of guilt aversion. The most compelling explanation of this seemingly ambiguous state of the literature is that the effect of second-order beliefs on action is heterogeneous: Khalmetski et al. (2015) find no aggregate effect of second-order beliefs on action. But for a little more than half of their dictators they can show that second-order beliefs induce action. These actions, however, correlate positively with second-order beliefs for some dictators and negatively for others.

¹ The authors statistically control for one source of the false consensus effect.

Given this heterogeneity, it seems important to investigate the factors that determine how strongly – or even whether at all – people are affected by guilt aversion. It seems that so far merely two studies speak to this point. Ederer & Stremitzer 2017 find an effect of second-order beliefs on action only if the trustor had previously promised to cooperate.² Balafoutas and Sutter (2017) find evidence for guilt aversion only if participants could communicate, while they did not find any if communication was not possible. Both studies raise the question what in promises or communication respectively activates or deactivates guilt aversion. The social integration that communication creates is an obvious candidate. To the best of my knowledge, no experiment has ever tested to which degree social distance is a factor that influences the strength of guilt aversion.

There is a large literature showing that decreasing social distance (Charness & Gneezy, 2008; Frey & Bohnet, 1999; Hoffman, McCabe, & Smith, 1996; Leider, Möbius, Rosenblat, & Do, 2009, 2010; Rankin, 2006), increasing social integration (Brañas-Garza et al., 2010), or inducing a common group identity (Chen & Li, 2009; Dawes, Van de Kragt, & Orbell, 1988) between participants leads social preferences to play out more strongly in dictator games. But none of these studies treats belief-dependent preferences. Psychological literature shows that a relationship reinforces empathy (Aron et al., 2003; Aron et al., 1991; Baumeister et al. 1994; Brewer 2007; Hewstone, Stroebe, & Jonas 2008). However, none of these papers has tested whether the closeness of a relationship has an effect on how second-order beliefs induce action. The literature studying the relevance of second-order beliefs has not produced an answer to the question whether guilt aversion is mediated by some form of social closeness either. Rankin (2006) studies whether recipients' demands in a dictator game

² This gives rise to a selection problem. Promising trustors may be different from non-promising trustors.

have different effects if recipients and dictators communicate face to face or anonymously. I study the effect of second-order beliefs and not of demands. In contrast to outright demands, the beliefs I work with do not have any normative or imperative appeal. Bicchieri and Chavez (2010) study the impact of first- and second-order beliefs on transfers in an ultimatum game. In the ultimatum game, however, second-order beliefs are strategically relevant. Guilt aversion claims an influence of strategically irrelevant beliefs. Therefore the study does not provide evidence on whether people have a preference to act in accordance with second-order beliefs.

It has been shown that shared identity induces ingroup members to expect more trust from each other (Masella et al. 2014). Because shared identity increases a partner's expectation, Güth et al. (2009) as well as Ockenfels and Werner (2014) hypothesize that dictators treat ingroup recipients preferentially because they know that ingroup recipients expect them to send more. Güth et al. do find that ingroup recipients' beliefs are higher than those of outgroup recipients, but they do not find clear support for their hypothesis that ingroup favoritism is driven by guilt aversion. Ockenfels and Werner find that dictators indeed treat ingroup recipients better if the latter know that they share the dictator's group identity. This effect is attenuated if the recipient does not know the dictator's group identity. While these studies show that group identity matters for belief-dependent preferences, they are distinct from the study reported here, in that I am not interested in the effect of changing levels of second-order beliefs on action. I ask for the effect of shared group identity on the capacity of second-order beliefs to induce action.

III. Experiment

I run an experiment with two treatments, an ingroup treatment and an outgroup treatment. The on-screen experiment is programmed in z-tree (Fischbacher, 2007). Group identity is induced, conditional on the subjects' preferences for paintings (Chen

& Li, 2009; Tajfel et al., 1971), and reinforced by letting groups compete in a real-effort task (Böhm, Rockenbach, & Weiss, 2013). In November 2013, I conducted 15 sessions in the BonnEconLab. Each session comprised 16 participants, recruited via ORSEE (Greiner, 2004). Each subject received a show-up fee of 4 €. In the experiment, the subjects played for the experimental currency “Talers”. Participants were paid in Euros. One Taler converts to 0.11 €. The experiment proceedings can be described as follows. All participants are seated in front of a computer terminal, separated by cubicles. They are first asked to state a preference for paintings by Klee and Kandinsky, respectively (group segregation stage); then, they compete in a real-effort task (group reinforcement stage) and complete a pre-experiment questionnaire (questionnaire stage); and finally, they play a dictator game (game stage).

a. Group Segregation Stage

In the group segregation stage, each participant is assigned to one of two groups according to his/her preference for one of two painters – Klee and Kandinsky. The procedure is adapted from Chen and Li (2009), who take it from Tajfel et al. (1971): Participants consider five pairs of paintings by Klee and Kandinsky (the same paintings as in Chen and Li (2009)), placed side by side on the screen. Subjects are asked to state how much they prefer one painting to another. To answer this question, they use a slider bar below the paintings (labeled in three steps: I strongly prefer left, I like both paintings equally, I strongly prefer right). The position of the slider bar is translated linearly into a distribution of 10 points between the two paintings (I strongly prefer left = 10 points to left, I like both paintings equally = 5 to left and 5 to right, etc.). Then, for each participant, the points allocated to Klee are summed up. The same is done for the points allocated to Kandinsky. Subsequently the computer labels that half of participants who allocated the highest amounts of points to Klee the “Klee group”

and labels the other half the “Kandinsky group”. Participants then are informed about their group membership.³

b. Group Reinforcement Stage

In the group reinforcement stage, the two groups compete against each other in a real-effort task to intensify the perception of belonging to a group by experiencing interdependence and a common fate. The task subjects compete in is the following (Böhm et al., 2013): Participants receive a 15-page text on paper. Then, on their screens, I ask them for letters in the text that I define by page, line, word, and position. Participants have four minutes to identify as many letters as they can. The group that jointly accumulates the largest number of correct answers wins. Each participant of the winning group receives 26 Talers. If the groups tie, all participants receive 13 Talers. Participants do not receive any feedback on the between-group competition until the very end of the experiment. This preserves the independence of observations.

c. Questionnaire Stage

The computer randomly assigns half of each group to the role A (dictator) and the other half of each group to the role B (recipient). Then the computer randomly pairs each dictator with a recipient. Half of the dictators in a session are paired with a recipient from their own group (ingroup treatment) and the other half are paired with a recipient from the other group (outgroup treatment).

³ Ties were resolved by the order of the randomly assigned subject ID. Subjects could allocate fractions of points, so that virtually an infinite number of possible sums of points for Klee paintings was possible. The slider bar did not have a scale beyond the three labels mentioned in the text. Therefore it was virtually impossible to set it to precise integers (other than 10.0; 5.5; 0.10). Because ties thus were extremely unlikely, the tie-breaking rule was not included in the instructions. However, a tie did occur twice. Of 120 dictators, ten ended up in the Kandinsky group despite having awarded more points to Klee. Two who had given exactly the same amount to Klee as they gave to Kandinsky ended up in the Kandinsky group.

In the questionnaire stage, the dictators (A) are not informed about their role. They answer a questionnaire on how much they identify with their group (Doosje, Ellemers, & Spear, 1995).

The recipients (B) are informed about their role and the group membership of the participant in role A they have been paired with. On their screens they are informed that in the subsequent stage they will be paired with a dictator, which group this dictator belongs to, and that the dictator can freely split 100 Talers between himself and the recipient. They are then asked to predict the average amount a person in their situation – i.e., a recipient paired with a dictator from the same [the other] group – would receive in the experiment. The recipient can enter any guess, which can be expressed in a full amount between 0 and 100 Talers. It is announced that each subject who predicts an average amount, which is no more than 1 Taler off the actual average amount received by recipients in their situation during the session, will receive an extra payment of 85 Talers = 9.35 €. ⁴ After all participants have completed their respective questionnaires, the questionnaire stage ends.

d. Game Stage

Subjects play a dictator game. A dictator game is the simplest setup that allows to tackle our question. In a dictator game, I have full control about social integration because nothing in the game interferes with it: neither the trust put into a trustee nor any messages or promises exchanged between the parties.

On the first screen, all participants are informed about their roles. Also, all participants are reminded about their own group membership and informed about the group membership of the participant they have been paired with. Dictators receive a pie of 100 Talers. They can send any share to their respective recipient, which can be expressed in full Talers. The amount sent is elicited by means of a method akin to a

⁴ Four out of 120 recipients earned this extra payoff.

strategy method (Khalmetski et al. 2015; Bellemare, Sebald, & Suetens, 2017b). Dictators are asked what they would like to send, conditional on their respective recipient's belief. They express the amount they wish to send for the recipient's expectation of 0, 10, 20, 30, 40, 50, 60, 70, 80, 90, and 100 Talers. It is explained to them that the computer will activate the choice closest to the recipient's actual stated belief.⁵ After the dictators have filled in the vector, a screen will reveal the true stated expectation of the recipient, and the computer will put into effect the allocation for the case closest to that belief. To decrease the risk of confounding the treatment effect with an experimenter demand effect, the design holds the experimenter's "demand" to condition the amounts sent on second-order beliefs constant over treatments.

My design uses a well-established procedure of eliciting first-order beliefs and reporting them on to dictators (Ellingsen et al., 2010, which has been amended by Khalmetski et al., 2015, and Bellemare et al., 2017b, to comprise a strategy method eliciting amounts sent for many hypothetical beliefs of recipients). From this literature I also inherit that recipients only learn after their statement that their stated beliefs are used for letting dictators condition their action on second-order beliefs. The recipients' ignorance about the use of their statement may raise doubt about experimental control. Despite this concern, manipulating dictators' beliefs by reporting recipients' predictions became standard in the literature on guilt aversion (Bellemare et al., 2017b; Ellingsen et al., 2010; Hauge, 2016; Reuben et al., 2009; now also Balafoutas & Fornwagner, 2017; for an approach improving experimenter control now, see Khalmetski et al., 2015). In comparison to Ellingsen et al. and to Reuben et al., the method is less problematic in my case (just as it is less problematic in the cases of Bellemare et al., 2017b, and Balafoutas & Fornwagner, 2017). In contrast to Ellingsen et al. and Reuben et al., I focus on between-subject treatment differences (ingroup vs.

⁵ The recipients' stated beliefs are rounded to 0 or the closest multiple of 10 according to general rounding conventions.

outgroup), and the procedure is constant over treatments. Consequently, the unlikely possibility of some dictators not believing the instructions should add the same degree of noise to the data of both treatments.

Finally, in a posttest, I measured perceived closeness between the dictator and the recipient, using a one-item test by Aron et al. (2003),⁶ and I ask participants for some demographic data, such as gender, age, and occupation.

IV. Behavioral Predictions

I will first focus on predicting decisions for recipient expectations between 0% and 50% and discuss predictions for higher expectations thereafter. My Hypothesis 1 is straightforward:

Hypothesis 1 (H1): In line with guilt aversion, for recipient expectations between 0 and 50%, expectations influence the amount sent positively.

Furthermore, in line with the literature on the effect of social distance on social preferences cited above, I hypothesize that guilt aversion plays out more strongly across a short social distance. I implement this short social distance as shared social identity and predict that amounts sent correlate more strongly with the recipients' beliefs in the ingroup treatment than in the outgroup treatment. I have two reasons for this conjecture.

⁶ This is not the standard way of measuring group identification. Usually, self-extension to the group is measured (Smith & Henry, 1996; Perreault & Bourhis, 1999; Tropp & Wright, 2001; Dorrough, Glöckner, Hellman, & Ebert 2015). I measure self-extension to the recipients' personality. I do that mainly to check whether indeed, as Aron et al. (2003) briefly claim, the self "extends" to group members, or whether it merely "extends" to the group as a whole.

First, two of the papers finding that social integration reinforces social preferences (Chen & Li, 2009; Dawes, Van de Kragt, & Orbell, 1988) found that shared group identity specifically reinforces social preferences. Guilt aversion is a social preference, so that I have reason to expect guilt aversion to be boosted by shared group identity. Second, guilt aversion theory was inspired by psychological research which focused on relationships (Charness and Dufwenberg, 2006, and Battigalli & Dufwenberg, 2007, referring to Baumeister, Stillwell, & Heatherton, 1994, and Tangney, 1995). Psychologists think that the concern for other individuals depends on the “relationships” between individuals. In “relationships”, the self “expands” to encompass the other’s self, so that “oneness” increases. This leads to genuine empathy: the self experiences the other’s welfare as the self’s own welfare (Aron et al., 2003, 1991; Brewer, 2007; Hewstone, Stroebe, & Jonas, 2008).

The required relationship can be operationalized as shared group identity. Naturally, “participants in a close relationship include each other into their selves” (Aron et al., 2003): The self’s relationships to friends, parents, or spouse (Aron et al., 1991) are typically “close”. But ingroups merely defined by gender, ethnicity (Tropp & Wright, 2001), college major, fraternity membership (Smith & Henry, 1996), or university affiliation (Dorrough, Glöckner, Hellman, & Ebert 2015) have also been shown to be included in the self. Perreault & Bourhis (1999) used a measure of self expansion in their minimal group paradigm experiment, indicating that even a minimal group paradigm can induce different levels of self-expansion to the ingroup versus to the outgroup. When participants expand their self to the ingroup, they also expand it to the ingroup members (Aron et al., 2003). In my experiment, dictators would be predicted to experience their recipients’ utility. If the recipients’ utility is reference-dependent, the recipients’ utility will depend on their expectations (Köszegi & Rabin, 2006; Abeler, Falk, Götte & Huffman, 2011) – i.e., recipients will be disappointed if they receive less than they thought. Therefore, the utility of dictators who experience their recipient’s utility will depend on their beliefs about their recipients’ beliefs. In the

ingroup treatment, dictators will experience the utility they cause in recipients to a higher degree than dictators of the outgroup treatment. Therefore, second-order beliefs should have a stronger effect on action in the ingroup treatment than in the outgroup treatment. Accordingly, I derive the second hypothesis, which is at the core of this paper:

Hypothesis 2 (H2): The influence of recipients' expectations between 0 and 50% of the pie on the amount sent is stronger if the dictator and the recipient are from the same group than if they are not.

For expectations above 50% of the pie, this study is exploratory. Amounts sent that exceed half of the dictator's endowment are extremely rare (Engel, 2011). Related work has suggested that the influence of second-order beliefs on amounts sent in dictator games is inversely u-shaped. Up to 50% of the pie, recipients' expectations seem to increase the amounts sent; for expectations above 50%, the correlation appears to be negative (Harth & Regner, 2014; Balafoutas & Fornwagner, 2017). However, there are studies suggesting that high expectations tend to be ignored in a trust game (Pelligra, Reggiani, & Zizzo, 2016). What drives the behavior of dictators confronted with expectations of above 50% of the pie does not appear to be settled yet. Conflicting forces are likely and they may play out differently for ingroup interactions and outgroup interactions. The first force may be guilt aversion in the traditional sense. Even in the realm of expectations above 50% of the pie, dictators may increase their amounts sent in response to increasing second-order beliefs. If beliefs are that high, however, dictators may also negatively condition their amounts sent on beliefs, "punishing" exaggerated beliefs. Dictators may actually do so more, the more exaggerated the beliefs are. Balafoutas & Fornwagner (2017) and Harth & Regner (2014) found evidence that the more exaggerated beliefs are, the less trustees send in a trust game. Harth & Regner (2014) explained their findings with reciprocity

dominating guilt aversion in that domain. Theories of reciprocity do not apply to my design – at least not in their traditional form (Dufwenberg & Kirchsteiger, 2004; Falk & Fischbacher, 2006; Rabin, 1993). The recipient does not act. So the dictator cannot reciprocate on any kind or unkind action.

The “punishment” of exaggerated beliefs could also be flat. This would mean dictators do discount their amount sent if expectations are above 50% of the pie and would send the same low amount for all these beliefs. Finally, the dictators would have good reasons just to ignore expectations above 50% of the pie (Pelligra et al., 2016). All these forces may interact with a shared group identity. But again the signs of these interaction effects seem unclear. There is evidence that participants tend to be more forgiving towards people who share their group identity (Chen & Li, 2009, p. 445). But an exaggerated belief could also be interpreted as particularly presumptuous if it were held by a group-mate meriting harsher “punishment”.

V. Results

Participants earned 11.08 € on average (including the show-up fee, minimum: 4 €; maximum 19.51 €). Not surprisingly, dictators (14.77 € on average) earned more than recipients (7.41 € on average).

Of the 120 recipients, 58 were female and 62 male. 105 recipients were university students, while 15 were not. The mean age of recipients⁷ was 24.27, the median age was 22, and the standard deviation was 6.58 years.

⁷ For the summary statistics of the recipients’ age, two observations were dropped because the participants had implausibly stated ages of 80 and 100, respectively.

Of the 120 dictators, 61 were female and 59 male. 114 dictators were students, while 6 were not. The mean age was 24.15, the median age was 23, and the standard deviation was 5.18 years.

I measured the dictators' group identification as the average score on the 7-interval scale on which dictators answered the questions of the questionnaire stage. The mean dictator scored 4.84 on this measure (median=5, mode=5, sd=1.2). 75% of dictators scored strictly higher than the midpoint of the scale. This is considered high group identification in the literature (Böhm et al. 2013). Dictators who were later allocated to the ingroup treatment scored slightly lower on the group identification scale than dictators allocated to the outgroup treatment (4.75 vs. 4.92).

After briefly exposing my analysis strategy, I test H1 and H2. Then, I analyze some additional findings on ingroup favoritism, dictators' heterogeneous reactions to recipients' expectations, and the persistence of group identity. I close the section with analyzing the effect of shared group identity on decisions in the realm of expectations above 50% of the pie.

a. Analysis Strategy

I analyze the decisions that 120 dictators made for eleven possible levels of second-order beliefs. 60 dictators took their decisions in the outgroup treatment and 60 in the ingroup treatment.

I expect a main effect of the ingroup treatment, which is due to the famous phenomenon of ingroup favoritism (Tajfel & Turner, 1979). This effect has to be separated carefully from the interaction effect of the ingroup treatment with second-order beliefs, which tests my hypothesis H2. Therefore I will estimate a linear regression model of the following form with random effects on the participant level. The regression predicts amounts sent using second-order beliefs (sobelief), a dummy for the treatments (ingroup), and the interaction of both (sobelief*ingroup) as

independent variables. The ingroup dummy is equal to 1 if the dictator and the recipient are from the same group, and 0 otherwise.⁸

$$\text{Amount Sent} = \beta_0 + \beta_1 * \text{sobelief} + \beta_2 * \text{ingroup} + \beta_3 * \text{sobelief} * \text{ingroup} + \text{error (participant random effects)} + \text{error (residuals)}$$

The coefficient β_1 should pick up any effect of second-order beliefs, which does not depend on the treatment manipulations. A positive and significant β_1 in the realm of realistic beliefs is evidence in favor of H1. β_2 should pick up the level effect induced by in-group favoritism. Finally, β_3 should pick up whether the ingroup vs. outgroup manipulation reinforces and attenuates the effect of second-order beliefs on action. If β_3 is positive and significant in the realm of expectations below 50%, this is evidence in favor of H2 this paper set out to test.

On top of the simple linear regression model, I will report results from a Tobit regression with random effects at the participant level. It has been shown in the literature that if the experimental design allows not only sending to recipients, but also taking from them, some dictators do actually “steal” from recipients’ endowments (Engel, 2011). In my experiment, dictators cannot transfer negative amounts. Therefore the data may be censored at zero. Tobit is a common approach to take such censoring into account.

All coefficients in the linear random effects model are virtually identical to those of the linear fixed effects model. Accordingly, the Hausman test yields an insignificant result.

⁸ I use a random-effects estimator instead of a fixed-effects estimator, insofar as it does not bias the results away from the result of the fixed-effects estimator. Using a fixed-effects estimator, one of my two main effects (ingroup) would drop out because of a lack of within-subject variance. Therefore the purpose of using random effects is merely to make all effects visible in one model.

b. Testing the Predictions of Guilt Aversion over Expectations up to 50% of the Pie

Graph 1 shows that, in the realm of expectations between 0 and 50%, the latter influence the amount sent positively in both the ingroup (IN) and the outgroup (OUT) treatment. It is also clearly visible that this influence is stronger ingroup than it is outgroup. Both results are confirmed by the regression analysis summed up in Table 1.

Table 1

Data for second-order beliefs from 0 to 50%

Dependent variable: Amount sent	Model 1: Linear Regression	Model 2: Linear regression	Model 3: Tobit regression	Model 4: Tobit regression
Second-order belief	0.152*** (0.0189)	0.100*** (0.0266)	0.241*** (0.0314)	0.141*** (0.0431)
Ingroup (dummy)	1.441 (2.889)	-1.155 (3.039)	-0.236 (5.925)	-5.756 (6.172)
Ingroup x Second-order belief		0.104** (0.0377)		0.207*** (0.0623)
Random effects at participant level	Yes	Yes	Yes	Yes
Constant	9.366***	10.66***	-4.909	-2.321
N	720	720	720	720
Number of Groups	120	120	120	120

*=p≤0.05; **=p≤0.01; ***=p≤0.001

Standard errors in parenthesis

The number of total observations is 720. “Number of groups” refers to the observations grouped by individual participants. The regressions consider amounts sent for six different second-order beliefs per dictator (0, 10, 20, 30, 40, and 50).

The null hypothesis that in the realm of realistic beliefs second-order beliefs do not influence actions positively has to be rejected. This is evidence in favor of general guilt aversion (H1). The result has to be taken with a grain of salt, however. By letting dictators fill in the vector, I exercised a certain demand over participants to condition their amount sent on second-order beliefs. Therefore it cannot be completely ruled out that the effect I find is driven by experimenter demand (Zizzo 2010). On the other hand, Bellemare et al. (2017a) now find that the “menu approach” I use did not strengthen the correlation between second-order beliefs and kind behavior in a mini trust game relative to the traditional approach on the basis of self reported second-order beliefs. Their results speak against a demand effect specifically caused by offering a menu of hypotheticals.

More importantly, the null hypothesis that the influence of second-order beliefs is not stronger if the dictator and the recipient are from the same group has to be rejected. Here, a demand effect is even less likely. The request to condition amounts sent on second-order beliefs was constant over treatments and no direction of the expected effect was suggested by the design. Therefore, the positive and significant interaction effect of second-order beliefs and group identity is reliable evidence for the influence of shared identity on guilt aversion. Accordingly, I derive results one and two.

Result 1: In the realm of expectations between 0 and 50% of the pie, the effect of second-order beliefs on action is positive, as predicted by guilt aversion. (Model 2, random effects regression, $N=120$ $p<0.001$, $\beta_1=0.1$).

Result 2: The effect of second-order beliefs on action is stronger if dictators and recipients share a common group identity. (Model 2, random effects regression, $N=120$, $p<0.01$, $\beta_3=0.104$).

My results can also be shown by a Tobit random-effects regression, which accounts for the possibility that dictators would actually have taken money from recipients if I had let them (Model 4, Tobit random effects: $\beta_1=0.14$, $p<0.001$; $\beta_3=0.2$, $p<0.001$).

c. Additional Results on Ingroup Favoritism, Heterogeneity, and the Persistence of Group Identity

With regard to the expected effect of ingroup favoritism (H2), over all hypothetical beliefs dictators send an average of 16.55 Talers to recipients who share their group identity. Dictators who are from a different group than their recipients send only 13.06 Talers on average. This is a difference of 28%. The literature suggests that payments to ingroup members are about 30% higher on average than payments to outgroup members (Lane, 2016; Balliet, Wu, & DeDreu, 2014). However, the difference I find does not reach conventional levels of statistical significance in my regression analysis reported in Table 1. And a rank-sum test of comparing average amounts sent per participant between treatments yields no significant result (rank sum, $N=120$, $p=.68$). Nor does a regression including only ingroup as independent variable and random effects on the participant level indicate a significant difference. This is true no matter whether I include only decisions for expectations up to 50% (rank sum: $p>.88$; regression: $p>.61$) or the full range of beliefs (rank sum: $p>.67$; regression: $p>.28$). Even searching for differences between the levels of amounts sent by treatment and by second-order beliefs does not provide any statistically significant results (rank sum, all $p>.22$). For very low expectations (0% and 10%), dictators even tend to send less on average to ingroup recipients than to outgroup recipients. However, this difference is not statistically significant either. It is worth noting that the share of published studies not finding any statistically significant ingroup favoritism is substantial and ranges from roughly 20 % overall (Balliet et al, 2014) to 55 % of all studies inducing group identity artificially (Lane, 2016).

Result 3 (Null result): I do not find general, i.e., belief-independent, ingroup favoritism. The average amount sent by dictators ingroup is 28% higher than outgroup but the difference is statistically insignificant (Model 1, random effects regression, $N=120$, $p>0.27$, $\beta_3=1.44$).

Shared group identity is normally known to go with ingroup favoritism. Result 3 can therefore raise doubts whether I indeed manipulated shared identity and whether it was shared identity that boosted guilt aversion.

That is why I set out to show the essence of my main result 2 in a way, linking guilt aversion more directly to the individual strength of participants' identification with their groups. For each individual dictator, I measure first differences between the amounts sent for the different recipient expectations, respectively. For every dictator, this gives me five first differences for the realm of recipient expectations from 0% to 50% of the pie. I call dictators with a positive sum of first differences "guilt-averse", because they display the positive correlation between amounts sent and recipient expectations. Correspondingly, I call dictators with a sum of first differences equal to or smaller than 0 "not guilt-averse", because they do not display the predicted positive correlation.

The number of guilt-averse dictators in both treatments is identical: 21. But in the ingroup treatment, guilt-averse dictators score on average 5.32 on the seven-item scale measuring their identification with their group. In contrast dictators who are not guilt-averse score only 4.46 on average in the ingroup treatment. The difference is significant ($p=0.016$, Wilcoxon rank-sum). In the outgroup treatment guilt aversion descriptively goes with lower scores on the identification measure. Here, guilt-averse dictators score 4.88 on the identification scale while dictators who are not guilt-averse score higher, 4.92. The difference is not statistically significant ($p=0.72$). I derive result 4.

Result 4: In the ingroup treatment, guilt-averse dictators score higher on group identification than dictators who do not display guilt aversion ($p=0.016$, Wilcoxon rank-sum, $N=60$). In the outgroup

treatment, dictators who do not display guilt aversion descriptively score higher on group identification than dictators who are guilt-averse. However, the latter difference is not statistically significant ($p=0.72$, Wilcoxon rank-sum, $N=160$).

This pattern of findings is evidence that indeed shared identity boosts the effect of guilt aversion in my experiment. Where dictators interact with members of their ingroup, guilt aversion goes with strong group identification. Where dictators interact with members of their outgroup, strong identification with their group does not make a difference. If anything, stronger group identification tends to go with less guilt aversion in this context.

There are more results suggesting that the induction of shared identity was successful. Recipients' beliefs differ between ingroup and outgroup treatment with marginal significance in a non-parametric ranksum test (Wilcoxon ranksum, $N=120$, $p=0.073$). Ingroup recipients expect 25.35 Talers on average; outgroup recipients expect 19.26 Talers on average. As the group manipulation was identical for dictators and recipients, I take this as confirmation that the group segregation has actually worked. Expecting nothing is frequent and in fact the mode of the elicited recipient beliefs in both treatments. The overall mean is 22.30 Talers. 97.5% of recipients' expectations are below or equal to 50% of the pie. Ex post, this can be regarded as a justification for generating hypotheses only for the realm of expectations between 0 and 50% of the pie.

Graph 2: Histogram of Recipients' Expectations
by Treatment

Result 5: Ingroup recipients expect dictators to send more than outgroup recipients (Wilcoxon rank sum, $N=120$, $p=0.073$).

Result 6: Only 2.5% of recipients expected dictators to send more than half of the pie.

A last finding is that, by the end of the experiment, ingroup dictators do not score higher on self-extension than outgroup dictators. I do not find any treatment difference between the dictators' perceived self-extension towards their respective recipients. Indeed, the outgroup dictators seem to feel slightly closer to their recipients (mean score: 2.56) than ingroup dictators seem to do (mean score: 2.53). This can mean that group identification only induces self-expansion with regard to the group as such and not with regard to particular group members.

d. Including Decisions for Expectations Above 50 % of the Pie

The regression analysis up to here does not change qualitatively when including the full range of second-order beliefs (*random effects regression*, $N=120$; $\beta_3=0.08$, $p=0.000$,

see Table 2, Model 1, below). But it is obvious from plotting the average amounts sent against the whole range of second-order beliefs by treatment (Graph 1) that dictators react differently to expectations up to 50% than they react to expectations above 50%. To explore the data in the realm of expectations above 50% of the pie, I amend the original regressions by including a dummy for exaggerated beliefs I call “larger50”. The dummy is equal to 1 for those amounts sent that are conditioned on the recipient’s expectation that the dictator sends more than half of the pie. For amounts sent conditioned on the recipient expecting not more than half of the pie, the dummy is 0. β_4 denotes the coefficient of this dummy. Including this dummy confirms the observation that the effect of second-order beliefs is attenuated once these expectations rise above 50% (*random effects regression*, $N=120$; $\beta_4=-3.563$, $p=0.001$). Adding an interaction effect of the larger50 dummy with the ingroup dummy into this regression reveals that this reduction is not stronger in statistically significant terms if dictators send to outgroup members (*random effects regression*, $N=120$; $\beta_5=-0.346$, $p=0.875$). Again, the results can also be shown using the Tobit random effects model also introduced in the main body of the paper.

Table 2

All Data: second-order beliefs 0-100%

Dependent variable:	Model 5:	Model 6:	Model 7:	Model 8:	Model 9:	Model 10:
Amount sent	Linear	Linear	Linear	Tobit	Tobit	Tobit
	regression	regression	regression	regression	regression	regression
Second-order belief	0.0129 (0.0124)	0.0615** (0.0195)	0.063** (0.025)	-0.0142 (0.0221)	0.0837* (0.0347)	0.084+ (0.0437)
Ingroup (dummy)	-0.524 (3.371)	-0.524 (3.370)	-0.098 (3.407)	-5.515 (7.204)	-5.572 (7.200)	-5.33 (7.26)
Ingroup x Second-order belief	0.0802*** (0.0175)	0.0802*** (0.0174)	0.075* (0.035)	0.157*** (0.0316)	0.158*** (0.0314)	0.155* (0.0773)
Larger50 (dummy)		-3.563** (1.105)	-3.74* (1.563)		-7.185*** (1.982)	-7.281** (2.816)
Larger50 x ingroup 1 0			-0.346 (2.211)			-0.195 (3.962)
Random effects at participant level	Yes	Yes	Yes	Yes	Yes	Yes
Constant	12.42*** (2.384)	11.61*** (2.396)	11.57*** (2.409)	-2.938 (5.166)	-4.578 (5.183)	-4.59 (5.202)
N	1320	1320	1320	1320	1320	1320
Number of groups	120	120	120	120	120	120

*= $p < 0.06$; **= $p < 0.05$; ***= $p < 0.01$; ****= $p < 0.001$

Standard errors in parenthesis

Result 7: The effect of second-order beliefs is attenuated once the expectations increase above 50% (Model 7, random effects regression, $N=120$; $\beta_4 = -3.74$, $p < .05$).

(Null) Result 8: This attenuation is not different between treatments (Model 7, random effects regression, $N=120$; $\beta_5 = -0.346$, $p = .87$).

To look more closely at whether and, if so, how dictators condition amounts sent on expectations above 50% of the pie, I run the regressions explained above (V.a.) with the data on exaggerated second-order beliefs.

Table 3

Data for second-order beliefs from over 50%

Dependent variable: Amount sent	Model 1: Linear Regression	Model 2: Linear regression
Second-order belief	0.013 (0.0655)	0.0005 (0.092)
Ingroup (dummy)	5.94 (1.853)	3.873 (10.65)
Ingroup x Second-order belief		0.0258 (0.131)
Random effects at participant level	Yes	Yes
Constant	11.87*	12.90
N	600	600
Number of Groups	120	120

*= $p < 0.05$; **= $p < 0.01$; ***= $p < 0.001$

Standard errors in parenthesis

With these regressions, no significant effect can be shown. It appears that, in the realm of excessive second-order beliefs, second-order beliefs do not have any effect on the amount sent – independently of whether dictators interact with ingroup or outgroup recipients. This effect is in some conflict with the findings of Balafoutas and Fornwagner (2017) and Harth and Regner (2014), who find a negative correlation of second-order beliefs above 50% and amounts sent; by contrast, it is more in line with findings from Pelligra et al. (2016), who find that very high requests in a trust game tend to be ignored.

Result 9: In the realm of exaggerated beliefs, no influence of second-order beliefs on action can be shown. That is true independently of whether dictators interact with ingroup or outgroup recipients (Model 2, random effects regression, $N=120$; $\beta_1=0.0005$, $p>.983$; $\beta_3=0.026$, $p>.45$).

Result 10: In the realm of expectations above 50%, no statistically significant level effect can be found between the treatments either. (Model 1, random effects regression, $N=120$; $\beta_2=5.94$, $p=.14$).

VI. Discussion and Conclusion

To the best of my knowledge, this experiment is the first to show that social closeness – implemented as shared group identity – determines how strongly the dictators' second-order beliefs influence the amounts sent in a dictator game. I used a minimal group paradigm to induce a shared identity and reinforced it slightly. The total intervention is extremely faint. Therefore, the effect is likely to be a lot stronger in the field, where relationships are based on family ties, friendship, co-workership, and the like.

I further find that, only if dictators and recipients are from the same group, those dictators who previously stated that they identify strongly with the group are more likely to be guilt averse. This further corroborates my main result that shared identity determines the effect of second-order beliefs on action.

In the exploratory part of the experiment, I find that expectations above 50% of the pie generally have an attenuated influence on the amount sent. The difference of attenuation is not statistically significant between treatments. Within the realm of expectations above 50%, second-order beliefs seem to have no effect on the amount sent, independently of the treatment.

My study is informative with respect to shared group identity as a factor that can help explain the heterogeneity of the effects of guilt aversion. My results suggest that subjects interacting with a partner they feel close to (families, friendships, co-workers) experience more guilt aversion than they would in anonymous contexts (anonymous market transactions). This could imply that it may be challenging to pick up the effects of guilt aversion in a very anonymous experimental setting. My results make a first step towards spelling out more explicitly what determines the weight of the guilt-term in a utility function with guilt aversion.

In my experiment, a general level effect of ingroup favoritism is absent. This is in line with what Güth et al. (2009) and Ockenfels & Werner (2014) have suggested: Ingroup favoritism possibly does depend on second-order beliefs, such that the difference in amounts sent ingroup and outgroup are due to a higher level of second-order beliefs in ingroup interactions. Also in line with Güth et al. (2009) and Ockenfels & Werner (2014), I show that shared group identity does indeed translate into elevated expectations of recipients – which dictators may well anticipate. But beyond Güth et al. (2009) and Ockenfels & Werner (2014), I also show in this experiment that their explanation may at least be incomplete. Shared group identity not only raises recipients' expectations, but leads to a stronger influence of the dictators' second-order beliefs on actions. Both effects together may just reinforce each other. However, according to my results, the increase in expectations required to trigger ingroup favoritism can be smaller than implied by Güth et al. (2009) and Ockenfels & Werner (2014). In fact, my results suggest that ingroup favoritism may also materialize under identical second-order beliefs, ingroup and outgroup. In case of very strongly shared identity, ingroup favoritism could arise merely through the stronger impact of a given level of second-order beliefs ingroup. Güth et al. (2009) and Ockenfels & Werner (2014) would not make this prediction. My experiment, which only induced group identity very gently, could not test this prediction conclusively. This test appears to be a promising avenue for future research.

From my results, it appears plausible that communication boosts guilt-aversion because communication induces shared identity. Correspondingly, people may hold a promise to a stranger because the promise creates a shared identity between the two, activating or boosting guilt aversion. Future research could test whether building a relationship by means of a promise is one of the channels inducing people to keep their promises.

With regard to the effect of exaggerated beliefs on guilt, this experiment solicits further research. While preceding studies have suggested that the effect of second-order beliefs on amounts sent is inversely u-shaped, ingroup dictators in this experiment sent the same amount as for expectations of 50% for higher expectations also, while outgroup dictators seemed to “punish” excessive expectations by a fixed amount over all expectations above 50% of the pie. The difference between the treatments did not, however, reach levels of statistical significance. The insignificant difference notwithstanding, the data of this experiment do not support an inversely u-shaped pattern over the full range of expectations.

These results can find practical applications in the management of teams. Shared identity has long been identified as a means of team management. It can, for instance, reduce shirking in a team production environment (Eckel & Grossman, 2005). The findings reported here refine our understanding of the role of shared identity for team production. Shared identity can boost guilt aversion. This can be the channel by which shared identity prevents shirking and increases cooperation and trust. The clear transmission of goals and expectations can therefore be used as an effective tool to incite well integrated team members. And possibly the expectations set out can be ambitious. The experiment reported here suggests that setting out exaggerated expectations may be less harmful to the beneficial effects of guilt aversion than prior literature has suggested. However, if there was harm by exaggerated expectations, this experiment does at most cautiously suggest ever more social integration as a fix.

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