No. 61 november 2010

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Research Paper Series
Thurgau Institute of Economics and Department of Economics
at the University of Konstanz

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THURGAU INSTITUTE
OF ECONOMICS
at the University of Konstanz

Taking the initiative. What characterizes leaders?*

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This version: June 2013 | First Version: December 2010

Abstract

Taking the initiative is a crucial element of leadership and an important asset for many jobs. We assess this element of leadership in a game in which it emerges spontaneously since people have a non-obvious possibility to take the initiative. We can show that leadership in this game correlates with real life activities associated with taking the initiative. Combining this game with other experimental games and with questionnaires, we investigate the personality characteristics that entail leadership. We find efficiency concerns and generosity to be important determinants of leadership. Leaders have an internal locus of control and are more patient than non-leaders, but they are not different from the non-leaders with respect to risk attitude. Response time patterns and the results from the cognitive reflection test show that cognitive resources are relevant in the decision to lead.

Keywords: leading-by-example, social preferences, experiment

JEL-Classification: A13, C92, D03, D83

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*We thank Katharine Bendrick, Paul Heidhues, Dorothea Kübler, Lydia Mechtenberg, Daniele Nosenzo and participants of the ESA world meeting 2010 in Copenhagen, the 2011 Florence Workshop on Behavioural and Experimental Economics, the GfeW meeting 2011 in Nuremberg, and of research seminars in Berlin, Konstanz and Kreuzlingen for very helpful suggestions. Nathalie Hock and Natalie Zimmer provided valuable research assistance. For providing laboratory resources we kindly thank *LERN* of the University of Erlangen-Nuremberg.

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1 Introduction

Imagine the following situation that recently occured in the department of the authors of this paper. A group of researchers is sitting in a lecture room, the presentation of a guest of the department is about to start. Trying to connect the speaker's laptop with the projector, people notice that the latter is defect. Some people hectically start pressing buttons and switches, but the projector continues not to be working. Everybody knows that a fast solution is needed so that the presentation can start in time, but only one postdoc actually solves the problem. He inspects the neighboring rooms, finds one in which the lecturer does not use the projector, enters that room and kindly asks the people there to switch rooms. Two minutes later the presentation can start.

The behavior of the postdoc is named *taking-the-initiative* and it has two distinct features, which generalize to many other situations in our everyday life. First, the situation concerns several people who are generally all capable of solving the problem. However, only one or a few of them actually do take the initiative. Second, in many of these situations, the best solution initially requires some creativity or innovation, even though with hindsight it seems quite obvious that this action was needed. Further examples are citizens' initiatives or neighbourhood centers creating cultural or sportive offers for disadvantaged young people to keep them busy and out of trouble, or enterprises arranging flexible childcare solutions for their employees when sufficient public childcare is not available. Often this initiative also has the third feature that it encourages others to follow the good example. The open source community is an economically relevant example. Without people like Linus Torwalds who initiate a project and contribute a significant code base, open source projects will never start (Lerner and Tirole 2002). Taking the initiative is a channel through which cooperation can arise in any bad state of social interaction, be it mutual freeriding in team work or a negotiation with parties irreconcilably opposed. In such situations, it is highly desirable that someone breaks the vicious circle by giving a good example, but only some people actually do so. We are interested in the determinants of this behavior. What characterizes people who take the initiative?

We build on a game introduced by Dufwenberg and Gneezy (2000) to study the Bertrand paradox and interpreted by Bruttel (2009a) in terms of leading-by-example. In this game, pairs of two people choose a number between 2 and 100. The person setting the lower number gets

the number she chose as a payoff; the other gets nothing. In the case of a tie both receive half the price. This game is repeatedly played with changing pair composition within groups of 12 subjects in total, with all 12 subjects in a group being informed about the decisions of all players in their group after the end of each round.

In this game, typically a cyclical movement of average numbers is observed. In the beginning, the 12 numbers within a group are more or less arbitrarily selected between 2 and 100. During the first few rounds, average numbers decrease, because all participants try to choose a number slightly lower than most of the others. After some rounds, the group reaches a relatively low level of numbers. Eventually though, one player raises the number to a very high level. Many of the other players follow, so a temporary increase of the numbers occurs. This behavior is in no way induced by the experimental instructions, but appears to be very robust. It crucially depends on the feedback condition, because such signaling to the group is only possible because of the group feedback. If subjects only get feedback from their own pair, the chosen numbers continuously decline. We use this game with 8 participants per group, and we consider the initiative to coordinate at a higher level to be endogenous leading-by-example. The person who takes the initiative, we call a "leader". We connect this classification to the decisions in other games and questionnaires eliciting other-regarding preferences, beliefs, risk attitude, patience, cognitive abilities, and other personality characteristics.

According to our results, leaders are characterized by above-average cognitive skills and are predominantly male. They have strong preferences for efficiency, generosity, and against advantageous inequality, and do not primarily seek to maximize their personal monetary benefit or to obtain a positive public image. They have accurate beliefs about the extent to which others will follow their example. Leaders in the experiment have an internal locus of control and are more patient than non-leaders, but we do not find an impact of the big five personality traits or risk attitude on leading-by-example. Finally, we can show that leaders as identified in the experiment are also more likely than non-leaders to engage in activities associated with taking the initiative in their real lives.

The leaders we are interested in have the ability and willingness to improve the outcome of the behavior of a group of people who are "stuck" in a bad situation, but they do not primarily give the good example because they are or want to be a formal leading authority. This makes

our definition different from most concepts of leadership discussed in the literature (see Yukl 2009 or Kouzes and Posner 2007), where the focus is on advising designated leaders. The notion most closely related to our research topic is charismatic or transformational leadership (Bass and Avolio 1994). However, unlike in situations in organizations, where charismatic leaders inspire followers by their impressive personality (see, e.g., Conger and Kanungo 1987), leaders in our experiment have relatively weak measures to communicate their "vision" of a cooperative solution to others as face-to-face interaction is ruled out in the computerized experiment. It is particularly the motivation of people taking the initiative which we borrow from this part of the leadership literature.

Hermalin (1998) defines leadership in a very similar way as we do, but the theoretical model of leadership he develops is different from our approach. In the model he discusses, a leader is the only member of the team to have information about their common effort return. By choosing effort before the other group members, the leader can signal this information to the followers, inducing them to provide high effort as well. The experiment of Potters et al. (2005) illustrates that such an informational setup in fact yields coordination on sequential moves to the benefit of efficiency. In our experiment, the action of the leader serves rather as a coordination device but not as a measure to resolve information asymmetries between leaders and followers as all players in our framework receive exactly the same information.

Because of the incentive structure of leading-by-example, this kind of leadership is often studied by introducing a sequential move structure in public good experiments (Gächter and Renner 2006; Güth et al. 2007; Gächter et al. 2012; Drouvelis and Nosenzo 2012; Moxnes and van der Heijden 2007; Pogrebna et al. 2011; Potters et al. 2007; Levati et al. 2007). These studies focus on the mechanism of leadership and typically show that groups with leaders on average contribute more than groups without, but only due to the higher contributions of the leaders.

Public good games capture nicely the incentive structure of leading-by-example. However, in these games it is obvious to all players what constitutes the good example and, therefore, they do not cover the innovative facet of the act of taking the initiative. Furthermore, it is always clear to the subjects that the experiment they are participating in is about leading and following. This may induce experimenter demand effects (Zizzo 2010) possibly manipulating leadership

in either direction. It may reduce leadership because involuntary leaders perform worse; or it may enhance leadership, because even natural non-leaders infer from the experimental design that leadership is socially desirable. In our design, there is no predefined leader. Different from other experiments about leadership, the leading behavior to initiate a number increase in this experiment is neither explicitly nor implicitly induced by the experimenter. There is no explicit assignment of the leading role to a certain subject. Decision making occurs simultaneously, so no player has a distinct role. So we do not address the question of whether a person accepts to be a leader when she is assigned the role but whether a person decides to take the initiative. Thus, our setup avoids the experimenter demand effect problem and includes the innovative element of leadership.

Our research question is related to a recent study by Arbak and Villeval (2013). They investigate the motivations of leaders by combining different variants of a two-stage public good game with personality tests. Similar to our setup, leadership is voluntary in their experiment, as subjects decide themselves whether they would like to make their contribution to the public good in the first or in the second stage of a round. However, the basic experimenter demand effect argument with respect to the two-stage structure still holds. In line with our results, they find that social concerns (measured as donations to a charity) are a driving force for at least some of their leaders and that men are more likely to lead than women. Furthermore, Arbak and Villeval (2013) argue that a positive social status[1] of leading drives the decision to give a good example, a finding which is rather not supported by our data.

The strategic interaction of our experiment is different to the one in a public good game and more related to price setting games. However, while cooperation of firms setting prices is usually inefficient for society because it implies a welfare loss at a cost for consumers, cooperation in our setup is socially desirable, because there is no consumer side which may suffer from collusion. Nevertheless, the behavioral patterns in price setting games often resemble similar cyclical movements as the numbers in our experiment, in particular if price setting occurs sequentially. For example, Leufkens and Peters (2011) and Bruttel (2009b) report cyclical price patterns for sequential price setting duopolies, which are initiated by a drastic price increase of one of the two

[1]While in Arbak and Villeval's (2013) study status arises endogenously from a high contribution to the public good, Kumru and Vesterlund (2010) show that also an ex ante induced high status makes leaders contribute more, because they anticipate that low-status followers will closely imitate the behavior of a high-status leader.

firms. Similarly, Selten and Apesteguia (2005) report "cooperative attempts", price increases with the intention to make others follow this step, for a simultaneous-move game with price competition on a circle. Comparing sequential with simultaneous price setting, Datta Mago and Dechenaux (2009) report higher average prices for the sequential game when capacities are asymmetric, while Kübler and Müller (2002) find average prices to be lower in the sequential case with differentiated products. In our experiment, the main feature triggering coordination is the public feedback within matching groups.

In section 2 of this paper, we present the design we use in the experiment. In section 3 we provide the behavioral predictions. Section 4 presents our findings and section 5 concludes.

2 Design and Procedures

2.1 Design

In order to determine the characteristics of leaders, we combine a game in which spontaneous leadership occurs regularly with a series of experiments and questionnaires that allow the measurement of beliefs, risk and social preferences as well as other characteristics. We start with the explanation of the game that we use as our leadership game.

The basic design of the game in our experiment is a variant of the stylized Bertrand pricing game in Dufwenberg and Gneezy (2000). In this game, two participants simultaneously choose a number from the interval [2,100]. The participant choosing the lower number wins the game. The prize is equal to the winning number. In case of a tie, each participant gets half of the prize. The game is repeated for 30 rounds. Players are divided into groups of eight participants. In each round, the eight participants in one group are randomly matched in pairs of two. Thus, four pairs play the game simultaneously in a group of eight participants. After each round the subjects were informed about their own number and the number of their partner in this period. Furthermore, all eight numbers were made publicly known in the group, ordered by size of the number. Thus, subjects received both, individual feedback and group feedback about the other participants whom they did not meet in the current round but might meet in the next rounds.

Each number choice decision in this main part of the experiment was surrounded by a belief formation stage and a publicity choice stage. In the belief formation stage before the number choice, players had to submit beliefs about the minimum, maximum, and average number of the other seven players in the next round. For each of the three values, they had to submit a probability distribution over the intervals 2-20, 21-40, 41-60, 61-80, 81-100. To facilitate submission of their beliefs, they were provided a graphical tool on the computer screen. Figure 9 in the Appendix shows a screen shot. The bars of the single intervals could be moved with mouse clicks. A click on "update" next to one of the distributions automatically increased or decreased all five bars proportionally to balance the sum of weights to 100 percentage points. If participants were done with their belief formation, they had to click "next". In the beginning of the next round, their past estimates were shown as default values and could be adapted with the same procedure. The quality of their prediction for each of the three values was determined with the quadratic scoring rule (Brier, 1950). They received a payment proportional to this measure.

By asking subjects before their decision for their belief about the probability distribution of the maximum number of the other group members in the next period, we learn how likely they think it is that someone else will lead. In particular, we need the probability weight leaders assign to the categories equal to or larger than their own leading number. The estimated average number of the other group members one round after a leading number provides an approximation of the leader's belief on the extent to which the others will follow. For our analysis, we re-calculate the estimated average from the submitted probability distribution. By comparing the beliefs of leaders to the beliefs of other not leading participants, we learn whether leaders are different from others with respect to their estimate of the benefits of leadership. The stronger the increase in the average number after a leading number, the higher are the potential gains to a participant undercutting opponents by a small amount. If leaders systematically overestimate others' average numbers after a leading number, this would indicate that leaders lead because they overestimate their monetary benefits from leading.

In addition to the maximum and the average number, we asked players to submit their belief about the minimum number of the other group members which we do not need for the analysis at all. We elicit beliefs in such a detailed way to receive an accurate belief of leaders on whether there will be another leader. Asking for the probability of the maximum interval only,

however, could introduce the experimenter demand effects again which we were able to avoid by the design of the main part. In order not to lead subjects into thinking about leadership, we therefore included the minimum belief and applied the distributional belief elicitation procedure to all three values, minimum, maximum, and average.

In the publicity stage after the number choice, we allowed players in the given round to give up anonymity and publish their seat number on the other participants' computer screens beside their own chosen number. Publication of the seat number in one round cost 10 points and could be decided upon by ticking a box on a separate screen after the number choice. Use of this feature allows us to control for whether appreciation by others motivated extraordinary number choices. Paying for publishing the seat number might not only be due to the leader's desire to become publicly known as a leader but also be used to strengthen the signaling effect of the leading number. It certainly emphasizes the leading number on the other participants' computer screens if the additional seat number entry is displayed as well. To test whether leaders do not want attention for themselves but for their number, we added a highlight option to the publicity feature in the second half of the sessions we conducted. In addition to the option to display their seat number at a cost of 10 points, participants could also choose to highlight their number anonymously on the others' screens by displaying three exclamation marks aside their number in that round at a cost of 5 points. To make sure that the highlighting effect of the publicity option is now not weaker than the one of the highlight option, in these sessions also the display of the seat number included three exclamation marks.

Treatment	Description
1	Distribution games (efficiency, inequality, generosity)
2	Risk elicitation
3	Belief trial phase (only in series 1)
4	**Number choice game**
5	Feedback about outcomes and payoffs
6	Questionnaires

Table 1: Order of Treatments

Before the main part of the experiment, we conducted some short games to elicit preferences for efficiency, generosity, inequality aversion and risk attitude. Table 1 includes an overview of the order of the different games in the experiment. We applied the same order of these

experiment to all the subjects. This procedure has the disadvantage that there might be spillovers between the games for which we do not control. However, we are interested in the difference between leaders and non-leaders and as long as there is no interaction between the type and the spillover, we can draw valid conclusions. The first part of the experiment was a series of six simple two-player distribution games using the strategy method, similar to Engelmann and Strobel (2004). As Bruttel (2009a) argues, there seem to be spillover wealth effects from the main experiment to the decisions in such distribution games. For this reason, we conducted these games before the main part of the experiment and not afterwards. In each game, participants had to choose between two distributions of money between themselves and another player. Table 2 shows the payoffs of the options between which player 1 could choose. The six games were designed in order to create tradeoffs between selfishness, equality and efficiency.[2] In the first column, there is a tradeoff between selfishness and equality on the one hand and efficiency on the other. The second column contains games with a tradeoff between selfishness on the one hand and equality and efficiency on the other hand. In the third column there is one game. In this game there is a tradeoff between equality in the form of envy and efficiency. The roles of players 1 and 2 were randomly assigned to the players after they had decided for both roles. Afterwards, one out of the six games was randomly selected for payment by the computer program. After completion of the six choices, we elicited risk attitude using the Holt and Laury (2002) procedure. The random draws from these initial parts of the experiment and the corresponding payoffs were revealed only after the main part of the experiment.

After the main part of the experiment, players first had to answer a questionnaire about their decisions in the number choice game. After that, they were asked to fill in several questionnaires, including the BFI-S big five questionnaire as used in the German Socioeconomic Panel (see Dehne and Schupp 2007), a locus of control[3] questionnaire according to the Rotter (1966) scale, a shortened version of the scale of patience[4] developed by Dudley (2003), the risk

[2]Originally, we presented one more choice measuring inequality aversion to the subjects. It was excluded afterwards because of a typo on the computer screen in the first series of the experiment leading to an inconsistency in the presentation of this choice.

[3]Broadly speaking, the locus of control measures the extent to which an individual feels to have control about the things happening in her life.

[4]The questions are included in Appendix B.

	Selfishness and Equality vs. Efficiency		Selfishness vs. Equality and Efficiency		Envy vs. Generosity	
	Option A	Option B	Option A	Option B	Option A	Option B
Player 1	2	1	7	6	1	1
Player 2	2	6	2	6	1	3
Player 1	2	1	6	5		
Player 2	2	5	2	5		
Player 1	2	1				
Player 2	2	4				

Table 2: Parameters in the distribution games

questionnaire used in Dohmen et al. (2011), a cognitive reflection test (Frederick 2005), and a short sociodemographic questionnaire. Among these questions, we asked for the following seven variables, referring to actual behavior in participants' real lives, which we interpret as taking the initiative: whether this person has ever been to a foreign country for an extended period of time, is working in an voluntary capacity, receives a student scholarship, took part in a youth research competition, has a function in an unincorporated association, has ever organized an event, or was class representative at school. Out of these seven variables, taking either the value zero or one, we create a "real life index" for participants' inclination to take the initiative. Furthermore, we used a variant of the personal initiative questionnaire in Meyer (2006) which builds on the questions of Bledow and Frese (2009). In this questionnaire, participants are given five hypothetical situations with four possible ways each to behave in these situations, where only some of the possible reactions represent personal initiative, but also the others are presented in a way that they can all be considered socially appropriate. Participants have to state which of the four reactions would most and least likely describe the way they would behave in such a situation. The questions and their coding are presented in detail in Appendix C.

2.2 Procedures

The experiment was computerized using z-Tree (Fischbacher 2007). A total of 224 students, 95 males and 129 females, from various disciplines took part in the experiment, divided into 28

groups of 8 participants each. They were recruited via ORSEE (Greiner 2004). The experiment took place in the *Lakelab*, the laboratory for experimental economics at the University of Konstanz between December 2009 and June 2010 (series 1, 13 groups), and in the *LERN* at the University of Erlangen-Nuremberg in June 2011 (series 2, 15 groups).[5] Sessions in series 1 lasted between 2 and 2.5 hours, sessions in series 2 about 1.5 hours. The sessions in the second series were shorter because they did not contain the belief elicitation procedure. We did not elicit beliefs in the second series to save time for filling out the large number of questionnaires. We did so because we learned after conducting the experiments in the first series that many participants were tired by the long duration of the experiment and rushed through the questionnaires without giving thoughtful answers.[6] In the second series we shortened the experiment by leaving out the belief elicitation so that they started filling out the questionnaire approximately 45 minutes after the beginning of the experiment. We also announced orally to the subjects that they may expect filling out the questionnaires to take about 45 minutes and emphasized that their answers are of great importance for the scientific evaluation of the experiment.[7]

The experimental currency was points. In the number choice game and in the belief elicitation procedure 30 points were converted into 1 euro after the experiment. In the distribution games and in the risk elicitation procedure there was a one-to-one exchange rate. On average, participants earned about 29 euros in the experiment with belief elicitation and 23 euros without. The questionnaires were not incentivized. The protocol during the experiment was as follows: After welcoming participants and explaining the main rules for participation in the experiment, they were randomly assigned seats in the laboratory. At their place, they read

[5]We had to move to another place for the second series, because while running some sessions (which are not included in this paper) in the LAKELAB in April and May 2011 information about this experiment had become known in the subject pool in Konstanz.

[6]We used two measures to identify careless answers. The first one was an item "you can trust my answers" in the big five questionnaire, according to which 18 percent of the participants cannot be trusted in their answering behavior, i.e. they select an answer of less than the maximum minus 1 unit to this item. The second one was an analysis of obvious patterns (e.g. choosing the very left answer for all items) in the answers to the locus of control questionnaire identifying 27 percent as very unlikely to be honest. As the two measures were also virtually uncorrelated (correlation coefficient 0.04), we decided that we needed a second series of experiments to obtain usable answers in the questionnaires.

[7]The two indices of careless answers in the second series were 4 percent and 8 percent, respectively.

short general instructions about the sequence of experiments they would participate in.[8] For the distribution games and the risk elicitation, subjects received instructions on their computer screen and made decisions immediately after reading the instructions. For the main part of the experiment, they received written instructions explaining the decisions and their consequences as well as the belief formation stage including the payment method with the quadratic scoring rule and the publicity choice stage. Next they were given the possibility to familiarize themselves with the computer screen for the belief formation. Then the experiment started. At the end of the session, the participants were asked to complete several questionnaires.

3 Behavioral predictions

In this section, we focus on our research question - what characterizes leaders. At the beginning of the next section, we will give the exact description of how we classify leaders. For now, we just note that if there is common knowledge about rationality and selfishness, subjects should choose 2 as their number. So, even when subjects try to coordinate on a higher number at the beginning, directing the behavior towards the best reply of the previous period will cause a decline in the numbers and, hence, in the payoff (Selten and Stöcker 1986). A subject displays leadership when she breaks out of this vicious circle and increases her number. In this section, we discuss the potential motivations for this behavior and the likely characteristics of such leaders.

First, let us consider the selfish motivation to lead. Some leaders in our experiment might initiate a number increase not for the purpose of the benefit of the group, but rather because they intend to undercut others at a higher level in the next round. Such selfishly motivated leadership crucially hinges on the belief on extent to which the other players will follow. Actually, Gächter et al. (2012) find that cooperative leaders have over-optimistic beliefs about the cooperativeness of followers, and that this can (aside from social motivations of leaders) explain their high contribution as first mover in a sequential public good game. These over-optimistic beliefs might be a consequence of the false consensus effect (Ross et al. 1977). It seems likely that such over-optimism is not only present for the randomly assigned leaders in the sequential

[8]The Appendix of this paper contains a translation.

public good game in Gächter et al. (2012) but also in the context of our number choice game. Thus, our first prediction is:

Hypothesis 1 *Leaders over-estimate the average numbers of their group members after leading.*

Our second hypothesis refers to other-regarding preferences. Successful leadership will provide a higher payoff for the group, but it is potentially costly for the leader and it could in particular create inequality that is disadvantageous for the leader. In the framework of a sequential public good game Arbak and Villeval (2013) find that voluntary leadership is related to preferences for efficiency and generosity. In Table 2, option B is always the efficient outcome. If leaders care more about efficiency than non-leaders, we expect that leaders more frequently choose option B. In the first column, efficiency does not only go against selfishness, it also creates disadvantageous inequality, which envious people will dislike. Since leaders risk disadvantageous inequality, we expect them to be more tolerant towards disadvantageous inequality and to choose option B more frequently that non-leaders in particular in the games in the first column of Table 2.

Hypothesis 2 *Leaders have stronger pro-social attitudes than non-leaders. They attach a higher value to efficiency, they are more generous and they are more willing to accept disadvantageous inequality.*

Leadership could be a signal of prosociality. As a prosocial attitude is generally seen as a positive trait it might be that leaders lead because they want to signal their "good character" to others. In our experiment, we offer participants an opportunity to make their seat number publicly know. We expect that leaders use this option more frequently than non-leaders.

Hypothesis 3 *Leaders are more likely to give up anonymity than non-leaders. Leaders are more likely to give up anonymity in their leading rounds than in other rounds.*

The considerations a leader makes before deciding to lead are relatively complex. In the beginning of the game most players, including the later leaders, follow a best reply strategy

against the distribution of numbers in their group in the previous round. This best reply dynamic leads to decreasing numbers from round to round, because all players try to choose a lower number than their representative opponent. The leader first has to understand the dynamic that all players react in a similar way to the group feedback. Second, the leader must be innovative in exploiting this behavior. By drastically increasing the own number, the leader manipulates the distribution of numbers the others are reacting to and reaches a temporary coordination of the group at a higher level of numbers. This understanding and manipulation of the dynamic decisions in this game requires a lot of innovation, creativity, and cognitive ability, as well as the willingness to use them. It also requires the willingness to break out of the simple responding to the other players' behavior. The cognitive reflection test described in Frederick (2005) captures the essence of these abilities. This is summarized in the next hypothesis.

Hypothesis 4 *Leaders have a higher score in a cognitive reflection test than non-leaders.*

According to Frederick (2005), the score in a cognitive reflection test is on average higher for males than for females. Furthermore, Arbak and Villeval (2013) hypothesize that particularly male participants may be concerned with maintaining a positive public image as men in their sample act more often as voluntary leaders than women. The latter result is also found in Gächter et al (2012), though it is not significant there. Matched with the information about participants' gender, we can also test whether male leaders are more publicity seeking than female leaders. We formulate this as our next hypothesis.

Hypothesis 5 *Men are more likely to lead than women. In particular, men are more likely than women to give up anonymity as leaders.*

Leadership is risky. When deciding to provide a good example, the leader hopes that the others will follow the example. In this case, future social welfare and potentially the leader's individual payoffs will increase. However, the leader cannot be sure that others will follow. A risk averse player might therefore be reluctant to lead even if this person was willing to set the example if it were guaranteed that others would follow. Similarly, the leader has to be patient, because for the leader any potential monetary gains from leading are realized only in

the future while initial costs (foregone possible gains) materialize immediately. We derive our next hypothesis:

Hypothesis 6 *Leaders are less risk averse and more patient than non-leaders.*

Leading-by-example is an optimistic act to improve the inefficient situation the group is in after a phase of mutual underbidding. Taking the initiative, the leader has to trust in his ability to change the circumstances of the interaction. We therefore hypothesize that leaders have an internal locus of control.

Hypothesis 7 *Leaders have an internal locus of control.*

The literature on charismatic or transformational leadership reports mixed evidence on the impact of the Big Five personality factors on these leadership theories. While Crant and Bateman (2000) and Judge and Bono (2000) both report a positive impact of extraversion on charismatic and transformational leadership, De Hoogh et al. (2005) find no such effect, and that neither in a stable nor in a dynamic work environment. Similarly, both De Hoogh et al. (2005) and Judge and Bono (2000) report a positive effect of agreeableness on charismatic and transformational leadership, but Crant and Batemann do not. So we control for the Big Five personality factors, but have no specific hypotheses concerning their impact on leadership.

Finally, the power of our results depends on the external validity of the leader classification with respect to taking the initiative outside the lab. The experiment contains two measures for this aspect, the "real life index" and the "index stories". We predict that these two indices correspond to leading behavior in the experiment. In contrast, we expect no significant correlation between taking the initiative in the experiment and the self-stated attitude of being a leader. Leadership in the experiment is likely to be driven by an implicit power motive (Fodor 2010), i.e. a concern for having an impact. The explicit power motive, in contrast, reflects the self-proclaimed goal to be in a leading position. According to a review by McClelland et al. (1989) the implicit and the explicit power motive are not correlated. In our context this means that people who take the initiative do not differ from others with respect to their inclination to seek a formal leading position.

Hypothesis 8 *Leaders in the experiment are also more likely to take the initiative in their real life outside the lab. A self-stated goal to be a leading personality does not correspond to leadership in the experiment.*

4 Results

We start the review of our results with an overview of the average numbers in all 28 groups. Figure 1 illustrates them, ordered by the timing of conducting the session. In all groups, average numbers fluctuate quite considerably, indicating dynamics within the groups. Average winnings numbers follow a very similar pattern. Looking at the initial phase of the game, we see that average numbers in most groups decrease from round to round, while in some groups (groups 2, 5, 9, 17, 24 and 25) they start by increasing. In these groups, at least one player chooses the number 100 in the first round which triggers the first upward movement of average numbers right in the beginning of the game. The number 100 in the first round of the game already seems to be an instrument of leadership.

Let us next consider groups with decreasing average numbers in the first rounds. In almost all of these groups the downward trend of average numbers stops after at most 10 rounds and turns into an increase instead. This later increase is always initiated by one player (sometimes also two at the same time) increasing the number substantially. Different from leadership in the first round, these leaders in later rounds do not necessarily increase their number to 100. In order to systematically disentangle intended leadership from casual number increases without a leading purpose, we use a refinement of the definition introduced by Bruttel (2009a). There, a leading number has to be more than 30 points larger than the leader's number in the round before and it has to be larger than all numbers of all other players in this group in the previous round.[9] We base our classification also on these criteria. In our understanding leadership contains the expectation that others will follow. We therefore define leadership to only be possible up to two rounds before the end of the game. Later high numbers may be observed for other reasons but cannot be motivated by the intention to lead. The value of 30 points is necessarily arbitrary. Bruttel (2009a) explains that 30 is a relatively low threshold including almost all

[9]In Bruttel (2009a) no leading numbers in the first round of the game were considered.

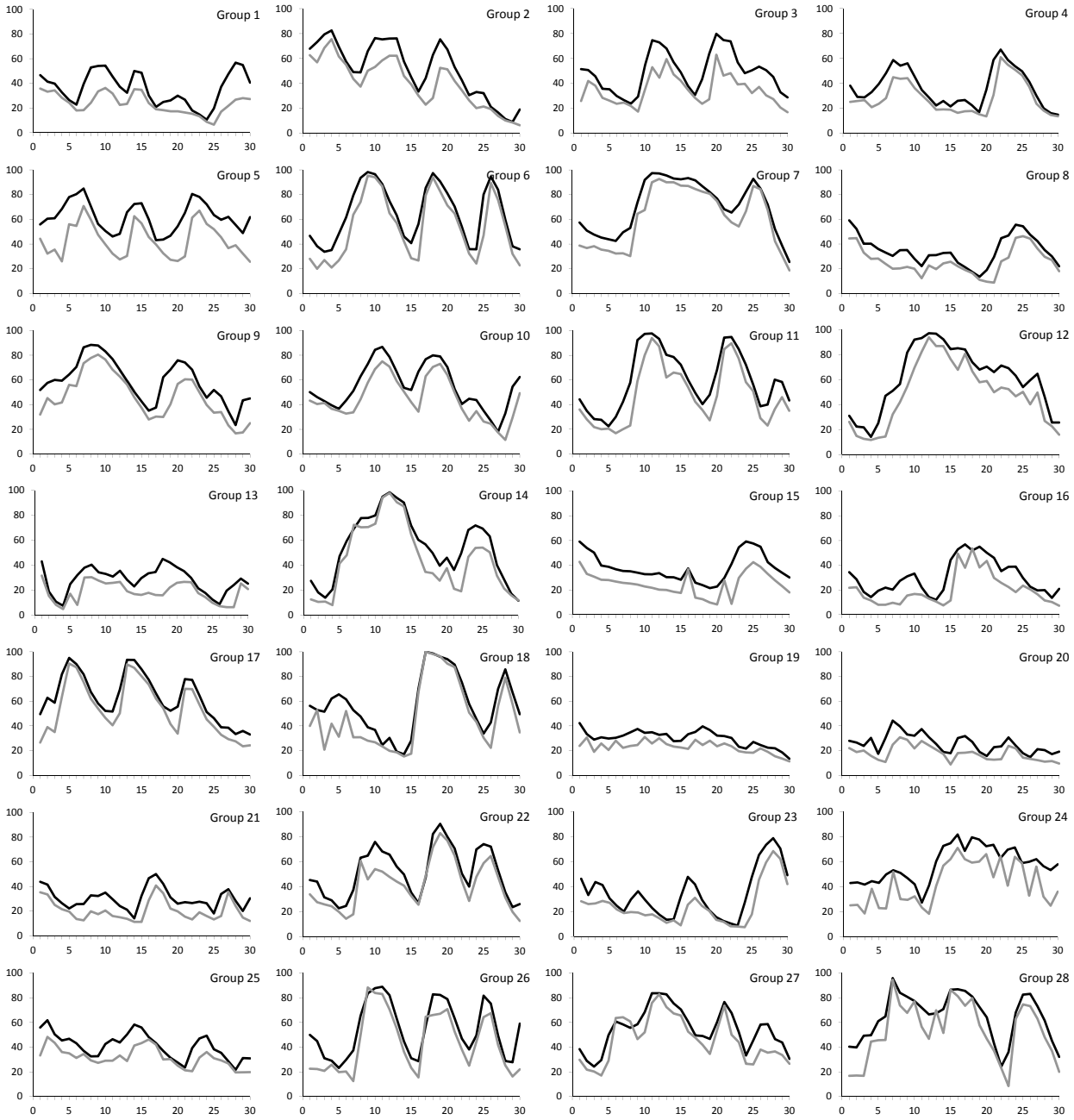

Figure 1: Average numbers (black line) and average winning numbers (grey line) in the 28 groups over all 30 rounds.

potential leading bids. Thus, the criterion provides a conservative classification ensuring that the differences in characteristics between leaders and non-leaders are not artificially amplified. Variations of the leadership criterion, such als shifting the value of 30 points to 20 or 40 do not have a qualitative impact on the results. Appendix D provides an overview of changes in the main results if different criteria of leader definition are applied.

Definition: A number n_{it} of player i in round t is called a *"leading number"* if one of the following conditions is satisfied:

1. $n_{it} = 100$ if $t = 1$ or
2. $n_{it}(t) > n_{j,t-1} \forall j \in [1;8]$ and $n_{it}(t) > n_{i,t-1} + 30$ if $t \in [2;28]$

The player i who places the leading number is called a *"leader"*.

Once leaders are identified according to the above definition we group them into the two subcategories "early" and "late" leaders. Early leaders are the first leaders in their group, late leaders are all subsequent leaders. Late leadership is a weaker variant of leading for several reasons. The innovative aspect of leadership disappears if the leader has already observed someone else leading. Thus, late leaders do not necessarily have to have above-average cognitive skills. Late leaders have also already observed the reaction of their group to leadership. Therefore, they have an easier task in forming a belief about the potential gains and losses of leadership for the leader and the consequences for group efficiency.

	early	late	no leader
Series 1	1.33	1.92	4.75
Series 2	1.15	2.23	4.62

Table 3: Number of leaders per matching group.

With this definition, we identify 31 out of 200 subjects[10] as early leaders[11] and 52 as late

[10]We excluded the data from matching groups number 12, 14 and 15 from the analysis. In these groups, one participant continuously set the number 100 over almost the whole duration of the game. This disabled us from classifying the remaining seven participants in these groups into late leaders and non-leaders, because they had no chance to lead during the whole experiment, even if they wanted to.

[11]There are more early leaders than matching groups, because it happened several times that two subjects led early in the same round.

leaders. Accordingly, 117 subjects are classified as non-leaders. Table 3 summarizes the average number of early, late and non-leaders per matching group across the two series' of sessions. A chi-squared test clearly rejects differences in the two distributions (p-value > 0.7). In each group but one, we are able to identify at least one early and one late leader. The exception is group 7, where we have two early leaders in the first round, but no late leader. More than half of the leading numbers had the value 100. Out of the 31 early leaders, 42 percent were leading at least once more in later rounds, among the 52 late leaders this holds for 33 percent. Roughly half of the leaders keep or even increase their number in the next round after leading (43 percent if we double-count leaders leading more than once, 51 percent if we count them only the first time they are leading), about one third (32 or 40 percent) do not lower their number for at least two subsequent rounds. The longer the first leader in a group stays at or above the leading number, the stronger is the effect on followers' behavior. On average, an additional round of early leading increases the average number of the other subjects in that group in all subsequent rounds up to round 28 by 2.3 points (see the GLS regression in Table 4). Furthermore, the scatterplot in Figure 2 illustrates that the higher a leading number is, the larger is the reaction of the other group members.

	Number
Duration of early leadership	2.32***
	(0.544)
Period	-0.106
	(0.188)
Constant	41.18***
	(5.532)

Table 4: Regression coefficients: subsequent numbers of the other group members depending on the duration of initial leadership. Standard errors in brackets. *** denotes significance at the 1% level, ** at the 5% level and * at the 10% level. Standard errors are clustered by matching group. Subject random effects are included.

4.1 Does leadership pay for the leader - and what do they expect?

We start our analysis of the characteristics of leaders with the question of whether leadership is profitable for the leaders. On average, early leaders earn 498 points in the experiment, late leaders 533 points, and non-leaders 595 points. Thus, leaders earn significantly less than non-

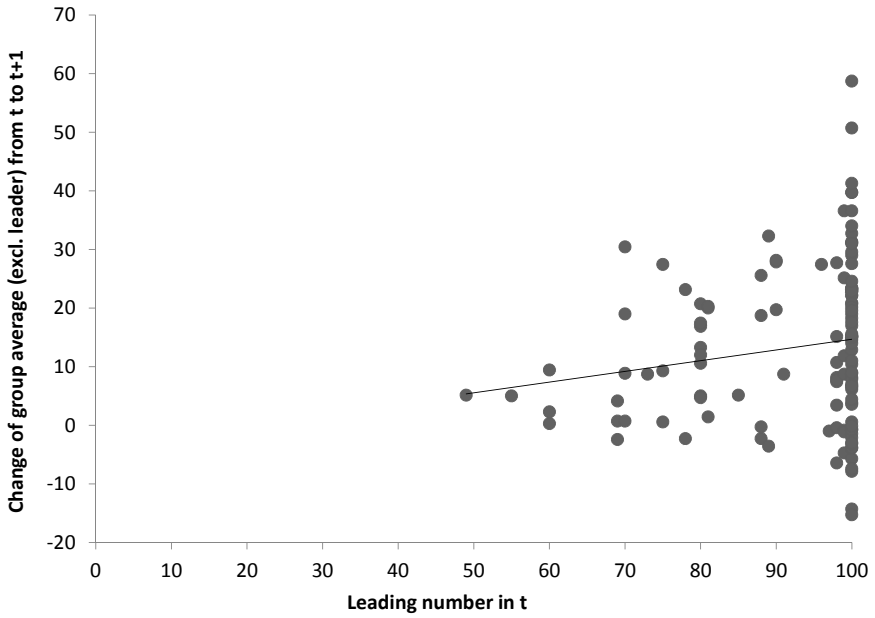

Figure 2: Average reaction of other group members to different leading numbers.

leaders (p-value = 0.01).[12] [13] This finding is not surprising given that leaders deliberately forgo the possibility to win their match while leading and also in other rounds choose higher numbers (56.58) than non-leaders (45.15) on average (p-value = 0.00). More relevant for the motivation of leaders (and much more difficult to answer) is the question whether leaders benefit from leading compared to the counterfactual situation in which they do not lead - and whether they correctly anticipate their net monetary loss or benefit from leading. We cannot answer the first question because we do not have a reference point which we could compare leaders' profits with. However, we can say that they are very good in anticipating the extent to which others will follow their good example, which is the basis for their own expected loss or gain from leading. We use the beliefs submitted for the average number of the seven fellow

[12]For a statistical comparison of leaders and non-leaders we treat each matching group of eight participants as one independent observation. Thus, we consider 25 independent observations, 12 for the first series and 13 for the second series. Within each group, we average the scores for each measure, e.g. the profit, over all early leaders, late leaders and non-leaders separately. All reported significance levels in this paper are then obtained (if nothing else is stated) in one-sided Wilcoxon signed rank tests testing the measures of (early or late) leaders against the non-leaders in each matching group. We do not correct for multiple hypothesis testing since we have ex ante hypotheses for almost all tests that we conduct.

[13]Average profits per matching group are not significantly different in the two series' (p-value = 0.28, two-sided test).

participants.[14] If leaders overestimated the average number of the others, it was very likely that they overestimated their gain from leading as well. Leaders are generally very good in estimating the reaction of others after their leading bid. If at all, they slightly under- rather than overestimate the average number of the seven other participants in the round after their leading bid (by about 4 units on the scale from 2 to 100). The quality of their estimate does not depend on whether they were leading in the previous round or not, and it is also not different from the quality of the estimates of the non-leaders. We conclude that over-optimistic beliefs as in Gächter et al. (2012) are not driving leadership in the framework of our number choice game. This makes it unlikely that selfish motives are the major driving force for taking the initiative. Nevertheless, we cannot exclude that some leaders are leading because they expect to earn more by leading than in the counterfactual situation without any leader.

Result 1 *(i) Leaders earn less than non-leaders. (ii) Leaders have realistic beliefs about how much the followers respond to their leading decision.*

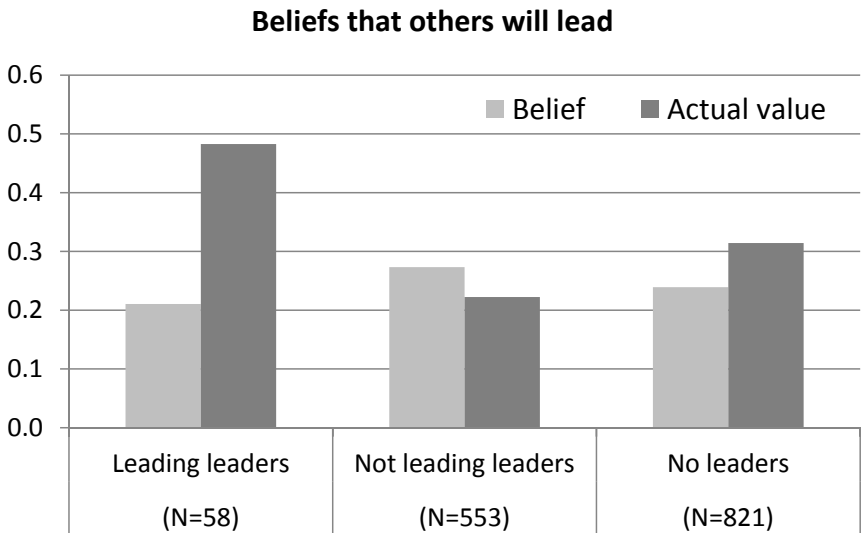

Figure 3: Belief that others will lead.

The decision to lead might not only depend on the belief whether others will follow but also on the belief whether someone else in the group will take the initiative instead. To capture

[14]Given the simplifying assumption that beliefs b are uniformly distributed within each of the five intervals 2-20, 21-40, 41-60, 61-80, and 81-100, we computed a point estimate for the predicted average of the other seven numbers as $(b_{2-20} * 11 + b_{21-40} * 30.5 + b_{41-60} * 50.5 + b_{61-80} * 70.5 + b_{81-100} * 90.5)$.

this belief, players had to submit an estimate for the probability distribution over the intervals 2-20, 21-40, 41-60, 61-80, 81-100 of the maximum of the other seven numbers in their group in each round. We approximate the belief on whether there is another leader with the percentage weight given to the upper interval from 81 to 100.[15] Figure 3 contrasts this belief with the actual frequency of at least one other group member choosing a number larger than 80, separating between leaders who are currently leading, leaders who are currently not leading and non-leaders. The data behind this illustration contains only values from rounds where leading was generally possible, i.e. rounds in which the maximum number in the round before was smaller than 100 and the minimum number was smaller than 70. In line with the argumentation of a false consensus effect (Ross et al., 1977) leaders would generally overestimate others' willingness to lead (p-value = 0.12, two-sided test) while non-leaders underestimate the probability that there would be a leader (p-value = 0.11, two-sided test). However, in their leading round, leaders underestimate others' willingness to lead (p-value = 0.03, two-sided test). In our interpretation, this pessimistic belief about the probability that others will lead additionally motivates leaders to take the initiative.

4.2 Leaders attach a high value to efficiency

Figure 4 summarizes the decisions of all participants in the role of player 1 in the distribution games. Leaders have stronger preferences for efficiency and they are more generous. We find the most notable difference for early leaders when there is a conflict between efficiency and equality. In this game, early leaders have an average efficiency score[16] of 1.52, while late leaders and the followers have an efficiency score less than 1. Table 5 summarizes the significance levels. For efficiency concerns it holds that early leaders choose the efficient option more often than all

[15]This approximation does not perfectly match actual leading bids, which could also be lower than 81. Eliciting beliefs perfectly fitting our leadership criterion would have meant using an even more detailed and complex elicitation procedure which we abstained from for practical reasons.

[16]This score takes the highest value 3 if a player chooses the efficient option in the game where the efficiency gain of this option compared to the selfish option is the smallest. If a player does not choose the efficient option in this game but does so in the next game where the efficiency gain increases by one unit, this player gets a score of 2. Similarly, a score of 1 is assigned to players choosing the efficient option only in the third game. Otherwise, the score is zero. If we simply take the sum of efficient choices as a subject's score, total scores are slightly smaller, but the relative differences between them and the statistical significance levels remain almost the same.

other players, and late leaders score higher than non-leaders. For generosity we observe a similar pattern, only the difference between late and non-leaders is just not significant. With respect to the games testing a preference for equality and efficiency versus selfishness, the major difference is between leaders and non-leaders, with no significant distinction between early and late leaders.

To find out whether social preferences are purely coincidental attributes of leaders or in fact a motive for leading, we analyzed their answers to the open questions in the post-experimental strategy questionnaire. In this questionnaire 69 percent of the early leaders explain that they started choosing a very high number because they wanted their group to coordinate on the socially optimal outcome, compared to null early leaders stating that they were leading in order to increase their own profit in future rounds (p-value = 0.00, one-sided test).[17] For the late leaders, the efficiency motive occurs in 29 percent and the selfish motive in 20 percent of the statements (p-value = 0.05). Taken together, the results from the distribution games and the answers from the questionnaires indicate that concerns for others' outcomes are a driving force for leading-by-example. Leaders are more pro-socially minded than non-leading players.

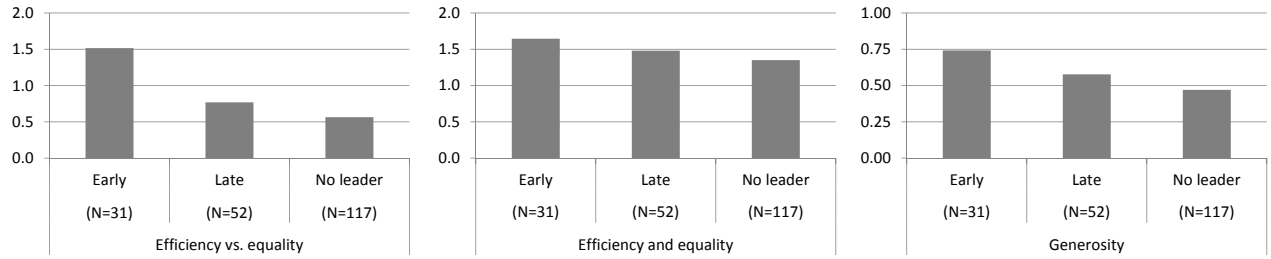

Figure 4: Valuation of efficiency, equality and generosity.

	early vs. no leader	early vs. late	late vs. no leader
Equality vs. Efficiency	0.00***	0.01**	0.07*
Equality and Efficiency	0.02**	0.17	0.09*
Generosity	0.02**	0.03**	0.12

Table 5: Wilcoxon signed rank tests (one-sided) for decisions in the distribution games. *** denotes significance at the 1% level, ** at the 5% level and * at the 10% level.

[17]We had an independent sample of 27 subjects classifying the answers into categories predetermined by us. Each of them received one third of the statements for classification. The statements were presented in random order to the different subjects. They were paid 15 euros for this task. A certain statement is defined to belong to a category if the majority of these subjects classified it accordingly.

Result 2 *Leaders attach a high value to the maximization of others' welfare. Early leaders are more willing to accept disadvantageous inequity than others when it is in conflict with efficiency.*

4.3 Do leaders like attention?

Leaders might lead not because they want to do something good for their group, but because they want their group to see that they are doing something good. Comparing the use of the publicity and the highlight option after the number choice allows us to disentangle these two motives. Figure 5 shows that leaders indeed use the publicity feature. In leading rounds, around 17 percent of the leaders opt for publicity; one round later (when the leaders often still have the highest number in their group), this holds for 12 percent of them. In comparison, non-leaders pay for publicity in less than 1 percent of their decisions and currently not leading leaders in only 3 percent. The differences between currently leading leaders (in t or $t-1$) and non-leaders as well as between currently leading leaders and currently not leading leaders are statistically significant (the p-values are 0.02 and 0.03, respectively). As expected, men use the publicity feature on average more often than women, but the differences between them are far from being significant.

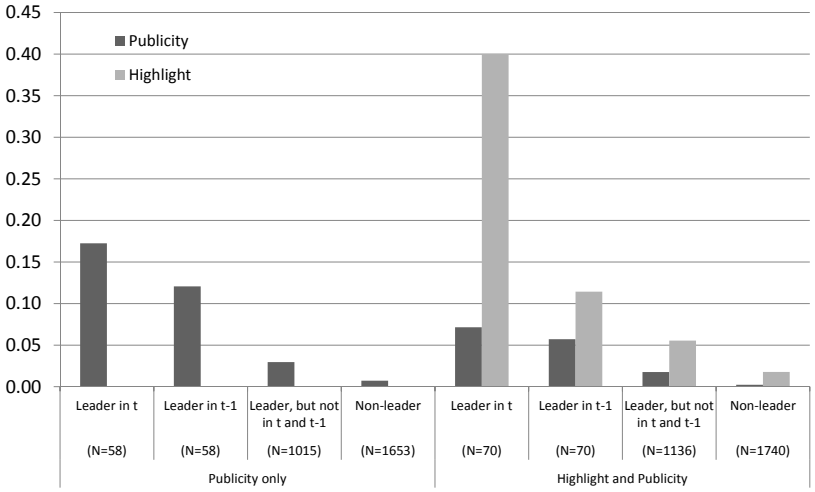

Figure 5: Frequency of payment for giving up anonymity and for highlighting a number.

Adding the highlight option remarkably reduces the use of the publicity feature by leaders in the second series while the highlight option, if available, is used by 40 percent of the leaders. This indicates that leaders in the first series mainly used the publicity option to strenghthen the

effect of giving a good example while the benefit from the positive image of being a leader plays if at all a minor role. When the highlight option is available, the difference between currently leading leaders and currently not leading leaders in the use of the publicity feature is no longer significant (p-value = 0.11) and the difference between currently leading leaders and non-leaders is only weakly significant (p-value = 0.08). Again, men use both the publicity feature and the highlight option more often than women without the difference being statistically significant.

Both highlighting and giving up anonymity have the desired effect to attract attention and strenghthen the signal for coordination within the group. More participants increase their number after a leading bid with (61 percent) than without (44 percent) three exclamation marks aside (p-value = 0.01). Publication of the leader's seat number has a similar, though not significant effect.[18]

Result 3 *Leaders want to direct other's attention to the good example they give, but they are only slightly more likely to reveal their identity than non-leaders.*

The finding that leaders in our experiment are rather not status-seeking seems to contradict the related finding of Arbak and Villeval (2013). However, the results are in fact very much in line with each other. Arbak and Villeval (2013) observe that subjects who volunteered to contribute in the first stage often contribute a much lower amount when assigned to make their contribution in the second stage. More specifically, they say that subjects, who contribute much to the public good when others can see their contribution before contribution themselves, are driven by status concerns. However, this behavior is perfectly comparable to anonymous leading in our experiment, that is, choosing a high number to make others imitate. Thus, what Arbak and Villeval name a "positive social image" for the leader could actually be very similar to "attention for my number" in our experiment.

4.4 Leaders show a high degree of cognitive reflection

After the main experiment, all participants had to answer three questions from a cognitive reflection test (CRT). There was no incentive for giving a correct answer and no feedback.

[18] In only 5 of 25 groups we observe both a leading bid with and without the leader using the publicity option.

Each correct answer gives one point in our evaluation so that participants could get between zero and three points in this task. Figure 6 shows that leaders have a significantly higher score on the cognitive reflection test than non-leaders (p-value = 0.00). The difference is also significant between early and late leaders (p-value = 0.01) and between late and non-leaders (p-value = 0.02).

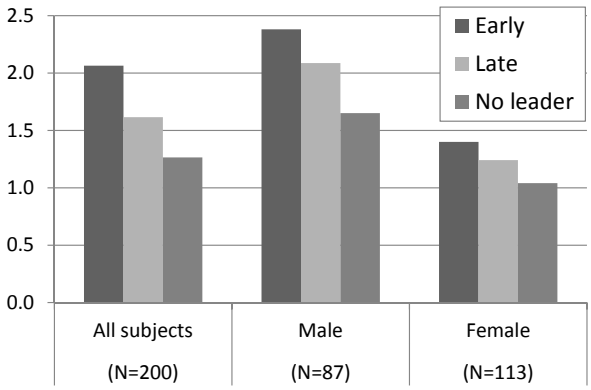

Figure 6: Scores in the cognitive reflection test.

As in Frederick (2005), men score higher in the cognitive reflection test than women. Men are significantly more often early leaders than women (24 percent of the males, 9 percent of the females, p-value = 0.01) and women are significantly more often classified as non-leaders than men (49 percent of the males, 65 percent of the females, p-value = 0.03). However, the relative difference of the CRT scores for leaders and non-leaders does not depend on gender as can be seen in the second and third group of bars in Figure 6 (p-values leaders vs. non-leaders: males = 0.03, females = 0.10). Thus, the special kind of intelligence measured by the cognitive reflection test and more frequently observed with men, seems to be associated with taking-the-initiative in the experiment. The probit regression in Table 6 reveals that this result is statistically significant. While the variable male significantly affects the leading probability without further control, it does not when we control for the performance in the CRT task.

Result 4 *Leaders perform better than non-leaders in a cognitive reflection task.*

Result 5 *Men are more likely leaders. The difference between men and women disappears when controlling for performance in the cognitive reflection task.*

	Leader	Leader
Male	0.413**	0.214
	(0.188)	(0.234)
CRT-score		0.254**
		(0.0991)
Constant	-0.398***	-0.694***
	(0.0907)	(0.126)

Table 6: Regression coefficients: leadership depending on crt score and gender. Standard errors in brackets. *** denotes significance at the 1% level, ** at the 5% level and * at the 10% level. Standard errors are clustered by matching group.

The result that women take the initiative in this experiment less often than men is in contrast to the empirical finding that female leaders apply a transformational leadership style more often than male leaders (see the meta-study by Eagly at al. 2003). The importance of the depth of reflection - as measured by the CRT score - for leadership in our experiment may explain this discrepancy. Furthermore, the difference could stem from a selection effect in empirical studies considering only established real-life leaders, where the strength of selection into leading positions may be stronger for women than for men. As our student sample of subjects is more general than samples consisting of real-life leaders only, we have no such selection.

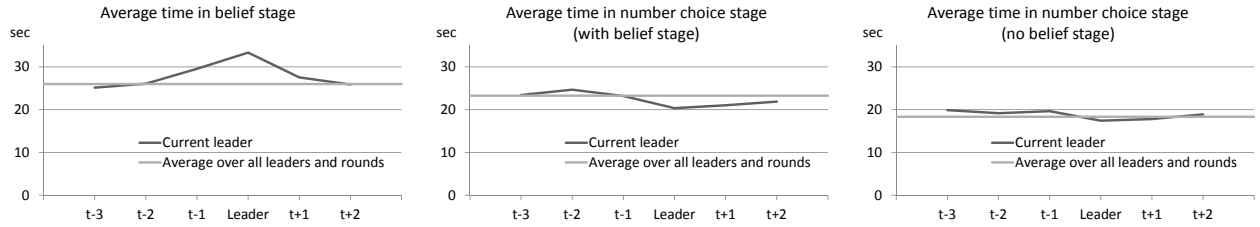

Figure 7: Reaction times.

A second piece of evidence from the recording of reaction times supports the interpretation that cognitive skills are a determinant of leadership in the experiment. Figure 7 illustrates the average time which leaders spend in the belief formation stage and in the decision stage of the experiment. Given the cognitive effort needed before a player decides to lead, we would expect that leading decisions take longer than other decisions.[19] In fact, leaders' belief formation times slow down significantly before their leading decision while the actual decision making gets even

[19]For an interesting application of response time to economic decision making see Rubinstein (2007).

faster in the leading round. Similarly, in the session without the belief stage, decision times slow down before the leading round and get faster in the leading round itself. The regressions in Table 7 show that reaction times of leaders slow down before the decision to lead. "Ever leader?" distinguishes leaders from non-leaders, because it might be that leaders are generally slower or faster than non-leaders in their decisions. The variables "Leader in t", "Leader in $t+1$?", and "Leader in $t+2$?" are dummy variables equal to one if the subject is a leader in the respective round. Using them, the regression captures changes in the response times in the leading round and two rounds before compared to rounds in which the person in consideration acts as a leader neither in the current nor in the two subsequent rounds. As can be seen in the first regression, the formation of beliefs lasts significantly longer in the leading round and already one round before. The time for the actual number choice in the second and third regression slows down in the two rounds before leading and quickens in the actual leading round, significantly only in series 1 with the preceding belief stage.

	Log belief time	Log decision time	
		Series 1	Series 2
Round	-0.0308***	-0.0114***	-0.0166***
	(0.00308)	(0.00169)	(0.00201)
Ever leader?	-0.0211	0.110*	0.0214
	(0.0620)	(0.0566)	(0.0449)
Leader in t?	0.269**	-0.187**	-0.112
	(0.0989)	(0.0822)	(0.0779)
Leader in $t+1$?	0.310***	0.0653	0.0786
	(0.0922)	(0.0593)	(0.0743)
Leader in $t+2$?	0.115	0.0688	0.0621
	(0.119)	(0.0558)	(0.0647)
Constant	3.443***	3.066***	3.008***
	(0.0710)	(0.0444)	(0.0418)

Table 7: Regression coefficients: log of time spend in belief stage and decision stage. Standard errors in brackets. *** denotes significance at the 1% level, ** at the 5% level and * at the 10% level. Standard errors are clustered by matching group.

Our understanding of this change in reaction times is that the decision to lead develops while participants are forced to think about others' behavior in the belief formation stage. The fast leading decision itself could be interpreted to be a self-commitment not to rethink the courageous decision to take the initiative. As the decision to lead seems to be formed already during the belief stage, it could also be that finally entering the leading number simply gets faster than selecting a number potentially maximizing profit against the numbers of the other players shown on the decision screen.

The reaction times of followers after a leading number further support the idea that reaction times provide a measure for the intensity of thought before a decision. The decision times for the number choice of followers significantly (p-value = 0.00) slow down in the round after a leading number (21.99 seconds) compared to rounds where no leading number was set in the two previous rounds (19.00 seconds). In the second round after a leading number, average decision times are with 19.83 seconds still slower (p-value = 0.00). The differences remain significant when considering the data from the first and second series separately. Belief formation times in series 1 get slower as well. In the round immediately after a leading number, the average belief formation time is 27.60 seconds, compared to 24.33 seconds in rounds without a leading number in the two previous rounds (p-value = 0.03). Two periods after a leading number, the belief formation lasts 28.59 seconds, which is again significantly (p-value = 0.01) more than in normal rounds.

4.5 Personality measures

Risk aversion, as measured by the Holt and Laury (2002) lottery procedure, has no significant effect on leadership. Early leaders are a little less (average number of safe choices 5.35), late leaders a little more (5.81) risk averse than non-leaders (5.70), but the p-values in Table 8 are far from any reasonable level of significance. Also the self-stated risk attitude does not differ significantly between leaders and non-leaders. This contradicts our hypothesis that leaders have a more positive attitude towards risk than non-leaders. The reason might be that the risk of leadership is different from (and hardly correlated with) the risk measured with the Holt and Laury (2002) lotteries. Their procedure generates risk as random draws between lotteries while the risk of leadership is a behavioral risk depending on the reaction of followers. The

former requires calculation of expected values while the latter depends on the ability to deal with strategic uncertainty.

	early vs. no leader	early vs. late	late vs. no leader
Holt & Laury risk aversion	0.22	0.17	0.47
Self-stated risk aversion	0.14	0.50	0.17
Patience	0.03**	0.35	0.02**
Locus of control	0.08*	0.03**	0.50

Table 8: Statistical tests for decisions in the distribution games, risk attitude, and gender. *** denotes significance at the 1% level, ** at the 5% level and * at the 10% level.

Patience is required from both early and late leaders as they have to wait for future rounds to regain the profits waived while leading. As can be seem from Figure 8 and Table 8, both types of leaders score significantly higher on the scale of patience than non-leaders,[20] but the absolute difference is rather small.

Result 6 *Leaders are more patient than non-leaders. Risk aversion does not play a role for the decision to take the initiative.*

Following Piatek and Pinger (2010), we consider the locus of control as a unidimensional concept. The distinction into an internal and an external dimension of the locus of control as implemented, for example, by Caliendo, Cobb-Clark and Uhlendorff (2010) does not seem convincing to us, because we cannot imagine what else a non-internal locus of control should be if not an external locus of control and vice versa. The results of a factor analysis for the ten items of the locus of control questionnaire point in the same direction as the intutive argument above: It indicates that item 1 has a negative loading on the main factor, while items 4, 6 and 9 neither load on the same factor as the other seven items nor on a joint second factor. Consequently, our index of the locus of control recodes item 1 and excludes the items 4, 6 and 9. This procedure also delivers the highest value for internal consistency as measured by

[20]The questionnaires for patience, locus of control and big five personality traits included the statement "You can trust my answers." as the last item. Subjects selecting an answer of less than the maximum minus 1 unit to this item were not considered in the analysis. This concerns four subjects in the big five questionnaire, six in the locus of control questionnaire, and three in the patience questionnaire, with considerable overlap between questionnaires.

Cronbach's Alpha, 0.637. For the patience and the big five questionnaire no such adjustments are necessary as already the full scales deliver very reasonable values for Cronbach's Alpha.

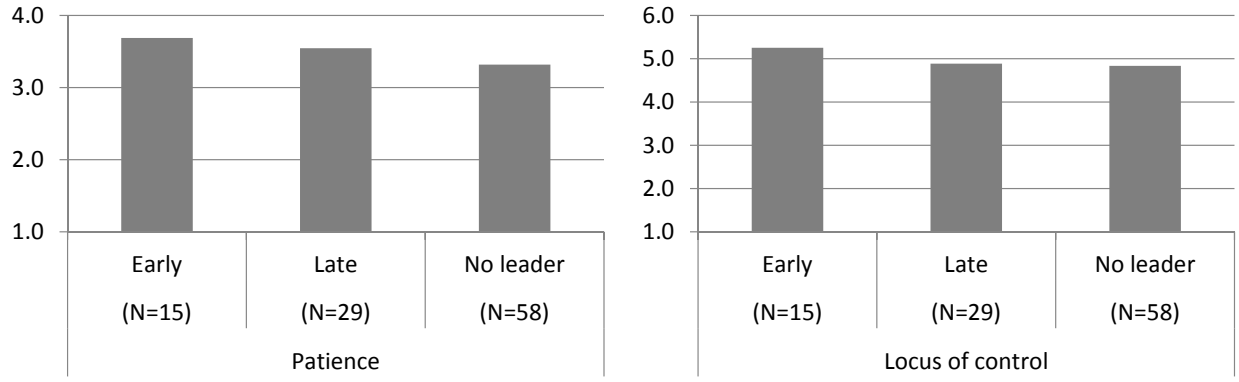

Figure 8: Patience and locus of control (a high value indicates an internal locus of control).

Figure 8 illustrates that an internal locus of control corresponds to leadership in the experiment. The main (but nevertheless rather small) difference for the locus of control in our experiment is between early leaders and late leaders with rather no difference between late leaders and non-leaders. Intuitively, only early leaders need intrinsic confidence that their behavior can change their environment while late leaders have already observed that leading behavior by other players does have the desired effect.

None of the big five personality traits has a significant effect on leadership. The average scores of early leaders, late leaders and non-leaders with respect to all five traits show almost no differences. If at all, a low score on the scale for neuroticism seems to be associated with leadership in the experiment (p-value early vs. non-leaders = 0.09, two-sided test), a result which is also reported by Arbak and Villeval (2013).

Result 7 *Leaders have a more internal locus of control. Other personality traits do not play a role for leadership in the experiment.*

4.6 External validity

Table 9 reports two measures for the external validity of our classification of subjects into leaders and non-leaders, reflecting the implicit power motive. The data stems from the second series of the experiment. The "real life index" consists of the sum of the values of the seven variables

revealing participants' actual propensity to take the initiative in real life. "Index stories" refers to the sum of points received for the stated behavior in the hypothetical situations.[21] This index can vary between +5 and −5.

		Early	Late	Non-leader
Implicit power motive	Real life index	3.20	2.17	2.35
	Index stories	2.47	2.31	1.45
Explicit power motive	Self-stated personal initiative	0.68	0.66	0.66
	Leader in a student work group	0.53	0.38	0.42
	Wants to be in a leading position	0.60	0.45	0.55

Table 9: Measures for the implicit and explicit power motive.

Both indices show that being classified as a leader in the number choice game corresponds to taking the initiative in life outside the lab. For the real life index, early leaders on average receive a score of 3.20 while non-leaders have a score of only 2.35 (p-value = 0.02). Late leaders are not significantly different from non-leaders with respect to this index. For the story index, we find that both early and late leaders score higher than non-leaders (p-value leaders vs. non-leaders = 0.05).

In addition to the implicit power motive revealed in the real life index we also assessed participants' self-evaluation of their proneness to take the initiative, reflecting their explicit power motive. They had to classify themselves as more or less "initiative-taking", they were asked whether they would like to work in a leading position, and what role they typically have in a student work group. None of these self-stated measures corresponds significantly to taking the initiative in the experiment. We interpret this finding as evidence that taking the initiative is a facet of personality which is different from the self-stated claim to be a leader.

Result 8 *(i) Leadership according to the behavior in the number choice game corresponds to the degree of taking the initiative in participants' real lives. (ii) The self-stated claim to be a leader does not correspond to leadership according to the behavior in the number choice game.*

[21] We excluded the data of two subjects (number 202 and 223) from this part of the analysis, because they stated orally during the experiment that they misunderstood the question and answered the first questions in this part randomly before asking for an explanation of the question.

5 Conclusion

What are the characteristics behind leadership? We address this question for a particular type of leadership, taking the initiative. So far, the implicit power motive behind such behavior seemed difficult to assess. In a simple questionnaire, people who just want to be a leader without having a particularly social motivation would also state that they take the initiative whenever possible. The most common alternative measurement method is the picture story exercise (Pang 2010). In this test, ambiguous pictures of everyday situations are presented to participants, who have to imagine and write stories explaining the picture. The written stories are interpreted by the experimenter as representing a stronger or weaker implicit power motive. In this paper we use an experiment which can identify people who take the initiative in an incentive compatible way.

We combine the experimental design in which leadership develops endogenously with several other small games and questionnaires to explore possible characteristics of leaders. Our design permits classification of subjects as leaders and non-leaders and to study the determinants of leadership. We find that traditional personality measures are not predictive for leading-by-example, but characteristics such as an internal locus of control and patience are. Further main determinants are that leaders attach a high value to efficiency, are not envious and they have better cognitive abilities than non-leaders. We can show that taking the initiative in the experiment corresponds to similar behavior in participants' real lives outside the lab, but it does not correlate with the self-stated intention to be a leader.

Our results have implications for the creation of an environment, be it in firms or public administration, where initiative is desired. As we find that leaders have above-average cognitive abilities and are willing to use them, we recommend creating a stimulating environment, as for example, the company Google is known for.[22] The rather internal locus of control leaders have calls for measures fostering trust in self-efficacy such as the assignment of responsibility. From the finding that leaders attach a high value to efficiency we conclude that it is worthwhile to give potential leaders reason to believe in the successful implementation of their suggestions.

[22]See, e.g. "A Place to Play for Google Staff", New York Times, March 16, 2013, p. B1.

Leadership, and in particular leading-by-example, is an important and desired trait for many jobs. Thus, measurement devices and the pattern of determinants of this behavior are highly desirable. Our results suggest that traditional personality traits are not very predictive for this behavior. This implies that leadership has to be assessed in a different way. We do not claim that our experiment provides the only way to do so. For example, it is not deception proof, and the measure of leadership depends on the comparison group. Once participants know about the purpose of the game, the classification into leaders and non-leaders according to behavior in that game is hardly possible. Nevertheless, it provides interesting insights into the mechanism of taking the initiative and suggests a new way to measure a disposition for taking the initiative.

Appendix A: instructions

General instructions at the beginning

Welcome and thank you for participating in this economic experiment.

This experiment consists of multiple parts. The instructions for the first two parts of the experiment will be displayed on your computer screen. The instructions for the third part will be handed out later in hard copy. All instructions are identical for all participants.

Please read the instructions carefully. If you have any questions regarding the experiment please raise your hand. We will then come directly to your place. Please be quiet during the experiment and do not talk to other participants. Failure to comply with these rules will result in an exclusion from the experiment. If this occurs you will not receive any payment.

After you have completed all three parts of the experiment please fill out the following questionnaires on your computer screen. Afterwards you will receive your payment for the entire experiment. The order in which participants receive their payments is already determined, beginning with the participant sitting at the computer "lakelab 1". So take your time to fill in the questionnaires. Your speed will have no influence on the timing of your payment.

Instructions for the number choice game

Now we will start with the third part of the experiment. After this part the experiment will be over and we will ask you to fill out some questionnaires.

Your gains and losses during the experiment are counted in points. The exchange rate is 30 points for 1 euro. Your payment in this part of the experiment depends on your decisions and on the decisions of other participants.

This experiment will last for 30 rounds. In each round you will be asked to choose a number between 2 and 100. Subsequently, the computer will randomly determine one participant out of a group of eight and compare the numbers you and the other participant have chosen. The participant who selected the smaller number receives as many points as her number. The other participant receives zero points in this round. If both of you selected the same number, each of you gets half of the points. At the end of each round you are informed about your payment in points and about the numbers all participants of your group have chosen. The composition of your group of eight does not vary during the 30 rounds. Out of this group in each round one participant will be randomly chosen and your numbers will be compared.

In each round before choosing a number you will be asked to make an estimate about the numbers which the other seven participants of your group are going to choose in this round. More specifically, you have to submit your belief about what is going to be the highest, the lowest and the average number of the other seven participants. We ask you to forecast the probability of these three numbers (maximum, minimum and average) being within the following intervals: 2-20, 21-40, 41-60, 61-80 and 81-100. For each of the five intervals you have to indicate the percentage value of the three numbers (maximum, minimum, average) being within these intervals. The five percentage values add up to a total of 100%, because the numbers have to be within one of the intervals no matter what. We place a graphical computer program at your disposal so you can enter your beliefs. You will have the opportunity to familiarize yourself with the program before the experiment begins. Here you can see what the program looks like:

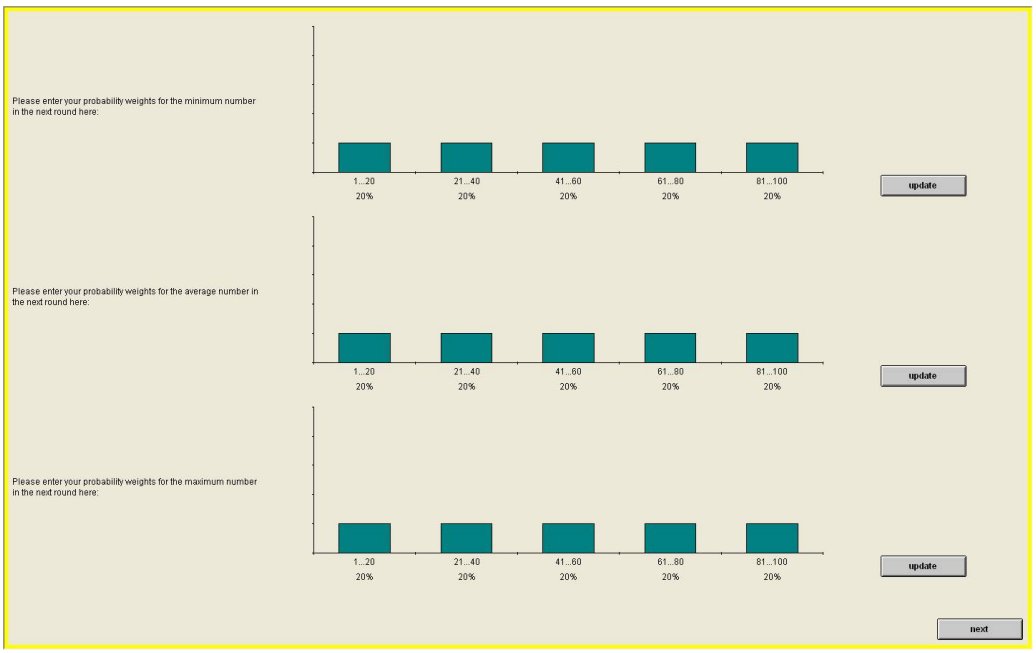

Figure 9: Belief formation tool.

You can change the height of the bars implemented in the program by clicking on a bar, holding the left mouse button and moving the mouse. Do not worry about whether the percentage values add up to 100 or not. Just change the heights of the bars until their proportions match the relative probability you propose. Then click on the button "update" next to the diagram. The bars are automatically adjusted so the values of your estimates sum up to 100. After entering your belief for minimum, maximum and average please click on "next". Next you can choose your number for the coming round.

There will also be a payment for the accuracy of your guess. The exact computation of this accuracy-dependent payment is described in detail in the appendix. If you have no interest in the details, feel free to ignore the explanations concerning this matter. The only important thing you have to know is that you maximize your payment by indicating your true beliefs.

From the second round on, your previous estimates will be the default setting, so you only have to indicate new numbers in case you want to adjust your previous estimates.

Your decisions in this experiment are always anonymous. The other participants of your group can only see the number you (and all the other participants) have chosen, but not the number of the computer you are sitting at. The numbers are ordered by size. So it is not

possible to draw conclusions about participants' seats from the numbers. If in a particular round you want the other participants not only to know the number you have chosen, but also the number of the computer you are sitting at, you can determine so with a mouse click on your computer screen. To disclose the number of your seat you have to pay 10 points.

Before the experiment begins you have the opportunity to familiarize yourself with the computer program. After the experiment please fill in the questionnaires. You will be paid in cash directly after the end of the experiment and after you have finished the questionnaires.

If you have any further questions regarding the conducting of the experiment, please give a short notice to the supervisors of the experiment. We will then come directly to your place.

Payment for probability estimates

As previously described, for the three numbers maximum, minimum and average you allocate five probability values p_i to the five intervals 2-20, 21-40, 41-60, 61-80 and 81-100. The actual number (for example the minimum) lies later in one of these intervals. For one probability estimate you can earn 2 points at most. If your estimate is not accurate there will be subtractions from the 2 points. The probabilities you have assigned to intervals in which the actual number does not lie, will be squared and subtracted from your maximal payment. For example, if you set 70% on the lowest interval but the actual number does not lie in this interval, $0.49 = 0.70 * 0.70$ points will be subtracted from your payment. Furthermore, it is disadvantageous if the probability value you distributed to the interval in which the actual number lies deviates significantly from 100%. This deviation will also be squared and subtracted from your payment. If you set 60% on the right interval, $(1 - 0.60) * (1 - 0.60) = 0.16$ points would be subtracted.

The smaller the sum of the squared wrong estimates is, the better was your guess. For those who are interested, here is the mathematical formula to calculate the quality Q of your guess:

$$Q = 2 - \sum p_{wrong,j}^2 - (1 - p_{right})^2$$

In each round the computer will calculate the quality Q of your estimate for minimum, maximum and average number. The higher the quality Q is, the better was your guess in that particular round. At the end of the 30 rounds of the experiment your 30 values of Q

for minimum, maximum and average will be summed up. This value will be added to your payment in points.

Examples

In the following we will describe some examples of the calculation of the quality of your estimate and demonstrate some useful tips on how to improve your estimate.

If you think that the smallest of the seven numbers of the other participants of your group definitely is equal to or smaller than 20, you say the probability of the minimum being within the interval 2-20 is 100% and the probability for the minimum being within one of the other intervals is 0%. In this case you gain 2 points if your guess is correct and no points are subtracted for false estimations, because you were 100% right. If you had distributed 20% to each of the five intervals, you would have scored only 1.2 points. In general: if you are sure about the actual number not being within a certain interval, it is better for you to assign a probability of 0% to this interval. Intentional probability "dispersion" does not pay off.

If you think that the highest of the seven numbers of the other participants of your group is either in the interval 61-80 or is higher than 80, but you are sure that the maximum definitely lies above 60, you should assign the value 50% to both intervals 61-80 and 81-100. In this case, your expected payoff is higher than in case you assigned 100% probability to only one of the intervals: If you assign 50% to both of the intervals, you surely gain 1.5 points. If you assigned the entire 100% to one of the intervals, you gained 2 points in case you were right and 0 points in case you were wrong. So your expected payoff would be only 1 point. In general: If you think that a given number is possibly within several intervals and the probability of the number being in each of these intervals is equal, it is best for you to enter equal probabilities to these intervals.

Appendix B: patience questionnaire

In the following you find a list with statements. You will probably agree completely to some and not at all to others. To some others you might be undecided.

Please answer according to the following scale. If you do not agree to the statement at all, then select the button to the left. If you agree to a statement completely, then mark the button to the right. In between you can grade your opinion.

Please state what you really think. Nobody is here you have to impress. The results can only be used scientifically if you answer honestly.

I frequently feel like hurrying others.
If I want something I get it.
I always have something to do in case I have to wait.
I am often in a hurry.
I often lose track of what people are saying if they go on for too long.
I consider myself as easy going.
I have trouble finding time to get my hair cut.
I wait too long to act.
I get things accomplished without undue stress.
I have enough time to do the things that are important to me.
I work fast.

Remark: Items 1, 2, 4, 5, 7, and 11 were recoded in the analysis.

Appendix C: questionnaire "index stories"

In the following different scenarios are going to be presented to you.

For each scenario we offer you different action alternatives.

Please select for each scenario which action alternative you would pick most likely and which you would pick the least.

Question 1:

The bus you have to take to the university every day is overcrowded. Since your stop is near the beginning, everyone has a spot on the bus. However, this is not true for later stops, some people had to wait for the next bus. What would you do?

a) I am going to write a letter to the bus company and ask them to reduce the problem by putting in another bus on this line. (-1; 1)[23]

b) As long as I get in I do not care. (1; -1)

c) Because these many people bother me in the morning, I decide from now on to take a bus earlier or after the busy times whenever possible. (0; 0)

d) If it goes on like this the bus driver will soon realize that a change is necessary - and after all it is his task to make sure to transfer all passengers. (1; -1)

Question 2:

A good friend of yours is celebrating his birthday in two days. Among your friends it is common to buy a present from all of you. It is in the middle of February and exams are right ahead. Since everybody is studying nobody volunteers to get the present. What would you do?

a) As everybody knows I am going to write one more exam than the others. The others will consider this for sure and are going to leave me out of the organisation of the present. (1; -1)

b) I propose that I will think about a present and somebody else will organize it. (-1; 1)

c) I will go to the city after my class and check if I can find something suitable. (-1; 1)

[23]In brackets after each statement are the points for answering: (fits the least; fits the most).

d) Since I have to study and I am hesitating to go to the party anyway I will keep out of it. (1; -1)

Question 3:

Since the introduction of tuition fees the university library has more financial resources. But there are still not enough copies of a standard reference which is needed by the second term students for their exam. What would you do in this situation?

a) I will buy the book at Amazon. (0; 0)

b) I will organize a study group with fellow students. So we can study together with one book. (-1; 1)

c) I will go to the information desk of the library and ask them to get another copy of the book. (-1; 1)

d) I assume that the professor knows about the shortage of the books and that he will not ask too many details in the exam. (0; 0)

Question 4:

Recently you moved in with two friends. So far there are not any agreements about the cleaning of the shared rooms (kitchen, bathroom). What would you do?

a) I will get an organizer in which I will list who will have cleaning duty in which week. I will start. (-1; 1)

b) I will clean the kitchen and bathroom when the rooms become too dirty for me. (-1; 1)

c) Since I am at the university all day and going home at the weekends, I make little dirt and do not feel responsible for cleaning. (1; -1)

d) I plan to talk to my roommates on the next occasion. (0; 0)

Question 5:

The cleaning staff did not refill the soap in the washing room of your working place for some days. How would you react?

a) I will post a note for the cleaning staff at the door to the washing room, they should remember refilling. (-1; 1)

b) I will bring my own soap and put it at the sink. (-1; 1)

c) That does not bother me. I rarely wash my hands with soap.(0; 0)

d) The cleaning staff gets controlled regularly at a random basis, somebody who is responsible for it will realize it soon. (1; -1)

Appendix D: variations of the leadership criterion

The following table provides an overview how variations in the leadership criterion affect our main results. The first block repeats data from the criterion used in the paper. The second block adds the condition that the leader's number has to exceed the previous maximum by at least 10 points. Blocks 3 to 5 vary the required increase of the leader's number from round $t-1$ to the leading round t from its original value of 30 to 10, 20, and 40. Finally, the sixth block applies the same criterion as in the paper but includes only leading numbers which have the value 100.

The values for efficiency, CRT score, and risk aversion are computed based on the whole dataset of 200 participants. For the stories index and the real life index, the data comes from the second series only.

Qualitatively, the results of the different criteria look very similar to the definition used in the main text, and also the results of statistical tests are robust to the variation, with only one major exception. When only increases to 100 are allowed as leading numbers, differences between leaders and non-leaders with respect to the real life index turn out to be no longer significant.

	Early	Late	Non-leader
$n_{it} = 100$ if $t = 1$ or $n_{it}(t) > n_{j,t-1} \forall j \in [1;8]$ and $n_{it}(t) > n_{i,t-1} + 30$ if $t \in [2;28]$			
Absolute Number	31	52	117
Efficiency	1.52	0.77	0.56
CRT score	2.06	1.62	1.26
HL risk aversion	5.35	5.81	5.7
Stories index	2.47	2.31	1.45
Real life index	3.2	2.17	2.35
$n_{it} = 100$ if $t = 1$ or $n_{it}(t) > n_{j,t-1} + 10 \forall j \in [1;8]$ and $n_{it}(t) > n_{i,t-1} + 30$ if $t \in [2;28]$			
Absolute Number	31	51	118
Efficiency	1.52	0.78	0.56
CRT score	2.06	1.65	1.25
HL risk aversion	5.35	5.84	5.69
Stories index	2.47	2.39	1.42
Real life index	3.2	2.14	2.36
$n_{it} = 100$ if $t = 1$ or $n_{it}(t) > n_{j,t-1} \forall j \in [1;8]$ and $n_{it}(t) > n_{i,t-1} + 10$ if $t \in [2;28]$			
Absolute Number	31	61	108
Efficiency	1.39	0.80	0.56
CRT score	1.94	1.59	1.29
HL risk aversion	5.29	5.75	5.74
Stories index	2.75	2.09	1.42
Real life index	3.25	2.18	2.33
$n_{it} = 100$ if $t = 1$ or $n_{it}(t) > n_{j,t-1} \forall j \in [1;8]$ and $n_{it}(t) > n_{i,t-1} + 20$ if $t \in [2;28]$			
Absolute Number	31	58	111
Efficiency	1.48	0.74	0.58
CRT score	2.00	1.60	1.27
HL risk aversion	5.23	5.79	5.74
Stories index	2.67	2.13	1.45
Real life index	3.20	2.16	2.37
$n_{it} = 100$ if $t = 1$ or $n_{it}(t) > n_{j,t-1} \forall j \in [1;8]$ and $n_{it}(t) > n_{i,t-1} + 40$ if $t \in [2;28]$			
Absolute Number	31	49	120
Efficiency	1.52	0.71	0.59
CRT score	2.06	1.63	1.27
HL risk aversion	5.35	5.78	5.72
Stories index	2.47	2.33	1.47
Real life index	3.02	2.15	2.35
$n_{it} = 100$ and $n_{it}(t) > n_{j,t-1} \forall j \in [1;8]$ and $n_{it}(t) > n_{i,t-1} + 30$ if $t \in [2;28]$			
Absolute Number	27	30	143
Efficiency	1.44	0.67	0.66
CRT score	2.00	1.73	1.33
HL risk aversion	5.52	5.57	5.73
Stories index	2.46	1.21	1.85
Real life index	2.85	2.36	2.36

Table 10: Impact of variations of the leadership criterion on the main results.

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