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Max Planck Institute for Research on Collective Goods, Kurt-Schumacher-Str. 10, D-53113 Bonn
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Crime as Conditional Rule Violation*

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Abstract

Most of the time most individuals do not commit crime. Why? One explanation is deontological. People abide by legal rules just because these are the rules. In this perspective, the power of normativity is critical. It is supported by experimental evidence. To an impressive degree, participants even abide by arbitrary, costly rules, in the complete absence of enforcement. Yet do they also do that if they learn that some of their peers violate the rule? The experiment shows that rule following is conditional on social information. The more peers violate the rule, the more participants are likely to do so as well, and the more severely the violation. This main finding replicates in a vignette study. The effect is most pronounced with speeding, weaker with tax evasion, and absent with littering. In the lab, social information has an effect whether it is framed as the incidence of rule violation or of rule following. If they have no explicit social information, participants condition choices on their beliefs. Even merely knowing that they are part of a group, without knowing how others behave, has an effect.

Keywords: decision to engage in criminal behavior, normativity, deontological motives, rule following, social context, social information, conditional rule following

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1. Introduction

Why do individuals commit crime? There have been very different answers to this question. Gary S. Becker believes that it is not crime, but its absence that bears explanation. Provided committing crime gives the potential criminal whatever utility, the individual will do so, unless she is deterred by the threat with a sufficiently probable and sufficiently severe sanction (Becker 1968). This is a radically individualistic concept of crime, and one that assumes perfect rationality, including the proper translation of uncertainty into expected values, and the anticipation and evaluation of any future effects of acting or not acting today. Michael Gottfredson and Travis Hirschi adopt an equally individualistic position. They also explain crime as a choice made by the individual criminal. Yet they make the completely opposite behavioral assumption: individuals only commit crime since they suffer from a personality defect: their self-control is so limited that they fall for the immediate "benefit" from crime, and completely neglect the long-term consequences, including punishment (Gottfredson and Hirschi 1990). This approach assumes that a reasonable person would not commit crime. The irrationality of crime is to be explained.

Criminologists have long questioned purely individualistic explanations of crime. A large literature studies peer effects (this literature is surveyed by Hoeben and Thomas 2019, McGloin and Thomas 2019). A graphic illustration is provided by Philip Zimbardo. He positions identical cars, at the same point in time, in the Bronx, and in Palo Alto. Neither car has a license plate, and the hood is open. The car in the Bronx is pillaged and totally destroyed within 26 hours. The car in Palo Alto remains untouched for a whole week. Yet when Philip Zimbardo himself breaks one of the windows, the Palo Alto car is also destroyed within hours (Zimbardo 1969). The experiment has been influential in developing the "broken windows" theory (Wilson and Kelling 1982), which has informed policy makers in New York (Zimring 2007) and Los Angeles (Wagers 2008).

Zimbardo's result can be rationalized in multiple ways, discussed in the next section. This paper focuses on a motivational channel that has thus far escaped the attention of criminologists. It hypothesizes that individuals may be willing to abide by a rule just because they know it to be the rule.

In philosophical terms, rule following is a deontological motive (on the distinction between utilitarian and deontological motives see Zamir and Medina 2011). The individual does not calculate her own (short- or long-term) well-being. She accepts authority per se (at least as long as it is not patently illegitimate). She may for instance be a Hobbesian, who sees peace as a first order problem, and individual well-being as only a second order problem (Hobbes 1651). Or she may be a Kantian and believe in the categorical imperative: it is everybody's moral duty to behave in a way that could be universal (and therefore feel obliged to obey the rule just because everybody is better off if rules are obeyed) (Kant 1785).

This is not a merely theoretical construct. In earlier experimental work it has been shown that rule following is a surprisingly powerful motive. One graphic test is provided by Kimbrough and Vostroknutov (2016). In their experiment, participants earn money by moving forward on a virtual line. Yet on this line there are "traffic lights". Individuals are instructed to stop at each

traffic light. Surprisingly many participants indeed do, although this means that they earn less money.

Together with Pieter Desmet, I have adapted this paradigm (Desmet and Engel 2021). We have made three main changes to the design. First we have removed any analogy with learnt rules in the real world. Participants earn money by moving any one of 48 sliders to the midpoint of the scale. Yet there are deliberately and patently arbitrary rules, stating that participants should not move more than x sliders. Second we manipulate the opportunity cost of rule following, by making the rules differently strict. This allows us to measure in which ways deontological (rule-following) and utilitarian motives (experimental earnings) compete with each other. The critical manipulation is a repetition of the original design, in the second stage of the experiment. In this stage, participants are able to condition their own choice on the choices of other participants. This design builds a bridge to research on the effect of social information. Social information has been shown to have a strong effect on behavior in the field (see e.g. Frey and Meier 2004, Shang and Croson 2009) and in the lab (see e.g. Berg, Dickhaut et al. 1995, Krupka and Weber 2009). It can be manipulated by giving participants (potentially biased) information upfront about behavior to be expected from others (Engel, Beckenkamp et al. 2014, Engel, Kube et al. 2021).

Even in this setting, that is deliberately devoid of any real life context, surprisingly many participants follow the rule, but the less so the more the rule is demanding. Most interestingly social information never hurts: even if participants learn that only one of six group members has been faithful to the rule, they do not become more likely to break the rule themselves. The more others follow the rule, the more they are likely to follow the rule themselves. This effect is strongest for participants who have very little inclination to follow arbitrary rules when alone. We conclude that people are “conditional rule followers” (Desmet and Engel 2021).

The present experiment puts the design and the research question upside down. I wonder whether conditionality also works in the negative domain. In experimental jargon, the novelty is a framing effect. Rather than telling participants how many of their peers have followed the arbitrary rule, I tell them how many of them have violated the rule. Does this social information increase the incidence or the degree of rule violations? The framing manipulation holds the information constant, but gives it a different meaning. Social information is not presented as information about abiding, but about violating a rule. In multiple contexts, framing has been demonstrated to be powerful (De Dreu and McCusker 1997, Kahneman and Tversky 2000, Hargreaves Heap, Rojo Arjona et al. 2014, Engel and Reuben 2015, Banerjee 2016, Crawford 2018). When informed about the frequency of rule violation, participants might be led to reason: apparently this rule is eroding. Social information might create ambiguity about the character of the rule: maybe it is merely a recommendation? It has been shown that individuals tend to exploit ambiguity to their advantage. It creates “moral wiggle room” (Dana, Weber et al. 2007), which facilitates selfish behavior (Haisley and Weber 2010, Dai, Hogarth et al. 2015), including the decision to commit crime (Horovitz and Segal 2006, Loughran, Paternoster et al. 2011). Understanding whether this valence frame matters has practical importance. Frames are open to purposeful intervention. If government is concerned about an increase in violations

of a socially important rule, they might promulgate information about the degree of rule following ("95% of all tax payers report their income correctly"). They might react if the media use a negative frame ("5% of the tax payers cheat on their tax return"), and stress the high number of rule followers.

The main experiment has the advantage of creating very clean data. This allows me to identify the causal effect of social information, rule severity, and their interaction, on the willingness to follow a rule although it is patently arbitrary and not enforced. But the price I have to pay for identification is the artificiality of the setting. The main experiment focuses on internal validity, at the expense of external validity. As a complement, I therefore add a second experiment that checks, with the help of three vignettes, whether conditionality is also to be expected in three areas of life where rule violations are frequent, and the threat with enforcement is credibly negligible: littering, speeding, and tax evasion: do participants state that they would be more likely to break these rules when noticing that anonymous others violate them as well? The three scenarios also differ in the second dimension of the lab experiment. Arguably, rule breaking has a much higher benefit when evading taxes, and a rather small benefit when properly disposing waste, while the benefit from speeding is intermediate: many drivers find it painful to slow down, and they miss the joy of speed.

I first relate the present paper to the criminological literature on peer effects (section 2), then report on the lab experiment (section 3), and on the vignette study (section 4), and conclude with general discussion (section 5).

2. Peer Effects

For almost a century, criminologists have been interested in the effect of peers on crime (see the surveys by Hoeben and Thomas 2019, McGloin and Thomas 2019). The idea that people might stop deciding autonomously when being a member of the "mass" (Le Bon 1895) has largely been abandoned (McGloin and Thomas 2016: 459). Social context does not deprive individuals of agency. But social context affects how individuals decide, also about committing crime.

Peers may be more or less narrowly defined. In the narrower sense, a peer is a person with whom someone interacts over a protracted period of time, and whom she sees as a relevant part of her social fabric, like one's school, or one's gang. The longterm exposure, and the importance of belonging for one's standing and self-esteem, have a formative potential. What peers do, or what they approve or disapprove, may shape the individual's desires, and her values. Peers may contribute to socialization, which can also be a socialization into a criminal career (Akers 1973). This is the core idea of "differential association" (Sutherland 1947, Matsueda 1988), and of the "social learning" theory of crime (Burgess and Akers 1966). Individuals may, for instance, learn from their peers to tolerate a greater risk of punishment (O'Brien, Albert et al. 2011). In the experiment, the formative channel is closed, as groups are randomly composed, interaction is completely anonymous, and there is no communication.

In a broader sense, a peer is anyone in the social context whose acts or attitudes the individual considers relevant. Criminology theory calls this a "situational" effect (Osgood, Wilson et al. 1996, Warr 2002, Osgood and Anderson 2004). Peers may actively instigate crime, for instance by calling on group members to join them in a criminal act (Warr 1996). This is ruled out in the experiment, as participants cannot communicate with each other, and must decide on their own. Peers affect each other when co-offending (Van Mastrigt 2017). Yet in the experiment, there is no room for joining forces.

Learning that others violate a rule is informative. Being part of a group may make offenders believe that punishment is less likely, or will be milder (McGloin and Thomas 2016). In the experiment, sanctions are excluded by design, which rules this effect out. Within a group, not participating in crime may lead to social sanctions, or even ostracism (McGloin and Thomas 2019: 255). In the experiment, this concern is ruled out by anonymity, and by the one-shot design. Learning that peers violate a norm may serve as a signal that helps individuals update their estimate about the benefit from rule violation (Anwar and Loughran 2011), which may induce the group to cross the "threshold" for engaging in criminal activity (Granovetter 1978, McGloin and Rowan 2015). In principle, acting as part of a group may increase the sensation of fun and thrill (Burt and Simons 2013). But the experiment is ostensibly devoid of any non-pecuniary benefit from violating the rule. Seeing others violate a rule may alert individuals to the opportunity for delinquency (Giordano, Cernkovich et al. 1986). But the experiment is so simple that this opportunity is obvious.

This leaves me with one channel. Learning that others violate the rule may give individuals an excuse. This excuse may help the individual subjectively justify a selfish choice (McGloin and Thomas 2019: 254), and may lead to a "drift" towards rule violation (Matza 1964). If she observes that others violate the rule, the individual may conclude that the rule is actually not as strict, and that it is eroding. The effect is related to de-individuation (Postmes and Spears 1998) and to neutralization (Sykes and Matza 1957). But it is reduced to the bare minimum. In the experiment, whether or not a peer violates the norm does not affect the violator's own well-being, or some overarching group well-being, in whatever form. Only rule abiding per se is at stake.

To the best of my knowledge, there is one other experiment that intends to isolate the effect of peers on the decision to commit crime. McGloin and Thomas (2016) test two groups of student participants on one vignette each. They manipulate the number of others who violate the norm not to destroy the property of others, or to steal. They do, however, have a different research question, and accordingly different dependent variables. They are interested in the impact of peer behavior on the perceived cost and benefit from crime, not on their choices. The closest relation to the present experiment and research question is their measure for "anticipated responsibility". With no other perpetrator, it is on average 4.42 (on a 5 point Likert scale). It is on average reduced to 3.32 with the maximum of 75 peers engaging in the criminal act. But their scenarios are such that they invite a diffusion of responsibility, so that the victim will have a hard time identifying who has caused which portion of harm to them. As I am interested in rule following, in my experiment there is no victim, and hence no room for the dissipation of responsibility. I also elicit how the peer effect interacts with the opportunity cost of rule

following, and I measure how uncertainty about the decisions of peers affects choices, whether mere social context already has an effect, and whether the perception as others abiding by the rule vs. others violating the rule matters.

My experiment also contributes to the large literature (reported in McGloin and Thomas 2019) responding to Travis Hirschi's doubts about the causality of peer influence on the decision to commit crime (Hirschi 1969, Gottfredson and Hirschi 1990). The experiment rules out the two main critiques: since I elicit conditional choices, the measurement of the explanatory variable, i.e. perceived peer influence, is beyond any doubt. And peers are randomly assigned, so that delinquent individuals cannot themselves have sought out peers with a proclivity to crime, which would create a selection effect.

3. Lab Experiment

a) Method

178 participants randomly selected from the pool of more than 6,000, jointly curated by the Max Planck Institute for Research on Collective Goods, Bonn, and the Econ Department of Bonn University, participated in the experiment. 114 of them (64.04%) were female. Mean age was 26.69. 19 were not students. The remaining held various majors. 156 (87.64%) reported that they had already had a paid job (which could also be part time). The experiment was programmed in oTree (Chen, Schonger et al. 2016) and implemented online. It took about 30 minutes. Participants earned 4 € for their participation, and at maximum 9 €, mean 6.39 € (equivalent to 7.55$ on the first day of the experiment).

The design of the experiment and the main hypothesis have been preregistered.[1]

The main experiment uses the strategy method (Selten 1967): conditions are manipulated within, not between participants. It has been shown that the strategy method is not only convenient for generating more data with less participants. It is also generally behaviorally valid, in that results tend to not substantially differ between the strategy method and the play method (Fischbacher, Gächter et al. 2012). Hence generally, conditional choices are a valid equivalent for unconditional choices. Actually, in terms of statistical theory one may even prefer the strategy over the play method. It not only potentially creates cleaner data, in that one may compare choices made by the same participant under different conditions, and thereby partial out individual idiosyncrasies. Actually the strategy method is the most powerful technique to mitigate the fundamental problem of inference: one truly observes the same individual treated and untreated, which is impossible in a between subjects design (see Angrist and Pischke 2008).

The main experiment has two stages. In the first stage, participants decide on their own. Social information is only introduced in the second stage of the experiment. In the first stage, participants only know that the experiment has further parts, but not what these parts are about.

[1] Open Science Framework, August 7, 2021, *https://osf.io/mu943/*. I am grateful to Jean-Louis van Gelder to suggest a complementary vignette study.

Participants are informed that they can earn money by a very simple action. For each of 48 sliders that they position to the midpoint of a line, they earn a piece rate. The midpoint is clearly marked, and moving the slider is easy, so that earning money requires neither skill nor pronounced effort.

However the real effort task, and hence the possibility to earn money, is postponed until the very end of the experiment. The main motive for this design is statistical. Effort choices are not contaminated by feedback, and hence independent. From each participant I can elicit a complete reaction function, rather than merely the choice in the one task that finally defines her payoff. To enable this, in the earlier stages of the experiment, participants are requested to make a commitment. They commit to the maximum number of sliders they are allowed to move at the very end of the entire experiment. They know that they will not earn additional money if, at the implementation stage, they move more sliders than this stated maximum. Participants make this commitment conditional on one of five rules. The rule asks them to move no more than 5, 11, 23, 32 or 41 of the 48 sliders. These rules very transparently manipulate the severity of the rule, and hence the degree of conflict between the deontological motive to follow the rule and the utilitarian motive to make money from the experiment. If a participant faithfully obeys the most severe rule 5, she gives up 48 – 5 = 43 units of income, while the opportunity cost is only 48 – 41 = 7 units with the most lenient rule.

The instructions stress that these rules are not enforced. Participants remain free to choose whatever upper limit they deem fit. After the entire experiment, with 50% probability this first part of the experiment is paid out, and with the counterprobability of also 50% probability the second part. Participants know this. After the entire experiment, there are two random draws. First it is decided whether the first or the second part is payoff relevant. Second, if the first part is payoff relevant, one of the five rules is singled out. Each rule has the same probability to be selected. This too participants know. On the computer screen, participants see 48 sliders. They are reminded of the applicable rule, and of the commitment they have made should this rule be in place. They may now at most move as many sliders to the midpoint as they have decided for this case.

The second stage repeats the first. Hence there are the same five rules. If this stage is payoff relevant, a second random draw determines the applicable rule. Each rule has the same probability to be selected. But now participants are randomly assigned to groups of six. Conditional on the respective rule, participants now make two decisions, one unconditional and one conditional. The unconditional decision is an exact replication of the first stage of the experiment, except that participants know that the remaining five group members simultaneously decide. Subsequently, participants decide again, but now not only conditional on the respective rule, but additionally conditional on the number of group members who have violated the rule, from zero to five violators. To make the conditional choice incentive compatible, participants are informed that one of them is randomly singled out, and that her payoff is determined by her choice conditional on the number of violators (if the second stage of the experiment is payoff relevant in the first place). For the remaining five participants, if the second stage of the experiment is payoff relevant, their choice is only conditioned on the rule, not further on the number

of rule violators in their group. This design is analogue to the design used by Fischbacher, Gächter et al. (2001) to measure conditional cooperation.

In the third stage of the experiment, participants are asked how many of their group members they believe have violated each of the five rules when when not conditioning on the number of rule violators.[2] These estimates are incentivized the following way: one rule is randomly selected (and each rule has the same probability to be selected); if the estimate is precisely correct, the participant earns two experimental currency units. If it is one above or below the actual number, the participant earns one experimental currency unit. The experiment concludes with a short demographic questionnaire.

The main dependent variable of interest is the choice a participant makes conditional on both the rule and the number of members who violate this rule. As the experiment uses the strategy method, from each participant I elicit the complete reaction function. It consists of 5 rules x 6 numbers of violators, i.e. 30 choices. I analyze these choices in two dimensions: does the participant obey, or does she violate the rule? Economists would call this the extensive margin. Which commitment does the participant make? Economists would call this the intensive margin. For the analysis, I work with the distance of the absolute number from the rule in question. This variable is a measure for the degree of rule violation.

The experiment is designed such that I need not second-guess the effect of social information. But in real life, participants often have no social information in the first place, or they have reason to doubt its precision. In either situation, they must replace hard social information with their subjective estimates. This is what I test with choices from the first stage of the experiment: are these choices explained by the beliefs that I elicit in the third stage of the experiment?

With my data, I can also test whether merely making salient that others also decide whether to abide by the respective rule already affects the willingness to follow rules. Does mere social context already affect rule following, short of true social information? To that end I compare the choices in the first stage of the experiment with the first choices in the second stage of the experiment, i.e. with the choices participants make when knowing they are part of a group of six, but not conditioning on rule violations by other group members.

The natural next step is comparing the choices participants make when alone with their conditional choices when in a group. This comparison allows me to test whether the content of social information matters: are individuals more likely to violate a rule, and do they violate it more intensely, if they know how many of their peers violate the rule as well?

In the spirit of criminology, in the main experiment social information is framed as the number of group members (i.e. the experimental analogue of peers) who violate the rule in question. One could, however, also frame social information the other way round. If none of the remaining five members of the group violates the rule, this means that all of them abide by the rule. Conversely, if all of them violate the rule, this means that none of them abides by the rule. In

[2] We thus ask for beliefs about the unconditional choice in the second stage of the experiment.

an earlier experiment, my co-author and myself had framed the otherwise identical design in this positive way (Desmet and Engel 2021). This provides the opportunity to pool the data from both experiments and to test, in a between subjects design, whether the present negative frame affects the willingness or the degree of rule violation. In this earlier experiment, 120 students participated. At the then time, experiments could still be run in the physical lab. University of Hamburg has been kind enough to implement that experiment for us. That earlier experiment was run four years before the present. When testing the effect of framing, I will come back to these differences in implementation.

All independent variables are directly taken from the design of the experiment. Except for this final comparison, they are within subjects manipulations.

The experiment generates panel data (Wooldridge 2002). From each participant I have 5 decisions from the first stage, 5+30 decisions from the second stage, and 5 decisions from the belief stage. These choices are of course not independent. This is taken into account when estimating standard errors. There is also the risk that individual idiosyncrasies bias estimated coefficients. This is always checked with the help of the Hausman test (Hausman and Taylor 1981). If it turns out significant, the more efficient random effects model is replaced by a more conservative fixed effects model. I estimate linear models, also for the dichotomous dependent variable to obey or violate the rule. For this dependent variable, I thus estimate a linear probability model. This specification does not only make it easier to interpret the coefficients (they are marginal changes in the probability, conditional on a one unit change in the explanatory variable in question). I also, and most importantly, avoid the statistical problems when adding interaction terms to non-linear models (Ai and Norton 2003).

b) Results

Social Information. The design of the experiment exactly matches the research question. This is why the hypotheses may be directly read off the design of the experiment. I first consider the main question of interest, i.e. the choices in the second stage of the experiment that are both conditioned on the severity of the rule, and on information about the number of rule violators in the randomly composed group of six. I expect:[3]

- **H_{1a}:** Participants are more likely to violate the rule the more the rule is constraining.
- **H_{1b}:** The gap between the commitment and the rule is the wider the more the rule is constraining.
- **H_{2a}:** Participants are more likely to violate the rule the more of their peers do so as well.
- **H_{2b}:** The gap between the commitment and the rule is the wider the more of their peers violate the rule.
- **H_{3a}:** The more the rule is constraining, the more enhancing the number of other group members on the probability that the participant violates the rule.

[3] Hypotheses 1a, 1b, 2a and 2b have been preregistered.

H_{3b}: The more the rule is constraining, the more enhancing the number of other group members that the participant violates the rule on the gap between the commitment and the rule.

As Figure 1 shows, descriptively the severity of the rule has a clear effect. The more the rule is constraining, the more participants are likely to violate it, and the more intensely the violation. If the rule requests participants to only move five sliders, the opportunity cost of rule abiding is 48 – 5 = 43 experimental currency units. Participants forego almost 90% of their potential profit when obeying the rule. If they know that all other group members obey regardless, 44.38% of them do so as well. But the fraction of rule abiding increases with decreasing severity of the rule. If the rule is 11, and hence the opportunity cost of rule abiding is 37 experimental currency units, more than half of the participants (52.81%) abide by the rule if they know that all other members do so as well. If the rule is even more lenient and only reduces potential profit by 25 experimental currency units (rule 23), 56.74% of the participants commit to 23 or less sliders provided no other group member violates the rule. With rule 32, the fraction of rule abiding participants reaches 61.80% if all others are rule abiding as well. Finally with the most lenient rule 41, i.e. with an opportunity cost of merely 7 experimental currency units, the fraction of rule abiding reaches 66.29% if all other members respect the rule as well.

There is an analogue picture when considering the degree of rule violation, i.e. the gap between the choice and the rule (intensive margin, right panel of Figure 1). With the most lenient rule of 41, if they know that all other group members are rule abiding, the mean gap is even negative (-3.19); some participants do even make commitments below their allowance. The mean gap remains very small if the opportunity cost of rule abiding is no more than a third of the maximum profit (rule 32, mean gap 1.5). With rule 23, the mean gap rises to 6.35. With rule 11, it is already 12.69. It reaches 15.71 with rule 5.

Social information (the number of other group members violating the rule in question) is more important for the categorical decision to violate the rule (left panel of Figure 1, extensive margin) than for the degree of rule violation (right panel of Figure 1, intensive margin): the lines in the left panel of Figure 1 are steeper. In the left panel of, all lines have an upward slope (and only for the most lenient rule 41, there is a kink). By contrast, in the right panel, the upward slope is only pronounced for the two most stringent rules 5 and 11. For the most lenient rule 41, the line even slopes downwards: if they know that more of their peers violate the rule, descriptively they are even further below the limit themselves.

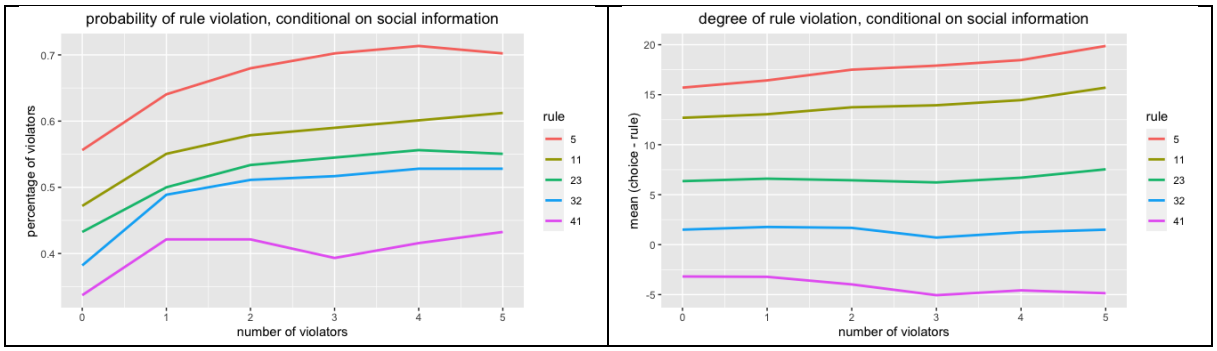

Figure 1
Choices Conditional on the Severity of the Rule and the Number of Rule Violators
left panel: mean probability of rule violation
right panel: mean gap between the rule and the commitment

As the regressions in Table 1 show, the hypotheses are fully supported by the data. The probability of rule violation and the degree of rule violation are the less pronounced the less the rule is stringent (models 1, hypothesis 1).[4] The more of the remaining group members violate the rule, the more a participant is likely to also violate the rule in question, and the more pronounced the violation (models 2, hypothesis 3). I finally find that the less the rule is stringent the less the participant is sensitive to the number of other group members who violate the rule (interaction terms in models 3, hypothesis 3). However, for the categorical dependent variable, the interaction term is only marginally significant (p = .077), while it is significant at conventional levels, and hence fully credible, for the degree of rule violation.

	violate			degree		
	model 1	model 2	model 3	model 1	model 2	model 3
rule	-.006*** (.0003)	-.006*** (.0003)	-.005*** (.0006)	-.6033*** (.009)	-.6033*** (.009)	-.524*** (.016)
violators		.022*** (.002)	.030*** (.005)		.210** (.069)	.924*** (.134)
rule*violators			-.0003^{+} (.0002)			-.032*** (.005)
cons	.671*** (.030)	.616*** (.031)	.597*** (.032)	20.610*** (1.004)	20.084*** (1.018)	18.301*** (1.058)
N obs	5340	5340	5340	5340	5340	5340
N uid	178	178	178	178	178	178

Table 1
Choices Conditional on the Severity of the Rule and the Number of Rule Violators
sample: conditional choices (from stage 2 of the experiment)
linear random effects models
Hausman test insignificant on all models
violate: dummy that is 1 if choice > rule
degree: number of sliders to which participant commits – rule
rule: continuous variable, maximum permissible number of sliders
violators: continuous variable, number of group members who decide to violate the rule
standard errors in parenthesis
*** p < .001, ** p < .01, * p < .05, $^{+}$ p < .1

[4] As the regressions are linear, effect sizes can be directly read off the coefficients. To illustrate, model 1 predicts that, with the most stringent rule (do not move more than 5 sliders), .769 [cons, implicitly assuming the rule to be 0] – 5 * .006 [coef of a one unit increase in the rule] = .739, or 73.9% of all individuals presented with this choice problem, will violate the rule. If the rule is one (of 48) points less stringent, the regression predicts that the probability of violating the rule decreases by 0.6%.

Beliefs about rule violations. In the interests of cleanly identifying the effect of social information, through the strategy method, in the main experiment participants know with certainty how many of their peers violate the rule when they decide themselves about their commitments. In the field, individuals do of course not have that much information. They must rely on whatever signals are available, or merely on their beliefs. Now in the main experiment, information about rule obedience in the population has turned out important. This motivates

- **H_{2c}:** Participants are more likely to violate the rule the more they believe their peers do so as well.
- **H_{2d}:** The gap between the commitment and the rule is the wider the more of their peers participants believe to violate the rule.
- **H_{3c}:** The more the rule is constraining, the more enhancing the expected number of other group members on the probability that the participant violates the rule.
- **H_{3d}:** The more the rule is constraining, the more enhancing the expected number of other group members that the participant violates the rule on the gap between the commitment and the rule.

I can test these hypotheses with the help of choices in the first stage of the experiment, plus participants' beliefs.[5] Comparing Figure 1 and Figure 2, one sees that removing uncertainty about the choices of other participants matters. Descriptively the content of the rule has approximately the same effect on the degree of violation (right panel, the more the rule is stringent, the higher the intercept of the line; lines only very rarely intersect). But this is not true for the categorical variable, i.e. for the probability of violating the respective rule. In the left panel, only the line for the most severe constraint is clearly distinct of, and above, the remaining lines. Yet in both panels all lines have a pronounced upward slope, more clearly even than in Figure 1. This suggests that participants are even more sensitive to beliefs than they are to exact social information.

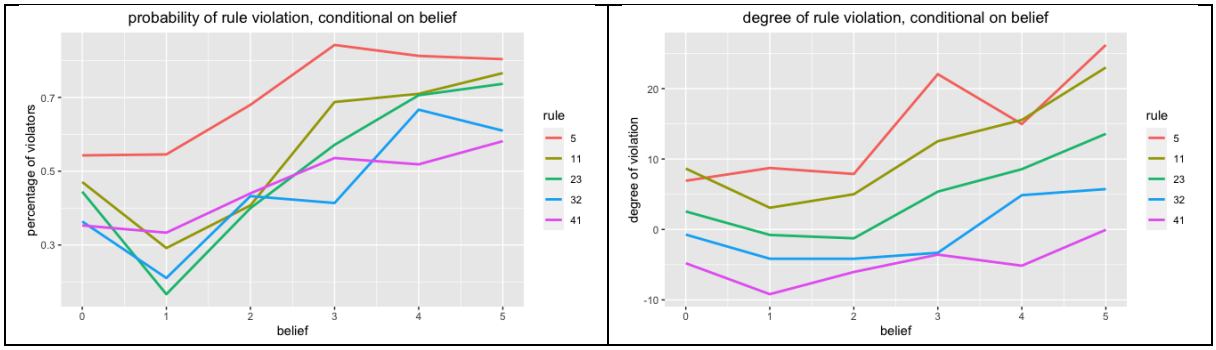

Figure 2
Choices Conditional on the Severity of the Rule and the Belief about the Number of Rule Violators
left panel: mean probability of rule violation
right panel: mean gap between the rule and the commitment

[5] Beliefs are elicited after the main experiment, to make sure that eliciting this supplementary measure does not contaminate the data that is central for the research question. At this point, participants know that they have been assigned to a group of six. This makes asking for beliefs about the choices of the remaining group members meaningful. Asking for their choices when not knowing that they are members of a group might have appeared contrived, which is why I instead ask for beliefs about their unconditional choices when knowing about the existence of the group. Of course, this ex post measure is only a proxy for considerations participants may have had when deciding in the first stage of the experiment.

Table 2 provides statistical tests.[6] The severity of the rule has the same negative effect, and is significant in all specifications: the less the rule is stringent, the less the participant is likely to violate it, and the lower the degree of the violation. The effect is also sizeable. If the rule is 1 (of 48) points less stringent, i.e. if the opportunity cost of rule abiding is 1 point lower, participants are about 0.7% less likely to violate the rule, and the gap between the rule and their choice approximately shrinks by .5 points. For the degree of violation, in model 3 I also find a significant positive effect of beliefs: the more others the participant thinks will violate the rule, the more intensely she violates the rule herself. The significant negative interaction effect shows that beliefs matter most for rules that severely limit the scope for profit. For the categorical variable I do not find a significant effect of beliefs though. I thus support hypotheses 2d and 3d, but not hypotheses 2c and 3c. If participants have to replace objective with subjective social information, results become less robust.

	Violate			degree		
	model 1	model 2	model 3	model 1	model 2	model 3
rule	-.007*** (.0006)	-.007*** (.0006)	-.009*** (.001)	-.580*** (.018)	-.577*** (.018)	-.443*** (.036)
belief		.003 (.009)	-.017 (.012)		.284 (.251)	1.335*** (.350)
rule*belief			.0009* (.0004)			-.047*** (.011)
cons	.710*** (.036)			19.153*** (1.114)		
N obs	890	890	890	890	890	890
N uid	178	178	178	178	178	178

Table 2
Choices Conditional on the Severity of the Rule and the Belief about the Number of Rule Violators
sample: conditional choices only (from stage 2 of the experiment)
if Hausman test is insignificant linear random effects models
otherwise fixed effects models (and no constant)
violate: dummy that is 1 if choice > rule
degree: number of sliders to which participant commits – rule
rule: continuous variable, maximum permissible number of sliders
belief: continuous variable, estimated number of group members who decide to violate the rule
standard errors in parenthesis
*** p < .001, ** p < .01, * p < .05, $^{+}$ p < .1

Social context. The second stage of the experiment makes social context salient. Participants are made aware that they are not alone when facing the normative expectation of one of the rules. This alone might make them think twice. They are alerted to the possibility that peers violate the rule. The explicit possibility of others violating the rule might change how participants construct the task. The construction might switch from unquestioning rule following to considering the pros and cons. If these considerations are critical, I should see

[6] Apparently, beliefs are idiosyncratic, and not distributed evenly in the experimental population, which is why for specifications with this explanatory variable, the Hausman test turns out significant. As it drops out via demeaning, in these regressions, no constant is reported. All other explanatory variable vary within subjects, and are therefore also estimated in models with participant fixed effects.

H_4: Whenever participants decide as members of a group, they are more likely to violate an arbitrary rule, and their choices are further away from the rule.

To test this hypothesis, I compare choices from the first stage of the experiment (when social context was not made salient) with the unconditional choices from the second stage (which makes social context salient, as participants know that, immediately thereafter, they will be asked to chose conditional on the choices of the remaining 5 members of their random group). Descriptively, I only find the hypothesized effect for the degree of rule violation. By contrast for the categorical decision to violate the rule, I find the opposite, Figure 3.

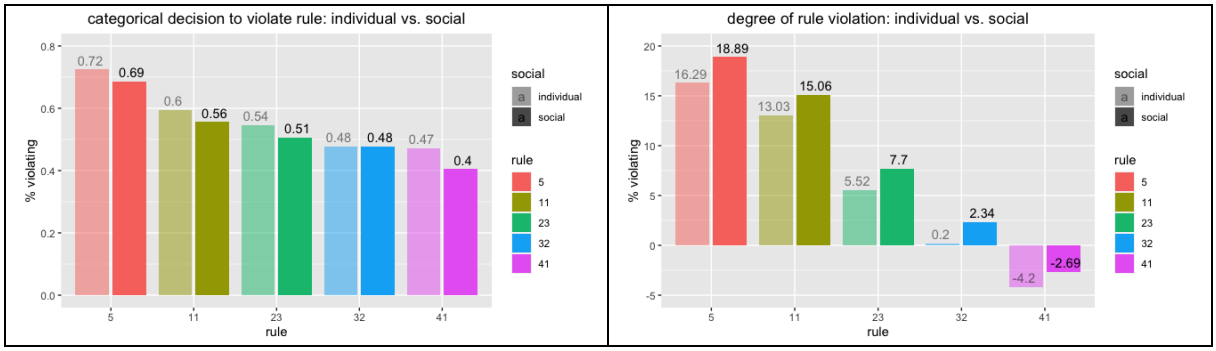

Figure 3
Effect of Social Context on Rule Violation
left panel: mean probability of rule violation
right panel: mean of choice – rule, per condition

This visual impression is supported by statistical analysis, Table 3. In all specifications, the gap between the choice and the rule is larger if participants know that they are deciding as members of a group of six. By contrast, the likelihood of rule violations is about 4% smaller with social context.[7]

	violate			degree		
	model 1	model 2	model 3	model 1	model 2	model 3
social context	-.037* (.016)	-.037* (.015)	-.033 (.029)	2.090*** (.572)	2.090*** (.419)	2.562** (.826)
rule		-.007*** (.0006)	-.007*** (.0008)		-.590*** (.016)	-.580*** (.022)
social context*rule			-.0002 (.001)			-.021 (.032)
cons	.563*** (.030)	.712*** (.033)	.710*** (.035)	6.170*** (.993)	19.390*** (1.036)	19.153*** (1.095)
N obs	1780	1780	1780	1780	1780	1780
N uid	178	178	178	178	178	178

Table 3
Effect of Social Context on Rule Violation
sample: choices when alone, and unconditional choices when a member of a group
linear random effects models
Hausman test insignificant on all models
violate: dummy that is 1 if choice > rule
degree: number of sliders to which participant commits – rule
social context: dummy that is 1 if participant decides (unconditionally) as a member of a group
rule: continuous variable, maximum permissible number of sliders
standard errors in parenthesis
*** p < .001, ** p < .01, * p < .05, $^{+}$ p < .1

Beneficial vs. detrimental effect of social information. While social context thus has an effect per se, the effect of true social information might be more pronounced. Observing how many of their peers violate the respective rule would inform participants that the rule is actually not strict. The fact that a peer violates the rule might make the situation morally ambiguous. It might give participants an excuse. In the spirit of the broken windows approach, a single bad apple might spoil the barrel. If these motives are at work, I should see

H_{5a}: Whenever participants decide as members of a group and at least one of them violates the rule, they are more likely to violate an arbitrary rule, and their choices are further away from the rule.

However the reaction to social information might also be gradual. The more peers abide by the rule, the more pronounced peer pressure, and the more intensely self and social image are at stake. If these effects are critical, I should see

H_{5b}: If participants decide as members of a group, they are the more likely to violate an arbitrary rule, and their choices are the further away from the rule, the more of their peers violate the rule.

[7] But not significantly different from 0 when interacting social context with rule, model 3.

To test these hypotheses, I compare the choices from the first stage of the experiment with any one of the conditional choices from the second stage of the experiment, Figure 4 and Table 4. If participants learn that all of their peers abide by the rule, they become more likely themselves to follow the respective rule, not less. The degree of rule violation is not significantly different from their choice when deciding in the absence of social information.

The direct test for hypothesis **H_{5a}** is the comparison of their choice in stage 1 of the experiment with their conditional choice in stage 2 if participants learn that no more than one of their peers violates the rule, they are still significantly less likely to violate the rule themselves, compared to their choice taken in isolation. The degree of violation is still not significantly different at conventional levels., Table 4. I thus have no support for **H_{5a}**.

As the left panel of Figure 4 shows, if participants learn that all of their peers follow the rule in question, they are substantially less likely to violate the rule themselves, compared to the choices they make when not having access to social information. The right panel shows that, even if they learn that all of their peers violate the rule, on average rule violation is about as frequent as if participants decide on their own. Even with the negative frame of violation, I thus replicate the finding from the earlier experiment. In that experiment social information was framed as the number of peers who abide by the rule. On average this information never reduced rule abiding, even if participants learned that they were the only ones to follow the rule (Desmet and Engel 2021). Table 4 summarizes statistical tests. They support these descriptive findings.

With the violation frame, I do however find an increase in the degree of rule violation (but not in the probability of violation) if social information is strongly negative, Table 4. If participants learn that four or even all five of their peers violate the rule, the gap between the rule and their choice increases more than if they decide without access to social information. This provides qualified support for **H_{5b}**.

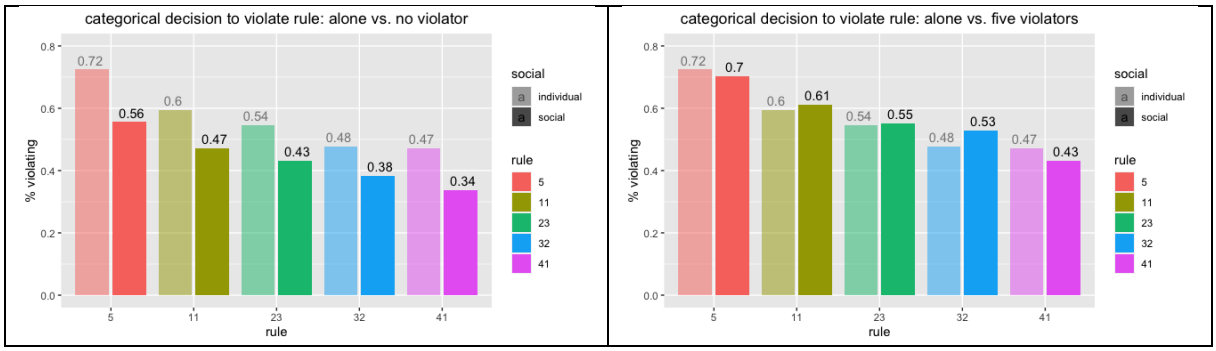

Figure 4
Effect of Social Information on Rule Violation
dependent variable: mean probability of rule violation
left panel: choice in first stage of the experiment compared with second stage, no other group member violates
right panel: choice in first stage of the experiment compared with second stage, all other group members violate

# violators	model	violate	degree
0	social	-	
	social + rule	-	
	social * rule	-	
1	social	-	
	social + rule	-	(+)
	social * rule	-	
2	social		
	social + rule		+
	social * rule		
3	social		
	social + rule		
	social * rule		+
4	social		(+)
	social + rule		+
	social * rule		+
5	social		+
	social + rule		+
	social * rule		+

Table 4
Effect of Social Information on Rule Violation
summary account of a series of 15 regressions,
comparing choices from the first stage of the experiment with the choice conditional on one selected social information
second column: list of explanatory variables
third column: effect of social information on probability of rule violation
fourth column: effect of social information on degree of rule violation
-: significantly less violation / lesser degree of violation in the presence of social information
+: significantly more violation / higher degree of violation in the presence of social information
(): weakly significant effect
the underlying regressions are available from the author upon request

Rule violation vs. rule abiding frame. It is a matter of perspective whether the glass is half full or half empty. This perspective is open to external intervention. In the behavioral disciplines, this intervention is called a frame. By couching the decision problem in appropriate words, individuals can be led to see a situation as either full of good news (many of my peers abide by the rule), or as full of bad news (many of my peers violate the rule). Mathematically the two presentations are of course exchangeable. In a group of six, one violator is the same as five rule abiders. But the former presentation highlights that the rule is not unanimously followed. By contrast the latter presentation highlights that the rule is widely implemented. These considerations yield a straightforward hypothesis

H_6: If participants are informed about the number of violators, rather than the number of rule followers, they are more likely to violate the rule themselves, and the gap between rules and choices widens.

To test this hypothesis, I am pooling the data from the earlier experiment with the rule following frame (Desmet and Engel 2021) and the data from the present experiment with the rule violation frame. Descriptively I have a surprising finding. Against expectations, the rule violation frame leads to more, not to less rule abiding, Figure 5.

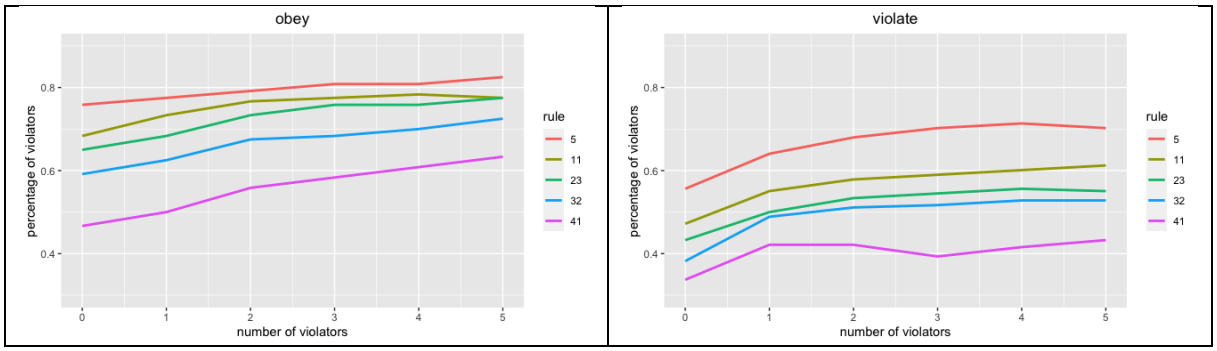

Figure 5
Rule Violation Conditional on the Severity of the Rule and the Rule Abiding vs. Rule Violation Frame

Yet the experiments have been run with different subject pools (University of Hamburg versus Max Planck Institute Bonn), at different points in time (2017 versus 2021), and in the physical lab (Hamburg) vs. online (Bonn). The surprising finding could therefore be due to a difference in the general propensity to follow a rule. The regressions in Table 5 put this explanation to the test. They exploit that the design of the experiment has two stages. In the first stage, there is no social information. The mean choice made by the participant in question in the first stage may therefore be used to partial out this individual's propensity to abide by an arbitrary rule.[8] As the regressions show, personal type does indeed matter (the mean choice when alone is highly significant in all specifications, and has a positive coefficient). More importantly, with one exception in these regressions the effect of the frame is no longer significantly different from zero.

The exception is the final model explaining the degree of rule violation, i.e. the gap between the participant's choice and the respective rule. In this specification both the main effect of a rule abiding frame and its interaction with the content of the rule are significant. Yet coefficients have opposite sign. The net effect is positive for rules 5 and 11, close to 0 for rule 23, and negative for rules 32 and 41. The net effect is significant for rules 5, 11 and 41. Hence the rule abiding frame hurts (leads to more intense rule violations) for strict rules, and it helps (leads to less intense rule violations) for lenient rules.

8 In principle, a regression with a participant fixed effect would of course have the same effect. But in a fixed effects model, between subjects' explanatory variables, and hence the main effect of the frame, would drop out, due to demeaning.

	violate			degree		
	model 1	model 2	model 3	model 1	model 2	model 3
rule abiding frame	-.057 (.038)	-.057 (.038)	-.065 (.040)	.645 (1.212)	.645 (1.212)	5.743*** (1.246)
rule		-.006*** (.0002)	-.006*** (.0003)		-.695*** (.006)	-.603*** (.008)
rule abiding frame*rule			.0004 (.0005)			-.228*** (.013)
mean choice when alone	.019*** (.001)	.019*** (.001)	.019*** (.001)	.701*** (.042)	.701*** (.042)	.701*** (.042)
cons	-.013 (.044)	.126** (.044)	.129** (.044)	-12.927*** (1.383)	2.641^{+} (1.390)	.587 (1.395)
N obs	8940	8940	8940	8940	8940	8940
N uid	298	298	298	298	298	298

Table 5
Choices Conditional on the Severity of the Rule and the Rule Abiding vs. Rule Violation Frame
sample: conditional choices only
linear random effects models
Hausman test insignificant on all models
violate: dummy that is 1 if choice > rule
degree: number of sliders to which participant commits – rule
rule abiding frame: information about number of group members who obey
rule: continuous variable, maximum permissible number of sliders
standard errors in parenthesis
*** p < .001, ** p < .01, * p < .05, $^{+}$ p < .1

c) Discussion

In the experiment, participants react to social information. The more of their peers violate a rule, the more they do so themselves. If they have no hard information on the choices of others, they replace it with their beliefs. Even merely making it salient that they are part of a group has an effect. It reduces the likelihood of violating an arbitrary rule. But those who do violate the rule more intensely. Learning how many others violate the rule does never make it more likely that participants violate the rule themselves. In that sense, social information never hurts. If participants learn that no or only very few others violate the rule, they become less likely to violate the rule themselves. However those who do violate the rule do so more intensely if they learn that many others violate the rule. Finally the effect of social information does not depend on the way social information is framed: as the incidence of rule violation, or as the frequency of rule abiding.

A lab experiment is able to perfectly identify these effects. Moreover, rule following can be made costly. This makes choices credible. The number of those who follow the costly rule can be manipulated, so that participants react to the real choices of other participants. In the lab, the number of peers who violate the rule can be perfectly fine grained, as can the cost of following the rule, and the positive (do others follow the rule?) or the negative frame (do others violate the rule?). Hence it is possible to elicit complete reaction functions from each individual participant, and thereby data with a high resolution.

Yet there is no such thing as perfect empirical evidence. The limitation is not only due to the fact that, normally, the researcher may only study a sample, but wants to extrapolate to the population. An equally severe limitation results from the necessity to position the empirical exercise on a continuum between maximum internal and maximum external validity. Ignoring a patently arbitrary normative expectation is certainly not a crime in the sense of the criminal code. The norm is visibly devoid of any moral connotation, except for a pure deontological concern. Violating the rule does, in particular, not cause any harm, neither to other experimental participants nor to the wider community. Yet arguably this is not an important limitation. This feature of the design works against the experimenter. If there is an effect of social information even under these extreme conditions, the effect should be expected *a fortiori* if the well-being of third parties or a larger community is at stake.

Lab experiments may be conceptualized as experimental games. While this conceptualization is mainly building a bridge to game theory and facilitates the definition of hypotheses, the gamification of the normative problem might also change how participants see it. They might perceive themselves as participating in a game, and exhibit choices they would no longer make if it mattered. As long as the researcher is concerned about excessive selfishness, there is again an *a fortiori* objection. It should be easier to reconcile choices with self-esteem and social esteem in a game of trifles, rather than when imposing serious harm on an innocent party. Yet as there is not much money at stake in the first place, and participants have voluntarily agreed to participate in the experiment, the willingness to live up to social expectations might be more pronounced than in the field. If true, the experimental effect would overestimate the effect in the field. Yet even if this concern were to be taken seriously, it is more likely to affect the level of rule abiding. It is harder to doubt about the marginal effect of a more versus a less stringent rule, or the marginal effect of information about a more or less rule abiding community.

Still, as a safeguard this paper combines incentivized, maximally clean evidence from the lab with a survey that evokes three mundane situations in which rule violations are frequent. This data is not incentivized and less fine-grained. But it allows to check whether context matters, and whether there is converging evidence from the lab and from (hypotheticals about the) field.

4. Vignette Study

a) Method

221 participants randomly selected from the pool of more than 145,000 persons in the subject pool of Prolific who are resident in the UK, Ireland, the US or Canada, and fluent in English, participated in the experiment. 113 of them (51.13%) were female. 3 reported their gender as diverse. Mean age was 42.85 (min 16, max 78). 80 (36.20%) were single, 101 (45.70%) married, 2 widowed, 7 divorced, and 30 indicated their family status as “other”. Participants reported on

average to have 2.04 brothers or sisters.9 115 (52.04%) have no child, 34 one, 49 two, 16 three, and 7 four children.

All participants reported to have formal qualifications: 26 secondary education, 45 high school diploma/A-level, 14 technical/community college, 81 an undergraduate degree, like BA or BSc, 44 a graduate degree, like MA, MSc or MPhil, and 11 a doctorate degree, like PhD. 118 (53.40%) reported their income as "average", 25 as "very low", 51 as "low", 27 as "high", nobody as "very high".

On a scale from -50 (very liberal) to 50 (very conservative), participants on average were slightly liberal (mean -10.03, median -9). 175 believe that government should do more about littering, 169 that it should do more about tax evasion, and 102 that it should do more about speeding. Overall only few of them believe that regulation is excessive in these domains: 92 for tax evasion, 38 for speeding, and 13 for littering.

The experiment was programmed in oTree (Chen, Schonger et al. 2016). It only took a couple of minutes. Participants earned 2 £ for their participation (equivalent to 2.77$ on the first day of the experiment).10

Participants saw the following three vignettes:

> **Speeding [alone]**
> "On a trip through the countryside, you pass multiple small villages. You enter village V. At the entrance, you see a 20 mph post. You see no people or cars on the street, nor surveillance cameras or police officers. Do you stay within the speed limit?"
> **Speeding [social information]**
> „The next village also has a 20 mph speed limit posted at the entrance. You see no people on the street, nor surveillance cameras or police officers.
> [plus randomly one of the following three sentences]
>
> There are however other cars. They all seem to stay within the speed limit.
> [or]
> There are however other cars. One of them drives considerably faster.
> [or]
> There are however other cars. Many of them drive considerably faster.
>
> Do you stay within the speed limit?"
> **Littering**
> "You are hiking in a national park. At the entrance, a number of rules are posted. One of them says that you have to carry all your waste with you, and dispose it outside the park, for instance in large bins posted for the purpose near the entrance. You have a rest in a lodge put up by the management of the park. During your rest, you empty a can of food, and two plastic bottles with beverages. Do you take your waste with you after your rest?"
> **Littering [social information]**
> „As you are on a long hike, a few hours later you have a second rest, at another lodge. You again empty a can, and two plastic bottles.
> [plus randomly one of the following three sentences]

9 1.90 when removing one outlier; this participant reported to have 32 brothers or sisters.

10 It is only when analyzing the data that I have noticed that there has been a technical problem with OSF. While the preregistration has started on Aug.6, 2021 - *https://osf.io/6nd2p/* - apparently the input has not been stored on the website. I had however exported the website to .pdf and have shared this document with my research assistant on that same day. The document is in the online supporting material.

During your rest, you enjoy the pristine surroundings of the lodge.
[or]
During your rest, you spot an empty can, from an earlier visitor, in a corner of the lodge.
[or]
During your rest, you spot cans, bottles, and paper scattered in and out the lodge.

Do you take your waste with you after your rest?"

Tax Evasion

"You have invested some of your wealth in stocks listed at a foreign stock exchange. As prices for the stock have gone up, you have sold them, at a nice profit. You are aware of the fact that, in your country of residence, the difference between the price at which you have bought and at which you have sold your stocks, is taxable income. You do, however, consider the probability to be very low that the tax authorities will ever learn. Do you declare the profit from selling these stocks in your next tax return?"

Tax Evasion [social information]

"A year later, you have sold a few more of your stocks, again at a nice profit. You are aware of the fact that, in your country of residence, the difference between the price at which you have bought and at which you have sold your stocks, is taxable income. You do, however, consider the probability to be very low that the tax authorities will ever learn.

Before preparing your next tax return, you read a newspaper article about a recent study from a group of scientists. They have joined forces with the tax authorities, and have randomly audited 15 tax payers that are likely to hold foreign stocks. It turned out that 10 indeed did.

[plus randomly one of the following three sentences]

All of them had declared income from selling stock.
[or]
One of them had not declared income from selling stock.
[or]
Many of them had not declared income from selling stock.

Do you declare the profit from selling these stocks in your next tax return?"

The order in which participants saw the three blocks of two vignettes was randomized.

To all questions, they responded „yes" or "no". "Yes" always stands for rule following, "no" for rule violation. All independent variables are directly taken from the design of the experiment. 3 scenarios are crossed with 2 social context options (absent vs. present) and 3 levels of social information (no rule violation; occasional rule violation; widespread rule violation). Scenarios and social context options were manipulated within subjects, the levels of social information between subjects.

The vignette study again generates panel data (Wooldridge 2002). From each participant I have 6 decisions. The resulting dependence is taken into account when estimating standard errors. There is also the possibility that individual idiosyncrasies bias estimated coefficients. This is always checked with the help of the Hausman test (Hausman and Taylor 1981). If it turns out significant, the more efficient random effects model is replaced by a more conservative fixed effects model. I estimate linear probability models. This specification does not only

make it easier to interpret the coefficients (they are marginal changes in the probability conditional on a one unit change in the explanatory variable in question). I also, and most importantly, avoid the problems when adding interaction terms to non-linear models (Ai and Norton 2003).

b) Results

Social context. In the first step I check whether the mere presence of the social context affects the stated willingness to abide by a normative expectation that individuals know will not be enforced. For this purpose, I am pooling the data over all three strengths of social information: no peer violates the rule, only one peer does so, or there are many rule violations. Descriptively, there is an effect for speeding, and an even more pronounced effect for tax evasion, but no effect for littering, Figure 6. Almost no participant is prepared to litter, whether or not the social context is made salient.

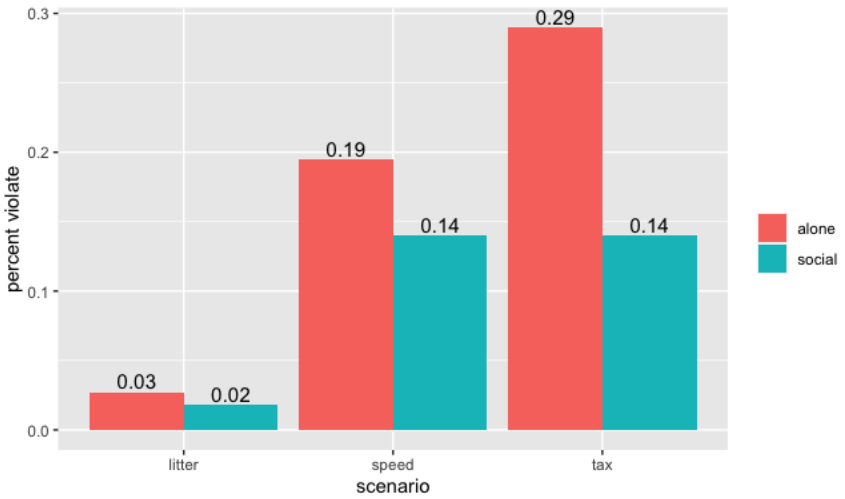

Figure 6
Effect of Social Context on Rule Violation
social: pooled over no, one, or many rule violators

When additionally pooling over the three scenarios, I do indeed find a significant effect of social context, Table 6 Model 1. If social context is made salient, participants are 7.1% less likely to violate the respective rule. The effect stays the same, and remains highly significant, if I control for scenarios. For speeding, rule violation is 14.5% more likely than for littering. Tax evasion is even 19.2% more likely (Model 2). If I further interact the presence of social context with scenarios, the main effect of social context becomes insignificant (Model 3). Given Figure 6, this is expected, as the main effect now captures the effect of social context in the littering scenario. Visibly, in this scenario, social context is immaterial. For tax evasion, the interaction term is strong and highly significant. In this scenario, social context makes a big difference. The descriptive difference in the speeding context does not turn out significant.

	model 1	model 2	model 3
social	-.071*** (.018)	-.071*** (.017)	-.009 (.029)
speeding		.145*** (.021)	.167*** (.029)
tax evasion		.192*** (.021)	.262*** (.029)
social*speeding			-.045 (.041)
social*tax evasion			-.140*** (.041)
cons	.170*** (.015)	.058** (.019)	.027 (.022)
N obs	1326	1326	1326
N uid	221	221	221

Table 6
Effect of Social Context on Rule Violation
linear probability models with participant random effect
Hausman test is insignificant on all models
social: dummy that is 1 if participants have received (any) social information
standard errors in parenthesis
*** p < .001, ** p < .01, * p < .05, $^{+}$ p < .1

Social information. Most relevant for the present paper is, however, the marginal reaction to a difference in the content of social information. Descriptively it is most important for speeding. In an environment where nobody speeds, almost all participants are happy to respect the speed limit. But if they observe only a single other person speeding, their willingness to stay within the speed limit is already reduced by 11%. It is reduced by another 8% if they observe that multiple others speed. For tax evasion, a single other tax evader makes virtually no difference. But descriptively tax evasion increases by 7 to 8% if a person observes that many others do not pay their taxes.

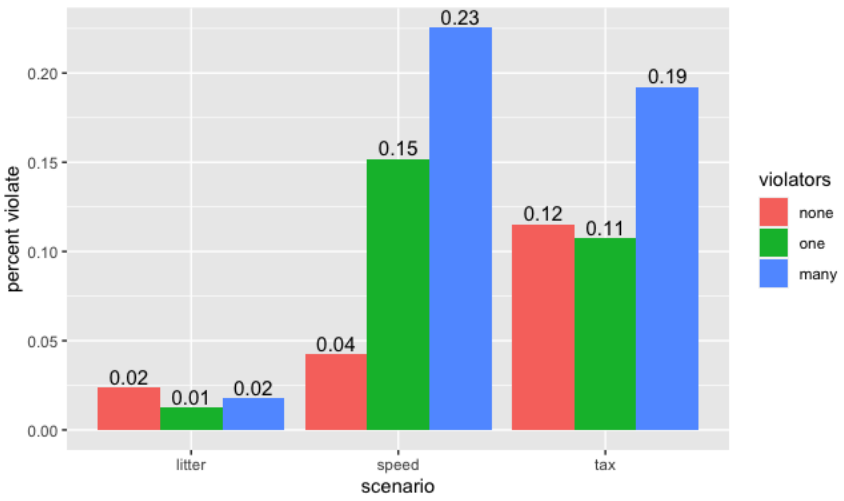

Figure 7
Rule Violation Conditional on Scenario and the Number of Rule Violators

Model 1 of Table 7 shows that, when pooling over all three scenarios, violations are almost 10% more likely if an individual observes that many others do not respect the rule in question. The estimate is similar when controlling for scenarios, Model 2. The significant coefficients of speeding and tax evasion capture that, overall, rule violations are more likely than with littering. If, however, interacting the number of violators with scenario (Model 3), only the effects for speeding remain (weakly, or at conventional levels) significant. When considering scenarios in isolation, the effect of social information is only visible with speeding. Put differently, the significant overall effect documented in Models 2 and 3 is predominantly driven by speeding.

Model 4 shows a pronounced and significant effect of political attitude: if a participant thinks that government should do more to foster the regulative goal in question, she is about 6% less likely to violate the rule. If she thinks that the domain is overregulated, she is about 9% more likely to violate the rule. When adding (all) demographic controls, the effect of the conviction that government should do more rises to 10%.

	model 1	model 2	model 3	model 4	model 5
one violator	.032 (.028)	-.032 (.027)	.005 (.045)	-.005 (.045)	.048 (.054)
many violators	.097*** (.028)	.087** (.028)	-.008 (.050)	-.023 (.050)	-.036 (.059)
speeding		.116*** (.027)	.016 (.046)	-.015 (.047)	-.044 (.054)
tax evasion		.116*** (.027)	.093* (.045)	.058 (.046)	.096^{+} (.055)
one violator* speeding			.120^{+} (.066)	.122^{+} (.065)	.112 (.079)
many violators* speeding			.198** (.070)	.206** (.069)	.291*** (.084)
one violator* tax evasion			.000 (.067)	.004 (.066)	-.025 (.082)
many violators* tax evasion			.084 (.069)	.092 (.068)	.093 (.083)
government should do more				-.059* (.027)	-.102** (.034)
regulation is excessive				.087** (.031)	.012 (.041)
political orientation				.001 (.000)	
controls	NO	NO	NO	NO	YES
cons	.059** (.020)	-.016 (.025)	.022 (.032)	.074^{+} (.038)	
N obs	663	663	663	663	663
N uid	221	221	221	221	221

Table 7
Rule Violation Conditional on Scenario and the Number of Rule Violators
sample: only data with social information

linear probability models with participant random effect
unless (in model 5) the Hausman test turns out significant
standard errors in parenthesis
*** p < .001, ** p < .01, * p < .05, $^{+}$ p < .1

5. General Discussion

Why do people commit crime? This paper argues: because their peers do. More specifically it argues: in principle, individuals are willing to follow the rules they know to be in force. But the more they learn that others do not obey, the more they desist from rule abiding themselves. Or in short: rule following is conditional on social information. This peer effect results from the fact that social information provides individuals with the opportunity to put the normative expectation into perspective. They not only learn about the expectation, but also about the way how their peers react to it, and hence about the degree by which the expectation is reflected in social practice.

With observational data, this claim would be hard to test. It would already be difficult to measure social information, not the least since crime is illegal and criminals are prosecuted. The individual criminal therefore has an incentive to conceal her acts. Of course, in the spirit of broken windows theory, the individual may observe signals of normative erosion. But these signals require interpretation. The proverbial window may be broken since the area has been hit by an earthquake. Even less would it be possible with observational data to prove that (negative) social information is causal. If the crime rate in a neighbourhood increases, this may be due to the fact that it has become harder to earn an income in the official economy, to mention just one alternative explanation.

Criminology as a discipline is traditionally more invested on the external validity side. It for instance studies the crime register, or interviews prison inmates. This has the obvious advantage that the object of study is identical with the object of interest: real crime, or the effect of real criminal sanctions on real convicts. But ultimately, empirical research makes causal claims. With data from the field, identifying causal effects is notoriously precarious. Directly investigating the ultimate object of interest is even more challenging if the research question is not interested in the effect, but in the (for instance behavioral) mechanism that produces the effect. With observational data, it is close to impossible to isolate a single behavioral channel. This is where the strength of lab experiments lies. In the lab, the researcher may create an environment so clean that plausible alternative explanations are ruled out by design. If she observes a (sufficiently clear and pronounced) effect, it must be caused by the experimental manipulation. Yet this researcher has a price to pay. Due to the very features of the design that enable causal inference, the object of investigation is no longer the ultimate object of study. What the researcher analyses is only analogue to the real world phenomenon that triggers the research question.

In the interest of isolating the causal effect of social information, in this paper I use a lab experiment. It relies on a series of within subjects manipulations. Participants know that, ultimately, one of 40 situations will randomly be selected to determine their payoff. Actually the within subjects design even makes possible what cannot be achieved in a classic between

subjects experiment: one simultaneously observes what the participant decides in any of these 40 situations; one observes the counterfactual.

The experiment has a clear result: the more of their peers violate the rule in question, the more a randomly selected individual is likely to do so as well. On top of this result on the extensive margin, there is also a complementary finding on the intensive margin: the more of her peers violate the rule in question, the more intensely a randomly selected individual violates the rule as well. Moreover, the more the rule is constraining, the more likely it is to be violated, and the more intensely the violation. These two determinants of rule violation interact: the more the rule is constraining, the more pronounced the socially detrimental effect of social information.

In the interest of identification, in the main experiment participants know with certainty how many of their peers violate the rule. In real crime scenes, social information is never that precise. Quite often, all the participants have is their subjective beliefs. Yet the main experiment in principle replicates if participants have to rely on their beliefs. But the effect is less cleanly observed.

Mere social context, without social information, has an ambivalent effect: it reduces the likelihood of rule violation. But participants who decide to violate the rule do so more intensely.

The experiment is designed such that a situation without social information can be compared with criminal decision making with such information. It turns out that, on the extensive margin, having access to social information never hurts. Even if the participant learns that all of her peers violate the rule in question, she is not more likely to violate the rule than when deciding on her own. Yet on the intensive margin, strongly negative social information hurts. Those participants who violate the respective rule do so more intensely.

Finally, it does essentially not matter how social information is framed. Participants react the same way whether they are alerted to the number of peers who violate the rule, or whether they are informed about the number of peers who abide by the rule.

As always, the lab experiment has the advantage that it isolates the effect in question, and hence the deontological motive to follow rules just because they are in force. But to achieve identification, the experiment creates an artificially clean situation. In the interest of checking whether the findings from the main experiment are externally valid, a series of vignettes translates the key effect into a more contextualized, crime like setting. Participants in this second study are members of the general public, not university students as in the main experiment. Each of them receives three scenarios in two versions each. The vignettes capture situations where rule violations are not infrequent, criminal enforcement is unlikely, and social information is readily available. These three scenarios are littering, speeding, and tax evasion. In each scenario, participants are first asked what they would do when alone. Thereafter they receive a variant of the scenario, and randomly selected social information: none of their peers violates the rule; there is a single violation; violations are frequent. The participant is asked whether she would violate the rule.

Again, social context never hurts. It helps with speeding and tax evasion. More importantly, the content of social information matters. The more social information is negative (others violate the rule), the more the participant is likely to indicate that she would violate the rule as well. Yet there are pronounced differences between the scenarios. There is practically no rule violation in the case of littering. In this context, social information has no discernible effect. In the tax evasion case, overall rule violations are most frequent. This resonates with the finding from the main experiment that rule violations are the more frequent, and the more pronounced, the more the rule is onerous. Declaring taxable income has a much higher opportunity cost than refraining from littering. But in the tax evasion case, the strength of social information has no significant effect. This is different with speeding. Here the content of social information clearly and significantly has the expected effect.

Hence the results from the vignette study are less clean than the results from the main experiment. This should not come as a surprise. The price for greater external validity is a less clean design. One potential confound is addressed with a control variable. After responding to the vignettes, participants are asked whether they would want government to be more active in the respective domain, or whether, to the contrary, they see government as too intrusive. These policy preferences significantly explain the data. Participants might also be influenced by beliefs about the attitudes of their peers. They might follow routines (the routine not to litter?). They might have (correct or false) expectations about the vigilance of others, and the likelihood of social sanctions. They might be afraid that the information leaks out and their social image is tainted.

Consequently the main (lab) experiment and the supplementary vignette study have competing strengths and weaknesses. Yet happily with respect to the main finding, the evidence converges. Individuals have a pronounced willingness to implement rules just because they know them to be in force. But this socially beneficial aspect of normativity has an open flank. The willingness to follow rules is conditional on social information. If this information is too bad, rule following decreases, the more so the more the rule is onerous.

This paper has been mainly interested in the motivational force of rule following. In real life, government often does considerably more than merely stipulating an arbitrary rule. Much more often than not, government prefers to convince the population, rather than nakedly exercising sovereign powers. Government also rarely completely and patently refrains from the threat with adjudication and enforcement. From a policy perspective, pure normativity is much less important than normativity as one ingredient in a richer institutional arrangement. In such an arrangement, motivational effects may be complements or substitutes. One ultimately wants to understand how these competing motivational effects interact. Yet understanding the interplay of multiple effects requires that one first has understood, and empirically tested, the isolated effects. Isolating rule following, and demonstrating that it is conditional on social information, has been the contribution of this paper.

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Appendix
[English Translation of the Online]
Instructions for the Lab Experiment

Welcome to the experiment. The experiment consists of three stages and a questionnaire. Either the first or the second stage is relevant for your payoff. In the end of the experiment, the computer will randomly select the relevant stage. Either stage is equally likely to be payoff relevant. The third stage is paid out with certainty.

Your payoff is calculated in the currency experimental currency unit ECU. In the end of the experiment, we convert ECU into Euro, at a rate of 1 ECU = .1 Euro.

Additionally you receive 4 Euro for your participation.

***

In the following we describe five different environments. In the end of the experiment, the computer will decide which of these environments is relevant for your payoff. Each environment has the same probability to determine the payoff. Each environment is defined by a number. There is a rule: do not move more sliders to the number 10 than defined for the respective environment.

Yet the rule is not enforced. Whatever the rule says: for each slider that you position at the number 10, you earn 1 ECU. You can at most position 48 sliders.

In the end of the experiment, you learn which environment is relevant for your payoff. For this environment you get the chance to position sliders. The computer will remind you of the number you have determined for this environment. If you position more sliders to the number 10, you are not remunerated for these additional sliders.

***

We want to be sure that you have understood the design of the experiment. Please do therefore answer the following control question:

The computer selects rule 23. For this rule you have decided to position at most 25 sliders at the number 10. You position 28 sliders to the number 10. Which is your earning?

***

Please do now decide how many sliders you want at most to move, conditional on the rule randomly selected by the computer.

	your decision
rule 5	
rule 11	
rule 23	
rule 32	
rule 41	

***

The second stage of the experiment is identical to the first. But you now are a member of a randomly composed group of six. In this stage of the experiment, you make two choices, one unconditional and one conditional. For five members of the group, the unconditional choice is payoff relevant. For the sixth group member, however, the conditional choice is payoff relevant. The computer will randomly decide which of your choices is payoff relevant, provided this stage of the experiment is paid out.

If your conditional choice is payoff relevant, your payoff depends on how many other members of your group have decided to violate the rule in question.

In the following we describe five different environments. In the end of the experiment, the computer will decide which of these environments is relevant for your payoff. Each environment has the same probability to determine the payoff. Each environment is defined by a number. There is a rule: do not move more sliders to the number 10 than defined for the respective environment.

Yet the rule is not enforced. Whatever the rule says: for each slider that you position at the number 10, you earn 1 ECU. You can at most position 48 sliders.

In the end of the experiment, you learn which environment is relevant for your payoff. For this environment you get the chance to position sliders. The computer will remind you of the number you have determined for this environment. If you position more sliders to the number 10, you are not remunerated for these additional sliders.

***

In this stage of the experiment, we also want to be sure that you have understood the design of the experiment. Please do therefore answer the following control questions:

The computer has selected rule 23, and has decided that for you the unconditional choice is payoff relevant. You have decided that, for this case, you want to position at most 23 sliders at 10. However, if more than three members of the group violate the rule, you want to position 40 sliders. How many sliders can you position at most?

The computer has selected rule 41. You have decided not to position more than 41 sliders to 10 if no more than two members of your group violate the rule. For you, the conditional choice is payoff relevant. One member of the group has violated the rule. How many sliders can you position to 10?

***

Please do now decide how many sliders you want at most to move, conditional on the rule randomly selected by the computer.

	your decision
rule 5	
rule 11	
rule 23	
rule 32	
rule 41	

***

Please do now decide how many sliders you want at most to move, conditional on the role randomly selected by the computer, and on the number of group members who violate the rule.

	all other group members obey the rule	one group member violates the rule	two group members violate the rule	three group members violate the rule	four group members violate the rule	five group members violate the rule
rule 5						
rule 11						
rule 23						
rule 32						
rule 41						

***

We now ask you for your estimate. What do you think, how many of the other members of your group have decided to position more sliders to the number 10 than permitted by the respective rule? Hence how many members of the group do you think have violated the rule?

We ask for your estimate regarding the unconditional choice of the remaining group members.

You may also earn money in this stage of the experiment. For this stage, the same environment will be payoff relevant that is also relevant for the payoff from the first two stages of the experiment.

If your estimate for this environment is exactly right, you additionally earn 2 ECU. If your estimate is too high or too low by one person, you additionally earn 1 ECU.

Please do always indicate a number between 0 (no other group member violates the rule) and 5 (all other members violate the rule).

	your estimate
rule 5	
rule 11	
rule 23	
rule 32	
rule 41	