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How Little Does It Take to Trigger a Peer Effect? An Experiment on Crime as Conditional Rule Violation

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Abstract

Objectives: Peer effects on the decision to commit a crime have often been documented. But how little does it take to trigger the effect? *Method:* A fully incentivized, anonymous experiment in the tradition of experimental law and economics provides fully internally valid causal evidence. A companion vignette study with members of the general public extends external validity. *Results:* (a) the more of their peers violate an arbitrary rule, the more participants do; (b) a minority has a threshold and switches from rule-abiding to violation once a sufficient number of their peers violate the rule; (c) the more the rule is constraining, the more participants are sensitive to the number of others who violate the rule; (d) if participants do not have explicit information about the incidence of rule violations in their community, they rely on their beliefs. *Conclusion:* In terms of substance, the paper shows that mere social information is the core of peer effects. In terms of

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methodology, the paper demonstrates the power of incentivized, decontextualized lab experiments for isolating mental building blocks of the decision to commit a crime.

Keywords
Peer effects, rule following, social information, speeding, tax evasion, littering

Introduction

Ample evidence demonstrates that individuals are more likely to commit a crime when in the company of peers (Agnew 1991; Haynie 2001, 2002; Hoeben and Thomas 2019; Matsueda and Anderson 1998; McGloin and Thomas 2019; Osgood and Anderson 2004; Thornberry et al. 1994; Warr 2002; Weerman 2011). This has for instance been shown for property crime (Agnew 1991; Matsueda and Anderson 1998), violent offending (Agnew 1994; Kreager 2007), substance abuse (Akers et al. 1979; Osgood et al. 1996), and drunk driving (Osgood et al. 1996).

Peers can have an influence on the decision to commit a crime on multiple channels. This makes it difficult to identify what drives the effect. As Warr (2002: 44) has noted, "questions concerning causality surround all theories, of course, but they have plagued theories and research on peer influence since the moment they first entered the arena of criminology" (see also Paternoster et al. 2013: 477 f.). More specifically, Gottfredson and Hirschi (1990: 156) have argued that "the 'delinquent peer group' is a creation of faulty measurement and the tendency of people to seek the company of others like themselves." Concern has been voiced that peer effects are actually only ex-post rationalizations (Rowan et al. 2022: 5f.).

This paper reports on a lab experiment that aims at excluding as many alternative explanations for peer effects as possible, to find out how little it takes to trigger a peer effect. In a nutshell, the experiment proceeds as follows: in a perfectly neutral setting, participants earn money by choosing how many actions to take. They are informed about a rule that constrains the permissible number of actions, akin to a crime defined by choosing a socially harmful activity level.[1] They do, however, also learn that there is neither audit nor enforcement. Finally, participants are informed how many other members of a randomly composed group of six violate the rule. They choose the number of actions conditional on the number of others who violate the rule.

The experiment is designed such that the dependent variable (violation of an arbitrary, unenforced rule) can be measured with perfect precision, and

that the independent variable (the number of peers violating the rule) need not be measured; it can be manipulated. The empirical test is implemented under the fully controlled conditions of the lab. In the experiment, violating the arbitrary rule leads to a higher payoff. That way, choices are incentivized, which creates higher credibility than mere self-report. Participants do not know with whom they interact. There is no communication. This closes multiple alternative channels of influence. Participants decide conditional on the possibility that others do or do not violate the rule. Hence the counterfactual is even observed at the individual level.

These design precautions make it possible to identify the causal effect of knowing how many peers violate a rule. Arguably this basic mental mechanism is present whenever individuals commit a crime in the company of peers. Future research can investigate how this basic effect interacts with more involved behavioral channels, and how it plays itself out with more involved acts of crime. This paper starts by showing how little it takes to trigger a peer effect.

Peer Effects

At a high level, peer research distinguishes a long-term effect, often defined as socialization (Melde and Esbensen 2011), and ad hoc effects, usually characterized as situational (Barnum and Pogarsky 2022; Hoeben and Thomas 2019: 764; Osgood and Anderson 2004; Osgood et al. 1996). In the present experiment, participants have been randomly selected from a pool of several thousand registered persons. They are randomly put into groups of six. They only know that the remaining members of their group have been selected from the same pool. At no point do they learn each other's identity. They also have no chance to communicate with each other. They only learn how other group members have decided after deciding for themselves. This is made possible by the use of the strategy method (Selten 1967), that is, by eliciting choices that are conditional on any possible number of group members violating the rule.

Arguably, these features of the design rule out long-term peer effects. The only remaining source of influence, outside the experiment, is the composition of the subject pool. Participants know that they interact with participants from the same pool. They might know that most, but not all of them are university students. This feature of the design at the least reduces the formative potential of long-term exposure to others to its absolute minimum. It could at most consist of behavior observed or expected among students, rather than other stratums of society. This makes it very unlikely that the forces

observed in long-term interaction matter. The long-term effects postulated in the literature include: What peers do, or what they approve or disapprove, may shape the individual's identity (Buchanan and Krohn 2020; Hogg 2018; Paternoster and Bushway 2008), self-concept (Buchanan and Krohn 2020), attitudes (Augustyn and McGloin 2021; Megens and Weerman 2012; Thornberry et al. 2003), values, norms (Anderson 2000; Miller 1958; Weerman, Wilcox, and Sullivan 2018: 433), preferences (O'Brien et al. 2011), habits, and routines (Uggen and Thompson 2003). Peers may contribute to socialization, which can be socialization into a criminal career (Akers 1973). Peers may create an "alternative moral culture" (Warr 2002). This is the core idea of "differential association" theory (Matsueda 1988; Sutherland 1947). It argues: "a person becomes delinquent because of an excess of definitions favourable to violation of law over definitions unfavourable to violation of law" (Sutherland and Cressey 1978: 81). This explanation is in the spirit of the "social learning" theory of crime (Akers 2017; Burgess and Akers 1966; Pratt et al. 2010). Individuals may, for instance, learn from their peers to tolerate greater risk of punishment (O'Brien et al. 2011).

A fortiori, the formative effect of being in a gang is immaterial (Augustyn and McGloin 2021; Melde and Esbensen 2011, 2013; Moore and Stuart 2022). As interaction is anonymous, the desire to be in harmony with, impress or please one's friends (Battin et al. 1998; Haynie, Doogan, and Soller 2014; Rees and Pogarsky 2011; Thornberry et al. 2003; Warr 2002; Weerman 2011) can also not trigger peer effects.

Many potential "situational" effects are also ruled out by the design of the current experiment. The random composition of groups excludes ad hoc selection into the company of delinquent peers (Gallupe, McLevey, and Brown 2019b; Thornberry et al. 2003). Participants have no chance to seek out each other for their (crime-prone) "homophily" (McPherson, Smith-Lovin, and Cook 2001; Ragan 2020), for mutual prior affinity to crime (Thomas and McCuddy 2020), or for feeling attached to each other (Agnew 1991). Participants may also not have actively sought out another individual as their partner in crime (Pyrooz and Densley 2016). Likewise, the individual may not engage in crime as this is required to be a member of a subjectively attractive group (Melde and Esbensen 2011, 2013; Pyrooz and Decker 2013; Weerman, Lovegrove, and Thornberry 2015). If there is an effect, it must result from what the peer's literature calls "influence" (Steglich, Snijders, and Pearson 2010).

In the field, crime requires an opportunity. The presence of peers may be a condition for making the criminal act promising (Decker, Decker, and Van

Winkle 1996; McGloin and Thomas 2016b; Osgood et al. 1996). Peers may make the implementation of the crime easier (Decker and Wright 1994; Hochstetler 2001), or they may even be required as co-offenders (Carrington 2009; Hoeben and Thomas 2019: 770; McGloin and Piquero 2009; Van Mastrigt 2017). In the present experiment, none of this can matter. The act consists of typing in a number while sitting in isolation in front of a computer screen. In the field, the benefit from crime may exceed its instrumental value. Within a peer group of delinquents, there may be reputation gain (Brantingham, Yuan, and Herz 2021; Hoeben and Thomas 2019: 767; Short and Strodtbeck 1965). Being a member of a delinquent group may protect oneself against outside violence (Gravel et al. 2018). In the current experiment, all of this is ruled out by the fact that others never learn the choices a participant has made. This design feature deprives criminals of a social audience (Buchanan and Krohn 2020). Seeing others violate a rule may alert individuals to the opportunity for delinquency (Giordano, Cernkovich, and Pugh 1986). But the experiment is so simple that this opportunity is obvious.

Crime may come with thrill and excitement (Thomas and McGloin 2013), which may be heightened when in the company of peers (Brezina and Piquero 2007; Burt and Simons 2013; Hoeben and Thomas 2019: 768; Nguyen and McGloin 2013). Yet the experimental task implemented here consists of the abstract choice of an activity level, so that excitement is unlikely in the first place. At any rate, anonymity excludes the presence of other offenders increasing the excitement.

The presence of peers on the crime scene may make it more difficult for the individual offender to be apprehended (McGloin and Rowan 2015: 488), and it may make it harder for prosecutors to prove beyond a reasonable doubt that this individual is guilty as charged (McGloin and Thomas 2016b; Pogarsky, Piquero, and Paternoster 2004). The fact that others violate a rule may also help individuals update the probability of being caught (Gino, Ayal, and Ariely 2009). In the present experiment, participants are informed upfront that there is no audit, and hence no sanction if they violate the rule, so that implications for the risk of a sanction cannot explain peer effects.

Conversely, not engaging in crime may come with social cost; there may be peer pressure to participate (McGloin and Piquero 2009: 340). The individual may be afraid to be ridiculed (Anderson 1999; Decker 1996; Warr 2002: 46), to lose status (Gravel et al. 2018; Matsueda, Kreager, and Huizinga 2006) and loyalty in her social environment (Costello and Hope 2016). Social anxiety and fear of peer rejection may induce them to

participate (Hoeben and Thomas 2019: 767). All of this is excluded by the fact that, in the present experiment, the identity of group members is not disclosed.

In the field, peers may instigate crime (Hoeben and Thomas 2019: 768; McGloin and Nguyen 2012; Warr 1996), and trigger an impulsive reaction (Thomas and McGloin 2013). More generally, the decision to engage in crime may be facilitated by conversation among would-be offenders (Barnum and Pogarsky 2022; Hoeben and Thomas 2019). Criminals may learn about techniques required for offending (Sutherland 1947). They may be induced to repeat what has worked (Brantingham et al. 2021; Samuelson and Zeckhauser 1988). Yet in the current experiment, they cannot observe each other in the act. They react conditionally to the possibility that a certain number of the other group members violate the rule. Hence these effects cannot explain a peer effect either.

Mere Social Information

The explanations for peer effects reviewed in the previous section share one building block: before deciding whether to engage in the act, a person learns about the decisions or the intentions of others. Peer effects require social information. The present experiment is designed to isolate the effect of social information. In this section, the reasons are discussed why mere social information might already increase the incidence of crime and deviance. This question is related to the question of whether the "mere presence" of peers increases the risk of crime (Chou and Nordgren 2017; Hoeben and Thomas 2019: 759), and whether peer crime is "contagious" (Brantingham et al. 2021: 953; Thomas and McCuddy 2020: 716).

Recently criminology has been alerted to the possibility that the decision to commit a crime might be motivated by elements in the utility function that transcend expected profit, and by social preferences in particular (Agnew 2014; Jaynes and Loughran 2019). A rich literature in behavioral economics has theorized fairness (see the survey by Fehr and Schmidt 2006). Individuals tend to particularly dislike if others are better off, with no compelling justification (Bolton and Ockenfels 2000; Charness and Rabin 2002; Fehr and Schmidt 1999). Now not engaging in crime may be constraining. If this is how the individual perceives the situation, she may not want to be the "sucker." This might motivate her to violate a rule that she learns some or even many of her peers are violating.

The fact that others violate a rule may change how the individual sees the situation, and how she assesses the decision problem. Delinquent peers may serve as role models (Agnew 1991; Asch 1955; Chartrand and Bargh 1999; Hoeben and Thomas 2019: 766). They may change what the individual perceives to be the social norm (Gino et al. 2009). Delinquent peers may reduce the guilt the individual perceives when violating the rule herself (Rowan et al. 2022: 4). Learning about rule violations of others may provide an opportunity for "neutralization" (Sykes and Matza 1957). One could also argue that the presence of peers facilitates the diffusion of responsibility (Rowan et al. 2022: 5). If peers do not co-offend, their presence does not make it systematically more difficult to establish causation. But perpetrators may feel less responsible for disregarding the norm: in the actual context, and as part of the fabric of society. In this looser sense, one may also argue that the presence of delinquent peers contributes to "deindividuation" (Diener 1976; Postmes and Spears 1998). Seeing others commit a crime may lead to "disinhibition" (Weerman et al. 2018: 434).

Crime and Delinquency

In the interest of cleanly identifying the effect of mere social information, the present experiment uses a very thin definition of crime: earning more money by choosing an activity level above a patently arbitrary limit, knowing that there is neither audit nor enforcement. One may wonder whether this dependent variable is so thin that it is uninformative for criminology.

There has been a long-standing debate in criminology about the proper definition of crime (Hagan 1994), in the interest of emancipating criminology from criminal law (see already Cressey 1951) and of avoiding ideological bias to the detriment of the poor (Michalowski 2016). Dispositional theories of crime want the discipline to focus on the "propensity to use force and fraud" (Gottfredson and Hirschi 1990: 4). Routine activity theory argues that attention should be on situational conditions that favor the convergence of likely offenders, suitable targets, and the absence of guardians (Cohen and Felson 1979). Situational prevention scholars contend that preempting future crime requires understanding crime-specific opportunity structures (Clarke 1995).

These are perfectly legitimate approaches. But ultimately, committing a crime is a choice made by the individual offender (Clarke 1980). Constructing crime as a decision not only requires that the would-be offender is aware of the option not to commit the act. He or she must also

discern a gap between the act in which she is about to engage and a normative expectation. As the deviance literature rightly stresses, this expectation need not originate in criminal law. It may for instance result from the paternalistic intention to prevent harm resulting from smoking, drinking, or skipping school (Rees and Pogarsky 2011; Thomas and McCuddy 2020: 722). But crime as choice requires the decision to violate a normative expectation. In the interest of identification, the present experiment radicalizes normativity and makes the expectation patently arbitrary.

The present experiment shares its starting point with the situational action theory of crime. It exploits the fact that humans are rule-guided creatures (Wikström 2019). But for the purpose of isolating the motivating effect of rule-following, it strips the rule of any moral connotation and triggers normativity per se.

Experiments on Peer Effects

Experiments are increasingly used in criminology (McGloin and Thomas 2013). A few experiments have tested for peer effects. Paternoster et al. (2013) provide participants with the opportunity to earn more money by cheating in a memory task. In their baseline, nobody did. Yet in the treatment, a confederate indicated her intention to cheat, justified it, and engaged in it. In this condition, more than a third of all participants were cheating as well. The result was replicated in Mercer et al. (2018). Gallupe et al. (2019a) gave participants the opportunity to steal a gift card. They manipulated the presence and protocol of a confederate. If the confederate only talked about the possibility of stealing, this did not have an effect. But the incidence of stealing increased significantly if the confederate stole him/herself, whether or not he/she also discussed the opportunity for stealing. Likewise, in Gallupe et al. (2016) merely verbal prompting did not increase stealing, while there was more stealing if the confederate stole the gift card himself. In Gino et al. (2009), the effect was even more pronounced. If the confederate merely made the opportunity for cheating salient, others became even less likely to cheat. By contrast, cheating increased if the confederate engaged in cheating himself. However, this effect was only observed if the confederate was ostensibly an in-group member.

McGloin and Thomas (2016b) tested two groups of student participants on one vignette each. They manipulated the number of others who violate the norm not to destroy the property of others, or to steal. They were interested in the impact of peer behavior on the perceived cost and benefit of

crime, not in their choices. The closest relation to the present experiment and research question is their measure of "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 a maximum of 75 peers engaging in the criminal act.

McGloin and Rowan (2015) manipulated in a vignette study whether participants decide in the presence of a large number of peers or strangers about committing a crime against supporters of a competing baseball game. In the presence of peers they were 13 percent more likely to do so. As they are interested in the threshold model of crime, their experiment is designed to find the number of peers participants (hypothetically) require to join in. Gardner and Steinberg (2005) are strictly speaking not an experiment about crime, but about risk-taking. When in the presence of two peers, adolescents and young adults (but not older adults) take more risks in a computer game of "chicken," where they risk a car crash. Collins, Parks, and Marlatt (1985) find that a delinquent confederate increases alcohol consumption in college students, but only if the confederate behaves sociably. In a purported wine-tasting task among college students, Lied and Marlatt (1979) paired treated participants with a heavily drinking confederate. This significantly increased the amount of alcohol participants consumed.

Experiments on Rule Following

The paradigm used in this experiment is borrowed from experimental economics. The bulk of this literature is utilitarian. It starts from the assumption that individuals want to maximize profit, and investigates the conditions under which an individual is willing to give up (the opportunity for) some profit for herself, in the interest of someone else's well-being (an excellent survey is Fehr and Schmidt 2006). Only recently, experimental economists have wondered whether individuals are even willing to follow rules if this does not help a discernible person. A graphic experiment is 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.

While it is difficult to explain these choices on utilitarian grounds, one may still argue that participants have adopted the routine to stop at traffic lights, as this improves traffic safety for themselves and others. The experimental "traffic light" could trigger this routine, that has originally been formed for its long-term utility. This potential confound is ruled out by a

design in which the rule is patently arbitrary. Participants earn a piece rate for positioning the cursor to the number 10 on any one of the X radiolines. But there is a rule not to position the cursor on more than N < X radiolines. The rule is deliberately not motivated. There is no public project for which the money is needed. The rule does not have a redistributive effect either. It merely is an explicit expectation. The instructions do make it clear that there is neither audit nor enforcement. Nonetheless, many participants follow the rule (Desmet and Engel 2021). This design is adapted for the present project.

Main Experiment

Sample

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. One hundred and fourteen of them (64.04 percent) were female. Mean age was 26.69. Nineteen were not students. The remaining held various majors. One hundred and fifty-six (87.64 percent) reported that they had already had a paid job (which could also be part-time). The experiment was programed in oTree (Chen, Schonger, and Wickens 2016) and implemented online. The online experiment was embedded in a Zoom session. Via Zoom, the monitor could talk to participants. Participants could privately ask questions in the chat. But this was only a safeguard. The complete design of the experiment was explained on the screen. Participants also had to answer control questions, to make sure they understood the design. Participants' video was disabled to preserve anonymity. The whole experiment took about 30 minutes. Participants earned 4 € for their participation, and at a maximum 9 €, and the mean of 6.39 € (equivalent to 7.55$ on the first day of the experiment) from their choices in the experiment.

Design

The total experiment consists of four parts. The complete instructions (in the German original) are available from the author upon request. Translations of the critical slides are in the Online Appendix. The main part is the second. The other parts will be introduced in later sections.[2]

Participants are informed that they can earn money with a very simple action. On their computer screens, they see 48 radiolines,[3] as in Figure 1.

Figure 1. Example task.

On all lines, the cursor is initially at the number 1. For each of 48 radiolines in which they position the cursor to the number 10, they earn a piece rate. If they click on a different number, or if they do not touch the respective radioline, so that the cursors stay at the default (i.e., the number 1), they do not earn money.

The task is perfectly easy so that earning money requires neither skill nor pronounced effort. If a participant wants to earn the most money from the experiment, she repositions the cursor to the number 10 on all 48 radiolines. Participants are, however, informed that there is a rule: they are supposed to move the cursor on no more than five radiolines. The instructions stress, however, that this rule is not enforced. There is no audit, and no sanction if they position the cursor to the number 10 on more radiolines. The experiment is run in a lab with a strict no-cheating policy. Participants know this. Most of them have previously participated in experiments in the same lab, and therefore also know from first-hand experience that they have no reason to be concerned that the experimenter is not telling the truth. Participants therefore know that they are completely free to earn full compensation, by positioning the cursor on the number 10 for all 48 radiolines.

The purpose of this paper is to learn whether, and if so how, the decision to violate the rule is moderated by social information about the number of peers who violate the rule as well. If one wanted to study this question with actual choices, one would need an impractically large sample. Five of six members of each group would first have to decide in the absence of social information about the choices of their peers. One randomly selected group member would be informed about the number of others who have violated the rule, and would decide conditional on this information. Only one out of six choices would be relevant to the research question. And one would be at the mercy of what the remaining members of the respective group have decided. Very likely, in most groups some, but not all group members would violate the rule. Hence there would be no, or at least very little, data about reactions to a very rule-abiding, or a very rule-violating, group.

The present experiment parries these challenges with a well-established experimental trick. It uses the strategy method, that is, it elicits conditional choices (Selten 1967). Each participant decides on how many radiolines she

wants to move the cursor to the number 10, provided no other member of her group has violated the rule, or 1, 2, 3, 4, or 5 of them have. She knows that the one of her choices will be implemented that corresponds to the number of others who actually have violated the rule. To get this number, the experiment uses a second experimental trick (first used by Fischbacher, Gächter, and Fehr 2001). Participants actually take two decisions: one unconditional, and one conditional on the number of other group members who violate the rule. They know that one of them is randomly selected and her conditional choice is implemented. For the remaining five group members, the unconditional choice is payoff relevant. To make these elements of the design practical, the experiment finally disentangles the decision and its implementation. On the decision screen, participants commit to the maximum number of radiolines on which they position the cursor to the number 10, separately for every number of others who violate the rule. At the very end of the experiment, and after all random draws, they are informed about the condition that is payoff relevant. They are reminded of the decision they have made for this condition. If the number of radiolines on which they reposition the cursor to the number 10 exceeds their commitment for the relevant condition, their earnings are capped at their commitment. Participants are informed about this precaution before deciding.

The design of the experiment and the main hypothesis have been preregistered.[4] The Bonn Max Planck Institute has received block approval from the Ethics Council of the Max Planck Society for experiments that follow the protocol that is standard in experimental economics, as this experiment does.[5]

The purpose of the experiment is to isolate a peer effect on violating a patently arbitrary rule.

Hypothesis: The more members of the group decide to violate the rule that they should not move the cursor on more than five radiolines to the number 10, the more a participant does so as well.

Results

Figure 2 summarizes results. There are two complementary ways of analyzing the data. The bars show the percentage of participants who have decided to move a number of cursors greater than five. This dependent variable is thus the binary choice to obey or to violate the rule. The line is the corresponding continuous variable. It shows by how many cursors participants' decisions on average exceed the limit of five cursors. This continuous dependent variable represents the degree of rule violation.

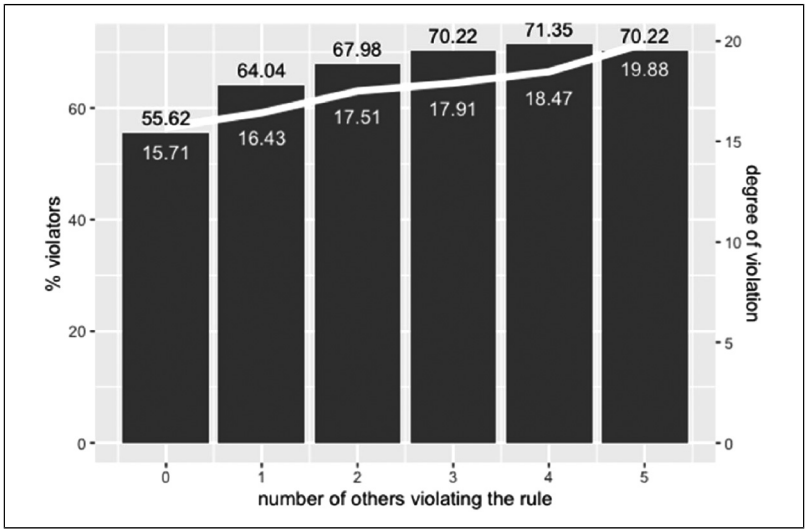

Figure 2. Decision to violate the rule not to move more than five cursors.

Visibly, both dependent variables react to the number of others who violate the rule. While 55.62 percent violate the rule when knowing that they are the only group member who does not obey, 70.22 percent violate the rule when knowing that all other group members do so as well. Likewise, the average gap between the rule and the actual commitment is 15.71 cursors if no other group members move more than 5 cursors, while it is 19.88 cursors if all other group members overstep the limit.

The regressions in Table 1 show that the effect is highly significant ($p < .001$) and substantial.[6] For every group member who violates the rule, the probability increases by 2.8 percent that the participant in question also violates the rule. Every group member who violates the rule on average increases the gap between the rule and the commitment by little less than 1 (of 48) cursors. We can confidently conclude that crime, defined as a rule violation, is conditional on knowing how many peers violate the rule.

Extensions

Threshold Model

The present experiment does not only manipulate whether a peer is happy to violate the (arbitrary) rule. It also manipulates the number of peers who do

Table 1. Decision to Violate the Rule Not to Move More Than Five Cursors.

	Binary	Continuous
Number of violators	.028***	.781**
	(.00822)	(.265)
Cons	.569***	14.915***
	(.042)	(1.599)
N obs	1068	1068
N uid	178	178

Linear models with participant random effects.
Hausman test insignificant on both models.
Robust standard errors in parenthesis.
Binary: dummy that is 1 if participant decides to move more than five cursors.
Continuous: decision to move x cursors – 5.
*** $p < .001$, ** $p < .01$, * $p < .05$.

so. In this second dimension, it is related to the "threshold" model of crime. Building on a more general concept from sociology about individual versus collective action (Granovetter 1978), the model argues that individuals have an idiosyncratic threshold (Granovetter and Soong 1988). They tolerate a certain (typically small) fraction of rule violations without adjusting their own behavior. But the more rule violations become prevalent, the more they are likely to switch from rule-abiding to rule-violating themselves (McGloin and Rowan 2015; also see McGloin and Thomas 2016b). Based on the concept of delinquency balance (McGloin 2009), it has further been suggested that there might not only be a lower bound of peer criminal activity, necessary for engaging in crime, but also an upper bound, leading to desistance from crime (McGloin et al. 2021: 739).

If participants have such a threshold, in the experiment they should obey the rule when knowing that all other group members do so as well, and switch to rule violation at their individual threshold, that is, at some number of other group members violating the rule. As Figure 3 shows, for a minority of participants, this is indeed the case: 17 participants do not move more than five cursors if all other group members do not overstep the limit either, but they move more cursors whenever a single other group member does so as well. Six participants switch if two others overstep the limit, and so on. Yet the majority of 82 participants never obey the norm, and 31 participants always obey the norm, however many others violate it.[7] This suggests that the threshold model matters, but only for a (relatively small) part of the population.

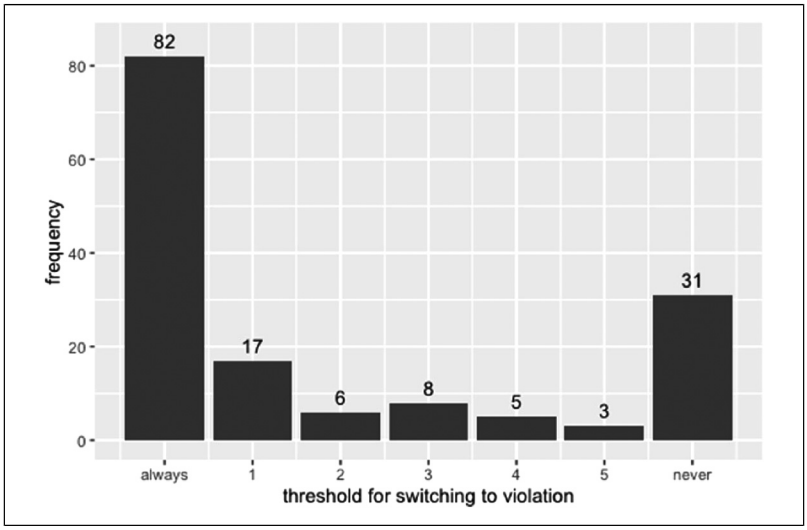

Figure 3. Threshold for switching to rule violation.

Interaction With Cost of Rule Following

In the rational choice model of crime, offenders decide based on the expected profit from engaging in crime (Becker 1968). There are certainly other motives at play, but empirical evidence suggests that incentives do matter (Loughran et al. 2016). It for instance has been shown that copper theft increases if copper prices go up (Brabenec and Montag 2018; Sidebottom, Ashby, and Johnson 2014; Sidebottom et al. 2011). In this example, the potential gain from crime increases. The experiment makes it possible to test the mirror question: does crime (operationalized as rule violation) increase if the cost of rule-following goes up?

In the experiment, this is implemented by testing participants not only on the (most onerous) rule to only move five of 48 cursors, but also on the more lenient rules to only move 11, 23, 32, or 41 cursors. Figure 4 graphically represents, for each rule, how costly it is to abide by the rule (hollow rectangle below the zero-line), and how much rule-abiding participants stand to gain (solid rectangle above the line).

Focusing on the case where no other group member violates the rule in question, we see that the severity of the rule does indeed affect how many participants on average violate the rule (left panel of Figure 5), and by how much (right panel of Figure 5).

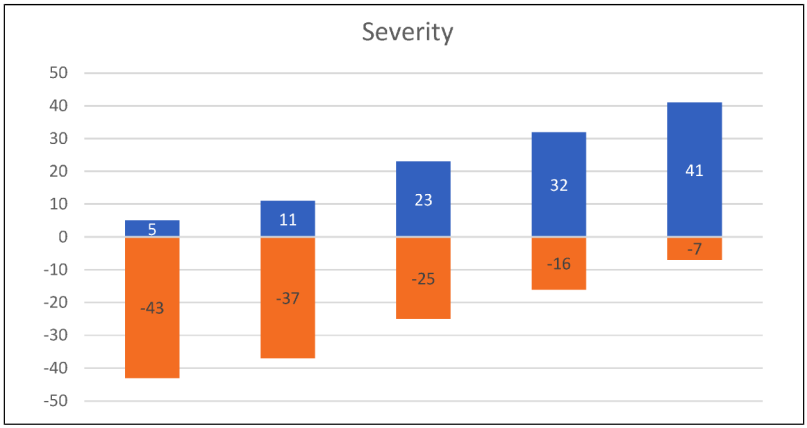

Figure 4. Severity of alternative rules.

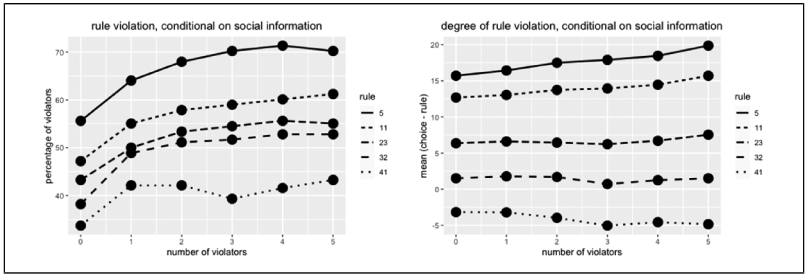

Figure 5. Decision to violate a rule conditional on severity of the rule and social information.

Most interesting for the present context is, however, the possibility of an interaction between the severity of the rule and the number of others who violate the rule. The binary dependent variable (rule violation yes or no) does have an upward slope for all rules. This shows that social information matters irrespective of the severity of the rule. Descriptively, there is no obvious interaction (slopes do not strongly differ by severity). This is different from the degree of violation. The more the rule is onerous, the more important is social information (the steeper the lines).

Model 1 of Table 2 shows that there is indeed a significant ($p < .001$) main effect of both the number of group members violating the respective rule and of the severity of this rule, on the decision to obey.[8] The

Table 2. Decision to Violate a Rule Conditional on Severity of the Rule and Social Information.

	Binary		Continuous	
	Model 1	Model 2	Model 3	Model 4
Number of violators	.022***	.035***	.021	1.397***
	(.007)	(.010)	(.018)	(.380)
Limit	−.060***	−.052***	−5.61***	−4.871***
	(.007)	(.007)	(.289)	(.352)
Number of violators × Limit		−.003*		−.297***
		(.0015)		(.063)
Cons	.714***	−.683***	29.02***	26.052***
	(.042)	(.044)	(1.823)	(2.079)
N obs	5340	5340	5340	5340
N uid	178	178	178	178

Linear models with participant random effects.
Hausman test insignificant on all models.
Robust standard errors in parenthesis.
Binary: dummy that is 1 if participant decides to move more than five cursors.
Continuous: decision to move x cursors − 5.
*** $p < .001$, ** $p < .01$, * $p < .05$.

coefficient of the limit is negative, indicating that participants are less likely to violate the rule the higher the limit, that is, the less the rule is constraining. The regression predicts that for any additional radioline the rule allows participants to make money with, the rule is 6 percent more likely to be obeyed. As model 2 shows, actually, there is also a small, but significant ($p < .05$) interaction: the less the rule is severe, the less participants are sensitive to information about the number of their peers who violate the rule.[9] Interestingly, for the degree of rule violation, only the severity of the rule turns out significant ($p < .001$), while social information about the number of peers violating the rule is not. The (statistical) picture clears once one interacts with both variables. One then also finds a strong and highly significant ($p < .001$) main effect of the number of violators: one more violator in the group is predicted to increase the number of radiolines the participant decides to solve by nearly 1.5. Actually, this is the predicted effect in the theoretical situation where the limit is zero: participants are not allowed to earn any money. The highly significant ($p < .001$) negative interaction shows that social information is less important for the degree of rule violation the less the rule is constraining.

Perception

The criminological literature has been highly attentive to the fact that crime-relevant information is filtered through perception. It has, for instance, been argued that perception is critical for deterrence (Nagin 1998) and that the presence of peers affects the perceived risk of sanctions (Pogarsky et al. 2004). More generally, perception matters in assessing the cost and benefit of crime (McGloin and Thomas 2016b; Thornberry et al. 1994). Learning that peers violate a norm may serve as a signal that helps individuals update their estimate about the benefit of rule violation (Anwar and Loughran 2011). All these potential distortions are ruled out by the design of the experiment. There is no audit and hence no risk of sanctions. The benefit from overstepping the respective limit is straightforward so that participants do not need social information to learn that rule violation is profitable.

The design of the main experiment also helps with another potential source of distortion, projection bias. A radical version stems from Gottfredson and Hirschi. They argue that measures of peer delinquency are no more than "another measure of selfreported delinquency" (Gottfredson and Hirschi 1990: 157). More generally criminologists have been alerted to the risk that those engaging in crime themselves are influenced by an inflated expectation that their peers would do so as well (McGloin and Thomas 2016a; Rebellon and Modecki 2014). Such a "false consensus" effect (Ross et al. 1977) is in line with the psychological proclivity to form self-serving beliefs (Babcock and Loewenstein 1997; Haisley and Weber 2010; Nagin and Pogarsky 2003). In the main experiment, this potential distortion is ruled out by manipulating, instead of measuring, the number of group members who violate the respective rule.

Two further elements of the design make it possible to directly measure the distortion. Before the beginning of the main experiment, participants are informed about the design of the experiment, short of the peers' dimension. They do thus learn that they can earn a piece rate by moving any of 48 cursors, but that one of five unenforced rules is in place. They are asked how many cursors they want to move, conditional on the respective rule. They know that, before they actually work on the task of positioning the cursor to the number 10, one of the five rules will be randomly selected. The number they have indicated for this rule in the first part of the experiment is the upper limit. If they position more cursors than they have indicated for the randomly selected rule, they do not earn additional money. Moreover, participants know that the first part of the experiment is only paid out with a 50 percent probability. With the counterprobability of also 50 percent, the second, still unexplained, part of the experiment will be paid out.[10] After the second part of the experiment, participants are incentivized for guessing

how many of the five members of the group randomly formed in the second part of the experiment actually have violated any of the five rules.

Recall that, in the first part of the experiment, participants were not induced to construct such a belief, as the social context was not made salient. Moreover, for choices in the main experiment, the belief was immaterial, as the number of group members violating the rule was exogenously manipulated. If beliefs (third part of the experiment) explain choices when deciding in the absence of social context (first part of the experiment), participants do indeed project their own choices on the choices of their peers. With this design it is, however, not possible to identify the direction of causality: beliefs may have motivated participants in the first part of the experiment, participants may have used their choices in the first part to elaborate beliefs in the third part, or beliefs and choices may have been constructed interactively. But as beliefs are incentivized, one learns whether participants have been unable or unwilling to correct for the bias, to the extent it exists.

The averages shown in Figure 6 suggest a clear association between beliefs and choices, both in terms of the binary decision to obey or violate the rule, and in terms of the continuous variable that measures the gap between the rule and the number of cursors the participant decides to move. Yet as the regressions in Table 3 show, the effect is only significant for the latter variable ($p < .001$). The significant negative interaction effect shows that the effect is more pronounced the more the rule is severe (and hence the limit is smaller).[11]

External Validity

Complementarity to Main Experiment

The main experiment has the advantage of creating very clean data. This makes it possible to identify the causal effect of social information, rule

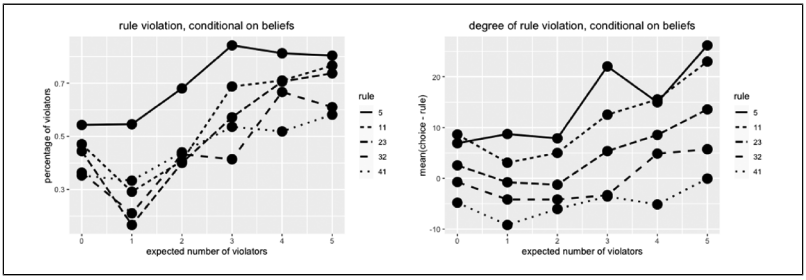

Figure 6. Decision to violate a rule conditional on beliefs about the number of others violating.

Table 3. Decision to Violate a Rule Conditional on Beliefs About the Number of Others Violating.

	Binary		Continuous	
	Model 1	Model 2	Model 3	Model 4
Expected number of violators	.003 (.010)	−.032 (.022)	.030 (.428)	2.023* (.807)
Limit	−.062*** (.008)	−.087*** (.017)	−5.361*** (.324)	−4.127*** (.607)
Expected number of violators × Limit		.0088^{+} (.0048)		−.435* (.176)
N obs	890	890	890	890
N uid	178	178	178	178

Linear models with participant fixed effects
(as Hausman test is significant on all models).
Robust standard errors in parenthesis.
Binary: dummy that is 1 if participant decides to move more than five cursors.
Continuous: decision to move × cursors − 5
***$p < .001$, **$p < .01$, *$p < .05$

severity, and their interaction, on the willingness to follow a rule although it is patently arbitrary and not enforced. But the price for identification is the artificiality of the setting. As a complement, a second experiment 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.

Sample

Two hundred and twenty-one participants randomly selected from the pool of more than 145,000 persons on Prolific who are residents in the United

Kingdom, Ireland, the United States, or Canada, and fluent in English, participated in the experiment. One hundred and thirteen of them (51.13 percent) were female. Three reported their gender as diverse. Mean age was 42.85 (minimum 16, maximum 78). Eighty (36.20 percent) were single, 101 (45.70 percent) married, two widowed, seven divorced, and 30 indicated their family status as "other." Participants reported on average having 2.04 brothers or sisters.[12] One hundred and fifteen (52.04 percent) have no child, 34 one, 49 two, 16 three, and 7 four children.

All participants reported having 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. One hundred and eighteen (53.40 percent) 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). One hundred and seventy-five 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.

Design

The experiment was programed in oTree (Chen 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).[13]

Participants read 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]

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. Three scenarios are crossed with two social context options (absent vs. present) and

three 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.

Results

As this is most relevant for the present paper, data analysis focuses on the marginal reaction to a difference in the content of social information. As Figure 7 shows, descriptive social information 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 percent. It is reduced by another 8 percent 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 percent if a person observes that many others do not pay their taxes.

Table 4 provides statistical tests. When pooling all three scenarios, there is a significant increase in rule violations when learning that many others have violated the rule, but not when only a single other person has been

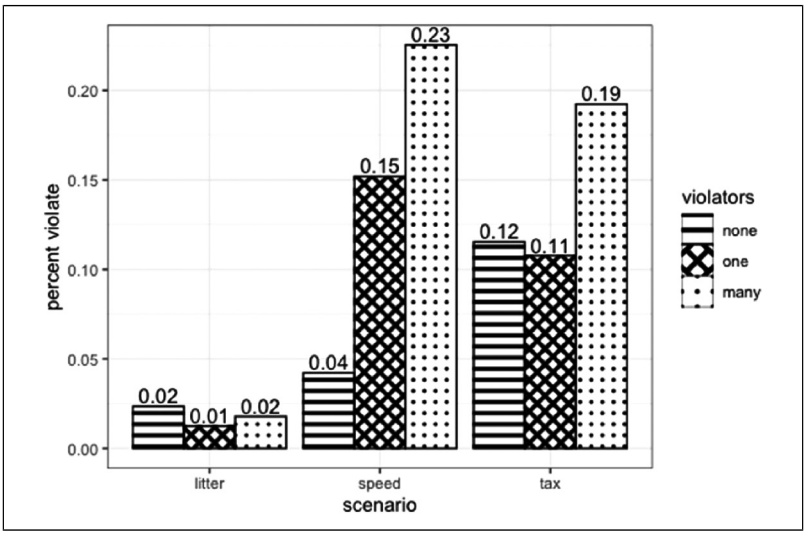

Figure 7. Rule violation conditional on scenario and number of rule violators.

Table 4. Decision to Violate a Rule Conditional on Scenario and Number of Others Violating.

One violator	.032
	(.026)
Many violators	.087**
	(.028)
Speeding	.1164***
	(.0244)
Tax evasion	.1157***
	(.0238)
Cons	−.016
	(.018)
N obs	663
N uid	221

Linear model with participant random effect.
Hausman test insignificant.
Robust standard errors in parenthesis.
dv: dummy that is 1 if participant decides to move more than five cursors.
***$p < .001$, **$p < .01$, *$p < .05$.

deviant. When analyzing each scenario in isolation, social information only turns out significant for speeding.[14]

General Discussion

Why do people commit a crime? An extended literature in criminology argues: the decision to commit a crime is facilitated, if not caused, by the fact that a person's peers commit a crime (see only Agnew 1991; Haynie 2001; Hoeben and Thomas 2019; McGloin and Thomas 2019). In the field, peer effects can have multiple sources. Long-term exposure to criminal peers may introduce people to a criminal career (Akers 1973; Augustyn and McGloin 2021; Melde and Esbensen 2011; Moore and Stuart 2022). The presence of peers may increase the benefit (Decker and Wright 1994), or reduce the expected cost of crime (McGloin and Rowan 2015: 488). Learning that peers commit a crime may serve as a signal that crime pays (Gino et al. 2009). The design of the present experiment rules out all these and many other effects (discussed in the introduction). It reduces a peer effect to its absolute minimum. All participants receive is social information. They learn that a defined number of others have taken the same decision, and how they have decided. This core element must be present

whenever there is a peer effect. By artificially stripping away any other element that in the field typically co-occurs if peers are present, the experiment is able to measure how little it takes to trigger a peer effect.

As the experiment shows, at their core, peer effects result from the fact that social information provides individuals with the opportunity to compare themselves with others, and to put the normative expectation into perspective. They learn about the degree to which the normative expectation is reflected in social practice. 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, and the more intensely she violates the rule. Moreover, the more the rule is constraining, the more likely it is to be violated, and the more intense 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.

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 the incentive to conceal her acts. Even less would it be possible with observational data to prove that (negative) social information is causal.

This paper does not stand in opposition to the rich empirical literature on peer effects. All of the effects reported in the literature[15] are plausible and, to the extent this is possible with the available data, backed up by empirical evidence. Yet clearly not all of these effects can be at work whenever there is a peer. Peers are not always from the same gang (Moore and Stuart 2022). Peers have not always had an impact on forming the perpetrator's identity (Paternoster and Bushway 2008). Peers need not actively seek each other out (Thomas and McCuddy 2020). The presence of peers need not be a precondition for committing a crime, or at least facilitate its commission (McGloin and Thomas 2016b). Peers may not add to the excitement and thrill of the act (Nguyen and McGloin 2013). There may not have been any peer pressure to participate (Gravel et al. 2018). Peers may not have instigated each other to commit a crime (McGloin and Nguyen 2012). Yet as this experiment shows, peers do still have an influence on the decision to commit a crime if none of these channels of influence is active. They need not even be physically present at the crime scene. It suffices if individuals receive social information about the prevalence of a certain crime among their peers.

Criminology has its own experimental tradition (see only McGloin and Thomas 2013). The present experiment deviates from this tradition and borrows a design and the experimental culture from experimental law and

economics. This experimental paradigm privileges are internal over external validity. It aims at removing (historically contingent) social context. Participants are exposed to a naked incentive structure. To increase credibility, participants engage in real money. The typical research question compares the profit-maximizing choice with the observed average response, in the interest of isolating a behavioral regularity. This experimental paradigm is best suited for testing theoretical expectations about basic behavioral mechanisms. The present paper argues: the impact of social information on the willingness to violate rules is one such mechanism, and one that speaks to a core research question of criminology.

Employing the paradigm of experimental law and economics thus offers a novel complement to the otherwise fairly rich criminological literature on peer effects. One gains the option to reconstruct observed peer effects as emanations of behavioral building blocks that are not specific to crime. One may investigate peer effects from the vantage point of normativity, which has even been documented in small children (Wyman, Rakoczy, and Tomasello 2009). In this perspective, crime is a violation of a normative expectation. The observed or expected behavior of peers moderates the otherwise widespread willingness to respect normative expectations.

The main advantage of this experimental paradigm is also its main limitation. One measures the willingness 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. 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 receive 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.

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 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 from speeding. Here the content of social information clearly and significantly has the expected effect. 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 effect 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, and 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 of a richer institutional arrangement. In such an arrangement, motivational effects may complement each other, or they may cancel out. 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. Just learning that one's peers violate a rule already suffices to reduce the willingness to abide by it. This is how little it takes to trigger a peer effect.

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Notes

1. Such rules are, for instance, part of environmental crime: nature tolerates a small amount of emissions, but suffers harm if their level increases. Increasing emissions above the tolerable limit is a criminal act.

2. For the ease of exposition, this part of the paper begins with one of five rules (do not put the cursor to the number 10 for more than five of 48 radiolines). Actually, in this part of the experiment, participants were asked to fill out a complete 6 (number of other group members who violate the rule) × 5 (differently constraining rules). The additional results from the remaining cells in the matrix are discussed below***.
3. In the German original, the radiolines are numbered as "ReglerN," with N being the numbers from 1 to 48.
4. Open Science Framework, August 7, 2021, https://osf.io/mu943/. I am grateful to Jean-Louis van Gelder to suggest a complementary vignette study.
5. Decision of the Ethics Council of the Max Planck Society of May 7, 2018.
6. Each participant makes a choice for every possible number of group members who violate the rule. These choices are not independent. Dependence is captured by a participant random effect (using the plm command in R). The Hausman test turns out insignificant, so that there is no need to shift to a fixed effects model. Standard errors are heteroskedasticity robust. To be on the safe side, the more conservative correction of Huber/White standard errors proposed by Bell and McCaffrey (2002) is implemented, using the coef_test, CR2 command from the clubSandwich package in R. To be on the safe side, we repeat the first model as a random effects logit model (using the glmer command from the lme4 package, cons 1.131*, coef number of rule violators 0.358***).
7. The number of observations is smaller than 178 as 25 participants exhibit some irregular pattern, like switching more than once.
8. The statistical specification is as defined in footnote 3.
9. Results from models 1 and 2 replicate in a nonlinear random effects model, using the glmer command from the lme4 package. These supplementary results are available upon request.
10. This precaution removes the risk of hedging (Blanco et al. 2010).
11. Again results from models 1 and 2 also replicate if we run a random effects logit model instead. Only the interaction term in model 2 turns out insignificant. These additional regressions are available upon request.
12. 1.90 when removing one outlier; this participant reported to have 32 brothers or sisters.
13. 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 August 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.
14. These additional regressions are available from the author upon request.
15. See the literature review above***.

Supplemental Material

Supplemental material for this article is available online.

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