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Johannes Buckenmaier
Eugen Dimant
Ann-Christin Posten
Ulrich Schmidt

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Efficient Institutions and Effective Deterrence: On Timing and Uncertainty of Formal Sanctions

Johannes Buckenmaier
University of Zurich

Eugen Dimant*
University of Pennsylvania & CESifo

Ann-Christin Posten
University of Cologne

Ulrich Schmidt
Kiel Institute for the World Economy

Abstract

Economic theory suggests that the deterrence of deviant behavior is driven by a combination of *severity* and *certainty* of punishment. This paper presents the first controlled experiment to study a third important factor that has been mainly overlooked: the *swiftness* of formal sanctions. We consider two dimensions: the timing at which the uncertainty about whether one will be punished is dissolved and the timing at which the punishment is actually imposed, as well as the combination thereof. By varying these dimensions of delay systematically, we find a surprising non-monotonic relation with deterrence: either no delay (immediate resolution and immediate punishment) or maximum delay (both resolution and punishment as much as possible delayed) emerge as most effective at deterring deviant behavior and recidivism. Our results yield implications for the design of institutional policies aimed at mitigating misconduct and reducing recidivism.

Keywords: Deterrence; Institutions; Punishment; Swiftness; Uncertainty
JEL: C91, D02, D81, K42

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1 Introduction

Governments use substantial resources to keep society safe and punish criminal activities. Annually, mass incarceration costs amount to approximately $182 billion in the United States (Wagner and Rabuy, 2017). The economic tradition to understand deviance and deterrence has its origins in the seminal work by Becker (1968), which stresses the importance of severity and certainty of punishments.[1] More recently, these concepts have been studied in the domain of uncertainty and ambiguity (see, e.g., DeAngelo and Charness, 2012; for a recent review of economic research, see Chalfin and McCrary, 2017, and for a cross-disciplinary discussion of experimental work, see Engel, 2016).

Understanding the deterrence mechanisms of deviant behavior yields important policy implications. However, a particular institutional aspect that is implicitly factored in by Becker's decision framework – the swiftness of punishment – has been under-researched in the economics literature. This concept is often referred to as *celerity* (see Bailey, 1980; Howe and Brandau, 1988; Yu, 1994; Nagin and Pogarsky, 2001, 2004).[2] Historically, this concept has its scholarly roots in the writings of Jeremy Bentham and Cesare Beccaria, and represents a fundamental component in deterrence theory that has been referred to as the 'neglected middle child of the deterrence family' (Pratt and Turanovic, 2018). However, despite theoretical advancements (see, e.g., Nagin, 2013), little empirical work has been conducted regarding the celerity of punishment using observational data since there are very few environments where nearly instantaneous punishment or resolution occurs. For this reason, we capitalize on a stylized experimental setting in which we can systematically vary the structure of celerity.

The goal of this paper is to experimentally study the role of *timing* and *uncertainty* of formal punishment in deterring deviant behavior - aspects that have received surprisingly little attention in this area of research. Specifically, we are interested in how the timing of formal sanctions (be it conviction or sentencing) and the timing of the resolution of uncertainties surrounding these sanctioning mechanisms affect deterrence. We systematically vary the swiftness of a sanction within a new, stylized, experimental

[1]Recent scholarly contributions have broadened this perspective and emphasized the importance of institutional structures, including staff rotations in the public administration, whistleblower protection, the bite of audits and formal punishment, and use of norm-nudge interventions (Shleifer and Vishny, 1993; Abbink, 2004; Nikiforakis, 2008; Sutter et al., 2010; Serra, 2011; Balafoutas and Nikiforakis, 2012; Abbink et al., 2014; Khadjavi, 2014; Engel et al., 2016; Buckenmaier et al., 2018; Bicchieri and Dimant, 2019; Hajikhameneh and Rubin, 2019; Dimant and Gesche, 2020).

[2]Classically, celerity referred exclusively to the temporal delay of a potential sanction following a transgression. We adopt a wider definition of celerity, using it as a catch-all phrase for the timing of the various facets of a deterrence mechanism (Pratt and Turanovic, 2018). Specifically, we distinguish between the timing of punishment and the timing of the resolution of uncertainty regarding the punishment.

paradigm along the following two dimensions: first, we vary the delay between offense and detection; second, we vary the delay between offense and sanctioning. We also study the combination thereof. Our main objective is to test the behavioral assumption that swiftness matters, which we derived from existing theories (e.g., Loewenstein, 1987; Frederick et al., 2002), and to advance our understanding of how to leverage swiftness and the dread of uncertainty as an effective third approach to deter deviance. We argue that swiftness can serve as a useful tool for policy makers to design more efficient and/or potentially also less expensive institutional deterrence mechanisms. Naturally, the concepts of decision-making under risk and uncertainty are not limited to the domain of criminal behavior. Most recently, this has been particularly apparent in the context of the COVID-19 pandemic where individual actions (e.g., to wear masks, to social distance, to obtain a vaccination) are guided by the same principles (Viscusi, 2020). The insights of celerity also find broader application beyond criminal behavior, such as finance and consumption (Palacios-Huerta, 1999; Chesson and Viscusi, 2003). For example, Kreps and Porteus (1978) and Kocher et al. (2014) show that preferences over temporal lotteries also depend on the point in time when the uncertainty is resolved: agents can show a preference for earlier or delayed resolution of uncertainty. Further evidence comes from consumer literature. Anticipatory emotions, compared with outcome-based emotions, are central in prospective consumption situations and the uncertainty associated with anticipatory emotions negatively affects intentions (Bee and Madrigal, 2013).

Given the high costs involved in increasing punishment's certainty (e.g. costs for an executive body) or punishment's severity (e.g. incarceration costs), we argue that the timing of punishment as well as the timing at which individuals are informed about the consequences, that is, their delay with respect to the transgression in question, can potentially serve as a powerful tool for deterrence. Our findings advocate for changes in institutional structures to increase the effectiveness of deterrence (Bigoni et al., 2015).

More generally, from an aggregate perspective, the speed of justice matters because it facilitates economic development and market efficiency (North, 1991; Djankov et al., 2008; Ponticelli and Alencar, 2016). Recent empirical results evaluating the impact of legal reform to increase judiciary adjudication in Senegal find an increase in procedural efficiency without any adverse effects on quality (Kondylis and Stein, 2018). From an individual perspective, existing research in various disciplines indicates that timing affects the association formed in one's mind between the deviant act and the ultimate punishment: without proper swiftness, sanctions risk losing their bite, regardless of how certain or severe they are (Chalfin and McCrary, 2017; Pratt and Turanovic, 2018) – a fact already prominently argued a long time ago (Watson, 1924).

In reality, the closest we can get to achieving maximal swiftness (celerity) of punishment is by catching deviants in the act and punishing them right away. For example, during the FIFA World Cup 2010 in South Africa, the local government agreed to establish 56 so-called 'World Cup Courts' across the country, assigning 1,500 dedicated personnel including magistrates, prosecutors, and public defenders. This was done to achieve speedy justice, in some cases leading to convictions on the same day.[3] Recently, the concept of celerity has entered the correctional arena through the project HOPE (Hawaii Opportunity Probation with Enforcement) as a new model for probation. "In 2004, First Circuit Judge Steven Alm launched a pilot program to reduce probation violations by drug offenders and others at high risk of recidivism. This high-intensity supervision program, called HOPE Probation [...], is the first and only of its kind in the nation. Probationers in HOPE Probation receive swift, predictable, and immediate sanctions – typically resulting in several days in jail – for each detected violation, such as detected drug use or missed appointments with a probation officer" (Alm, 2014). In a first pilot, the project was found to reduce drug use, crime, and incarceration. Simultaneously, it saved the government approximately $6,000 per participant per year through reduced incarceration (Hawken and Kleiman, 2009).[4]

The existing line of research that has acknowledged the relevance of celerity has often resorted to observational studies with mixed approaches and insights. However, as also recognized by this stream of literature, the common absence of reliable observational data and the ability to account for potentially confounding influences (i.e., perceived or actual certainty or severity of punishment) render the study of celerity methodologically challenging (for a critical discussion, see Pratt and Turanovic, 2018). A natural starting point to look for a clean effect is the laboratory environment in which institutional constraints are absent and where we can precisely control the incentives and relevant timings of resolution and punishment (Charness and DeAngelo, 2018). This is in the spirit of the recent surge of experimental economists studying related topics, such as corruption or tax evasion (e.g., Abbink, 2006; Serra, 2011) in controlled laboratory environments. This allows us to control the important elements of celerity: anticipation and revelation of information, timing and severity of punishment, and the opportunity to recidivate. Our experimental analysis is based on a novel cheating game where subjects may cheat in periodic instances to increase payoffs. These cheating periods are followed

[3]https://www.theguardian.com/football/2010/jun/20/world-cup-2010-fans-marketing-justice-fifa

[4]One can, of course, also find settings outside the correctional system (e.g., exam-taking) to test swift versus delayed punishments with variation in timing at which the uncertainty about the punishment could be resolved. Recently, a stream of literature has started examining the role of certainty of punishment in the exam-taking context (Walters and Morgan, 2019) so that our results are also informative for future research that seeks to incorporate celerity and timing of uncertainty resolution in other domains.

by an investigation such that cheaters will be detected and fined with a given probability. Across different treatments, we systematically vary the timing of both the resolution of uncertainty about whether one will be punished and actually being punished.

We analyze behavior alongside two dimensions: total propensity to cheat and recidivism (cheating conditional on having cheated at least once before). Our results show that delayed resolution of uncertainty has no direct systematic impact on behavior. With respect to the relation between the delay of punishment and deterrence, we observe a u-shaped relationship where deterrence is lowest for delayed punishment combined with no delay in resolution of uncertainty and for delayed resolution combined with no delay of punishment, whereas it is significantly higher for either no delay or a delay combined with delayed resolution of uncertainty. This result is at odds with discounted expected utility and theories of anticipatory utility. From a policy perspective, our results suggest that to improve deterrence mechanisms, punishment should either be swift or delayed and paired with the psychological dread of uncertainty.

Section 2 details our experimental design, procedures, and briefly discusses the existing literature on swiftness and deterrence. Results are presented and discussed in Section 3. We conclude in Section 4.

2 Experimental Design

Different theories come to different conclusions regarding the implications of the timing of a sanction for deterrence (see Section 3.1 for details). Clearly, this is an important point that has to be taken into consideration for the design of legal institutions. A systematic study of the role of celerity for deterrence poses a serious empirical challenge, because changing the celerity of an enforcement mechanism would most likely impact existing institutional structures on multiple levels. For that reason, isolating the impact of such an intervention is hardly possible in the field. In addition, it is unclear whether an actual or would-be offender is aware of this change or not, making identification almost impossible. Thus, a systematic study of celerity calls for a highly controlled environment that allows for the isolation of the direct effect of institutional changes varying celerity on behavior. Fortunately, the experimental laboratory provides such a controlled environment.

2.1 The Cheating Game

We use a simple card game, in which subjects have to guess which card was drawn from a deck of cards. The game is played repeatedly by our subjects for a total of 28 rounds. In certain rounds, subjects are presented with the option to "cheat."[5] Cheating guarantees them the maximum possible payoff for that round. Our goal was to design a simple task where the option to cheat was auxiliary – that is, we wanted the task to be easy to understand – but meaningful, regardless of whether or not the option to cheat was presented. Specifically, we wanted to make sure that cheating was not considered part of the game, but a clear violation of the said game's rules. In the game, a card is randomly drawn from a deck of 32 cards and subjects have to guess which card was drawn. A subject received 10 Experimental Currency Units (ECU) for a correct guess and 4 ECU for an incorrect guess. Given a 1 in 32 chance of being correct, the expected payoff of not cheating is about 4.2 ECU. In some rounds, participants are given the option to cheat. By cheating, participants are allowed to uncover the randomly drawn card before making one's guess, ensuring a correct answer and the maximum payoff of 10 ECU less a possible fine if detected.[6] Participants were informed that each instance of cheating will be followed by an "investigation" that would detect cheating with a fixed probability of 25%, the occurrence and outcome of which is only revealed to the participant.[7]

With that, cheating exposes them to the risk of being caught and fined. If caught, the consequences in the form of punishment are twofold. First, the subject has to pay a fine of 10 ECU. Second, the subject is suspended from the game for one round, is not allowed to make any decisions, and cannot earn any ECU. Hence, the total expected fine is about 14.2 ECU including the forfeited expected earnings of 4.2 ECU due to suspension. Consequently, cheating yields an expected payoff of about 6.5 ECU in that round, whereas the expected payoff of not cheating is about 4.2 ECU. Suspended participants are not allowed to play the game for one round and additionally are forced to wait 60 seconds before they are allowed to continue in the next period. We deliberately chose suspension combined with a short waiting period as part of the sanctioning mechanism

[5]We follow recent experimental literature on deviant behavior highlighting the importance of incorporating framing (e.g., using loaded language) that corresponds to the studied behavior. Most notably, while the literature finds that framing is helpful in making the decision-situation salient, it also finds that it does not conceptually change the decision-environment, e.g. no systematic change in the perceived social norms of a deviant action (Banerjee, 2016) or – at worst – produces no change in deviant behavior in the aggregate when compared to a *no framing* situation (Dimant et al., 2020). Thus, a priori we have little reason to assume that the use of framing changes the decision environment for our participants, much less so any systematic changes across treatments that could explain our main results. Ultimately, our decision to use framing is backed by Alekseev et al. (2017) who recommend resorting to context-rich language (instead of abstract language) in order to retain tight experimental control.

[6]When subjects decide to cheat, we automatically implement the "right guess" for them. Subjects are informed about this procedure in the instructions. We implement this forced guess to avoid "second thoughts" where a subject cheats, views the drawn card, but chooses a different card.

[7]To avoid floor and ceiling effects in total cheating behavior, we calibrated the probability of being caught on the basis of pre-tests that were run at the same lab. Such probabilities are common in related experiments studying deviant behavior (see, e.g., Abbink and Serra, 2012; Banuri and Eckel, 2012).

to increase salience with regard to the timing of sanctions.[8] While one might argue that a delayed fine in a laboratory context where all "actual" payments are realized at the very end of the experiment decreases the result's robustness, such concerns do not apply to the suspension as it is clearly linked to the particular round a subject is suspended.

In order to make the moral dimension of cheating more salient in our laboratory context, we introduce a third party, represented by a charity, that incurs a monetary damage as a result of cheating.[9] Specifically, for each experimental session, there is a charity pool of 250 ECU (worth $25) from which 50 ECU is deducted each time a particular subject decides to cheat. At the end of the experiment, one subject is randomly selected. This subject's behavior is implemented towards the charity and the remainder is donated to "Doctors without Borders."[10] The identity of the subject is not communicated and remains unknown to avoid reputation concerns.

2.2 Treatments

We vary the timing in a 2×2 design along the following two dimensions: the timing of punishment and the timing of the resolution of uncertainty. Resolution of uncertainty regarding whether cheating is detected (and hence whether there are sanctions) is either immediate (IR) or delayed by two periods (DR). Obviously, punishment cannot occur before a sentence is passed and, hence, necessarily occurs after the uncertainty is resolved. Relative to the timing of resolution of uncertainty, punishment is either immediate (no delay or ND) or delayed by 2 rounds (delayed or D). This gives us 4 treatments: *IR-ND, IR-D, DR-ND,* and *DR-D.* To ensure that decisions are not influenced by unresolved

[8]Existing research in psychology indicates that delay typically renders the impact of punishment less effective as the connection between the deviant act and the resulting punishment gets lost. Hence, in order to retain effectiveness, any minuscule delay of punishment must come with a clear verbal/cognitive component (for a discussion, see Pratt and Turanovic, 2018). Punishment consists of a monetary and non-monetary component, which is not only more realistic but also follows existing theoretical and experimental literature (e.g., Akerlof, 1980; Masclet et al., 2003; Bolton et al., 2020). From a theoretical perspective, criminal behavior involves a trade-off between the utility of a potential gain and the disutility of a sanction in case of detection. Which factors compose the disutility of the sanction is in principle irrelevant for our experimental analysis given our focus on treatment differences of punishment.

[9]Following Eckel and Grossman (1996), Bicchieri et al. (2020), Dimant (2019), and Bolton et al. (2020), we use a charity instead of a participant within each session to increase the salience of the pro- or antisocial decisions; behavior can either benefit or harm a credible institution delivering a public good.

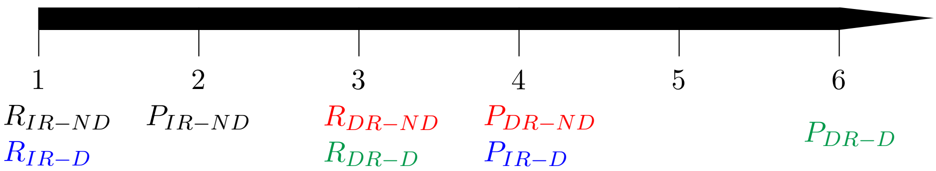
[10]We chose this charity because it frequently tops the rankings of the most trusted charities globally. That way we could maximize our chances that the implementation of the charity serves the targeted purpose: to evoke a moral dilemma. Importantly, participants were never told – and were not able to observe – the number of other participants taking part in the experiment so that the (perceived) impact of their actions on the charity remained constant across all treatments.

risk, each cheating decision was followed by 5 rounds without cheating.[11]

All treatments consist of 28 rounds: 4 training rounds followed by 4 blocks of 6 rounds each. In the first 4 rounds participants play the card game without cheating to familiarize themselves with the game and the interface. In the first round of each block, subjects can cheat. In the remaining rounds of a block (rounds 2-6), they play the card game without the option to cheat. Using blocks of 6 rounds allows us to vary both the timing of the resolution of uncertainty, as well as the timing of punishment without an overlap with subsequent cheating decisions.

Table 1: Overview of timing of resolution of uncertainty and punishment in the different treatments.

Treatment	Timing of resolution of uncertainty	Timing of punishment
IR-ND	immediate	no delay
IR-D	immediate	delayed (2 rounds)
DR-ND	delayed (2 rounds)	no delay
DR-D	delayed (2 rounds)	delayed (2 rounds)



Note: In the timeline, R and P indicate the timing of resolution of uncertainty and timing of punishment for *IR-ND*, *IR-D*, *DR-ND*, and *DR-D*, respectively.

Table 1 summarizes the four treatments where the timing of punishment is always considered relative to the timing of resolution of uncertainty. In treatment *IR-ND*, subjects experience immediate resolution of uncertainty - instant feedback within the same round about whether cheating was detected. The subjects also experience no delay in punishment. That is, the fine (if due) and the potential suspension are implemented immediately for the next period. In treatment *IR-D*, resolution of uncertainty is again immediate, but now there is a delay in punishment of two periods; when cheating in period t, the uncertainty will be resolved immediately, but the potential fine and suspension are executed only in period $t + 3$ (as opposed to $t + 1$ in *IR-ND*). We will also refer to *IR-D* as immediate resolution of uncertainty and delay of punishment. In treatment *DR-ND*, the investigation into cheating does not conclude immediately, but lasts

[11]For each subject, there were exactly four cheating opportunities, that is, in rounds 5, 11, 17, and 23, subjects were given the opportunity to cheat. Subjects were told that, "occasionally," they will be presented with the option to cheat, but they were not informed about the exact timing and frequency of the occurrence of this option.

for two additional periods. Only after that is the participant informed about whether his cheating was detected or not. As in *IR-ND*, there is no delay of punishment once uncertainty is resolved. We hence refer to *DR-ND* as delayed resolution of uncertainty and no delay of punishment. Finally, in treatment *DR-D*, resolution is again delayed, but now punishment is delayed for another two periods after resolution of uncertainty. That is, cheating in period t results in resolution of uncertainty in period $t+2$, followed by the actual punishment (if due) in period $t+5$.

2.3 Experimental Procedures

We conducted 32 experimental sessions at the Decision Science Lab at Harvard University. Participants were recruited vie e-mail invitation from the laboratory's database which contains students as well as non-students. A total of 296 subjects (46.6% male) participated in the experiment split between treatments as follows: 66 subjects in *IR-ND*, 85 subjects in *IR-D*, 69 subjects in *DR-ND* and 76 subjects in *DR-D*. The experiment was programmed and run using z-Tree (Fischbacher, 2007).[12] Within each session, participants were randomly assigned to a computer booth in which they would participate in the experiment anonymously. The consent forms and instructions for the corresponding treatment were distributed (see instructions in in Appendix A). Following an informed consent, the participants were given sufficient time to read the instructions carefully. Before the start of the experiment, subjects had to answer a series of comprehension questions in order to check their understanding of the game and its payoff structure.[13] Subjects then played 28 periods after which they were informed of their total earnings via a detailed summary screen. One subject was randomly drawn to determine the charity pool and all participants were informed about the final amount left in the pool to be donated to "Doctors without Borders."[14] At the end of the experiment, subjects completed a questionnaire containing questions on personal characteristics (demographics, education, income, age), risk-attitudes (SOEP; see Dohmen et al., 2011)[15], consideration

[12]We observed an influx of disproportionately older participants due to a bug in the recruitment software in our first sessions. This was quickly resolved. Participants of 41 years and older represent around 11% of our data set. Unless noted otherwise, our results are robust with respect to this subgroup.

[13]In all sessions, participants answered comprehension questions on a printout and were first verified by the experimenters before the experiment began.

[14]Prior to the experiment, subjects received a short description of the work of "Doctors Without Borders", which helps to enforce a basic level of common knowledge to increase salience of prosocial behavior. A receipt of the amount donated was made available to all participants via email.

[15]Because the SOEP question is an ordinal scale, we transform the variable into a dummy using the median-split: values [0,5] are coded as '0' and indicate risk aversion; values [6,10] are coded as '1' and indicate risk-loving (for a similar approach, see Crosetto and Filippin, 2013, among others). All of our results presented below are robust to alternative specifications such as a mean-split or representing all

of future consequences (Strathman et al., 1994) and self-control (Tangney et al., 2004).

Sessions lasted approximately 45 minutes excluding the time for payment. A participant's payoff was determined by the sum of his earnings over all 28 rounds. The total payoff in ECU was then converted to dollars at a rate of 10 ECU = $1. The average payment was $14.29, which included a show-up payment of $2.50.

2.4 The Role of Swiftness in Deterrence

Standard theories produce conflicting results with respect to the role of timing on deterrence. The classic theoretical approach towards the deterrence of criminal activity (e.g. Becker, 1968) is based on the assumption that potential offenders mainly weigh the expected gains against the expected adverse consequences of an offense. This poses an inter-temporal decision problem under uncertainty: the benefits of criminal behavior are usually immediate, whereas any proceeding detection, conviction, and implementation of legal consequences are generally delayed and stochastic. In the standard framework of discounted expected utility, delayed punishment should reduce deterrence due to a discounting effect, whereas the timing of resolution of uncertainty should have no effect on behavior. However, it has been argued that the uncertainty associated with anticipatory emotions negatively affects intentions (Bee and Madrigal, 2013). Psychological learning theories (Skinner, 1963; Tversky and Kahneman, 1986; Ehrlich, 1996; Hackenberg, 2009) second the argument that the time between a transgression and the punishment as well as the uncertainty that is associated with the punishment are driving forces for effective behavioral changes. If this is indeed the case, then the classical interpretation of celerity as the time between committing an offense and the actual punishment (e.g. fine or imprisonment) should be complemented by the time the uncertainty is resolved, that is, the time of sentencing.

Starting with the seminal paper of Loewenstein (1987), several theories propose that anticipation of future events is an important determinant of intertemporal utility (see e.g., Wu, 1999; Lovallo and Kahneman, 2000; Caplin and Leahy, 2001; Dillenberger, 2010; Strzalecki, 2013; Golman et al., 2019). These models are based on the idea that a non-negligible proportion of the overall consequences from future consumption (be it negative or positive) is already consumed in the form of so-called anticipatory utility before actual consumption takes place. As a consequence, resolution of uncertainty may affect deterrence and the effect of delayed punishment may be reverted, leading to an increase in deterrence (for recent experimental evidence studying preferences for different forms of resolution of uncertainty see e.g. Zimmermann, 2014 and Nielsen,

11 levels as dummies. These results are available upon request from the authors.

2018). While there is growing theoretical literature supporting anticipatory utility theory and its implications, little empirical, and especially experimental, work exists.[16]

In light of these conflicting predictions, it remains an empirical question to show how the timing of punishment, the timing of resolution of uncertainty, and the interaction thereof affects both deviance and recidivism (learning from experiencing punishment and uncertainty). Our experiment is set out to test these important open questions by systematically varying the structure of the timings. In a last step, we will use our insights to point to future research directions.

3 Results

Here, we present our results using parametric and non-parametric comparisons[17], as well as various regression models to analyze differences in cheating behavior. Please note that not only were the number of cheating opportunities (4) the same in all treatments, but also their position within the block was constant (in the first round of each block). Hence, any difference in behavior can only result from our systematic variation in the timing of punishment and the timing of resolution of uncertainty.

There are a number of factors that might affect the propensity to cheat. First and foremost, risk attitudes might affect the individual propensity to engage in cheating. Hence, it is important to note that we find no differences in risk attitudes across treatment groups (chi-square test of independence, $N = 296$, $\chi^2(30) = 30.422$, $p = 0.296$). It is also conceivable that subject who were detected cheating in a previous round, are more likely to cheat in future rounds to make up for the missed income opportunity. Consequently, differences in punishment frequencies across treatments, especially in early blocks, might introduce a confound. To rule out this possibility, we ran chi-square tests of independence comparing total punishment frequencies and punishment frequencies in the first block across treatments. Both tests show no significant difference across treatments (Overall: $N = 1184$, $\chi^2(3) = 0.6806$, $p = 0.878$; first block: $N = 293$, $\chi 2(3) = 1.6887$, $p = 0.639$) indicating that the frequency of (randomly) received punishments are comparable across treatments.

First, we look at the mean differences in total cheating across all treatments. Total

[16]Two recent exceptions are Falk and Zimmermann (2016), who experimentally tested the implications of anticipatory utility in the context of information preferences and Kogler et al. (2016), who showed that delayed resolution of a tax audit results in higher tax compliance.

[17]We follow Moffatt (2015) and employ the bootstrap two-sample t-test method (hereafter BSM) with 9999 replications to analyze mean differences of average behavior. This has the advantage of retaining the rich cardinal information in the data without making any assumptions about the distribution. Unless noted otherwise, the results are consistent with non-parametric tests.

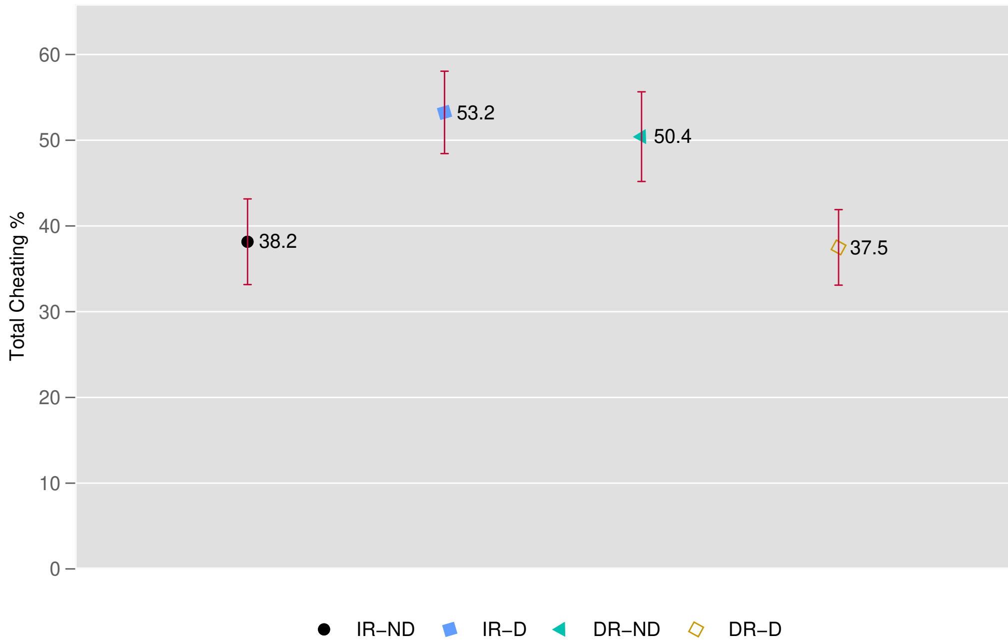


Figure 1: Total Cheating. Percentage of individual cheating attempts relative to the maximum of possible cheating attempts (four).

cheating is defined as the proportion of all individual cheating incidences across all four blocks. We present a graphical illustration in Figure 1 and regression results in Table 2. We find no significant difference between *IR-D* and *DR-ND*. However, our results illustrate an interesting inverted u-shaped relationship between total cheating and delay of punishment: compared to the *IR-D* case (immediate resolution, delayed punishment), cheating is significantly lower in both *IR-ND* (BSM, 53.2% vs. 38.2%, $p = 0.03$) and *DR-D* (BSM, 53.2% vs. 37.5%, $p = 0.02$). This result alone indicates that the most deterrent form of punishment is either swift or delayed and coupled with a period of unresolved uncertainty.[18]

In order to check for robustness, we capitalize on the panel structure of our data by performing a random effects Logit regression (coefficients represent odds ratios). Our dependent variable in Table 2 is a dummy variable indicating the decision to cheat in each block. For ease of comparison and to improve readability of the results, we use *IR-D* (immediate resolution, delayed punishment) as our reference category. The regressions include a battery of relevant covariates such as gender, age, block number, number of correct card guesses, and risk preferences.[19] In model 2, we also control

[18]Compared to *DR-ND*, cheating is marginally lower in both *IR-ND* (BSM, 50.4% vs. 38.2%, $p = 0.09$) as well as *DR-D* (BSM, 50.4% vs. 37.5%, $p = 0.07$).

[19]We used the ordinal risk measure of Dohmen et al. (2011) that asked respondents "How willing are you

for previous cheating decisions (*L.Cheat*) and experience with punishment from past cheating (*L.Punishment*).[20]

Table 2: Propensity to Cheat (Random Effects Logit Regression)

Cheat	model 1 All	model 2 All	model 3 Female	model 4 Male
IR-ND	0.2985**	0.5529**	0.4518**	0.3851
	(0.1722)	(0.1286)	(0.1679)	(0.3391)
DR-ND	0.9763	0.8078	0.6702	1.1419
	(0.5558)	(0.1817)	(0.2113)	(1.0800)
DR-D	0.3406*	0.5563***	0.4940**	0.3505
	(0.1898)	(0.1249)	(0.1621)	(0.3250)
Willing to take risks	0.8275	0.8434	1.0129	0.3437
	(0.3497)	(0.1446)	(0.2508)	(0.2530)
Age	0.9396***	0.9811**	0.9841	0.9343**
	(0.0174)	(0.0074)	(0.0132)	(0.0272)
Block	0.8346**	0.9688	0.8997	0.9182
	(0.0665)	(0.0971)	(0.1307)	(0.1753)
GuessCorrect	0.7560	0.9099	1.1024	0.5046*
	(0.1903)	(0.0927)	(0.1709)	(0.2030)
Male	6.0081***	2.0533***		
	(2.6317)	(0.3536)		
L.Cheat		8.4844***	12.1630***	0.4812
		(1.4882)	(3.1187)	(0.3622)
L.Punishment		1.6401*	1.5647	4.0618**
		(0.4577)	(0.6123)	(2.7446)
Observations	1184	888	474	414

Note: Odds ratios reported. SEs in parentheses, clustered at the individual level. * $p < 0.1$,** $p < 0.05$, *** $p < 0.01$. *IR-ND*, *DR-ND*, and *DR-D* are treatment dummies (reference category is *IR-D*). *Willing to take risks* is the median split of our ordinal risk measure (0–10): Values 0 – 5 are coded as '0' indicating risk aversion, whereas values 6 – 10 are coded as '1' indicating risk-loving. *Block* (1–4) refers to the four blocks (cheating opportunities). *GuessCorrect* is the number of total correct guesses. *Male* is a gender dummy. Lagged variables (*L.Cheat* and *L.Punishment*) for cheating and punishment are dummies indicating whether an individual cheated or was punished for cheating in the previous round.

to take risks, in general?" Respondents rate their willingness on an ordinal scale from 0 to 10. The scale was converted into a dummy variable using a median split. Consequently, all values above the median value of 6 were coded as 1 and all values below 6 were coded as 0.

[20]We also have measures for awareness of future consequences, self-control, and a participant's previous participation in economic experiments. When we add these as further controls, none are significant and the significance levels of our variables of interest remain unaffected. Hence, we decided to not include them in the reported regression table in the interest of brevity, but results are available upon request.

Our analysis in Table 2 on the full sample (model 1) suggests that, relative to immediate resolution and delayed punishment (*IR-D*), both swifter punishment (*IR-ND*) as well as delayed punishment coupled with delayed resolution (*DR-D*) are significantly less likely to render individual cheating decisions. The introduction of delayed uncertainty resolution, holding the timing of punishment constant (*DR-ND*), does not significantly affect cheating behavior. A direct comparison of our treatments mirrors this finding, indicating that higher deterrence can be achieved by either implementing swift punishment (*IR-ND*) or through the combination of delayed uncertainty resolution and delayed punishment (*DR-D*). Post-estimation tests yield a marginally significant difference between *DR-ND* and *DR-D* ($p = 0.071$), and no significant difference between the coefficients of *IR-ND* and *DR-D* ($p = 0.823$), suggesting that the effectiveness of deterrence is of a similar magnitude in both cases. All of these results corroborate our previously obtained non-parametric comparisons.[21]

The results (model 2) also suggest that cheating behavior increases with both past rounds' cheating behavior and having been caught and punished for cheating. This finding indicates that individuals try to make up for incurred losses by increasing the frequency of cheating and taking larger risks, thus being more risk-seeking in losses. Further, a participant's age is inversely and significantly correlated with one's probability to cheat.[22] This indicates that potential wealth effects cannot explain cheating behavior. It is worth noting that we observe substantial gender heterogeneity, indicating that males cheat significantly more than females.[23] To further explore potential heterogeneous effects for males and females, we ran additional regressions with restricted subsamples using only male or female subjects, respectively (model 3 and 4).[24] All this suggests that swift punishment or delayed punishment in combination with delayed resolution of uncertainty significantly increases the deterrence of deviant behavior. The delay of uncertainty alone remains ineffective.

We conclude that both very efficient (no delay of punishment) and very inefficient

[21] Applying the conservative measure of one independent observation per participant, post-hoc power calculations indicate that the comparisons to *DR-ND* achieve statistical power in excess of 87%.

[22] Importantly, the amount of correct guesses in non-cheating rounds, which are the driving force behind potential wealth accumulation in our setting, has no significant predictive power for cheating.

[23] One may argue that this result is partially driven by gender differences in how positive the charity used in this experiment is perceived. We deem this explanation as unlikely based on existing evidence using the same charity that find no gender differences (e.g., Bicchieri et al., 2020; Bolton et al., 2020).

[24] We thank an anonymous reviewer for suggesting these additional analyses. Although male subjects cheat more frequently than females, we find that in these restricted regressions the previously observed treatment effects are only significant for female subjects but not for males. Although gender differences are not the focal point of our investigation, our results are consistent with Croson and Gneezy (2009) who found that females are generally more sensitive to the contextual frame.

(delayed punishment combined with delayed resolution of uncertainty about the status of discovery) punishment institutions are equally effective in deterring deviant behavior. What is more, one could also plausibly assume the presence of learning effects. A large body of existing literature suggests that the learning effects that emerge through experience are shaped by the timing of rewards and punishments. Due to this, they affect subsequent behavior (cf. Camp et al., 1967; Parke and Deur, 1972). This is of particular importance in the punishment context, because such learning effects would directly speak to the occurrence of recidivism among former felons. Following this logic, the experience of uncertainty and punishment following transgressive behavior could lead to differences in subsequent transgressions. We refer to this as *recidivism*. Recidivism in this context is defined as the propensity to cheat conditional on at least one previous transgression (which can occur at the beginning of any of the first three blocks). The idea behind this measure is to understand whether experiencing the drain of uncertainty of punishment following the first cheating decision will affect the individual's subsequent propensity to cheat. Our results indicate that cheating behavior following the experience of uncertainty and punishment is congruent to our previous findings on unconditional cheating behavior. We present a graphical illustration in Figure 2.

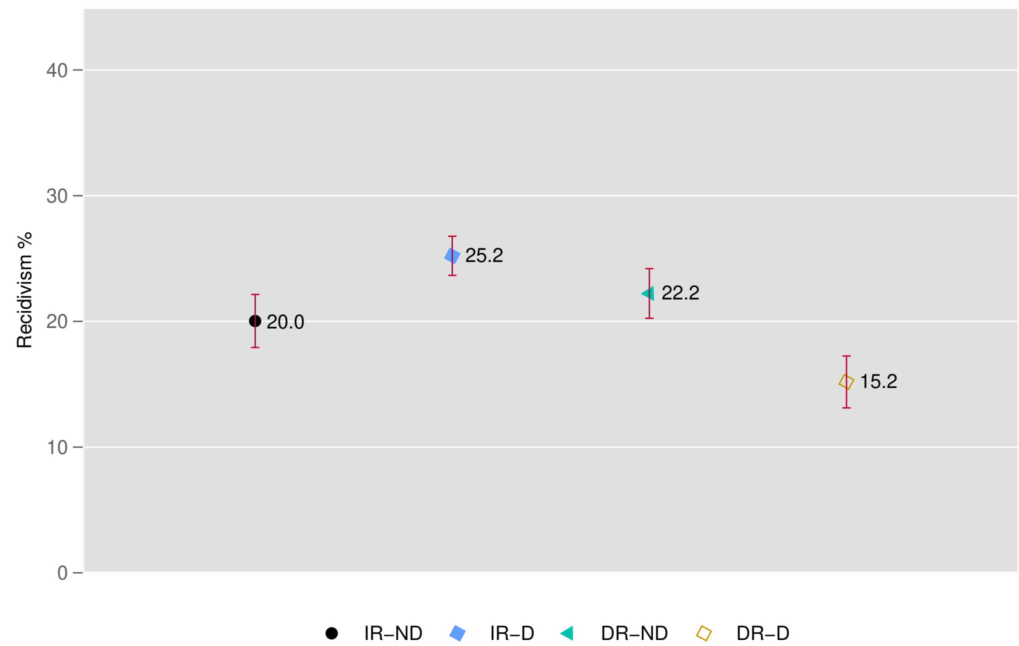


Figure 2: Recidivism. Percentage of individual cheating attempts relative to the maximum of possible cheating attempts following their first cheating decision.

In order to shed light on this mechanism, we employ the same estimation approach as before, but now restrict our sample to observations where participants have cheated at least once before. Our dependent variable of interest continues to be the propensity to cheat in any given period. Through these regressions, reported in Table 3, we look to analyze the propensity to recidivate and continue to cheat.

Table 3: Propensity to Recidivate (Random Effects Logit Regression)

Cheat	model 1	model 2	model 3
IR-ND	0.3008**	0.2045**	0.2437*
	(0.1719)	(0.1605)	(0.2030)
DR-ND	0.6005	0.5095	0.5164
	(0.3311)	(0.3794)	(0.4094)
DR-D	0.2192***	0.1229***	0.1018***
	(0.1235)	(0.0978)	(0.0877)
Willing To Take Risks	0.6751	0.6312	0.6084
	(0.2797)	(0.3553)	(0.3568)
Age	0.9608**	0.9475**	0.9441**
	(0.0183)	(0.0252)	(0.0263)
Block	0.8588	0.6229**	0.6002**
	(0.1377)	(0.1436)	(0.1443)
GuessCorrect	0.5165***	0.4269**	0.4161**
	(0.1293)	(0.1489)	(0.1510)
Male	2.1710*	2.7598*	2.8255*
	(0.9201)	(1.6057)	(1.7118)
L.Cheat		0.2031***	0.1781***
		(0.1228)	(0.1107)
L.Punishment		3.5587**	5.0887
		(1.7547)	(5.8578)
L.Punishment×IRND			0.2687
			(0.3842)
L.Punishment×DRND			0.7242
			(1.0636)
L.Punishment×DRD			1.5303
			(2.1741)
Observations	493	493	493

Note: Odds ratios reported. SEs in parentheses, clustered at the individual level. * $p < 0.1$,** $p < 0.05$, *** $p < 0.01$. *IR-ND*, *DR-ND*, and *DR-D* are treatment dummies (reference category is *IR-D*). *Willing to take risks* is the median split of our ordinal risk measure (0–10): Values 0 – 5 are coded as '0' indicating risk aversion, whereas values 6 – 10 are coded as '1' indicating risk-loving. *Block* (1–4) refers to the four blocks (cheating opportunities). *GuessCorrect* is the number of total correct guesses. *Male* is a gender dummy. Lagged variables (*L.Cheat* and *L.Punishment*) for cheating and punishment are dummies indicating whether an individual cheated or was punished for cheating in the previous round.

Our results for recidivism are consistent with our previous findings, suggesting that the recidivism of individuals is lowest when punishment is either immediate or late and paired with uncertainty. The delay of uncertainty alone is ineffective. We again find an inverted u-shaped relationship; when combining delayed punishment with uncertainty of resolution, recidivism rates return to levels similar to those found when immediate punishment is paired with immediate uncertainty resolution (*IR-ND*). In support of this, post estimation tests show that the drop in recidivism rates in *DR-D* is marginally significant compared to cheating in *DR-ND* ($p = 0.077$). In contrast to total cheating behavior, we do not observe gender heterogeneity for recidivism, except in *DR-ND*, where males are significantly more likely to recidivate (interaction not shown in the table). Age continues to be inversely related to cheating propensity. We observe a particularly interesting finding for the lagged cheating and lagged punishment variables. The results suggest that following one's previous decision to cheat, subsequent cheating decisions become less likely, *unless* previous cheating was accompanied with punishment. In the latter case, participants are substantially more likely to recidivate, which suggests recidivism is driven by one's desire to make up for previous losses. To further investigate whether previous punishment has a differential treatment effect, we ran an additional regression that interacts lagged punishment with the treatment dummies. We find all interactions to be non-significant. Post-estimation tests show that the overall effects identified above remain unchanged.[25]

In summary, we can conclude that the same institutional settings that are capable of reducing recidivism are also the ones deterring deviant behavior in the first place. Our results demonstrate that swift or delayed punishment combined with an extensive dread of uncertainty regarding one's detection reduces future criminal behavior.

3.1 Discussion

In this section, we discuss our results in light of existing theories of intertemporal decision making. Specifically, we seek to illustrate how deterrence should be affected by our two main treatment variables, the delay of punishment and the timing of resolution of uncertainty, given a certain theoretical framework. We consider two prominent theories that seem particularly important for our setting, discounted expected utility theory (DEU) and utility from anticipated emotions (UAE). For brevity, we will focus on the intuition behind derived implications and delegate the formal derivation to Appendix A.

Intuitively, most theories of temporal discounting suggest that future costs or benefits receive a lower weight than immediate ones; this weight decreases as one moves further

[25] Linear combination test: IR-ND + L.Punishment × IR-ND ($\beta = 0.0655$, $p = 0.070$).

into the future (Frederick et al., 2002). First, we consider a standard DEU model with discount factor $0 < \delta < 1$, and for simplicity, restrict attention to a single block of six periods involving one cheating opportunity only. Note that in this model, the timing of the resolution of uncertainty does not affect utility. Furthermore, note that only the utility generated from cheating differs across treatments and that this utility is larger the more the punishment is delayed. Hence, if a potential offender discounts delayed legal consequences, then deterrence decreases as the delay extends. As a consequence, higher celerity (less delay) would increase the efficiency of legal sanctions, which is the classical hypothesis in criminology literature (Nagin and Pogarsky, 2004; Paternoster, 2010). Thus, according to DEU, the efficacy of deterrence decreases as the delay of punishment increases, independent of the timing of resolution of uncertainty. In particular, the point in time where the uncertainty is resolved has no effect on deterrence.

Our results provide only limited support for this implication. When resolution of uncertainty is immediate, we find the expected effect of an additional delay of punishment. However, if resolution of uncertainty is delayed this effect is reversed.

We remark that empirically the above implication of DEU has to be taken with some caution. Gains are usually discounted with a positive rate, making earlier gains more attractive than delayed ones. However, fines are losses, and the evidence for discounting of losses is rather mixed. While studies consistently show that losses are discounted less than gains, it is unclear whether the discount rate for losses is positive, equal to zero, or even negative (Thaler, 1981; Loewenstein, 1987; MacKeigan et al., 1993).

Second, we turn to theories of anticipatory utility, which incorporate anticipatory feelings such as excitement, fear or dread into classical expected utility theory. DEU fails to take such considerations into account. These theories suggest that one might want to bring forward an unpleasant event to shorten the period of dread (or delay a positive event to enjoy the excitement for a longer period of time). The idea is that future events influence current utility. More precisely, negative future events cause more negative utility today the further away the event is (at least up to a certain point). If this effect outweighs the discounting effect described above, then a further delay increases deterrence due to the negative utility from anticipation. Consequently, in UAE, deterrence increases in the delay of punishment if the effect of anticipation is sufficiently high, for a fixed timing of resolution of uncertainty.

Anticipated emotions in Loewenstein's model refer to future consumption under certainty. Caplin and Leahy (2001) extend this model by allowing for uncertainty and point toward the importance of anticipatory feelings prior to the resolution of uncertainty. Anticipatory emotions, such as anxiety, are often predicated on an uncertain future and thus, are mainly relevant prior to the resolution of uncertainty. As a consequence, the point in time at which uncertainty is resolved matters. For example, Kreps and Porteus (1978) and Kocher et al. (2014) show that preferences over temporal lotteries also depend on the point in time when the uncertainty is resolved. That is, agents can show a preference for earlier or delayed resolution of uncertainty. Caplin and Leahy (2001) argue that anxiety experienced one period before the resolution of uncertainty should depend on both the detection probability and the severity of the punishment. Typically, this anxiety, if large enough, leads to a preference for early resolution of uncertainty for negative outcomes. That is, if the effect of anxiety experienced just before the resolution of uncertainty is large enough, a decision-maker discounting future events will prefer to resolve this uncertainty as late as possible to minimize the impact of anxiety. Thus, under UAE, delayed resolution of uncertainty increases deterrence if anxiety prior to the resolution of uncertainty is large enough and the timing of punishment is held fixed.

Given that the effect of anticipation and anxiety are large enough, the combined effect of delayed resolution and delayed punishment can be grasped by comparing, for example, *DR-ND* to *IR-ND*. Taken together the combination of both effects predicted by UAE imply an increase in deterrence when the delays of both punishment and resolution are increased. To summarize, UAE implies a positive effect of delayed punishment on cheating, in contrast to DEU. Delayed resolution of uncertainty may matter under UAE, potentially further increasing deterrence.

Finally, we want to briefly remark that, recently, Baucells and Bellezza (2017) proposed a new theory of inter-temporal decision making. They extend the existing models of anticipatory utility by a reference point which adjusts. It does so during the anticipation phase by altering a utility of recall in the periods succeeding the consumption and changing the magnitude effect in discounting. In this theory it is possible that the utility maximizing timing of an unpleasant event is somewhere in the middle of the time horizon, i.e. fines in earlier or later periods hurt more and should, therefore, lead to higher deterrence. While our experiment was not designed to test this theory, it is the only theory which is compatible with the findings of our experiment.

4 Conclusion

We investigate along two dimensions how timing can impact the effectiveness of sanctions. We use a controlled laboratory experiment designed to study the effect of delayed punishment and delayed resolution of uncertainty on deterrence. Our experimental findings show that the timing of resolution of uncertainty has no effect on deterrence. For the delay of punishment, we observe the following inverted u-shaped relationship: deterrence is highest for no delay or delayed punishment combined with delayed resolution, whereas it is lowest when there is delayed resolution but no delay in punishment or when there is delayed punishment but no delay in resolution of uncertainty. The observed inverted u-shape is at odds with both discounted expected utility theory and anticipatory utility theory. According to the first theory, deterrence should decrease monotonically with the delay of punishment. According to the second, there should also be a monotonic relation between deterrence and delay, which would be the inverse of that in the previous case if the effect of anticipation is sufficiently high.

It is important to note that the effects of treatments on the total cheating behavior can be obtained by two different, possibly simultaneous operating processes. First, the variations in the experimental treatments could have affected the participants' anticipatory reasoning about how a possible punishment would impact them. If the impact is anticipated to be severe, this could lead to delayed or no cheating. Second, learning processes may have affected cheaters who (at least once) underwent the respective treatments differently by experiencing the (non)waiting for a resolution of uncertainty and the potential execution of an immediate or delayed punishment. This may have influenced their likelihood to cheat again in the future. Inspecting the results for recidivism (i.e. future cheating upon having cheated before) shows that they closely mirror the results of the total cheating behavior. Even if some experience for the treatments to become effective would be needed, basic learning theories (e.g. Azrin, 1956; Banks and Vogel-Sprott, 1965) are at odds with the inverted u-shaped relation between deterrence and delay of punishment also observed for recidivism.

Arguably, the highly effective deterrence of deviant behavior in *DR-D* could be interpreted in one of the following two ways: firstly, only a delay of punishment, and not the existence of uncertainty resolution, is responsible for the decrease in cheating; secondly, it is the combination of both the delay in punishment and the existence of uncertainty that imposes additional dread and, thus, the interaction of both is driving the strength of deterrence. Our results and theoretical foundation suggest that it is most likely the former. We consider this as a promising venue for future research.

Our findings yield important insights for optimally designing sanctioning schemes in legal systems. Existing deterrence literature has almost exclusively focused on the role of severity and certainty of legal consequences in deterring proscribed actions. Our study shows that celerity, the timing of sanctions through sentencing, may also be a crucial component of an effective legal system. Our results imply that if deterrence is to be maximized, then the punishment should either follow the criminal act quickly or be delayed and combined with uncertainty. As immediate punishment may be relatively costly, an optimally delayed punishment along with a certain degree of uncertainty could be the most efficient solution to reduce both deviance and recidivism.

Our study provides a first step into analyzing the effects of deterrence in a sanctioning system. In order to draw conclusions for an optimal policy in the real world, future research needs to tackle several limitations of our study. In particular, it seems necessary to study celerity when the delay of punishment extends to the real payout of subjects. While the concept of celerity has already been backed theoretically in the scholarly literature for an extended period of time, only recently has the concept of celerity entered the correctional arena through the project HOPE (Hawaii Opportunity Probation with Enforcement) as a new model for sanctions and probation (Hawken and Kleiman, 2009; Kilmer et al., 2013; Alm, 2014; Nicosia et al., 2016). More research should follow and consider additional aspects such as the role of optimal delay, which can be very sensitive to the type of punishment (e.g. the optimal delay may be rather different for monetary fines than for imprisonment). With this in mind, we are confident that our study highlights the role of celerity in designing optimal sanctioning systems and points to fruitful avenues for future research.

Appendix A Theoretical analysis

In this appendix, we provide further details regarding the derivation of the behavioral implications outlined in Section 3.1. We present the formal derivation of those implications for discounted expected utility theory (DEU) and utility from anticipated emotions (UAE). In the standard discounted expected utility (DEU) model, optimal decisions do not depend on the timing of resolution of uncertainty. In the DEU model, a delay of punishment should decrease deterrence. The utility of not cheating (NC) is identical in all treatments and is given by

$$\text{DEU}(NC) = \frac{31}{32}4 + \frac{1}{32}10 \tag{1}$$

where we assume for convenience a linear utility function.[26] We restrict attention to a single block consisting of 6 periods, where cheating was possible in the first round of that block. Furthermore, we only consider the utility generated from the decision about cheating in the first period of such a block in all our analyses. The remaining utility components within a block are identical across treatments. In all treatments, detected cheaters are fined 10 ECU plus one round of suspension for a total expected fine of $F = 10 + \text{DEU}(NC)$, but the time at which the punishment occurs varies across treatments. In the baseline treatment, detected cheaters are punished directly in the next period. For a discount factor $\delta < 1$, the utility of cheating (C) amounts to

$$\text{DEU}(C, \text{IR-ND}) = 10 - \frac{1}{4}\delta F \tag{2}$$

as cheating is not possible in the next period. Compared to the baseline treatment, punishment is delayed by two further periods in *IR-D*. The same is true for *DR-ND*. As the timing of resolution of uncertainty is immaterial under DEU, we get

$$\text{DEU}(C, \text{IR-D}) = \text{DEU}(C, \text{DR-ND}) = 10 - \frac{1}{4}\delta^3 F. \tag{3}$$

Finally, we have

$$\text{DEU}(C, \text{DR-D}) = 10 - \frac{1}{4}\delta^5 F. \tag{4}$$

as punishment is delayed by a total of four periods in *DR-D*.

Since $\text{DEU}(C, \text{IR-ND}) < \text{DEU}(C, \text{IR-D}) = \text{DEU}(C, \text{DR-ND}) < \text{DEU}(C, \text{DR-D})$ where the utility of not cheating is independent of the treatments, we get the prediction:

[26] While risk aversion modeled by a concave utility function certainly influences the decision between cheating and not cheating, it does not imply differences between treatments.

Proposition 1. *Under DEU, increasing the delay of punishment decreases deterrence for a fixed timing of resolution of uncertainty.*

As a consequence, DEU predicts more violations in *IR-D* compared to *IR-ND* and more violations in *DR-D* compared to *DR-ND*. Since the timing of resolution of uncertainty does not affect behavior, following DEU violations in Treatments *IR-D* and *DR-ND* are identical.

Proposition 2. *Under DEU, the timing of resolution of uncertainty does not affect behavior, implying that violations in Treatments IR-D and DR-ND are identical.*

Proposition 3. *Since under DEU the timing of resolution of uncertainty does not change deterrence and increasing the delay of punishment decreases deterrence, we will have more violations in DR-ND than in IR-ND and more violations in DR-D than in IR-D.*

Following Loewenstein (1987), negative future outcomes can cause immediate disutility through negative anticipatory emotions such as fear, dread, or anxiety. DEU fails to take this into consideration. Suppose you were cheating in the baseline treatment. Then, you dread in the first period that you will be fined in the next one, i.e. you dread a loss of $F = 10 + \text{DEU}(NC)$. For a discount rate γ, which measures the degree to which current utility is influenced by anticipated emotions from consumption in the next period, the utility of cheating is given by

$$\text{UAE}(C, \text{IR-ND}) = 10 - \frac{1}{4}(\delta + \gamma)F \tag{5}$$

where UAE denotes utility with anticipated emotions. We now consider *IR-D* where there is a delay of punishment by two periods. Note that the utility from anticipation is discounted with discount factor δ. While the discounting effect in (3) increases utility compared to *IR-ND*, anticipation leads to decreasing utility as dread is now experienced in more than one period. More specifically, we get

$$\text{UAE}(C, \text{IR-D}) = 10 - \frac{1}{4}\delta^3 F - \frac{1}{4}(\gamma^3 + \delta\gamma^2 + \delta^2\gamma)F \tag{6}$$

Comparing (5) and (6), it may well be that the utility of cheating is lower in *IR-D* than in *IR-ND* if γ is sufficiently high. Since the utility of not cheating is identical across treatments, we get the opposite of Proposition 1.

Proposition 4. *If the effect of anticipation is sufficiently high, delaying punishment increases deterrence leading to less violations in IR-D compared to IR-ND and in DR-D compared to DR-ND.*

Anticipated emotions in the model of Loewenstein (1987) refers to future consumption under certainty. In Treatments *DR-ND* and *DR-D*, resolution of uncertainty is delayed which may alter anticipatory emotions. While in *IR-D* a detected cheater may feel dread in periods 1-3 due to anticipating the punishment in period 4, in *DR-ND* a cheater may experience the anxiety of being detected in the later investigation. Following Caplin and Leahy (2001), the anxiety experienced one period before resolution should depend on the probability of being detected and the size of the fine. As all these parameters are identical in Treatments *DR-ND* and *DR-D*, we simply use the term A to denote the anxiety of a cheater one period before resolution. We now introduce a third discount rate α, such that anxiety experienced t periods before resolution is given by $\alpha^t A$. This yields the following utility of cheating in *DR-ND*:

$$\text{UAE}(C, \text{DR-ND}) = 10 - \frac{1}{4}\delta^3 F - (\alpha + \delta\alpha^2)A - \frac{1}{4}\delta^2\gamma F \tag{7}$$

Typically, it is observed that people prefer early resolution of uncertainty for negative outcomes. In our model this is the case if

$$(\alpha^2 + \delta\alpha)A > \frac{1}{4}(\gamma^3 + \delta\gamma^2)F \tag{8}$$

and leads to the following hypothesis:

Proposition 5. *Delayed resolution of uncertainty increases deterrence leading to less violations in DR-ND compared to IR-D.*

If the resolution of uncertainty should be delayed to increase deterrence, punishment has to be delayed as it cannot precede the resolution of uncertainty. The combined effect of delayed resolution and delayed punishment can be grasped by comparing *DR-ND* to *IR-ND*. If both delaying punishment according to Proposition 4 and delaying resolution according to Proposition 5 increases deterrence, our UAE implies the following:

Proposition 6. *If delaying punishment increases deterrence due to dread and delayed resolution also increases deterrence due to anxiety, then the combined effect of delaying punishment and resolution results in less cheating and, therefore, less violations in DR-ND compared to IR-ND.*

Let us finally consider the utility of cheating in *DR-D*. Here we get

$$\text{UAE}(C, \text{DR-D}) = 10 - \frac{1}{4}\delta^5 F - \frac{1}{4}(\gamma^3 + \delta\gamma^2 + \delta^2\gamma^3 + \delta^3\gamma^2 + \delta^4\gamma)F \tag{9}$$

The cheater experiences anxiety prior to the resolution of uncertainty as in *DR-ND*, but there is also an extended period where he may experience dread due to delayed punishment. The second component is similar to the dread experienced in *IR-D*, additionally discounted as the experience starts two periods later. Assuming (8), a comparison of (9) and (6) reveals that the utility of cheating in *DR-D* will be smaller than that of cheating in *IR-D* under the conditions of Proposition 4. This leads to the following result:

Proposition 7. *If* (8) *holds and the effect of anticipation is sufficiently high* (γ *is large enough), then delayed resolution combined with delaying punishment results in less cheating leading to fewer violations in DR-D compared to IR-D and fewer violations in DR-D compared to IR-ND.*

Appendix B Instructions

General Instructions

Welcome to the Lab! You are about to participate in an experiment on decision-making. During the experiment you can earn money. The amount of money you will earn depends on your decisions during the session. The session consists of 28 rounds. Your earnings are determined by the sum of your earnings in each round. During the experiment all amounts will be presented in ECU (Experimental Currency Unit). At the end of the experiment all the ECU you have earned will be converted to Dollars as follows:

$$10 \text{ ECU} = 1\$$$

At the end of the experiment your total earnings, which is the amount you have earned during the session (converted to Dollars), will be privately paid to you in cash.

Additionally, there is a separate charity fund for each participant, which initially contains 250 ECU. Your decisions during the experiment will affect the final amount of ECU in your charity fund. At the end of this session, one participant will be randomly chosen and the final amount of ECU that remain in that participant's charity fund will be donated to Doctors Without Borders (for more information on the work and the mission of Doctors Without Borders please find enclosed the summary taken from their website). Therefore, if you are selected your decisions throughout the experiment would directly affect the amount of money that is actually donated. A copy of the receipt of the donations can be made available upon demand.

If there is something you do not understand or if you have any questions, now or at some point during the experiment, please raise your hand and remain seated. One of our colleagues will come to you and answer your question.

It is important that you read all instructions and explanations on the screen carefully before making your decision.

For the purpose of the experiment it is important that you do not talk or communicate in other ways with the other participants. Please turn off your cell phone and all other electronic devices.

After the actual experiment concludes, we will ask you to fill out a questionnaire. Please fill out the questionnaire carefully and truthfully. The whole experiment is completely anonymous and your answers cannot be traced back to you personally. Therefore, it is important that you fill out the questionnaire carefully even if you have already answered a similar questionnaire in another experiment in the past.

Figure B.1

On the next pages you will receive further information regarding the experiment and your decisions.

Experiment Instructions

In this experiment you have to make a series of decisions over 28 rounds. In each round the computer will draw a single card from a deck of cards. The deck contains the 32 cards shown in Figure B.1 below. Each card is drawn with the same probability.

There are two different types of rounds, type A and type B. This means any of the 28 rounds is either of type A or type B. For each round the round type is indicated on the computer screen at the top left corner. We detail the course of events for each type of round below.

Type A

In rounds of type A you have to play the "guessing game" where you have to guess which card was randomly drawn by selecting the corresponding card from the 32 cards shown on your screen (see Figure B.1 above). For a correct guess, that is if the card you have chosen is the exact same card drawn by the computer in suit and number, you will receive a payout of 10 ECU. For an incorrect guess, that is the computer has drawn a card different from the one you have chosen, you will receive a payout of 4 ECU.

Example:

1. Suppose your guess is "King Of Hearts," i.e. you have chosen "King Of Hearts" on the screen in Figure B.1, and the card actually drawn by the computer is "King Of Hearts". This means you guessed correctly and 10 ECU is added to your account.
2. Suppose your guess is "King Of Hearts" and the card actually drawn by the computer is "Queen Of Spades." This means you guessed incorrectly and 4 ECU is added to your account.
3. Suppose your guess is "King of Hearts" and the card actually drawn by the computer is "King of Clubs." This means you guess incorrectly and 4 ECU is added to your account.

Type B

In rounds of type B, you first have to decide whether you want to follow the rules of the guessing game or cheat in the guessing game. If you choose to cheat, the computer will guess the right card for you. In other words:

- If you decide to follow the rules, you can make your guess just as in rounds of type A with the same possible payouts, i.e. if you guess correctly you receive 10 ECU and if you guess incorrectly you receive 4 ECU.
- If you decide to cheat, you do not have to guess which card was drawn, but the computer will automatically choose the right card for you. This means when you choose to cheat you will always "guess" correctly and win 10 ECU. However, when you cheat, an amount of 50 ECU will be subtracted from the charity pool, i.e. every time you cheat the amount of money that will be donated to "Doctors Without Borders" is reduced by 50 ECU. This reduction in the charity fund will become a reality in the case that your decisions are randomly chosen to determine the ending amount in the charity fund at the conclusion of the experiment.

Any instance of cheating will be investigated. Cheating is detected with a probability of 25%. This means that one out of four times you will be detected. The investigation is completed in the same round. When the investigation is completed, you are informed whether your cheating was detected or not.

If your cheating was detected, you will be suspended for one round and you have to pay a fine of 10 ECU. When suspended you can neither cheat nor guess and will not earn any ECU in that round. Moreover, the fine of 10 ECU will be deducted from your account in the round you are suspended. Your suspension will be implemented in the first round after the investigation is completed and your cheating is detected (see the timeline in Figure B.2 below). This does not affect any other round except the round of your suspension.

There are no more than five type B rounds.

Examples:

Situation 1: Suppose we are in round 7 and this is a round of type A.

There are two possible payouts for this round, which depend on your guess:

1. You guessed correctly (your guess and the computers draw coincide). You earn 10 ECU in round 7.
2. You guessed incorrectly (your guess and the computers draw do not coincide). You earn 4 ECU in round 7.

Situation 2: Suppose we are in round 7 and this is a round of type B.

There are four possible payouts for this round, which depend on whether you cheat, are detected upon investigation, and, in case you did not cheat also on your guess:

1. You did not cheat and guessed correctly. You earn 10 ECU in round 7. There are no other consequences.
2. You did not cheat and guessed incorrectly. You earn 4 ECU in round 7. There are no other consequences.
3. You cheated and 10 ECU are added to your account in round 7. The investigation reveals in round 7 that you were not detected and do not face any further consequences.

   Because you cheated an amount of 50 ECU will be deducted from the charity pool if your decisions are chosen to determine the charity fund.

4. You cheated and 10 ECU are added to your account in round 7. The investigation reveals in round 7 that you were detected. You will be suspended for round 8, further you have to pay a fine of 10 ECU which will deducted from your account during your suspension in round 8. This means in round 8 you cannot make any decision, but the fine of 10 ECU will be deducted from your account.

   Because you cheated an amount of 50 ECU will be deducted from the charity pool if your decisions are chosen to determine the charity fund.

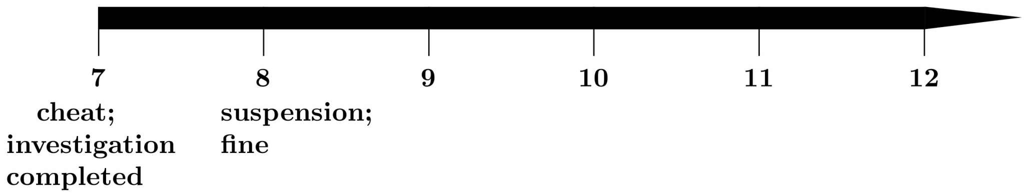


Figure B.2

Summary

We now summarize the course of action for a single round:

1. If you are suspended for this round a pop-up screen will inform you that you are not allowed to make any decisions in this period. Please wait for the next round to proceed.

2. In rounds of type A you have to play the guessing game (see Figure B.1).

3. In rounds of type B you have to decide whether you want to cheat or follow the rules. If you cheat the computer selects the right card for you. If you do not cheat you play the guessing game as in rounds of type A.

4. After you have made your decisions the outcome screen appears where you are informed of the outcome of this round and your payoff. If an investigation is completed you are informed about the outcome and consequences.

5. If you were detected for cheating, your punishment consists of a) losing the 10 ECU you won by engaging in elicit behavior, b) you have to pause for the next round (60 seconds waiting time), and c) you forgo the potential income of the round in which you are paused.

Comprehension Questions

Please answer the following comprehension questions. If you have any questions, please raise your hand and remain seated. Somebody will come to you and answer your question.

QUESTION 1: In rounds of type A, you have the option to cheat.
(Please circle the right answer.)

right wrong

QUESTION 2: In rounds of type B, you have the option to cheat.
(Please circle the right answer.)

right wrong

QUESTION 3: How many ECU are added to your account if you cheat in that round?

QUESTION 4: What is the probability that you are detected after you cheated?

QUESTION 4: What is your payout if you guess correctly/incorrectly in the guessing game?

QUESTION 5: Suppose you cheated in round 7. In which round is the investigation completed and you are informed whether you have been detected or not?
(Please circle the right answer.)

round 7 round 8

QUESTION 6: Suppose you cheated in round 7 and the completed investigation has revealed that you have been detected. In which round are you suspended and is the fine deducted from your account?
(Please circle the right answer.)

round 7 round 8

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