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## A Survey of Contributions from Law, Economics and Criminology

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**Abstract**

In three distinct disciplines, crime and punishment are studied experimentally: in empirical legal studies, in experimental economics, and an experimental criminology. These three disciplines have surprisingly little interaction. The current paper surveys the rich evidence, and discusses the methodological reasons for running experiments on these issues, the limitations of the method, and how they can be mitigated.

Keywords: crime, punishment, experiment, experimental economics, experimental criminology

JEL: A12, C91, C93, D03, H26, K14

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## 1. Why experiments?

Academia is a worthy topic for anthropological research. An anthropologist in search for a promising case study should pick experimental contributions to criminal law. If she first visits a few good law schools, she would probably be inclined to give up the project. In most countries, the doctrinal tribe has little contact with any serious empirics, and would probably claim that there is no such thing as experimental criminal law. Hopefully our anthropologist is not discouraged prematurely and follows the path towards the homestead of empirical legal scholars. Most of them will show her their shiny econometric armors. But there is a corner where a few experimentalists gather. If she stops by, the anthropologist might come to the conclusion that experimental criminal law might have a future, but is still very much a nascent enterprise. She would miss a lot. If she crossed the river that separates the law from the social sciences, she would find two large experimental communities working on crime and punishment. Yet she better had not tell one of those communities that she plans to visit the other. Because those two communities almost behave like enemies. One of these communities, experimental criminology, even has its own journal (*Journal of Experimental Criminology*). It garners substantial public money and is proud of its high standards. Pride is also pronounced among economists working on crime. They publish their papers in the best economics journals, they command a whole panoply of experimental methods, and they tend to believe that there is no serious (social) science outside economics. For an anthropologist, this state of affairs may not be all too surprising. Tribes have their culturally defined identities. Culture is by definition historically contingent. But for a criminal lawyer (broadly speaking, including prosecutors, judges, and legal scholars) who wants to benefit from experimental evidence, the scattered intellectual landscape is troublesome. This article is meant as a map of the landscape (for a related exercise see Horne and Rauhut 2013). It shall provide orientation: to work that is already available, and to authors and communities that will sure produce much more evidence on which criminal law can capitalize.

Why experiments? A typical experimental paper is a strange read for a doctrinal lawyer (for an introduction to experimental law and economics generally, see Arlen and Talley 2008). When she is first exposed to such a piece of scholarship, she will be inclined to skip the technicalities. Ultimately, they are of course important. In the long run, a lawyer interested in using quantitative evidence will have to become statistically literate (the relevant techniques are explained by Moffatt 2015). But for a first cut, the main results will do, and they are usually stated in plain English. But even if one puts all the math aside, a doctrinal lawyer will be puzzled by experimental papers extolling randomness. Why should one search for, or even purposefully create, a random shock? Isn't human life fraught with non-random determinants? Correct. But the name of the game is not description. A typical experimental paper is motivated with observations or political debates. They make the research question interesting. But the actual experiment is confined to testing a precise hypothesis about a cause and an effect. The typical design features a baseline and a treatment. Baseline and treatment are identical but for a single difference. This difference is the presence of the absence of the purported cause. The

typical hypothesis is not deterministic, but probabilistic. One does not claim that the presence of the cause makes a difference in each and every application. Rather one claims that an effect is more likely if the cause is present. This hypothesis is tested by comparing some outcome variable in the baseline and in the treatment. Statistical conventions define whether this difference is large enough to provisionally accept the hypothesis (or, more precisely, to not reject the null hypothesis that there is no effect). Provided baseline and treatment truly differ in no other respect, and participants have been randomly assigned to either the baseline or the treatment, a causal effect is shown (for background see Angrist and Pischke 2008, Angrist and Pischke 2010).

Why that much rigor? Wouldn't it be enough to watch out for the presence or the absence of the cause in the field, and compare the outcomes? Wouldn't that be a more powerful approach, since one avoids randomization and thereby artificiality? Not so rarely, and in particular when one wants to study crime, studying a purposefully created analogous situation is the only way to generate data at all, since one suspects criminals to successfully hide their activities. But even if observational data are available, there are three big problems. The practically most troublesome concern are omitted variables. Let us illustrate the problem with a question from criminal law. Do more severe sanctions reduce crime? Seemingly, there is plenty of evidence to study the question. All over the world, criminal codes do not treat all crimes alike. The threat with sanctions for petty crime tends to be considerably more lenient than for felonies. Most countries register convictions. Could one not simply count the number of convictions per crime, and correlate them with the mean sanction threat? The problem is obvious: quite likely the number of those tempted to kill others is much smaller than the number of those tempted to shoplift. Consequently, if one does find a statistically significant difference, it could be spurious. It could result from a difference in populations. On the other hand, the police exert much more effort to catch a murderer than to catch a shoplifter. If one does not find a statistically significant difference, this could just say something about the police, not about deterrence.

The second problem can be illustrated with the same example. Most legal orders leave the trial court little room for sentencing murder, while the band between the minimum and the maximum sentence for shoplifting is much wider. If one just compares the mean permissible sentence for both crimes, one likely misses that some potential offenders are more deterred by the minimum sentence, others by the maximum sentence, and yet others by information about the empirical average. Due to the imperfect determination of sentences by their code, the explanatory variable (the threat with punishment) is only measured with error. The frequency of convictions for either crime may overestimate or underestimate the true effect of severity.

To illustrate the third problem, let's assume we have data about convictions for shoplifting in two countries. One country threatens repeat shoplifters with a prison sentence, while the other country never goes beyond a fine. Why not just compare the two countries? Because it could be that the first country has harsher sanctions for a reason. Policymakers might have reacted to the fact that a sizeable fraction of the population is prone to shoplifting. The causal arrow

would point into the opposite direction. It is not the threat with sanctions that determines the frequency of crime; it is the frequency of crime that determines the threat with sanctions.

Random assignment is the technique to solve all three problems at a time. It precisely is the artificiality of the manipulation that makes identification possible. Artificiality is the price one has to pay if one wants to be sure about cause and effect. One should not be sanguine about this statement. The price can be substantial. Instead of studying what one is ultimately interested in, one studies an artificial, analogous situation. If one runs an experiment, one knows in advance that one will face a problem of external validity. Extrapolating from the experiment to the problem in real life that motivated the project always requires a leap of faith (more from Engel 2013). Ideally, one can rely on triangulation, i.e. on converging evidence resulting from the use of complementary methodologies (cf. Jick 1979). There is observational data that suggest the hypothesized effect. There are no glaring identification problems with the observational data. But there is still some residual doubt. This residual doubt is removed by a matching experiment.

In this chapter, I bracket external validity concerns, and focus on the experimental evidence that is already available. I do so with the intention to convince criminal lawyers that a lot is already out there on which they can capitalize. I also want to point them to the scientific communities they might approach with new research questions. Going back to my introductory anthropological metaphor, they should not expect that those foreign tribes will always serve them as desired. They have their own rites, mores and styles. But all of them are fascinated by a powerful, novel research question. It is not unlikely that doctrinal lawyers have such research questions for them, and will then be able to rally their support.

## 2. A Variety of Experimental Methods

Much of the acrimony in the conflict between the different experimental tribes working on crime and punishment originates in almost religious belief in methodology. As with all religions, the articles of faith can be rationalized. But there is no good reason to shun evidence that has been generated using a different methodology. All methods have their advantages and disadvantages (Zeisel 1973). If one believes, for a concrete question of criminal law, that one of the differences is important enough, triangulation would again be best policy. One should check, with a new experiment, whether results are robust to methodology (Rauhut and Winter 2012). And not so rarely, one faces the choice between no rigorous evidence at all, and the one experimental method that happens to be implementable. This does not remove its limitations. They should be acknowledged. But normally some evidence is better than no evidence.

To the extent that criminology is experimental, the predominant method is the randomized field trial. A characteristic example is the experiment by Ready and Young (2015). 100 police officers in Mesa, Arizona were split into two groups of equal size. Efforts were made to make these two groups comparable based on age, race and gender. Half of them were equipped with

action video cams. 5 days per month were randomly selected. On those days, officers filled out field contact reports. Over the duration of 50 observation days, 3698 contact forms were collected. The main dependent variable was officer behavior. Differences were pronounced. Officers equipped with a camera were much more likely to issue a citation for violating an ordinance (41.5% rather than 18.4%), and they were more likely to initiate the encounter with the citizen (50.3% rather than 36.7%). By contrast they were more reluctant to conduct a stop and frisk (7.3% rather than 17.1%) or to arrest a suspect on a felony and misdemeanor charge (20.4% rather than 27.3%). All differences were statistically significant.

The predominant method in experimental economics is the lab experiment. A characteristic example is the experiment by Abbink, Dasgupta et al. (2014). Corruption is a vexing social evil. It is particularly troublesome if a public official abuses sovereign powers to extract a bribe from citizens whom she is supposed to govern. The authors translate the interaction into a game where one participant may ask the other for a side payment. She may refuse to pay, "pay quietly" or "pay and report". If she refuses to pay, both players receive a lower payoff. If she pays quietly, with 95% probability the first player receives a higher payoff plus the requested amount, and the second player receives an equally high payoff minus the requested amount. With 5% probability the computer sanctions the interaction. If she pays and reports, the probability of computer intervention goes up to 40%. The treatment manipulation is the effect of sanctioning on the second player's payoff. In the symmetry treatment, both players receive the same medium payoff. By contrast in the asymmetric treatment, the second player keeps the benefit from the corrupt deal. The authors had 360 students play this game in four different Indian universities. They find a clear difference in the second player's choices. In the asymmetric treatment, 59% pay and report, while only 25% do in the symmetric treatment. First players react. While 38% of them demand a bribe in the symmetry treatment, only 24% do in the asymmetric treatment. The difference in reporting is highly significant. But with 8% probability the difference in demanding a bribe is not systematic.

Both experiments test a policy. In both cases, the method is wisely chosen. If one wants to predict the effects of an intervention on the behavior of police officers, it is helpful to study police officers. If one wants to predict the effects of an intervention on behavior that those engaging in have every interest to keep secret, it is helpful to study an artificial analogous situation. Both experiments have to pay a price for their advantage. The first experiment has made an effort to make the treated and the untreated group comparable. But it cannot exclude that group composition had been different in another dimension that had not been on the screen of the designers (cf. Berk, Smyth et al. 1988). What the experiment observes may be driven by the fact that both groups of policemen knew they were participating in an experiment (a so-called Hawthorne effect, McCarney, Warner et al. 2007). The second experiment may overestimate the incidence of corruption because stakes are low (cf. Fehr, Tougareva et al. 2014) or because participants might feel the experimenter has encouraged them to engage in bribery (cf. Zizzo 2010). There is no such thing as a perfect experiment. But experimental evidence showing that a policy works raises the bar for those who plead against the policy.

The experimental finding rarely settles the policy debate. But it makes an important contribution.

A hybrid between both methods is a "lab in the field" experiment. An illustration is Henrich, McElreath et al. (2006). They have participants play a dictator game. The dictator receives an endowment, and may share any fraction with a passive second participant. The twist is a third party who observes the game, and has the possibility to give up 20% of her own endowment to reduce the first player's payoff by 30%. The authors play this game in 15 different societies all over the world, running from nomadic foragers through inhabitants of big cities. Everywhere there is some punishment. But in some contexts (e.g. the Tsimane) punishment is rare even if the first participant has kept the entire endowment. In other contexts (e.g. the Maragoli), any division of the endowment short of the equal split attracts considerable punishment. Societies where punishment is frequent are also societies where altruism is prevalent. This suggests that retribution sentiments have coevolved with caring attitudes.

There are research questions where randomization is just not possible. The following is an illustration: Legal orders have to strike a balance between respect for the particularities of the case and consistency across cases in meting out sanctions. A compromise solution are sentencing guidelines. The judge may set it aside, but is supposed to take it into consideration. This is the solution adopted for instance by Maryland. Do such recommendations have a discernible effect on judicial decision-making? How strong is the effect? Of course, identification would be clean if one could randomly assign some (otherwise identical) judges to sentencing guidelines, while others are not. But judges decide about defendants' lives. This will make it difficult for legislators to condone the experiment. Bushway, Owens et al. (2012) have found a clever workaround. They have found out that a substantial fraction of the recommendations that Maryland judges received were actually wrong. The system was complicated enough to produce errors. Errors were not systematic, and therefore created quasi random variation. Unsurprisingly, sentences are positively correlated with the accurate recommendation. But if the recommendation was erroneously high, sentences for drug offences and for violent offences were more severe. And if the recommendation was erroneously low, sentences for these offences were less severe. For drug offences the distortion had about the same size in both directions. For violent offences, the downward distortion was about twice as large as the upward distortion. It perfectly neutralizes the effect of the accurate recommendation. All other distortions have about half the magnitude of the accurate recommendation. The authors conclude that recommendations do have an effect, but that they do not completely determine sentences. The example illustrates the power of "natural experiments". But the researcher is at the mercy of naturally occurring variance, and should always be wary of overlooked systematic drivers of the difference (more from Angrist 2006, Berk, Barnes et al. 2010, Weisburd 2010).

Experiments are tools for identifying causal effects. This purpose is thwarted if the experiment suffers from a confound. One cannot exclude that the treatment difference is driven by a cause that differs from the one the experiment is meant to study. This is why economic exper-

iments normally remove all frames, and expose participants to a naked incentive structure. Yet ironically this quest for cleanliness can backfire. In an attempt at making sense of the decision problem, participants frame the game themselves, in ways usually not under the control of the experimenter (Engel and Rand 2014). And sometimes the (legal) research question is no longer meaningful if one removes the characteristic context. On the other hand, randomly exposing participants to the actual situation one wants to understand may not be permissible, or feasible. In such situations, a way out is randomly exposing participants to vignettes that differ by the feature of the context one wants to study. This method is prevalent in psychology (more on differences between psychological and economic experiments from Hertwig and Ortmann 2001). It has been abundantly used in jury research (see e.g. Hastie, Penrod et al. 1983, Devine, Clayton et al. 2001, Breau and Brook 2007). An example using the tool for understanding punishment is Montag and Sobek (2014). Should similar offenses face similar fines? Or should the sanction reflect the sensitivity of the culprit for the intervention? Should richer persons hence have to pay higher fines? And should courts be more hesitant to inflict prison sentences on offenders from well to do backgrounds since arguably they would suffer more dearly? One determinant of this policy choice may be the attitudes of citizens. In order to measure these attitudes, the authors presented 215 students with two scenarios, and asked them which sanction they deemed appropriate. If a prison sentence was at stake, there was no difference between poor and rich defendants, while there was a clear and significant difference for fines.

## 3. Crime

### a) Theft

The quintessential crime is theft. Studying theft in the lab is straightforward. The experimenter endows one participant with an electronic entitlement that is exchanged against real money at the end of the experiment (skirting the doctrinal question whether this constitutes theft or computer fraud). Another participant has the possibility to take some or all of this endowment (Falk and Fischbacher 2002, Gravert 2013, Harbaugh, Mocan et al. 2013, Pecenka and Kundhlande 2013, Feess, Schramm et al. 2014, Engel and Nagin 2015, Khadjavi 2015, Fleming, Parravano et al. 2016). Participants even do so if stealing reduces expected profit (Schildberg-Hörisch and Strassmair 2012, Engel and Nagin 2015). If there is more to steal, theft becomes more frequent (Harbaugh, Mocan et al. 2013). In South Africa, black participants steal more from other black participants than from white participants (Pecenka and Kundhlande 2013). If participants learn how much others have stolen, stealing increases (Falk and Fischbacher 2002, Engel and Nagin 2015).

Substantial theft is also observed under more naturalistic conditions. Gravert (2013) pays participants with toy coins. She instructs them to keep the coins they are entitled to, and to put the remainder into an envelope they are asked to seal. The manipulation is what they receive

the coins for: either a random dice roll, or a challenging real effort task. The latter participants embezzle a larger amount. The author explains the finding with the fact that exerting effort reduces the moral cost of stealing. Castillo, Petrie et al. (2014) send letters from the US to Peru. 14.8% of them are lost on the way to the recipient, compared to a loss of 21.4% if a slight bump signals that the envelope might contain something valuable. The only criminological stealing experiment I have been able to trace uses a similar design (Farrington and Knight 1980). Participants pick up a letter that was apparently lost on the street. It contains a £ 1 note, and an indication of the intended recipient's gender. 39.3% of all those letters are not returned, compared with 10.7% if the letters do not contain money. If the purported recipient is male, 50% of the letters with money are not returned, while only 28.6% are not returned if the recipient is female. Belot and Schröder (2013) ask participants to classify Euro coins by the country in which they have been minted. They are paid by the number of coins they report to have classified. Almost no one takes the opportunity to steal coins from the experimenter (while there is some misreporting).

In a vignette study, Hardy, Krawczyk et al. (2013) aim at understanding why many users of online platforms do not seem to have much reticence to appropriate content. They ask participants to rate the acceptability of appropriating a valuable commodity in multiple scenarios. The two factors that have the highest explanatory power are the electronic (rather than physical) nature of a good, and the fact that appropriating content does not deprive the current owner of its use.

### b) Fraud

While the papers do not use the term from criminal law, many experiments test fraud. Participants are not perfectly observed, and can react by selfish misreporting (for a survey of this literature see Rosenbaum, Billinger et al. 2014). A typical design is Fischbacher and Föllmi-Heusi (2013). Participants receive a standard die. They roll it in private. The experimenter remunerates them for the number they report, by the equivalent number of CHF; if a participant reports 6, she receives nothing. If participants do not cheat, all 6 options should be reported with the same frequency. Actually only 6.4% report 6, while 35.0% report 5, and 27.2% report 4 (a similar finding is Lewis, Bardis et al. 2012). The authors conclude that there is dishonest reporting, but that many participants do not lie maximally. This behavior has been attributed to an attempt at maintaining a positive self-image (Shalvi, Dana et al. 2011). In a simple coin-flipping task, it has been found that unobserved participants heavily overreport the winning side of the coin. 85.3% of children of age 5 – 15 do (Bucciol and Piovesan 2011). But Abeler, Becker et al. (2014) do not replicate the finding in a representative sample of the German population. Pascual-Ezama, Fosgaard et al. (2015) could only replicate the effect in Denmark, Finland, Germany, the Netherlands, Spain, the US and the UK, but not in Austria, Belgium, Columbia, Greece, India, Indonesia, Italy, Japan and Turkey. Hugh-Jones (2015) also reports substantial differences in cheating across countries, but is only unable to replicate the effect in the UK and South Africa.

Cheating is more pronounced if there is more to gain from cheating (Conrads, Irlenbusch et al. 2014); if the own payoff with no cheating compares unfavorably to the payoff of other participants (John, Loewenstein et al. 2012); if there is a risk of being outcompeted by others (Schwieren and Weichselbaumer 2010); if cheating holds the promise of higher social status (Pascual-Ezama, Prelec et al. 2013); if the benefit from cheating is shared with another participant (Wiltermuth 2011); if participants observe that others cheat (Fosgaard, Hansen et al. 2013); if participants do not feel well treated (because they had received little from a dictator) (Houser, Vetter et al. 2012); if an incentive scheme is perceived as unfair, e.g. because it does not reflect performance (Gill, Prowse et al. 2013); if participants had previously done something moral (Ploner and Regner 2013, Clot, Grolleau et al. 2014). Participants cheat less if they have to commit to cheating before the opportunity presents itself (Hao and Houser 2013, Jiang 2013).

The only pertinent experiment in criminology I have been able to trace gave participants a chance to cheat on an incentivized quiz. Participants with a stronger present orientation, and those more liable to self-serving bias, were more prone to cheating (Nagin and Pogarsky 2003).

Cheating has also been documented in naturalistic settings. Taxi drivers in Athens take patrons on longer detours if they ostensibly lack knowledge of the city, or of the local tariff system (Balafoutas, Beck et al. 2013). Students are more likely to cheat than pupils or employees (Djawadi and Fahr 2015). Individuals who dodge fares also cheat more in the lab (Dai, Galeotti et al. 2016).

### c) Tax Evasion

Tax evasion is notoriously difficult to observe in the field. Also self-report data, and classifications by tax officers, do not significantly correlate with choices elicited in the lab (Elffers, Robben et al. 1992). These observations have spurred a very active experimental literature (for (partial) surveys see Torgler 2002, Alm and Jacobson 2007, Blackwell 2007, Torgler 2007, Alm 2012, Hallsworth 2014, Pickhardt and Prinz 2014, Slemrod 2016). The design of the typical experiment is straightforward: participants are given the opportunity to earn money, e.g. in a real effort task. Income is liable to tax, but participants have the opportunity to underreport. Regularly, a substantial fraction of participants do (e.g. Doerrenberg and Duncan 2014). Interest is in the determinants of this decision.

Tax evasion increases in income (Becker, Büchner et al. 1987, Anderhub, Giese et al. 2001); in the tax rate (Alm, Jackson et al. 1992, Kleven, Knudsen et al. 2011); (but see Anderhub, Giese et al. 2001), and especially in reaction to an increase in the tax rate (Bernasconi, Corazzini et al. 2014); if the tax regime changes from proportional to progressive tax (Heinemann and Kocher 2013); in the cost of effort (Bühren and Kundt 2014).

Tax evasion decreases the more public goods participants receive "in exchange" for their taxes (Alm, Jackson et al. 1992, Alm, McClelland et al. 1992), especially in terms of transfer income (Becker, Büchner et al. 1987). It also decreases if they know that tax income will be used to support other participants (Bosco and Mittone 1997), or for a worthy cause, like research or a well-regarded charity (Doerrenberg 2015). But Güth, Strauß et al. (2002) do not find that tax evasion reacts to induced changes in the effectiveness of a state pension system. There is no significant difference between the evasion of earned and assigned income (Durham, Manly et al. 2014).

Tax evasion is influenced by fairness considerations, and equality in particular. Tax evasion increases if, by the design of the experiment, others earn more (Doerrenberg and Duncan 2013, Bazart and Bonein 2014); if others underreport income (Wakolbinger and Haigner 2009, Bazart and Bonein 2014, Blaufus, Bob et al. 2014, Lefebvre, Pestieau et al. 2015); if a tax (Spicer and Becker 1980) or a tax reform is to the disadvantage of the participant (Heinemann and Kocher 2013); if lower income is taxed more heavily than higher income (Castro and Rizzo 2014); if participants compete with others who might evade taxes (Casagrande, Di Cagno et al. 2015). However Fortin, Lacroix et al. (2007) find that participants evade less taxes the more their peers evade, and vice versa.

The effect of individual morality and social norms is debated. Some experiments do not find a significant effect (Alm, McClelland et al. 1992), or find that it is only present in interaction with explicit sanctions for underreporting (Alm, Sanchez et al. 1995). But choices in an unframed gamble do not predict tax evasion when there is some risk of audit (Baldry 1986). When framing the payment as a tax, there is more evasion than with a neutral frame (Jacobsen and Piovesan 2014).

Tax evasion increases if participants construct the choice as a gain, rather than a loss, as when overreporting deductible expenses (Fochmann and Wolf 2015). Participants who know they do still have to pay past taxes evade more new taxes (Robben, Webley et al. 1990). Tax evasion is reduced for individuals scoring high on sympathy, and for subjects whose empathy has been primed (Christian and Alm 2014). Tax evasion is correlated with high arousal (Coricelli, Joffily et al. 2010) and psychic stress measured by heart beat variability (Dulleck, Fooken et al. 2016).

The fact that two participants must collude to make tax evasion practical does not affect the rate of tax evasion (Abraham, Lorek et al. 2015). Tax evasion may also result from the fact that tax law is complicated (Kosonen and Ropponen 2015).

Gërxhani and Schram (2006) give participants a choice between two options for making money. One is more profitable in expectation, but tax evasion is impossible. The other is a little less profitable. But audit probabilities are smaller, so that a risk neutral individual prefers the latter. They run the experiment in Holland and in Albania. 79% of Dutch, and 61% of Albanian university students seize the opportunity and pick the second option.

## d) Corruption

Corruption is as difficult to study in the field as is tax evasion, which motivates studying corruption in the lab (for surveys see Dusek, Ortmann et al. 2005, Abbink and Serra 2012, Lambsdorff 2012, Bobkova and Egbert 2013). A typical experimental implementation is Abbink, Irlenbusch et al. (2002): A first participant decides whether she wants to transfer a certain amount (the bribe) to a second participant. If she does not, the game is over and both participants receive modest payoffs. The second participant may accept or refuse the payment. If she accepts, she may grant the desired favor, or just cash in the bribe. While the favor increases the payoff of the briber, it hurts a passive outsider.

Interest is again in the determinants of the decision to offer and to accept a bribe. The fact that bribery reduces, rather than enhances, welfare does not affect the frequency of bribery offers, nor of their acceptance (Cameron, Chaudhuri et al. 2009). If it is known that public officials are likely to accept bribes, more bribes are offered (Bilotkach 2005). There occur also more bribe offers if participants are induced to believe that bribery is common in their social context (Corbacho, Gingerich et al. 2016). If an intermediary communicates information about the lowest bribe that the official will accept, more bribes are offered (Drugov, Hamman et al. 2014). If a principal has paid the official a fair wage for exercising discretion in her interest, this reduces her willingness to act corruptly (Jacquemet 2005).

If the situation is explicitly framed as corruption, bribery becomes more frequent in Barr, Lindelow et al. (2009), less frequent in Barr and Serra (2009), and remains without a significant difference in Abbink and Hennig-Schmidt (2006), Büchner, Freytag et al. (2008). If granting the desired favor hurts passive outsiders, this reduces the acceptance in Barr and Serra (2009), but does not have an effect in Abbink, Irlenbusch et al. (2002), Castro (2006), and only increases variability in Büchner, Freytag et al. (2008).

The effect of wages for the official differs across experiments. Azfar and Nelson Jr (2007), Van Veldhuizen (2013) find less corruption if officials earn more money. Armantier and Boly (2011) find that higher wages reduce the willingness to accept a bribe. But if officials accept the bribe, they are more likely to grant the favor. In Abbink (2002) bribery is not reduced if bribers and officials earn symmetric wages (also see Barr, Lindelow et al. 2009).

Female participants are less likely to engage in corrupt activities (Rivas 2013); (also see Lambsdorff and Frank 2011). In Indonesia, public officials are less likely to accept a bribe than students (Alatas, Cameron et al. 2009b). In Australia, women are less likely to offer a bribe, but not in India, Indonesia and Singapore (Alatas, Cameron et al. 2009a). Undergraduates studying in the UK and coming from countries with a high degree of reported corruption are more likely to act corruptly in the lab. But the effect does not hold for graduate students, and declines in the time spent in the UK (Barr and Serra 2010). In India, more bribes are offered and accepted than in Australia and Singapore (Cameron, Chaudhuri et al. 2009).

Corruption has also been tested in naturalistic settings. Graders have been offered a bribe for grading an exam. They respond more favorably to larger bribes. If they are paid better, this reduces their willingness to accept a bribe. But if they do, they are more likely to grant the favor (Armantier and Boly 2011). Participants who report that they have engaged in corrupt acts in real life are also more likely to do so in the lab (Campos-Ortiz 2011).

A corrupt deal is inherently instable. If the official takes the bribe, but does not deliver the favor, the briber cannot take her to court. Yet if the design of the experiment makes this possible, punishing sentiments kick in. A briber who has been let down takes revenge, despite the fact that this is costly for herself (Engel, Goerg et al. 2012). This effect is more pronounced if the bribe is openly flagged out as corruption, rather than a "gift" (Lambsdorff and Frank 2010), and it is more pronounced in men (Lambsdorff and Frank 2011).

The standard experimental paradigm is collusive bribery. The real world analogue is a citizen approaching a public official and proposing a side-payment in exchange for the official exercising discretion in her favor, or openly breaking the law. Recently, the literature has also become interested in harassment bribery. It requires a change in the structure of the game. The official moves first, and "offers" to behave as required by law if the citizen gives her a side-payment (Abbink, Dasgupta et al. 2014). Such behavior becomes less frequent if the interaction is framed as bribery (Banerjee 2016). It becomes more frequent if officials compete with each other (Ryvkin and Serra 2015). It is substantially reduced if the private person is not punished (Abbink, Dasgupta et al. 2014).

## 4. Intervention

Most experimental research on the determinants of criminality comes from experimental economics. There is also a solid body of economic experiments on intervention, and on punishment in particular (for a meta-study see Balliet, Mulder et al. 2011); (specifically on attempts at curbing tax evasion see Hallsworth 2014, Slemrod 2016). But this is also a topic rich in criminology experiments (for surveys and meta-studies see Farrington and Welsh 2005, Farrington and Welsh 2006, Petrosino, Kiff et al. 2006, Bruinsma and Weisburd 2007, Farrington and MacKenzie 2013).

### a) Deterrence

Many experiments have, explicitly or implicitly, tested the deterrence hypothesis: threatening a person with a sufficiently severe sanction that is sufficiently likely to be meted out reduces theft (Engel and Nagin 2015), fraud (Nagin and Pogarsky 2003), tax evasion (Friedland, Maital et al. 1978, Alm, Jackson et al. 1992), and corruption (Abbink, Irlenbusch et al. 2002, Azfar and Nelson Jr 2007). Deterrence effects have also been demonstrated in the field. Drago, Galbiati et al. (2009) exploit the fact that Italian Parliament on August 1, 2006 unex-

pectedly released 22,000 prisoners. This effectively reduced prison sentences, the more the longer the inmate would otherwise have had to do time. From crime record data they estimate that one more month in prison reduces the probability of recidivism by .16%. The Danish tax authorities randomly audited 20,000 tax filers, while they left another 20,000 unaudited. This had a small, but significant effect on filing self-reported income, as did letters that threatened individuals with audit in the next period (Kleven, Knudsen et al. 2011). Increasing the probability of audit reduced misrepresentation of cost in Indonesian village road projects (Olken 2007). Informing tax payers in Minnesota that their cases would be "closely examined" increased tax payments (Slemrod, Blumenthal et al. 2001), as did announcing additional monitoring on the VAT payments of Chilean firms. Yet the effect was less pronounced if the transaction in question automatically left a paper trail (and hence was more liable to scrutiny in the first place) (Pomeranz 2015). Households who have received a letter threatening them with audit and fining are more likely to register their TV sets, and then pay the legal fee (Fellner, Sausgruber et al. 2013). However the fact that new firms are visited by a tax collector does not reduce tax evasion in Austria (Gangl, Torgler et al. 2014), while such visits have proven effective with individual taxpayers in Columbia (Ortega and Scartascini 2015). In an attempt at curbing traffic fatalities, the then governor of Connecticut imposed a "crackdown on speeding". First violations led to a 30-day suspension of the traffic license, second violations to a 60-day suspension, and third violations to a withdrawal of the license. Time series analysis suggests that this increase in the severity of the sanction indeed led to the intended effect (Campbell and Ross 1968, Glass 1968). Khadjavi (2015) tests prison inmates on a game with the threat of sanctions, and finds that they react to the threat, but less so than students. Surprisingly, however, Schildberg-Hörisch and Strassmair (2012) find an *increase* in stealing if the expected value of the sanction increases, as long as the expected value of stealing remains positive.

Several lab experiments have compared the effect of increases in the certainty with increases in the severity of punishment. Nagin and Pogarsky (2003) find a certainty effect, but no severity effect. Harbaugh, Mocan et al. (2013) find certainty and severity effects of about the same magnitude.[1] Engel and Nagin (2015) find a more pronounced severity effect for risk averse participants. This also holds for risk-seeking participants, except if the expected value of stealing is negative (cf. on the effect of risk preferences Beck, Davis et al. 1991, Magessi and Antunes 2015). Friesen (2012) finds a complete severity effect. Feess, Schramm et al. (2014) replace certainty by the noise ratio with which another participant in the role of a judge correctly observes stealing. Decreasing this noise ratio has a much smaller effect than increasing severity. In a repeated game, participants react to the experience of having been punished by shifting behavior closer to the norm (Mittone 2006).

[1] They do not test for the difference. In the regression of Table 5 model 1, the effect of a 1% increase in certainty is estimated to reduce the propensity to steal by .003%. The maximum fine is 5.7 units. A one unit increase in the size of the fine is estimated to reduce propensity to steal by .058%.

In the field, the probability of audit, and hence the certainty of punishment, tends to be endogenous. Auditors for instance focus on contexts where the probability of tax evasion is high. A plausible signal of such a contexts is the number of tax payers reporting low income. This constitutes a strategic game among tax payers. If the number of others who could underreport increases, tax evasion decreases (Tan and Yim 2014); (also see Casagrande, Di Cagno et al. 2015). If the law makes this possible, individuals are likely to strategically manipulate the probability of audit. In Spain, tax authorities made it known that income over €6 Mio was substantially more likely to be audited. In reaction the frequency of reports below the threshold jumped up (Almunia and Lopez Rodriguez 2014). In reality, tax evaders and auditors play a strategic game. As a group, both sides would be better off if the evaders invested less in concealment, and auditors invested less in detection. Yet in the lab, this socially beneficial outcome is less likely the higher the tax rate, while the severity of punishment in the case of detection does not increase the welfare loss from this strategic game (Bayer and Sutter 2009). The detection game becomes more complicated if the design of the experiment makes it possible for auditors to take bribes. In the lab, the frequency of corrupt offers increases once it becomes known that bribes have been accepted, but auditors' actions are unaffected (Bilotkach 2005). The severity of punishment is endogenous as well. If fines increase, auditors (partly) offset the effect by decreasing their efforts, and hence certainty (Rauhut 2009, Rauhut and Junker 2009). High severity, and high uncertainty make authorities more hesitant to punish (Feess, Schramm et al. 2014). The incidence of crime increases if the individual can bribe an official who has power to sanction her (Castro 2006).

Government audit can be supplemented or replaced by private whistleblowing. In a field experiment with village road projects in Indonesia, this possibility did not reduce fraud (Olken 2007). In the lab, pledging a reward for whistleblowing had a much stronger effect if the parties did not interact repeatedly (Wu and Abbink 2013). In an antitrust game, rewards for whistleblowing were more effective than leniency (Bigoni, Fridolfsson et al. 2012). If bystanders have the possibility to report corruption to authorities that have power to intervene, this has a strong deterrent effect (Serra 2012). In a bribery context, legal orders may incentivize the briber by making her punishment more lenient. This helps deter harassment bribes (Abbink, Dasgupta et al. 2014), but increases the likelihood of collusive bribes (because the low punishment provides bribers with a psychologically credible threat to enforce the corrupt deal) (Engel, Goerg et al. 2012).

Simple deterrence models assume that the threat with losing money is the only reason why people desist from committing profitable crime (the classic model is Becker 1968). A rich experimental literature demonstrates that this view is too narrow. Individuals also care about losing reputation, which is why shaming is a deterrent, but Casagrande, Di Cagno et al. (2015) only find a conditional effect. In the field experiment by Perez-Truglia and Troiano (2015), shaming only works for tax evaders with small debt. Pate and Hamilton (1992) find an interesting interaction between shame and social status. If the police arrest men who beat their spouses, this deters repetition in employed offenders, but increases repetition in unemployed

offenders. Social esteem and self esteem are also reasons why rewarding those who do comply (in the concrete case: with the legal obligation to pay a local church tax) proves effective (Dwenger, Kleven et al. 2016).

The evidence on the effect of social norms is mixed (Bosco and Mittone 1997, Torgler 2002). Alm, McClelland et al. (1992) do not find that people desist from tax evasion simply because they consider it to be wrong. But Alm, Sanchez et al. (1995) find less tax evasion in the US than in Spain, especially if the probability of being caught is small (similar results are reported by Cummings, Martinez-Vazquez et al. 2009). They explain the difference with differently pronounced civic virtues. Baldry (1986) finds less violations if he frames the equivalent game as paying taxes, rather than a lottery. Beams, Brown et al. (2003) significantly explain the propensity to engage in insider trading with low guilt aversion.

Other behavioral effects also matter for the effectiveness of deterrence. Tax payers overweight the small probability of audit (Alm, McClelland et al. 1992). Offenders suffer from the illusion that they will not be caught (Nagin and Pogarsky 2003). They are subject to self-control problems, and neglect the risk of punishment (Nagin and Pogarsky 2003). The (extrinsic) threat with punishment may crowd out the intrinsic motivation not to commit crime (Schulze and Frank 2003), (but see Engel 2014). The threat with punishment may not be powerful enough since potential offenders do not want to fall behind their peers, who might gain a competitive advantage by committing crime (Songchoo and Suriya 2012). One important behavioral effect results from the perceived legitimacy of the enforced rule (Beams, Brown et al. 2003, Cullis, Jones et al. 2012, Castro and Rizzo 2014, Besley, Jensen et al. 2015), of the procedure that has led to the adoption of the rule (Feld and Tyran 2002, Azfar and Nelson Jr 2007, DeAngelo and Charness 2012), and of the sanction (Montag and Sobek 2014): the higher the legitimacy, the less the rule is violated.

There is a rich literature in experimental economics that studies what is usually called peer punishment. A typical design is Fehr and Gächter (2000): Participants are randomly assigned to groups of four. Each group knows that they will repeatedly be exposed to the same social dilemma. Each participant is individually best off if she keeps her endowment for herself. Yet the whole group is best off if all of them contribute their entire endowments to a joint project, the public good. This game has been tested very often. A standard pattern is some degree of cooperation in the beginning, but a gradual decay of contributions over time (Zelmer 2003, Chaudhuri 2011). If, however, participants are given the opportunity to punish other group members, at a cost for themselves, cooperation stabilizes at a fairly high level (see the meta-study by Balliet, Mulder et al. 2011). The effect has been shown in many cultures across the world (Henrich, McElreath et al. 2006, Cameron, Chaudhuri et al. 2009).

This design of course differs from criminal law sanctions in multiple respects: punishment is not centralized and entrusted to some authority (but see Engel 2014); there is no explicit norm that participants violate by not contributing to the joint project (but see Engel and Kurschilgen 2015); there is no penal code, so that participants ex ante have no information about the sever-

ity of punishment; the punishment institution has not been introduced by a legitimate procedure (but see Gürerk, Irlenbusch et al. 2006); there are no safeguards against the abuse of punishment power. Nonetheless this literature is quite informative for criminal law.

Why do participants punish if this is costly? One might think that they invest in a profitable future. But why not let other group members bear the cost of disciplining free riders (Yamagishi 1986, Heckathorn 1989)? In the lab, freeriding on punishment efforts indeed occurs, but punishment does not break down: the cost of punishment is distributed unequally (Dreber, Rand et al. 2008, Rockenbach and Milinski 2008). The literature has not stopped at noting that there is indeed "altruistic punishment" (Fehr and Gächter 2002). It has also aimed at identifying the underlying psychological motives (Falk, Fehr et al. 2005). The main driver is emotions (Xiao and Houser 2005, Coricelli, Joffily et al. 2010, Cubitt, Drouvelis et al. 2011, Crosetto, Güth et al. 2012, Coricelli, Rusconi et al. 2014). The punishing sentiments are so strong that participants even invest in disciplining others if they do not benefit themselves (Fehr and Fischbacher 2004, Leibbrandt and López-Pérez 2012, Engel 2014). This even holds for Italian Camorra prison inmates (Nese, Palomba et al. 2013). The link to the debate on criminal law on retribution still has to be spelt out. Yet if peer punishment is coupled with a probabilistic automatic sanction (which the authors interpret as a legal sanction), the latter partly crowds out peer punishment (Kube and Traxler 2011).

Are there behavioral advantages of institutionalizing criminal law intervention, beyond the wish to contain sovereign powers? Criminal law theory tends to invoke the risk of a Hobbesian war of all against all. In the lab, this has been tested by adding another stage to the game. Those who have received punishment are given the opportunity to strike back (Nikiforakis 2008). Results are mixed. While some experiments indeed find that cooperation breaks down (Denant-Boèment, Masclet et al. 2007, Nikiforakis 2008), other experiments do not replicate the effect (Nikiforakis and Engelmann 2011, Engel, Beckenkamp et al. 2014). Feuds are much more likely if the asymmetry of payoffs leaves room for normative conflict (Nikiforakis, Noussair et al. 2012). Another advantage of legalizing punishment is containing "anti-social punishment"; depending on cultural context, the willingness is quite pronounced to spend money for hurting those who behave better than oneself (Herrmann, Thöni et al. 2008).

Defendants do not tend to take their convictions lightly. If the sanction is adjudicated and enforced, ex post they tend to be worse off than had they never committed the crime. But for many crimes, the dark field is wide. Even if the crime is detected, criminals often get away with their deed since it cannot be proven beyond a reasonable doubt. Therefore in expectation crime may well pay. In a public good game with punishment, it can be shown that, nonetheless, the threat with punishment is not pointless. It stabilizes the behavior of those who are tempted to act selfishly, but are held back by a combination of a weak preference for behaving in a socially respectful way, and the weak threat with punishment (Engel 2014).

This paradigm may also be used to test premises of criminal law. One of these premises is Jeremy Bentham's claim that deterrence works the better the more the interventions of the

criminal system are made transparent (Bentham 1830). A lab experiment manipulating feedback disproves the claim. If individual sanctions are made public, cooperation drops. The counterproductive effect results from the fact that, inevitably, the severity of the infraction is made public as well. Participants learn how poorly (some) others behave (Engel and Irlenbusch 2010).

### b) Effect of Criminal Sanctions

Deterrence focuses on individuals who might commit crime, but desist from doing so since they dread the intervention of the criminal law system. Most of the experimental research on this topic comes from experimental economics. By contrasts criminologists have focused on the effect of criminal sanctions on the probability that the defendant recidivates (for a survey see MacKenzie and Farrington 2015).

While offenders are in prison, they have a hard time committing new crimes. Barbarino and Mastrobuoni (2014) exploit unexpected releases from Italian prisons to estimate the incapacitation effect. It is pronounced, and most important for Mafia killing, drug dealing and bank robbing. In France, a similar release is shown to significantly increase recidivism five years later (Maurin and Ouss 2009). In the Netherlands, having high frequency offenders do more time in prison reduces recidivism (Tollenaar, Van der Laan et al. 2014). A randomized trial in Maryland compares a 6 months sentence in a bootcamp with longer time in a traditional prison, and finds less recidivism with the former intervention (MacKenzie, Bierie et al. 2007, Bierie 2009). More drastic conditions in some Italian prisons than others do not significantly reduce recidivism though (Drago, Galbiati et al. 2011). In California, randomly assigning inmates with a high security classification to a high security, rather than a low security institution even increases the recidivism rate by 31% (Gaes and Camp 2009).

Multiple experiments have compared imprisonment with alternative interventions for low risk, and in particular first offenders. In France, putting such offenders on electronic maintenance reduces recidivism five years later by more than 6% (Henneguelle, Monnery et al. 2016). A Swiss study comparing a sentence for community service with a two-week arrest finds more rearrest in the group randomly assigned to the latter sanction in the short term (Killias, Aebi et al. 2000), but not 10 years later (Killias, Gilliéron et al. 2010). Studies from Denmark (Klement 2015) and the Netherlands also find more reconviction for those arrested (Wermink, Blokland et al. 2010).

Criminal courts routinely place offenders on probation, or release them from prison on parole. Either way the convict for an extended period faces the threat to go (back) to prison when recidivating. The 2006 Italian prison pardon randomly created this situation for a large group of convicts. The longer the probation period, the smaller the incidence of recidivism. Interestingly the length of the probation period for peers from prison, i.e. for their buddies, had a similar effect (Drago and Galbiati 2012). In the lab, it is possible to directly compare probation with a sanction that is immediately executed. The deterrent effect of the direct sanction is more pro-

nounced. While on probation, participants desist from norm violations. But once the probation period is over, they go back to reoffending (Engel, Hennig-Schmidt et al. 2015).

Many jurisdictions have tried to reduce the risk of recidivism by special interventions that transcend the mere sanction, with mixed results. A special Californian program targeting juvenile offenders did not significantly reduce their recidivism (Lane, Turner et al. 2005, Brank, Lane et al. 2008, Farabee, Zhang et al. 2014). Self-report data indicate that those in the program were even more likely to later commit violent crime (Lane, Turner et al. 2007). A Finnish program aimed at the rehabilitation of juvenile delinquents reduced reoffending in the first year after sentencing, but not in the long run (Huttunen, Pekkala Kerr et al. 2014). A program aimed at educating juvenile offenders in the Netherlands significantly reduced delinquency, as reported by parents, but not recidivism, as measured with data from the crime register (Asscher, Deković et al. 2014). In the UK, cognitive-behavioral treatment only reduced recidivism for moderate and high risk offenders (McGuire, Bilby et al. 2008). A program in Minnesota targeting high risk offenders with supplemental case planning, housing, employment, mentoring, cognitive-behavioral programming, and transportation assistance services reduced the risk of revocation by 28%, and the risk of reconviction by 43% (Clark 2015); (also see the meta-studies Landenberger and Lipsey 2005, Visher, Winterfield et al. 2005, Koehler, Lösel et al. 2013). Special, intense treatment for drug offenders ("drug courts") had a significant reducing effect on recidivism in Baltimore (Gottfredson, Najaka et al. 2006) and in Birmingham, Alabama, Jacksonville, Florida, and Tacoma, Washington (Walters 2016), but not in Wilmington, Delaware (Marlowe, Festinger et al. 2014) and in Pennsylvania (Welsh, Zajac et al. 2014); (also see the meta-analyses on measures specifically targeting sexual offenders by Lösel and Schmucker 2005, Schmucker and Lösel 2015).

Some crimes are conspicuously difficult to detect; tax evasion is an application. One option is to reward those who, upon audit, are found out to have reported truthfully. In a lab study, this is shown to work, but only if the reward is monetary; a mere expression of recognition, i.e. a (non-monetary) award, does not suffice (Fatas, Nosenzo et al. 2015); (but see Fochmann and Kroll 2016).

Lab experiments have a chance to manipulate factors that are hard to randomly assign in the field. Legal orders are concerned about the risk of convicting an innocent, and require criminal courts to convict the defendant only if guilt has been established "beyond a reasonable doubt". But this standard of proof does not, and is not meant to, exclude wrongful conviction at all cost. In the lab, a 10% risk that a signal of the defendant having misbehaved is false does not prevent punishment from disciplining participants. Yet if this risk is as high as 50%, interestingly, experimental punishers punish even more harshly. But punishment stops having an effect on punishees (Grechenig, Nicklisch et al. 2010).

## 5. Cross-Fertilization

Criminal law and the experimental analysis of crime and intervention work at very different levels of resolution. Experiments are fine-tuned to solve identification problems. One wants to be sure about a causal claim. The inevitable price for achieving this degree of confidence is a fairly narrow field of observation. In a strict sense, the individual experiment only shows a single effect. Moreover, lab experiments are conducted with convenience samples, usually students. If interest is in petty crime, this may not be much of a limitation. But external validity is more of a concern with serious crime. Lab experiments tend to have rather small samples. While they of course test for significance, false positive findings may not be ruled out completely. This may also result from the fact that non-results are more difficult to publish (well), which is why the sample of published experiments might be biased. Running experiments is a time-consuming (and relatively pricey) endeavor. For all these reasons, oftentimes a criminal lawyer will not find all the experimental evidence she would desire to use. She will always want to discuss how much can be learned from the available studies. If there are complementary studies using observational data, this creates a welcome opportunity for triangulation.

All these caveats notwithstanding, I hope this survey convinces criminal jurists that the existing body of experimental evidence is a valuable, rarely used resource. They can use this resource to back their normative choices with truly reliable empirical evidence. Maybe even more importantly: they learn how many of the empirical claims that underlie legal argument have not been tested, at least not with rigorous experimental methods. Sometimes, quantitative findings generated with alternative methods are available, or rigorous qualitative analysis; they are beyond the scope of this survey. But not so rarely, criminal lawyers will only have their intuition. Since the law must make decisions, and since the length of procedure is itself a normative issue, intuitive decision making cannot always be avoided. But intuition may be misled. If criminal lawyers do not find any controlled empirical evidence, they should at least become self-conscious. They should articulate their empirical claims as precisely as possible. Some such claims cannot be tested with experiments. For instance, personality traits cannot be randomly assigned. But all institutional features lend themselves to manipulation, and quite some determinants of criminal behavior can also be induced. There is ample room for cross-fertilization. Despite the fact that this paper has a substantial list of references, most of the empirical claims underlying criminal law doctrine are still uncharted territory.

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