Criminal Deterrence: A Review of the Missing Literature

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Criminal Deterrence: A Review of the Missing Literature

Alex Raskolnikov*

This review of the criminal deterrence literature focuses on the questions that are largely missing from many recent excellent and comprehensive reviews of that literature and even from the literature itself. By “missing” I mean, first, questions that criminal deterrence scholars have ignored either completely or to a large extent. These questions range from fundamental (the distributional analysis of the criminal justice system) to those hidden in plain sight (economic analysis of misdemeanors), to those that are well-known yet mostly overlooked (the role of positive incentives, offender’s mental state, and celerity of punishment). Second, I use “missing” to refer to the areas where substantial relevant knowledge exists but is largely disregarded within the criminal deterrence research program. The empirical analysis of environmental and tax compliance is a stark example. Finally, I stretch “missing” to describe topics that have been both studied and reviewed but where substantial challenges remain. These include the theoretical explanation for the role of offense history, the proper accounting for the offender’s gains, the estimation of the costs of various crimes, and the cost-benefit analysis of crime-reduction policies.

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Among the literature’s missing pieces, several stand out both on their own and because they combine to produce a highly unfortunate result. First, although the empirical side of the literature focuses almost exclusively on street crime, the literature makes only a minor effort to estimate the cost of crime and essentially no effort to estimate the cost of white-collar offenses. This adds to the impression—not supported by the available evidence—that street crime is a great social problem while white-collar crime is a minor one. Second, the literature fails to treat misdemeanors and misdemeanor enforcement as an independent subject of study. This failure contributes to the notion—also unjustified—that 13 million or so misdemeanor charges a year and countless millions of stops, frisks, and interrogations that lead to no charges—all heavily skewed by race and class—are also not a major social problem. Third, the literature is only starting to develop a benefit-cost analysis of various crime-reducing strategies. The analysis considers almost exclusively measures reflected in the optimal deterrence model. This creates an impression—almost surely false—that deterrence is the only means of reducing future crime. Finally, the literature ignores distributional analysis altogether, even though the burdens of crime and the criminal justice system vary dramatically, predictably, and disturbingly by race and income. By disregarding this variation, the literature may be reinforcing it. For all these reasons, the criminal deterrence literature may well be contributing to the overwhelming, singular focus of American society and law enforcement on the forceful deterrence of street crime. Addressing the missing pieces would enrich the literature, expand its appeal and policy relevance, and enable academics to contribute to the effort of setting the US criminal justice system on the path of long overdue structural reforms.

1. **INTRODUCTION**

Criminal deterrence is a big subject: big in terms of its role in people’s lives, big in terms of the scope and scale of government expenditures, and big in terms of the academic attention it has captured over decades.

Academic literature on criminal deterrence is vast, it is both theoretical and empirical, and it reflects a number of methodological approaches including law, economics, and criminology. Excellent,
comprehensive reviews of this literature are available, including recent ones (Chalfin and McCrary 2017; Levitt and Miles 2007; Nagin 2013; Polinsky and Shavell 2007; Tonry 2008). Just 2 years ago, an entire volume was published on the subject (Nagin, Cullen, and Jonson 2018). There is hardly a need for another similar review.

There is, however, a need to consider the questions that are largely missing from these reviews and from the criminal deterrence literature overall. By “missing” I mean, first, questions that criminal deterrence scholars have ignored either completely or to a large extent. These questions range from fundamental (the distributional analysis of the criminal justice system), to those hidden in plain sight (economic analysis of misdemeanors), to those that are well-known yet mostly overlooked (the role of positive incentives, offender’s mental state, and celerity of punishment). Second, I use “missing” to refer to the areas where substantial relevant knowledge exists but is largely disregarded within the criminal deterrence research program. The empirical analysis of environmental and tax compliance is a stark example. Finally, I stretch “missing” to describe topics that have been both studied and reviewed but where substantial challenges remain. These include the theoretical explanation for the role of offense history, the proper accounting for the offender’s gains, the estimation of the costs of various crimes, and the cost-benefit analysis of crime-reduction policies. Thus, the focus on the “missing” literature aims to offer a somewhat different perspective on the criminal deterrence research—a perspective that expands the field, emphasizes the need to tackle some basic yet important questions, and highlights promising directions for future work.

This review has four sections. The first section asks what, if anything, is missing from the basic building blocks of the criminal deterrence research: its concepts and its facts. This inquiry is hardly at the center of the literature. Yet the inquiry is important because it reveals that the literature’s shortcomings vary from one criminal deterrence subfield to the next. For instance, the economic theory of crime is plausible when applied to white-collar offenses, but it encounters major problems when applied to violent crime. In contrast, the empirical research of violent crime is vast while the econometric analysis of white-collar offenses is essentially nonexistent. Whether any of this is important depends on the social cost of violent crime and white-collar offenses—the subject of both great significance and great uncertainty. The conceptual ground clearing undertaken in the first section sets up the discussion of these and many other distinctions later on.

The second section addresses the missing pieces in the theoretical framework of criminal deterrence—the optimal deterrence theory. The
third section deals with empirical questions and results. And the fourth section addresses three open questions. The first question—the role of celerity of punishment—is known to be foundational but remains unresolved. The second one—the economic analysis of misdemeanors—appears to be well studied but actually is not. And the third question—the distributional analysis of the criminal justice system—is decidedly fundamental but is missing from the literature entirely.

The conclusion emphasizes that among the literature’s missing pieces, four stand out both on their own and because they combine to produce a highly unfortunate result. First, although the empirical side of the literature focuses almost exclusively on street crime, the literature makes only a minor effort to estimate the cost of crime and essentially no effort to estimate the cost of white-collar offenses. This adds to the impression, which is not supported by the available evidence, that street crime is a great social problem while white-collar crime is a minor one. Second, the literature fails to treat misdemeanors and their enforcement as an independent subject of study. This contributes to the notion, also unjustified, that 13 million misdemeanor charges a year and countless millions of stops, frisks, and interrogations that lead to no charges—all heavily skewed by race and class—are not a major social problem either. Third, the literature is only starting to develop a benefit-cost analysis of various crime-reducing strategies. The analysis considers almost exclusively measures reflected in the optimal deterrence model. This creates an impression, almost certainly false, that deterrence is the only means of reducing future crime. Finally, the literature ignores distributional analysis altogether, even though the burdens of crime and the criminal justice system vary dramatically, predictably, and disturbingly by race and income. By disregarding this variation, the literature may be reinforcing it. For all these reasons, the criminal deterrence literature may well be contributing to the overwhelming, singular focus of American society and law enforcement on the forceful deterrence of street crime. At the very least, the literature has not deployed its powerful tools to show that this focus may be misguided for many reasons, economic and otherwise.

2. KEY CONCEPTS AND DATA
2.1. What Does Criminal Deterrence Study?
A study of criminal deterrence is a study of crime. Yet the meaning of “crime” is by no means clear. It varies from one subfield of the
criminal deterrence scholarship to the next. And it is not well-defined within the various subfields.

Starting with empirics, “crime” to an econometrician is what can be measured. There are two main sources of crime data in the United States. The first source is the Uniform Crime Reports (UCR) containing standardized, nationwide information about certain crimes. The Federal Bureau of Investigation (FBI) collects and compiles that information and publishes it annually in Crime in the United States [Addington 2019; Levitt and Miles 2007]. The UCR tracks seven types of crime included in the FBI’s “Index I” offenses. Three of these seven are violent personal crimes: homicide, rape, and aggravated assault. Three others are property crimes: burglary, larceny, and motor vehicle theft. And the last one—robbery—is both a violent personal and a property crime. The UCR also collects data on arson, but it is substantially less reliable because of many missing values [Levitt and Miles 2007]. The FBI has been trying to expand its information collection efforts to other types of crimes, and it has recently taken a decisive step toward accomplishing this goal, as discussed later. But for now, the comprehensive data available from the UCR are limited to the seven index crimes.

The second main source of crime data is the National Crime Victimization Survey (NCVS) that was known prior to 1990 as the National Crime Survey. The NCVS aggregates surveyed households’ self-reports of exposure to crime. The US Census Bureau conducts the survey and publishes annual results. The NCVS tracks the same offense categories as does the UCR with the exception of homicide (there are no victims to report the crime).

Clearly, the two major databases used by empirical economists contain information about a very limited range of criminal behavior. Some empirical analyses go beyond the UCR and NCVS data and investigate such offenses as drunk driving and drug possession [Cook 2012; Kuziemko and Levitt 2004]. Some recent research takes advantage of more comprehensive and detailed data sets [Lee and McCrary 2017]. But overall, “little empirical evidence exists on illegal activities that are essentially voluntary transactions, such as prostitution or the purchase and consumption of controlled drugs, as well as on crimes in which victims are unaware that they have been harmed, such as insider trading or many frauds” [Levitt and Miles 2007, 458].

So to an empiricist, criminal deterrence is primarily a study of homicide, rape, robbery, aggravated assault, burglary, larceny, and motor vehicle theft. Following the convention in the literature, I refer to this list—and to “crime” as the term is used in empirical research—as “index crimes.”
Important as index crimes are to an empirical economist, they play no special role in theoretical analysis. Not only does deterrence theory extend beyond index crimes, it extends beyond all crimes. The optimal deterrence theory is also known as the theory of public enforcement of law for a reason. The theory’s basic model focuses on the trade-off between the private gain and the external harm and on the government’s ability to deter acts where the harm exceeds the gain by imposing a cost on the putative offender. The external harm need not be violent or personal or take any particular form. Assault gives rise to an external harm, but so do pollution, tax evasion, auto accidents, and defective products. Sanctions, too, are just a cost imposed on the offender and they may take any form.

The theory distinguishes between sanctions that are monetary and nonmonetary. The main difference between the two is that the magnitude of nonmonetary sanctions is not limited by the offender’s wealth. Nonmonetary sanctions are typically referred to as “imprisonment,” though other forms of nonfinancial sanctions that can be imposed on an offender unable to pay a large fine surely exist [Polinsky and Shavell 2007]. “Crime” is simply any offense that is punished by nonmonetary sanctions [Polinsky and Shavell 2007, 421; Shavell 2004, 541–42].

Some models investigate crimes that are punished by fines, essentially eliminating the difference between criminal and civil violations [or at least those civil violations that are enforced by government agents rather than private plaintiffs].

Another important difference is that monetary sanctions are transferable (the payor’s financial loss is someone else’s financial gain) while “imprisonment” is not. Imprisonment is in quotes here because any nontransferable sanction [e.g., a suspension or a loss of a professional license] is equivalent to actual imprisonment in the optimal deterrence framework. Notably, nontransferability of “imprisonment” is its unfortunate side effect [because it wastes resources]. In contrast, the opportunity to use “imprisonment” to impose a cost in excess of the offender’s total wealth is the key advantage of “imprisonment” from the optimal deterrence perspective.

Shavell’s more recent book discusses the reasons to treat some offenses as crimes, but it largely restates his earlier points [Shavell 1985, 2004]. Other scholars pointed out various distinctions between torts and crimes, mostly limiting the latter to index crimes [Boston University Law Review 1996; Friedman 2000, Mungan 2012]. Yet after a comprehensive analysis aimed at identifying the economic rationale for criminal law, Friedman [2000, 295] concludes that the “current sorting of offenses
concept of crime in the optimal deterrence theory is purely derivative of the type of sanction imposed. Importantly for our purposes, this concept is surely broader than the seven index crimes generally studied by empirical economists.

And then, of course, there is the law. Here, one would think, crime has a very precise and clear meaning: crime is whatever the law says it is. Alas, “the law” is anything but clear. Most crimes are defined by state law. And states vary greatly not only in how they define specific crimes (say, as a murder or an assault) but also in their definitions of crime as a general category.

Wyoming offers a clear definition: “No conduct constitutes a crime unless it is described as a crime in this act or in another statute of this state” (Wyoming Criminal Code 6-1-102[a]). In Washington, a crime is an offense punishable by imprisonment (Washington Criminal Code § 9A.04.040). In Wisconsin, a crime is “conduct which is prohibited by state law and punishable by fine or imprisonment or both” (2011–12 Wis. Stats. §939.12; emphasis added). And in the country’s most populous state, crimes are not defined as a category at all. Instead, under the California Penal Code, “a crime or a public offense [is a prohibited act punishable by] 1. Death; 2. Imprisonment; 3. Fine; 4. Removal from office; or 5. Disqualification to hold and enjoy any office of honor, trust, or profit in this State” (California Penal Code § 15). Clearly, the same act may be a crime in one state and not in another. As clearly, the number of offenses defined as crimes by state and federal laws—both felonies and misdemeanors—is vastly greater than the seven offenses in the FBI crime index. “Crime” to an empirical economist and a lawyer has very different meanings indeed.

The same is true for theorists and empiricists. Index crimes are surely “crimes” for modeling purposes because their commission leads to sanctions not limited by the offender’s wealth (i.e., imprisonment). But this is where the similarity ends. Many other violations of public law fit squarely in the optimal deterrence framework and may lead to imprisonment but are not index crimes. Just think about securities fraud, price fixing, tax evasion, and the like. Moreover, although index crimes are all felonies, criminal misdemeanors can and do lead to imprisonment as well (Natapoff 2019). So for a theorist, just as for a lawyer, crime is a much broader concept than what empirical economists study.

Finally, not all violations of criminal law are punished by imprisonment (that is, not all are crimes in the optimal deterrence sense). At the same time, some offenses that do lead to jail time are not criminal

between the categories of crime and tort has at most a modest relation to what that analysis suggests would be an efficient division.”
as a matter of law (the notoriously vague “disorderly conduct” is the prime example; Natapoff 2019). So the legal definition of crime is both broader and narrower than the one in the optimal deterrence theory. And, as already mentioned, the optimal deterrence theory also applies to a vast array of harm-producing activities that are not criminal in the legal sense. Thus, violations of public law modeled by deterrence theory (including violations punishable by fines and “imprisonment”) are the broadest category of offenses considered in the criminal deterrence literature.

These distinctions are not mere semantics. Their significance is revealed throughout this review, but two examples would give a sense of the problem. Say a theorist working on the economic analysis of crime makes a prediction of how individuals would respond if sanctions are adjusted in a particular way. The prediction is tested empirically, and the data do not support it. Is this because the prediction is wrong or because a very particular set of offenses—index crimes—poorly fit the theory (while other crimes in a legal sense would fit it perfectly well)? Similarly, if our deep moral intuitions raise difficult questions when the economic model of criminal behavior is applied to homicide, rape, and aggravated assault—the three personal index crimes—does this mean that the model is not apt for the analysis of all violations of state and federal criminal laws?

The misalignment in the empirical, theoretical, and legal meanings of crime leads to more than occasional confusion. At the most basic level, the subject of the criminal deterrence research is poorly defined. It varies by context, and this variation complicates the empirical analysis of theoretical conclusions and the application of both to policy making. Yet the discussion of the uncertain meaning of crime and the resulting complications is largely missing from the criminal deterrence literature.

2.2. What Is the Cost of Crime?

Few would argue that crime is not a serious social problem in the United States. But how serious is it? In economic parlance, what is the cost of crime? The answer to this simple question turns out to be remarkably unclear.

In 2017, after consulting with the National Academies of Sciences, the US Government Accountability Office (GAO) issued a report to Congress summarizing the best available evidence on the cost of crime in the United States. “Researchers have estimated
varying annual costs of crime, including totals of $690 billion, $1.57 trillion, and $3.41 trillion," GAO concluded [GAO 2017, 1]. How could the estimates vary so much? “GAO found that there is no commonly used approach for estimating the costs of crime, and experts face multiple challenges when making estimates. GAO identified four primary methods to estimate costs, each with limitations: [1] measuring effects on markets, [2] using jury awards, [3] surveying the public for its willingness to pay to reduce crime, and [4] calculating individual categories of cost to develop a total cost” [GAO 2017, highlights]. All of these methodologies, the experts told GAO, are inadequate. Crime is a big problem, but it is not clear how big it is.

The uncertainty about the cost of crime is not troubling if the question is whether social scientists should study it. All estimates suggest that the cost of crime is high, surely high enough to merit researchers’ attention. But the same uncertainty is indeed an issue if the question is where the researchers should focus their studies, that is, which specific crimes or types of crimes are most costly and important to analyze, understand, and deter.

A search for an answer to this question encounters two major obstacles. First, the cost-of-crime estimates ignore misdemeanors, the most common crimes committed in the United States. Every year, 3–4 million felony cases are filed nationally. In contrast, courts handle approximately 13 million new misdemeanor cases annually (Stevenson and Mayson 2018). These estimates are rough. The UCR does not track felonies and misdemeanors separately, and there is no definition of a misdemeanor that reflects the varying criminal laws of different states. Mayson and Stevenson (2020) take a close look at sub-felony cases in eight diverse US counties and conclude that “courts, policymakers, and scholars should take care not to generalize about ‘misdemeanors’ on the false assumption that the term describes a coherent set of universally criminalized behaviors” [Mayson and Stevenson 2020, 982–83]. Still, there is no doubt that misdemeanors are a major part of the US criminal justice system, at least in terms of raw numbers.

Felonies and misdemeanors give rise to very different costs. The main costs of felonies are borne by the victims. The main—or at least significant—costs of misdemeanors are borne by the accused who are overwhelmingly poor and nonwhite (Mayson and Stevenson 2020). These costs are far from trivial. A misdemeanor conviction or even a charge may land the accused in jail for days, cause the accused the loss of a job, child custody, nutritional assistance, or eligibility for student loans and may lead to deportation. A criminal record resulting from a misdemeanor conviction can add years of prison time to a sentence for a subsequent offense (Natapoff 2012). The existence of these
costs and their social significance are beyond doubt. Yet there appear to have been no efforts to estimate them and compare them to the costs of felonies.

The second major obstacle to evaluating the cost of crime is that another vast area of criminal law has largely escaped the gaze of criminal deterrence scholars. We know exceedingly little about the cost of white-collar crime.

There is no accepted definition of the term “white-collar crime,” and its use in the literature varies significantly. Two definitions are illustrative. The first one is focused on the nature of the offense. White-collar offense is defined as “an illegal act or a series of illegal acts committed by nonphysical means and by concealment or guile to obtain money or property, to avoid the payment or loss of money or property, or to obtain personal or business advantage” (Edelhertz 1970, 3). The second definition is based on the role of the offender: “White-collar offenses are any violations of law committed by persons or organizations in the conduct of their legitimate occupational roles or organizational functions” (Simpson and Yeager 2015, 47).

Several features of these two definitions are worth emphasizing. First, although the familiar term is “white-collar crime,” neither definition is limited to criminal acts. This makes perfect sense. In some settings, the only difference between a civil and a criminal violation is the burden of proof (Sedima S.P.R.L. v. Imrex Company, 473 U.S. 479, 491 [1985]; Saltzman and Book 2020, 12.06[5]). In most settings, the difference between a civil and criminal charge is a matter of prosecutorial discretion reflecting practical considerations like resource availability and the prosecutor’s preferences (Simpson and Yeager 2015). Moreover, because the offenses in question do not involve violence, it is difficult to justify a sharp line between acts that produce similar harms in a similar fashion. This is especially true because the law often draws this line based on a single number, be it dollars involved or the quantity of pollutant emitted. For all these reasons, the rest of this review refers to white-collar offenses rather than crimes. As should be clear by now, erasing the sharp distinction between criminal and civil white-collar offenses is entirely consistent with the optimal deterrence theory. Erasing this difference also accentuates the gap between index crimes studied in most of the empirical literature and crime as the phenomenon that society and the criminal deterrence scholarship need to understand and address.

Another point worth noting is that some white-collar offenses under both definitions quoted earlier give rise to tangible harms, such as the damage to the environment or to the health and safety of individuals. Such damage is typically one of the largest components of the overall social cost of these violations. But many white-collar
offenses are financial transfers from the victim (or victims) to the offender. For these, the amount of the transfer and the resulting social cost may differ sharply.

Finally, the first definition of white-collar offense—but not the second one—includes personal fraud ranging from tax fraud to welfare fraud, counterfeiting, forgery, telemarketing fraud, credit card fraud, cyber fraud, and the like. The literature has not arrived at a consensus about whether personal fraud should be viewed as a white-collar offense. Adding to the confusion, some sources treat regulatory offenses as a separate category (GAO 2017, 24; Miller et al. 1996, 6) while others view them as central to the concept of white-collar offenses (Simpson and Yeager 2015).

With limited exceptions, information about white-collar offenses is not collected by any federal agency. No white-collar offense is included in index crimes. Empirical economists do not study these offenses, and their costs are largely unknown (GAO 2017; Miller et al. 1996; National Academies of Sciences, Engineering, and Medicine 2016). As a result, and with the exception of an occasional financial scandal, white-collar offenses fail to capture the attention of the public and the lawmakers. As the National Academies of Sciences reported to the GAO, the prevalence of the information about street crimes in the nationally maintained databases “has limited the public’s perception of what constitutes a crime” (GAO 2017, 15). When the government’s report called “Crime in the United States” ignores all white-collar offenses, it is easy to conclude that these offenses are only a minor social problem (National Academies of Sciences, Engineering, and Medicine 2016, 25).

Yet whether the omission of white-collar offenses from the cost-of-crime estimates is a trivial or a major deficiency depends on the relative costs of street crimes and white-collar offenses. What are these relative costs? Although the data are scarce and not particularly reliable, some estimates do exist.

The most recent comprehensive cost-of-crime estimate cited by GAO (2017) is found in Anderson (2011). He estimates the cost of crime in the United States to equal $3,216 billion a year. Of that, he attributes $1,561 billion to white-collar offenses.

The largest components of Anderson’s (2011) $1,561 billion estimate are as follows: $762 billion cost of occupational fraud, based on the estimates of the Association of Certified Fraud Examiners; $294 billion net tax gap based on the estimate by the Internal Revenue Service [IRS] (the more recent number is $381 billion [IRS 2019]); $184 billion cost of health insurance fraud based on the estimates by the FBI and the National Health Care Anti-Fraud Association; and $145 billion in retail fraud based on the estimate of Javelin Strategy
& Research (Anderson 2011, 238–40). In contrast to these large numbers, the estimated annual cost of robbery—one of the seven index crimes—is only $727 million (Anderson 2011, 249).

Anderson (2011, 227) also estimates the cost of cybercrime based on the FBI’s assessment that computer-related crime cost businesses $78.1 billion in 2006. More recent numbers are much higher. In 2019, Accenture’s forecast for the cost of cybercrime was $1 billion a year worldwide for the next 5 years (Accenture 2019, 14). Given that the US economy represents about 20% of world gross domestic product (World Bank 2020), Accenture’s estimate suggests the US cost of cybercrime is on the order of $200 billion a year. The FBI (2019) report on internet crime states that the agency’s Internet Crime Complaint Center (IC3) received complaints reporting losses exceeding $3.5 billion, more than half of which came from “business email compromise/email account compromise complaints” (fraudulent and unauthorized transfers of funds). This extremely high reported number may well reflect net annual losses [after recoveries] of over $300 billion—a total comparable to a rough estimate based on Accenture’s analysis.5

Barkan (2018) also collects cost estimates for various white-collar offenses. The largest of these estimates are between $100 and $400 billion for insurance fraud and between $77 and $259 billion for healthcare fraud, both based on the data from the Insurance Information Institute. Barkan concludes that, conservatively estimated, the total cost of white-collar offenses is $588 billion annually (Barkan 2018, 262). When tax noncompliance is included, this number grows to $969 billion.

Note that although the tax gap is included in what Anderson (2011, 254–55) interchangeably calls “white-collar crimes” and “transfers,” the vast majority of the revenue lost to tax noncompliance is certainly not due to criminal activity.6 Given the earlier discussion, treating tax noncompliance as a white-collar offense makes perfect sense.7

Looking beyond the United States does not bring much clarity. Czabanski (2009) reports that as of 2009, there was a single cost-of-crime

5 The FBI has been successful in recovering some internet losses. It reports a recovery rate of its Recovery Asset Team of 79% (FBI 2019), leaving 21% of $3.5 trillion or $735 billion a year as not recoverable. Even if these losses are overreported by half, the actual net losses exceed $300 billion.

6 Tax crimes amount to a small fraction of tax enforcement, and the tax gap estimates published by the IRS do not break down revenue losses for criminal and civil violations separately.

7 Note that the tax gap number includes individual tax noncompliance, which would be covered by the offense-centered but not by the role-centered definition, and business-level noncompliance, which would be covered by both definitions.
estimate outside of the English-speaking world—a short 1999 article focused on France. Wickramasekera et al. (2015) review 21 cost-of-crime studies from six countries. The types of crimes included in various studies vary, as do the types of costs considered. Some of the studies estimate the cost of fraud, though none appear to evaluate white-collar offenses more broadly. The authors conclude that, on average, fraud accounts for 17% of the total cost of crime. The standard deviation of this estimate (13%) reveals the extent of variation, although the dispersion of estimates for many other types of crimes is even greater (Wickramasekera et al. 2015, 225–26).

Among the cost estimates for white-collar offenses just discussed, the US tax gap number is surely the most reliable one. So it is worth comparing with the conservative estimate of the cost of index crime. According to Chalfin (2016, 11), that cost for 2012 “is approximately $200 billion if only Uniform Crime Reports index crimes reported to the police are counted, and as high as $310 billion when unreported crimes are accounted for.” Chalfin (2016, 11) notes that these estimates “do not credibly account for dynamic responses of individuals to increases in crime nor are the costs of non-index crimes counted.” Chalfin and McCrary (2018) offer a similar estimate, and when the cost of police is included, the number rises to approximately $440 billion. These numbers are comparable to the $381 billion tax gap number. And tax noncompliance is just one white-collar offense out of many.

Of course, tax noncompliance is a transfer (from the state to the offenders) while index crimes lead to real social costs. Anderson’s (2011) estimate of $3,216 billion a year for the total cost of crime reflects the cost of crime to the victims. Anderson (2011, 248) attributes $1,516 billion of the total $3,216 billion to white-collar offenses and excludes that amount from the social cost of crime because “transfers of goods and money via fraud and theft do not necessarily impose a net burden on society.” This is familiar point in economics. “Mere transfers” (Donohue 2009, 286), as they are often called in the literature, are costly to the victims but not to the society as a whole given that no value is destroyed as resources are moved around in the economy (Shavell 1991). Yet the social cost of transfers is almost certainly greater than this logic suggests.

Tullock (1967) pioneered the literature on rent-seeking when he pointed out that the cost of tariffs, monopolies, and theft is greater than the deadweight loss triangle because domestic producers, monopolists,

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* Chalfin and McCrary (2018) estimate the cost of index crime per capita as $995 and the per capita cost of police as $341. Given the US population of about 330 million, the total cost is about $440 billion.
and thieves would deploy resources to secure their gains. “Transfers themselves cost society nothing,” Tullock (1967, 230) explained, “but for the people engaging in them they are just like any other activity, and this means that large resources may be invested in attempting to make or prevent transfers. These largely offsetting commitments of resources are totally wasted from the standpoint of society as a whole.” To take one stark example, Americans are estimated to spend $164 billion worth of time every year locking and unlocking doors and [not to be omitted!] looking for misplaced keys (Anderson 2011, 247).

Those engaged in securities fraud, welfare fraud, price fixing, and the like expend resources to secure the intended gains while remaining undetected. On the other side, putative transferees invest resources to prevent these transfers. At the extreme, an embezzler would invest up to a dollar to embezzle a dollar and a potential victim of embezzlement would invest up to a dollar to prevent it. So contrary to Anderson’s (2011) remark, the social cost of transfers may be as much as twice the amount transferred (Wenders 1987). These are real social costs of white-collar offenses.

As this review makes clear, data on white-collar offenses are scarce, and many of the estimates that do exist are speculative. So the point here is certainly not to condemn the empirical criminal deterrence literature for missing a major part of the problem by focusing on index crimes and ignoring white-collar offenses as well as misdemeanors. Rather, the argument is that there is no reason to think that the cost of white-collar offenses to the society is insignificant compared with the crimes that the literature has been studying for decades. So it is difficult to justify the literature’s almost exclusive focus on index crimes and its disregard of white-collar offenses. Moreover, given Anderson’s (2011, 254–55) estimate that most of the increase in the total cost of crime between the mid-1990s and 2011 came from white-collar offenses, the missing study of these offenses is even more problematic. In any case, the empirical analysis of white-collar offenses is a pressing need.

There is little doubt that the reason for the literature’s inattention to white-collar offenses is lack of reliable data. Fortunately, more data on some forms of white-collar offenses are about to become accessible. And vastly more information on regulatory offenses is not far out of reach.

The FBI has been running the UCR program since 1930. The UCRs are based on crime reports from 18,000 city, county, university/college, state, federal, and tribal law enforcement agencies. The reporting is voluntary, but the participation rate is very high. The reports cover over 98% of the US population (GAO 2017).
Until recently, the FBI relied on the Summary Reporting System (SRS) to collect the information. Since 1991, however, the FBI started collecting additional crime statistics using the National Incident-Based Reporting System (NIBRS). NIBRS covers many more offenses in greater detail, but many fewer reporting units participate. As of 2017, for example, no major US city used NIBRS to report crime statistics [Addington 2019]. After years of slow progress, the FBI announced in 2016 that starting in 2021, it will accept crime data only using NIBRS, with the historic (and still widely used) SRS finally being discontinued.

Nationwide NIBRS reporting would contain some data that would shed light on the cost of white-collar offenses. In 2014, the FBI added cyberspace as a new crime location [FBI 2019]. NIBRS offense categories include bribery, fraud (including wire fraud and welfare fraud), cybercrime, and computer hacking.9 They also include embezzlement and general fraud, just as SRS reports do. Still, the vast majority of white-collar offenses remain outside of the NIBRS program. Researchers would need to look elsewhere for that information.

Fortunately, there is a good place to start—a recent, detailed, and comprehensive report by Simpson and Yeager (2015). At the request of the Bureau of Justice Statistics, these scholars compiled an exhaustive list of data sources that are currently available—or may plausibly be made available—from many of the 44 federal agencies that have enforcement responsibilities and information about white-collar offenses. The authors focus on the Environmental Protection Agency, the Securities and Exchange Commission, the Federal Trade Commission, the Food and Drug Administration, and the Consumer Financial Protection Bureau. The authors also discuss data that are or may be made available by the fraud and environmental crime units of the Department of Justice, the Administrative Office of the US Courts, the Office for Victims of Crime, and the Department of Health and Human Services. The authors do not discuss the IRS, but the tax agency has been collecting data on tax noncompliance starting as far back as 1964 [Brown and Mazur 2003].10 Simpson and Yeager (2015) clearly show that more and better data on white-collar offenses are within reach.

This section’s focus on the issues of measurement and data discovery may seem excessive. Economists run regressions using data

9 Cybercrime includes “wrongfully obtaining and using another person’s personal data like name, date of birth, Social Security number” and the like, and hacking includes “wrongfully gaining access to another person’s or institution’s computer software or networks” (GAO 2017, 24).

10 The most likely reason for this omission is that the only way to get access to the administrative tax data is to participate in a special program run by the IRS (2018).
that are available. Cost estimates are not worth much if they are highly speculative. So it seems self-evident that the best economists can do is to take the available data and apply the econometric techniques to it. Although all of this is true, it may be illuminating to consider another area of empirical research that has undergone a major shift in focus in the past 2 decades—a shift driven by the realization that data discovery is at least as important as data analysis.

Public economics has a long history of empirical research. Early estimates of labor supply elasticities date back to the 1970s (Rosen 1976; Wales 1973). There are countless, increasingly sophisticated tests of people’s responses to income taxes, capital gain taxes, payroll taxes, value-added taxes, cigarette taxes, and so on. Yet the research program that has dominated all these studies in terms of both public influence and social importance has been focused on a much more basic question: Who earns what and how has the distribution of earnings changed over time?

The search for answers to these questions—questions that were simply not asked until the early 2000s (Piketty and Saez 2003)—has mushroomed into an entire subfield of public economics. The public attention is mostly drawn to the U-shaped trajectory of income shares going to the highest earners over the past century. But for researchers, perhaps the most sobering lesson has been the difficulty of answering even the most basic questions such as how incomes reported to the government on tax returns change over time (Auten and Splinter 2019; Piketty, Saez, and Zucman 2018) and whether the income of highest earners is derived mostly from labor or capital (Piketty, Saez, and Zucman 2018; Smith et al. 2019). Yet answering these questions has turned out to be much more important than making incremental refinements in estimating various elasticities.

If nothing else, this comparison should motivate empirical criminologists and economists. New research on the cost of crime—with crime being broadly understood to include all white-collar offenses as well as misdemeanors—may turn out to be the most important, consequential, policy-relevant, and exciting area of criminal deterrence research in the coming decades.

3. THE MISSING PIECES OF THE OPTIMAL DETERRENCE PUZZLE

Turning from measurements to models, this section address the theory of optimal deterrence. The goal is to consider whether several key propositions of that theory still contain “missing” pieces, in a
sense that some basic questions about these propositions remain unsettled.11

3.1. What to Do with Offenders’ Gains?

The foundational proposition of the optimal deterrence theory is that the government should deter private acts producing external harms in excess of private gains while allowing private acts for which the gains exceed the harms. This point was made by the theory’s progenitors, Jeremy Bentham ([1823] 2010) and Cesare Beccaria ([1767] 1995). And it was embraced by Becker when he formalized this trade-off in his seminal article (Becker 1968). Yet Becker’s view of the government’s optimization problem was immediately challenged by Stigler (1970). The two Nobel laureates disagreed about whose gains ought to count. Becker’s answer was “everyone’s.” Stigler’s view was “certainly not!” “What evidence is there that society sets a positive value upon the utility derived from a murder, rape, or arson?” Stigler asked incredulously. The “society has branded the utility derived from such activities as illicit” he added (527) without offering any evidence that society recognizes as much as the concept of utility, let alone brands some kinds of utility as different from others.

Adopting Becker’s view leads to the inescapable conclusion that society should allow efficient crimes (as well as efficient torts)—a result that at least in some cases economists find to be distasteful (Curry and Doyle 2016; Dharmapala and Garoupa 2004). Adopting Stigler’s view leads to an uncomfortable realization that society’s “branding” of “illicit utility” is quite contingent. Some acts that society “branded . . . as illicit” and criminalized just a few short decades ago are now constitutionally protected fundamental rights (e.g., same-sex relationships, interracial marriages). Likewise, some acts that society brands as “illicit” and criminalizes today used to be acceptable not long ago (e.g., marital rape; Bennice and Resick 2003; Hasday 2000). As the discussion in the previous section makes clear, certain acts are crimes in some states but not in others. Does the illicit utility differ from state to state?

The Becker-Stigler debate has not been resolved, but scholars have found ways to advance the theory while avoiding the issue. Curry and Doyle (2016) formalize Posner’s (1985) suggestion that criminal law aims to induce putative criminals to achieve their objectives through voluntary market exchanges—a notion that is easier to accept for some crimes (property theft) than others (rape or battery). When

11 Some of the discussion in this section overlaps with Raskolnikov (forthcoming-a).
market exchange is added to the choice of either committing a crime or doing nothing, Curry and Doyle (2016) show, maximizing social welfare becomes equivalent to minimizing the cost of crime. Because the offender’s gain is not part of this cost, there is no need to decide whether or not gains of some offenders should count. Curry and Doyle’s (2016) analysis explains several features of criminal law, such as the use of criminal history in sentencing and the necessity defense.

Raskolnikov (2014) avoids the same question by focusing on a subset of socially undesirable acts in which the offender’s gain is always equal to the victim’s harm. These acts, ranging from price fixing to market manipulation, securities churning, insider trading, and many forms of fraud, are intentional, nonconsensual transfers of money; they amount to quasi-theft. As already discussed, although the transfer itself neither adds to nor detracts from social welfare, victims incur defensive costs to prevent these transfers, and offenders incur costs to carry them out. These costs make all quasi-theft unambiguously inefficient whether or not the social welfare function includes the offender’s gain. Therefore the economic analysis of quasi-theft does not hinge on resolving the offender’s gain conundrum.

While some scholars deal with the “illicit gain” problem by narrowing the acts under consideration, others resolve the same issue by considering both alternatives. Mungan (2019, 11) builds a case for rewarding individuals who abstain from engaging in criminal acts by either including the utility of criminals in the social welfare function or ignoring it. The results, it turns out, do not depend on the treatment of the offender’s gains. Mungan (2014) follows the same strategy with the same indifference result in his analysis of escalating sanctions. Miceli and Bucci (2005, 77–78) also consider both alternatives in their study of escalating penalties, but their result holds only if the offenders’ gains are excluded.

The preceding discussion of the meaning of crime suggests yet another, new take on the illicit gains problem. Only three out of seven index crimes are violent personal crimes: murder, rape, and aggravated assault. The fourth one—robbery—is a both a violent personal crime and a transfer. The three remaining index crimes of burglary, larceny, and motor vehicle theft are all transfers of property. Looking beyond index crimes, a large share of white-collar offenses are transfers as well, in many cases transfers of money. Some of these transfers fit Posner’s (1985) market bypass theory, others do not. Either way, one’s moral objections to recognizing the private gain of a murderer

12 When the transfer is from the government, such as in the case of welfare fraud, tax noncompliance, and the like, no comparable market transaction exists. As we have seen, the two white-collar offenses just mentioned taken by themselves give rise
or a rapist do not apply with equal force—and perhaps not at all—to recognizing the private gain of a thief, a tax cheat, or a corporate insider trading on private information. Setting violent personal crimes aside takes much of the sting out of Stigler’s critique of Becker’s (as well as Bentham’s and Beccaria’s) decision to include the offender’s gain in the social welfare analysis of crime.

As we have seen, nonviolent crimes and white-collar offenses likely give rise to major social costs. If so, another solution to the offender’s gain conundrum would be to limit the optimal deterrence theory either to offenses involving money and property transfers or possibly to a broader category in which the offender’s gains are purely financial.

This approach is particularly appealing because violent personal crimes involve severe measurement problems. Whether or not a murderer’s or rapist’s gain is illicit, it is difficult to price it. The same is surely true of the losses of the respective victims. It is also extremely difficult to value offenders’ gains and victims’ losses resulting from crimes driven by animus (Hayashi 2019). Thus, instead of relying on a highly questionable concept of illicit gain, the optimal deterrence theory may conclude that violent and animus-driven personal crimes involve gains and losses that are so difficult to calculate that the theory is not well suited to the analysis of these offenses.

Finally, one can avoid the illicit gain problem by switching from a normative to a positive approach. If theoretical efforts focus on how society can deter exogenously defined offenses without asking what offenses should be deterred, that is, if the theory focuses on compliance rather than deterrence, the illicit gain problem would disappear altogether (Raskolnikov, forthcoming-a).

3.2. The Missing Explanation of Criminal Intent

The two most important features separating criminal from civil violations as a matter of law are the burden of proof and the offender’s mental state, frequently referred to as intent. Economic analysis of legal errors and evidentiary thresholds is considerable (Spier 2007; Spier 2008).

to private costs [treating the government—or, rather, taxpayers as a whole—as a private party] that may run in multiple hundreds of billions of dollars a year.

Not everyone agrees. Cowell (1990, 136), for example, suggested that tax evaders’ gain should not be fully included in the social welfare function, for which he was criticized [appropriately] by Slemrod and Yitzhaki (2002, 1447).

Needless to say, there have been many efforts to estimate the victim’s losses in these examples, though not the offender’s gains.

As Finkelstein (2000) points out, the mental state specified in many criminal offense definitions is knowledge rather than intent.
Zeiler and Puccetti 2018). In contrast, economic analysis of mental states is almost nonexistent.

This omission is problematic. The offender’s mental state often distinguishes crimes from noncriminal offenses (Finkelstein 2000). Even when it does not, a specific mental state is a necessary prerequisite for a criminal conviction. From the mens rea requirement in general criminal law to tests based on knowledge, purpose, willfulness, and scienter in environmental regulation, securities regulation, corporate governance, and taxation, the offender’s mental state determines not only the existence of liability but the severity of sanctions as well (Raskolnikov 2016). Yet “economic analysis of law has expressed puzzlement at the intent rules in the law... Under the standard economic approach, which focused on internalization of external costs, the actor’s intent would appear to be irrelevant” (Hylton 2010, 1242). It is revealing that Polinsky and Shavell’s (2007) comprehensive review of the optimal deterrence theory makes no mention of the offender’s mental state despite discussing such subjects as social norms and fairness.

Deterrence theorists have offered several explanations of the role of the offender’s state of mind, all limited to certain doctrinal areas and all lacking rigorous empirical support. Posner (1985) suggests that the intent requirement in criminal law is a proxy for the probability of apprehension and conviction, a proxy for the offender’s responsiveness to punishment, or a means of identifying what he calls pure coercive transfers. Shavell (1985) links the same requirement to the probability of harm and the likelihood of escaping from sanctions. Parker (1993) argues that the mens rea requirement in criminal law relates to a putative offender’s cost of acquiring information about the nature and consequences of his or her actions.

Raskolnikov (2014) identifies cases in which the intent requirement has a direct and obvious connection both to efficiency and legality (as well as, often, criminality). He points out that inefficient and illegal acts ranging from insider trading to naked price fixing, securities churning, embezzlement, and others all have efficient and legal counterparts that differ from their illegal “twins” only in the actor’s mental state. If companies in the same industry raise prices because raw materials have become more expensive, the act is both efficient and legal. If the same companies raise prices collusively, the act is both inefficient and illegal. If someone takes $20 out of my wallet without my knowledge while thinking that I owe him $20, there

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16 The point here is that even when the only factor that distinguishes criminal and civil violations is the burden of proof, both the civil and the criminal violations require a particular mental state, usually knowledge or some form of intent.
are no negative consequences in terms of either efficiency or legality. If someone does the same while thinking that the money is mine, the act is both inefficient and criminal. The role of the offender’s mental state in identifying and deterring all these inefficient and illegal forms of quasi-theft is obvious and intuitive.

Raskolnikov (2014) offers many examples of quasi-theft, and the earlier discussion of white-collar offenses suggests many others. Recall the costliest white-collar offenses: occupational fraud consisting of asset misappropriation, corruption, and financial statement fraud (Association of Certified Fraud Examiners 2018) as well as tax non-compliance, health insurance fraud, and retail fraud. Almost all of these are money transfers and quasi-thefts. For all of them, the agent’s intent separates a benign (and efficient) action from a harmful (and inefficient) one. So Raskolnikov’s (2014) explanation for the role of mental state applies to a significant portion of acts studied in the criminal deterrence literature, at least in terms of their economic significance.

Still, not all crimes—however defined—are transfers. Deterrence theorists are yet to offer a general explanation of why the offender’s state of mind matters in criminal law.

### 3.3. The Role of Offense History

Langan and Levin (2002) report that out of nearly 300,000 individuals released from prison in 1994, more than two thirds were rearrested and over half were back in prison within 3 years of release. Approximately half of all crimes committed in the United Kingdom were committed by individuals with criminal records (Wickramasekera et al. 2015). Repeat offending is a major social problem. And its punishment is a challenge for the optimal deterrence theory.

US Sentencing Guidelines escalate penalties for repeat offenders (US Sentencing Commission 2018, chaps. 4–5). State three-strike laws do the same (Durlauf and Nagin 2011; Shepherd 2002a). Prior misdemeanor convictions—or mere charges—increase the likelihood of a later charge, arrest, and conviction (Kohler-Hausmann 2018, 74–84). Higher sanctions for repeat offenders are a major feature of criminal law. They are also common in civil law statutes ranging from environmental law, to occupational health and safety law, to immigration law (Dana 2001; Polinsky and Shavell 1998).

The dependence of sanctions on the offense history has puzzled economists for some time. “At the very best the literature . . . has shown that under rather special circumstances escalating penalty schemes may be optimal” (Emons 2003, 254). Although this is hardly a ringing endorsement, researchers have offered multiple explanations for higher sanctions for repeat offenders.
In an early attempt to tackle the issue, Polinsky and Shavell (1998) suggested that escalating sanctions may feature in an optimal deterrence regime, though their model supports sanctions below what would have been optimal otherwise in the second period for non-repeat offenders rather than higher sanctions for recidivists. Among recent efforts, Miceli and Bucci (2005) show that if criminals’ opportunities to earn income in the labor market decline as they commit more crimes, sanctions should be higher for repeat offenders under some restrictive assumptions. Emons (2007) concludes that if the criminal market has a barrier to exit (think of joining a gang or knowingly installing faulty pollution control equipment), escalating sanctions are efficient if the gain from the offense is high in relation to agent’s wealth and several other assumptions apply. Mungan (2014) offers a behavioral justification for escalating penalties based on the assumption that potential offenders are “weak-willed . . . [meaning that they] ordinarily possess self-control, but . . . may lapse into committing crime” (Mungan 2014, 190). These individuals may rationally abstain from committing a profitable offense to avoid a higher penalty for a future offense that they may commit in their weak-willed state. Müller and Schmitz (2015) also emphasize the indirect effect of the sanction for the second offense on the deterrence of the first offense. If first-time offenders can only be sanctioned significantly below the socially optimal level, Müller and Schmitz (2015) show that escalating penalties may be optimal. Curry and Doyle (2016) demonstrate that escalating sanctions are optimal if offenders have a market alternative to achieving their criminal objectives and if criminal history reveals that the offender cannot be cheaply deterred. Endres and Rundshagen (2016) conclude that if the authorities minimize the sum of harm from crimes and enforcement costs, escalating sanctions are optimal given a particular distribution of criminal benefits among offenders with different offense histories. Buehler and Eschenbaum (2020) use a model of dynamic price discrimination to reveal the optimality of escalating penalties if the social planner chooses how to discount the utility of offenders and cannot commit to future transfers.

Mungan (2014) sorts explanations of escalating sanctions for repeat offenders into three categories: those relying on (1) the stigmatization effect of the first penalty, (2) the variation in the offenders’ propensities to commit crime, and (3) the offender’s learning how to escape punishment (Mungan 2014, 190–91, citing sources). Buehler and Eschenbaum (2020) add error-prone law enforcement as the fourth explanation.

Dana (2001) takes on all these arguments with a simple, powerful, and meticulously supported counter: the probability of detection is higher for repeat offenders in state and federal, civil and criminal
law enforcement (Dana 2001, 753–54). So it is not enough to explain—as the models just described do—why expected penalties should be higher for repeat offenders. One must establish that the increase in these penalties on account of the higher probability of detection is insufficient, so nominal penalties should increase as well.

Dana (2001) himself suggests two reasons for such an increase. First, informal sanctions that accompany the first act of lawbreaking are likely to be higher than those for subsequent acts. So higher formal sanctions for repeat offenders may be needed to counter the decline in informal sanctions. Second, escalating nominal sanctions may also serve an expressive function. Mungan (2010) adds another reason: repeat offenders may be better at learning how to avoid detection than the government is at learning how to catch them. If so, the overall probability of detection for repeat offenders may be lower, not higher, than for first-timers. Higher nominal sanctions are needed to offset this decline. All these suggestions are more plausible in some contexts than others. Overall then, despite many creative and interesting models explaining the role of offense history in punishment, a general explanation of the pervasive offense-based penalty escalation is still missing. Perhaps, it simply does not exist.

3.4. The Mostly Missing Participation Margin

Deterrence theorists have recognized for decades that legal rules may affect behavior along two margins. Because the initial analysis used torts as a paradigmatic legal regime, these margins came to be known as the level of care and the level of activity (Shavell 1980). More general terms for the same two margins are compliance and participation.

Economic analysis of joint optimization along these two margins is surprisingly slim. Png (1986) shows that errors in determining compliance reduce participation in potentially welfare-enhancing activities such as driving. Kaplow (2011) confirms that imperfectly accurate enforcement chills participation and studies the implications of this insight for the optimal burden of proof. Friedman and Wickelgren (2010) explain that the interplay between deterrence (compliance) and chilling (participation) may make litigation settlements welfare reducing.

When it comes to the economic analysis of criminal violations (as opposed to harm-producing acts in general), the participation margin has been ignored altogether. This is somewhat surprising. Posner’s (1985) early economic explanation of criminal law is that it deters offenders from bypassing the market, that is, prevents criminals from taking by force or guile what they can obtain for a price. Obviously,
market participation is an optional activity. The importance of the participation margin in Posner’s setup is difficult to miss.

Moreover, just as Shavell (1980) identified the need to consider the activity levels of potential tortfeasors, Polinsky (1980) made the same point about market participation by polluting firms. Pollution, of course, may lead to criminal liability (Scalia 1999). And firms too concerned about this liability, including its mistaken imposition, may exit the polluting industry. The same is true of firms concerned about price-fixing prosecutions and even of individuals chilled by potential mistaken charges of theft, an example that Posner (1985, 1221) himself used. Clearly, the optimal deterrence theory acknowledged the importance of the participation decision early on, yet it never developed models to investigate it in detail. Polinsky and Shavell’s (2007, 425) literature review has a rather brief discussion of activity levels. And as they explain, the “determination of the optimal level of activity presumes that individuals act optimally when engaging in the activity.” That is, participation is investigated only conditionally on optimal compliance.

Granted, lack of attention to the participation margin is hardly a problem when one thinks about violent crime. Posner’s (1985) market bypass explanation has few adherents when it comes to murder, rape, and assault. But the same explanation is much more plausible in the context of property crimes and transfers more generally. So the missing analysis of the participation margin in the optimal deterrence literature is a drawback.

Dari-Mattiacci and Raskolnikov (2020a) study agents engaged in a joint optimization of the compliance and participation decisions. The analysis yields a somewhat unexpected result: higher expected sanctions may undermine deterrence. Higher expected sanctions induce some violators to start complying. However, these sanctions may also induce some previously compliant agents to exit the regulatory regime altogether (or abstain from entering it). If the second effect dominates the first, higher sanctions would lead not only to a reduction in the number of violators but also to a decline in the number of compliers. If the latter effect dominates the former—and Dari-Mattiacci and Raskolnikov (2020a) show that this is entirely possible—higher sanctions would increase the share of violators among those choosing to participate in a regulatory regime. This outcome may be viewed as a decrease in deterrence, and it may surely be regarded as undesirable by law enforcers. The potentially counterproductive effect of higher sanctions—especially when added to their greater cost—should give pause to punishment enthusiasts regulating optional regulatory regimes. More broadly, the deterrence theory would clearly benefit from further study of the participation decision.
3.5. All Sticks, No Carrots?

A combination of the terms “criminal” and “deterrence” hardly elicits associations with rewards, benefits, and government inducements. So entrenched is the view that deterrence is all sticks and no carrots that sociologists and legal academics investigating a crime prevention program that incorporates both view it as self-evident that carrots matter for procedural justice but not for deterrence (Papachristos, Meares, and Fagan 2007, 237). Criminal deterrence theory has paid little attention to carrots: “The effectiveness of positive incentives is an understudied topic” (Durlauf and Nagin 2011, 40). But this appears to be changing.

Polinsky (2015) demonstrates a clear benefit of mixing sanctions with rewards in criminal punishment. Reducing a prison term by allowing parole or probation for well-behaving prisoners, he explains, may save enforcement costs without reducing deterrence. Good behavior in prison is costly to prisoners. If the government sets the reward at the level where the cost of good behavior to a prisoner is just equal to the benefit of a shorter prison term, the total disutility of the sanction would remain unchanged but the enforcement costs of imprisonment would decline. Rewarding good behavior in prison is socially beneficial.17

Mungan (2019) brings carrots into the criminal deterrence analysis on a more general scale. He considers a seemingly implausible idea of rewarding everyone who does not commit crime (during a given period, presumably). Upon closer inspection, however, the idea of rewards (financial or otherwise) is not that implausible. To take a stark example, imagine that a marginal disutility of a prison term above 1 year is zero. If so, prison sentences beyond 1 year have no additional deterrent effect but give rise to enforcement costs of maintaining a large prison population. These costs may be saved without reducing deterrence by cutting all prison terms to 1 year. Transferring some (or all) of the resulting cost savings to nonoffenders would increase deterrence because of a well-known insight that carrots and sticks are substitutes in their incentive effects (Ben Shahar and Bradford 2012). Mungan’s (2019, 4) first conclusion is that rewards “are optimal as long as the ratio between total imprisonment costs and the total costs of crime . . . is greater than the imprisonment elasticity of crime.” His second conclusion is that if rewards may be targeted toward likely offenders—even if imprecisely—rewards “can always be used to jointly reduce crime, sentences, and  

17 The few papers that considered the same subject before Polinsky (2015) featured significantly more restrictive assumptions, narrower optimization problems, or both (Garoupa 1996; Lewis 1979; Miceli 1994).
Programs such as the Chicago Gun Project described in Papachristos, Meares, and Fagan (2007) are very much in the spirit of Mungan’s (2019) argument.

Dari-Mattiacci and Raskolnikov (2020b) also focus on carrots but of a different kind. When regulators inspect potential noncompliers, a finding of a violation punished by a sanction is obviously costly for the violator. But a finding of no violations, Dari-Mattiacci and Raskolnikov (2020b) emphasize, is often more than just a neutral result. Passing an inspection may lead to a designation as a low-risk regulated party, reducing future inspection costs and possibly even future fines (Black and Baldwin 2012; Blundell, Gowrisankaran, and Langer, 2020). It may result in a regulatory stamp of approval for a practice, a design, or a reporting position of previously questionable legality. As a result, the future benefits of that practice or position no longer need to be discounted in light of legal uncertainty. And given the well-established practice of focusing enforcement on repeat violators (Dana 2001), a finding of nonviolation yields a clear benefit of avoiding the repeat violator status.

Highlighting all of these possible carrots resulting from successfully passing a regulatory inspection, Dari-Mattiacci and Raskolnikov (2020b) demonstrate two stark results, one more surprising than the other. The first result is that the presence of carrots negates the standard conclusion that the certainty and severity of punishment are substitutes. When carrots are in the picture, an increase in the nominal sanction is bad news for regulated parties. In contrast, an increase in the probability of detection (be it an audit, an inspection, or a certification) is a mixed blessing. This increase gives rise to both a higher expected cost and a higher expected benefit (because the possible carrot is more likely to materialize). Sanctions and detection probabilities are no longer interchangeable.

The second, particularly surprising, result is that when, in addition to potentially obtaining the carrots just described, regulated parties may choose to exit the regulatory regime or to abstain from entering it, the certainty and severity of sanctions may have not just a different effect on deterrence—they may have the opposite effect. The intuition is that if the nominal sanction is larger than the reward, the net incentive is a sanction. Its higher likelihood has the same directional effect as its higher magnitude. But if the reward is larger than the sanction, the net incentive is a reward. Its higher likelihood surely does not have the same directional effect as a larger sanction. Recognizing the complex incentives created by the existence and changes in rewards—especially when agents make compliance and participation decisions simultaneously—raises questions for future research about
some of the most fundamental conclusions of the optimal deterrence theory.

It seems that enriching the model of deterrence by exploring different types of rewards is a promising direction for future research. There is considerable empirical literature investigating the interaction between crime and unemployment or wages (Chalfin and McCrary 2017). A number of randomized controlled trials (RCTs) have studied the effects of programs aiding reintegration of former prisoners on recidivism. Some of these studies found significant impacts, others did not (Chalfin and McCrary 2017). Further theoretical analysis of the interaction between rewards and deterrence may point toward new policies and new directions of empirical research.

3.6. The Missing Welfare Analysis

Social welfare is at the center of the optimal deterrence theory. Granted, the economic analysis of crime occasionally sets the welfare-maximization objective aside. The analyst simply presumes that certain acts “definitely are undesirable” because their “harm done exceeds any legitimate private benefits” (Kaplow 1992, 3). If one investigates the most cost-effective way of achieving a particular level of deterrence, ignoring social welfare makes sense. But in general, the clear objective of the optimal deterrence theory is to identify the acts that should be deterred, the socially optimal level of deterrence, and the least costly combination of enforcement tools that achieves that level. The theory’s goal is to devise welfare-maximizing legal regimes.

Given the theoretical focus on welfare, one would think that the literature would develop analytical frameworks that would connect empirical findings to welfare evaluation. Yet with few recent exceptions discussed later (Abrams 2013; Chalfin and McCrary 2018; Yang 2017), this connection has gone missing.

Most of the empirical criminal deterrence research focuses on estimating elasticities. Chalfin and McCrary (2017) summarize and discuss the literature. They conclude that elasticity of crime with respect to police (meaning the percentage increase in index crimes in response to a 1% increase in the per capita number of police officers) is likely no greater than one and ranges from −0.2 for burglary to −0.7 for murder. The elasticity of crime with respect to incarceration (i.e., the percentage increase in index crimes in response to a 1% increase in the number of incarcerated individuals) is likely to be smaller, falling between −0.1 and −0.7, with “most recent estimates falling in the low end of that range” (Chalfin and McCrary 2017, 26). The precision of these estimates is not high. “Consequently, we still
know little about the elasticities that are central to a social welfare evaluation” (Chalfin and McCrary 2018, 168).

Estimating elasticities, however, is only the first step in a welfarist analysis. As Levitt (1997, 285) pointed out, “even if the impact of police on crime was known with certainty, the social costs of crime are not.” The social value of police, he noted, is not limited to reducing index crimes. Police also reduce crimes and other offenses that are not included in the FBI crime index. Moreover, police engage in socially valuable non-crime-related activities such as emergency medical responses. At the same time, police occasionally infringe on civil liberties and abuse their authority, imposing disproportionate costs on the poor and racial minorities. And, of course, extra police must be financed with extra tax dollars. Raising taxes gives rise to deadweight loss and administrative and compliance costs.

In the end, Levitt’s (1997, 270) “highly speculative cost-benefit analysis” concludes that even given very large elasticities that he identifies, the “null hypothesis that the marginal social benefit of reduced crime equals the costs of hiring additional police cannot be rejected.” Marvell and Moody (1996, 633) conclude that “cost-benefit calculations are not possible because one must include much more than police costs, and at present there is no basis for determining just what those other costs are.” Helland and Tabarrok (2007) are likewise noncommittal.

Similar questions bedevil the cost-benefit analysis of imprisonment. The elasticity of crime with respect to incarceration is usually estimated by focusing on index crimes. Yet many other crimes, including misdemeanors, lead to periods of incarceration. Nor are the costs of imprisonment limited to the government’s expenditures on the prison system. Imprisonment is obviously costly for the imprisoned as well as for their family members (GAO 2017, 25). Even without these complications, Donohue (2009, 320) finds it challenging to arrive at a clear conclusion about the cost-efficiency of the current level of incarceration, high as it is by comparison to the rest of the world. Moreover, if one considers reducing prison sentences, asking whether this reduction would increase crime is one of many possible questions. One can also ask, for example, what would happen if we reduce prison sentences and redeploy the saved resources toward additional police (Durlauf and Nagin 2011).

Although the last question may indeed be “provocative” (Chalfin and McCrary 2017, 40), many more provocative questions surely exist. Would it increase welfare if we shorten prison sentences and

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18 The work of Lee and McCrary (2017) is a notable exception. Their data include all recorded felonies in Florida from 1989 to 2002 [Lee and McCrary 2017, 83].
make unemployment insurance more generous? Or increase the main US wage subsidy—the earned income tax credit for individuals without dependents? Or boost expenditures on childhood education for disadvantaged children? These kinds of inquiries are scarce in the criminal deterrence literature. Research by Donohue and Siegelman (1998), Heckman and Masterov (2007), and Aos and his colleagues (Cohen 2005, summarizing results) are rare exceptions that prove the point. Welsh, Farrington, and Gowar (2015, 448) offer a review of 23 benefit-cost analyses of various crime prevention programs while noting that “benefit-cost analysis continues to be underused in study of crime prevention.”

In some respects, the paucity of benefit-cost analyses may not be a major problem. Imagine, for example, that the elasticity of crime with respect to imprisonment is close to zero. This is not a far-fetched hypothetical given Lee and McCrary’s (2017) recent findings. If an enforcement instrument, such as incarceration, is both costly and futile, one does not need sophisticated modeling to conclude that the instrument should not be used. To take another example, consider a policing strategy that changes the manner in which police are deployed without changing their total number. This, too, is by no means pure speculation. Evidence suggests that hot-spot policing, problem-oriented policing, and citywide police redeployments all reduce crime (Chalfin and McCrary 2017, reviewing numerous studies). When deterrence benefits can be achieved at no additional cost, the policy maker’s choice is rather simple. But in general, the apparent lack of models that would allow estimation of social welfare effects of various enforcement measures as well as broader social policies is a significant missing piece.

Chalfin and McCrary (2018) tackle this issue head-on. They construct “a framework for deriving the optimal number of police” in a few intuitive steps (168). First, they cost weight the standard police elasticity of crime, reflecting the fact that both the elasticities and the costs of different index crimes vary. Second, they scale this cost-weighted elasticity by the ratio of the expected cost of crime to the cost of police. Finally, they use this scaled, cost-weighted elasticity, which can be viewed as a benefit-cost ratio, to calculate the “social dollars saved from increasing spending on police by $1.00” (182).

And yet some programs continue to be funded despite growing evidence that the programs lack any crime prevention benefits (Cohen 2005, 91).

Even this seemingly clear case is not as clear as one might think. If the crime reduction is accompanied by extra (or extra aggressive) policing of poor and nonwhite neighborhoods, distributional issues (broadly understood) would immediately arise. These issues are discussed later on.
Their analysis accommodates “heterogeneity across persons, crowd-out of private precautions by government investments in policing, and externalities in private precaution” (168).

They conclude that medium-size and large US cities are under-policed due in large part to a very high social cost of murder. This conclusion is subject to many of the limitations discussed earlier in this section [though some of them, such as the socially valuable effects of police on non-index crimes, would strengthen the conclusion]. Even so, the work of Chalfin and McCrary (2018) is the most advanced effort to execute the optimal deterrence analysis of actual law enforcement policies.

Several other scholars have recently adopted a different approach to assessing the welfare impact of changes in specific components of the criminal justice system. Rather than devising a sufficient statistics model based on plausibly ascertainable elasticities, they work from the ground up. Yang (2017) identifies the likely costs and benefits of pretrial detention both for the society and the detainees. She then marshals the best empirical estimates of these costs and benefits while acknowledging that her cost-benefit analysis is only partial and the estimates are speculative and imperfect. She concludes that the net welfare cost of pretrial detention is approximately $55,385–$101,223 for a marginal detainee, suggesting significant welfare losses from current US pretrial detention policies. Although benefit-cost analysis of crime control measures is not a new idea (Cohen 2005; Welsh, Farrington, and Gowar 2015), the most credible source for Yang’s (2017) study is a very recent investigation that produced first-of-a-kind welfare estimates of the bail system in the United States (Dobbie, Goldin, and Yang 2018). An earlier effort in the same spirit is the work of Abrams and Rohlfs (2011).

Abrams (2013, 968) evaluates the welfare effects of imprisonment reforms. He “breaks new ground” by offering cost-benefit analysis based on recent, causal estimates of various deterrence and incapacitation effects. He finds that a truly one-time release of the least dangerous prisoners is the most cost-effective reform. A reclassification of some crimes (mostly downgrading nonviolent offenses) also yields benefits in excess of costs. Abrams (2013, 969) believes that his work reflects a new “era of a scientific approach to criminal justice policy.” Although other contributions just discussed support this view, progress has been slow thus far. Even the basic cost-benefit analyses are rare, and more advanced theoretical efforts are in their infancy.21

21 I use the term “basic” to refer to the idea of comparing costs and benefits, not to the task of executing this idea in the criminal deterrence context, which is by no means basic.
Overall, as this section’s discussion of the optimal deterrence theory makes clear, there is room to expand some aspects of the theory and to develop some important neglected areas. The next section turns to empirical challenges.

4. ADDING TO EMPIRICAL RESULTS

The main empirical questions facing criminal deterrence scholars are well-known and exhaustively covered in recent reviews (Chalfin and McCrary 2017; Levitt and Miles 2007; Nagin 2013). The following discussion considers whether answers to some of these questions can be clarified by looking outside of the criminal deterrence literature. For some questions, such outside perspective is indeed highly informative; for others, less so.

4.1. Getting Help from the Outside

The earlier discussion emphasized that the empirical analysis of crime is mostly the study of index crimes. These are seven offenses of major social importance but of limited range as objects of study. Yet are more data truly unavailable? Are criminal deterrence scholars missing a chance to consider offenses that could improve our understanding of crime and punishment?

Consider tax enforcement. Its economic analysis finds little reflection in the criminal deterrence literature. But the substantive connection between the two research programs is tight indeed. First, the entire theoretical analysis of tax compliance is, in legal terms, the analysis of tax evasion. Although legally questionable tax positions are pervasive, they are simply ignored. Tax compliance models are models of tax fraud (Raskolnikov 2006, 610). And tax fraud is a crime as a matter of law, or at least it may be. Second, all tax noncompliance, fraudulent and otherwise, fits comfortably under the rubric of white-collar offenses. And, third, the foundational economic model of tax noncompliance (Allingham and Sandmo 1972) is based on Becker (1968).

In other words, compared with the criminal deterrence research, the economic analysis of tax enforcement studies the same subjects (i.e., people and organizations) while testing the same model by using the same econometric techniques. Moreover, tax enforcement analysis takes advantage of both RCTs and very large data sets containing

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22 Tax fraud may be a criminal or a civil offense. The main difference between the two is the standard of proof (Saltzman and Book 2020, 12.06[5]).
highly granular administrative data—the kind of data that the criminal deterrence empiricists can only dream of. Naturally, one would expect that some of the results in the tax enforcement literature would be quite informative in answering the questions posed by the criminal deterrence researchers.

Much of what was just said about tax enforcement is true of environmental enforcement as well. Other than the IRS, the Environmental Protection Agency (EPA) has data collection systems “that are among the most advanced among federal law enforcement agencies, if not the most advanced” (Simpson and Yeager 2015, 60). Analysis of environmental enforcement relies on the familiar Becker (1968) model and uses standard econometric techniques (Shimshack 2014). Yet there are few connections between the empirical analysis of criminal deterrence and econometric studies of tax enforcement, environmental enforcement, or any other regulatory regime. The following discussion aims to highlight the likely benefits of changing this status quo.

In practical terms, empiricists working on criminal deterrence aim to answer questions that policy makers and the public are asking. Should we hire more police? Should we lock up criminals for longer terms? But although policy makers and the public may ask these questions based on intuition, economists have better reasons. They have a model of behavior, and their empirical research aims “to test whether [the core predictions of that model] hold in the real world” (Chalfin and McCrary 2017, 10). It is in answering this question that the literature on tax and environmental enforcement is particularly useful.

4.2. Does Deterrence Work?

Becker’s (1968) model is a model of deterrence. Its core prediction is that expected sanctions deter future violations, and higher expected sanctions deter more. Given that the sanction for index crimes is imprisonment, the key empirical question is whether longer prison sentences deter more crime.

This question has proved difficult to answer. Although scholars have pointed out a number of reasons for this difficulty, the main one is clear: it is often challenging—if not impossible—to separate the deterrent effect of prison from its incapacitating effect (Chalfin and McCrary 2017; Levitt and Miles 2007). Crime-prison population elasticity studies and the analysis of the effects of sentence enhancements are not particularly informative in answering the question. Conclusions regarding the deterrent effect of capital punishment are mixed at best. The same is true of the studies of individuals’ responsiveness to policy-induced
discontinuities in the severity of sanctions (Chalfin and McCrary 2017, 31–32). Overall, disentangling deterrence from incapacitation remains the “first-order issue” and an “open question” (Chalfin and McCrary 2017, 37).

For better or worse, exceedingly few tax offenders go to prison.23 Yet there is no doubt that variation in the expected cost of noncompliance affects taxpayer behavior. Without incapacitation as an alternative explanation, the conclusion in the tax enforcement scholarship is clear: the deterrence hypothesis captures an important part of human decision making.

Compliance rates are dramatically higher when the expected cost of noncompliance is greater. For income subject to both withholding and information reporting, tax underpayments are almost certain to trigger a payment demand from the IRS and may lead to penalties as well. So the compliance rate for this type of income is almost 100%. If only information reporting constrains underpayments, the compliance rate drops slightly to 95%. But the same rate plummets to 45% when no third-party verification exists [IRS 2019, 14]. This effect has been observed at the micro-level as well. Individual taxpayers pay their taxes fully on income subject to third-party reporting. But when it comes to income that the government cannot verify, according to a tax audit experiment in Denmark [Kleven et al. 2011], the same taxpayers are much more likely to underpay. Having asked a question of whether taxpayers are “unwilling or unable to cheat,” the study’s authors came away with a clear answer—taxpayers are willing indeed (Kleven et al. 2011).

RCTs involving letters from tax authorities confirm the effect of expected sanctions. When a random group of taxpayers receives official letters informing them about the consequences of noncompliance by highlighting sanctions and audit risks, taxpayers file more returns [Meiselman 2018] and report more income [Kleven et al. 2011; Slemrod, Blumenthal, and Christian 2001]. Notably, these findings are not specific to the United States. When it comes to deterrence, it turns out that people respond similarly all around the globe (Bérgolo et al. 2019; Kleven et al. 2011).24

The results are similar in environmental enforcement (Shimshack 2014, summarizing studies). Monitoring and enforcement actions by

23 Although over 2 million adults were incarcerated in the United States in 2013, just 927 people were imprisoned for federal tax evasion not related to illegal activities [Slemrod 2019, 911].

24 The results reported by Bérgolo et al. (2019) do raise questions about the realism of the deterrence model. Among the affected taxpayers who randomly received tax authority letters informing them about likely penalties or audits, taxpayers whose letters
the EPA and state environmental authorities increased compliance with the Clean Air Act regulations among monitored and sanctioned firms in the steel industry, paper and pulp industry, and among coal-fired power plants. Similar results came from studies of enforcement of water quality regulation as well as compliance by oil and gas processors. Likewise, “rule changes increasing liability or penalties significantly reduced hazardous waste violations and toxic releases in the late 1980s and 1990s” (Shimshack 2014, 353, citing studies). Deterrence strategies were also successful in environmental regulation in Canada, China, Denmark, Germany, India, Mexico, and Norway.

None of these results establish that higher expected penalties always lead to fewer violations. One of the main reasons why they may not do so—offenders’ poor understanding of the magnitude of sanctions—is discussed later. Another reason is that some offenses are unique, and offenders committing them may not respond to sanctions as the model would suggest. Murder and the (non)responsiveness to capital punishment are the starkest example (Chalfin and McCrary 2017, reviewing numerous studies). Human decision making is complicated, and it would be foolish to assume that a simple model perfectly describes every aspect of it. Rather, the conclusion bolstered by the tax and environmental enforcement literature is that at the basic level, human beings do take expected punishment into account among other considerations. So if particular individuals do not respond to a particular change in expected sanctions in a predicted manner, economists and policy makers should look for reasons why this basic relationship does not hold in a given case rather than wonder whether the basic relationship exists at all. Deterrence can work, but this does not mean that it always works.

4.3. Does the Likelihood of Punishment Matter?

Another challenge in the empirical criminal deterrence research is to determine how potential offenders respond to changes in the probability of detection. These changes are “operationalized as the study of the sensitivity of crime to police” (Chalfin and McCrary 2017, 5). Determining this sensitivity turns out to be a difficult task. Some econometric strategies fail to produce convincing evidence that any relationship exists at all. And when this relationship is found, the elasticity estimates are not large.
These mixed results raise a question: Do people fail to react to variations in the probability of punishment, as the deterrence model predicts, or do the specific policies studied by economists and criminologists fail to produce the expected results for some secondary reasons? Again, tax and environmental enforcement research helps answer the question.

It turns out that people do care about detection. The starkest examples of the responsiveness to the expected sanctions described in the previous section are actually responses to variations in the likelihood of detection. This is obviously true of the aggregate differences in compliance rates for income that is and is not subject to third-party reporting. Kleven et al. (2011) find the same difference at the micro-level. Pomeranz (2015) finds that detection probability significantly affects compliance with value-added tax in Chile. And a number of effective deterrence-related messages in letters from the tax authorities are about audits and the likelihood of detection (Bérgolo et al. 2019; Kleven et al. 2011; Meiselman 2018; Slemrod, Blumenthal, and Christian 2001). Moreover, when the enforcement regime is discontinuous, such as when the audit rate jumps at a particular income or revenue threshold, taxpayers take notice and bunch just below the threshold (Almunia and Lopez-Rodriguez 2018). An effort to avoid audits is an obvious explanation.

The literature on environmental enforcement comes to the same conclusion. Among the studies mentioned in the previous section in support of the deterrent effects of enforcement, a significant portion involved closer monitoring, threats of inspections, and actual inspections (Shimshack 2014, 352–53). In different countries, during different decades, and for different US statutes, these detection-related enforcement measures improved compliance. The full picture, of course, is complicated. For example, firms appear to react to more than just expected sanctions. “State inspections, federal inspections, state administrative sanctions, federal administrative sanctions, civil penalties, and criminal penalties generate different deterrence effects on average” (Shimshack 2014, 355). Still, evidence that in principle people pay attention to the probability of incurring the cost of noncompliance is strong.

4.4. Do Offenders Practice Rational Substitution?

When rational agents inhabiting the world of economic modeling are deterred from pursuing one illegal activity, they do not turn into model citizens. Rather, they look for alternative possible violations where the offenders would face lower expected punishment. These alternative offenses may be subject to lower nominal sanctions or
lower likelihood of detection. Finding these substitution effects (also known in the criminal deterrence literature as displacement) empirically would confirm the realism of the basic rational agent assumption underlying Becker’s [1968] model. Finding no such effect raises questions about this assumption.

Criminal deterrence studies “typically conclude that displacement is a minor phenomenon, finding either no evidence of displacement or that it is small in magnitude” (Yang 2008, 1). When it comes to geographic displacement, “a majority of the literature finds no evidence of displacement of crime to adjacent neighborhoods,” and some studies “have found that the opposite is true” (Chalfin and McCrary 2017, 18). For example, benefits of effective policing reduce crime near the treated area (Chalfin and McCrary 2017, 18). As for inter-crime displacement, studies finding effects raise further questions. Shepherd (2002a), for example, finds that California’s three-strikes laws significantly deterred burglaries while leading to an increase in larcenies, possibly due to a displacement effect. But Shepherd (2002b) concludes that truth-in-sentencing laws reduced larcenies slightly while increasing burglaries by 20%. Lott and Mustard (1997) and Levitt (1998) both find evidence of inter-crime displacement, though neither study aimed at evaluating its magnitude. Moreover, Levitt’s (1998) results reveal that property crimes are not substitutes for rape and robbery while Shepherd (2002b) appears to find the opposite, as do Lott and Mustard (1997).

Do these findings imply that the rational agent model fails to describe the decision making of actual offenders? Not if one looks outside of the four corners of criminal deterrence. Carrillo, Pomeranz, and Singhal (2017) report what happened when the Ecuadorian tax authority notified some firms that the government possesses third-party information showing that the firms underreported their income. Some of the firms responded by reporting higher income on amended returns. But the same amended returns also showed higher deductions, eliminating almost the entire additional tax liability resulting from the income increase. Slemrod et al. (2017) and Asatryan and Peichl (2017) find similar effects. Yang (2008, 1) reports that increased enforcement aimed at deterring one method of evading import duties reduced the targeted evasion but “led to substantial displacement to an alternative duty-avoidance method.” How substantial? “The hypothesis that the

25 The former result suggests substitution away from “strikeable” offenses that start the two- or three-strike count (burglary being one of them) into nonstrikeable ones (such as larceny). This behavior is fully consistent with rational decision making about property crimes of burglary and larceny. Yet it is not clear why truth-in-sentencing laws that, as Shepherd (2002b, 513) points out, should “cause some offenders to substitute out of violent crimes and into property crimes” have dramatically different effects on the same two property crimes.
reform led to zero change in total duty avoidance cannot be rejected” (Yang 2008, 1). Evans, Gilpatric, and Shimshack (2018) find similar spillovers in environmental enforcement.

All this evidence suggests that violators do indeed look for alternative noncompliance opportunities when the existing ones dry up. If researchers find no such substitution when they study particular forms of policing, it means simply that substitution does not always take place. This conclusion is entirely unexceptional given the multitude and complexity of factors affecting the choices of potential violators and the great variation in the nature of offenses.

4.5. The Black Box of Perceptions

If the empirical analysis of tax and environmental enforcement bolsters the deterrence model, but the observed behavior of criminals does not follow the model’s predictions, what causes this disconnect? One obvious explanation is that people respond to the perceived expected sanctions rather than to the actual ones, and people’s perceptions do not mirror reality closely. This is, indeed, one of the main findings of the perceptual deterrence scholarship (Apel 2013; Nagin 2013). “Most people are not particularly well-informed about criminal penalties” (Apel 2013, 73). However, researchers also find that perceptions of punishment are closer to reality when the punishment is more likely. Thus, the general public has relatively accurate perceptions of sanctions for drunk driving. Eighth and tenth graders have roughly accurate perceptions of various school sanctions. And high school seniors are fairly informed about the punishment for marijuana possession while adults overall are less informed (Apel 2013, reviewing multiple studies).

Tax enforcement research reinforces these findings. People misperceive expected costs of tax noncompliance, and there is some evidence that those who are more likely to underpay their taxes have a better idea of the potential costs involved (Raskolnikov 2009, 702, citing studies). It also turns out that the method of delivering information matters. Taxpayers who received information during a personal visit from a tax inspector responded to it more strongly than those who received a phone call, which in turn was more effective than letter mailings or emails (Boning et al. 2018; Slemrod 2019, reporting unpublished results of Ortega and Scartascini). Given that the information delivered by all these methods was identical, it is likely that taxpayers’ perceptions play at least some role in explaining the differential effectiveness.

A distinctive advantage of tax compliance studies is that they allow researchers to examine the effects of learning. Learning is obviously
important to our understanding of perceptual deterrence. It is easy to explain why entirely rational individuals would have a poor understanding of criminal sanctions. Quite simply, this is information that most people do not need to know. They are not planning to commit crimes, so they do not bother learning about punishment (Apel 2013). But if people are rational, they should respond to information that they do learn. Tax enforcement researchers have been getting glimpses of that learning process. The picture that has emerged so far is complicated.

Some studies find that audited taxpayers start paying more taxes (Advani, Elming, and Shaw 2017; DeBacker et al. 2018). Others discover that subsequent tax payments increase only after audits that uncovered noncompliance. Taxpayers found to be compliant during an audit reduce subsequent payments (Gemmell and Ratto 2012). The lesson is that learning about the expected cost of noncompliance is not a simple, easily predictable process. More information about deterrence is not guaranteed to improve compliance.

Another lesson is that learning is nuanced. Press releases by the Occupational Health and Safety Administration (OSHA) revealing major health and safety violations and related coverage in local newspapers significantly improved compliance of firms located close to the noncompliant site, but the effect declined with distance (Johnson 2020). Similarly, peer facilities in the same industrial sector as the OSHA-sanctioned firm responded strongly to the press releases while firms in other sectors did not respond at all.

To make matters more complicated, tax enforcement research reveals that whatever reactions taxpayers have to new information, those reactions tend to be short-lived. Audits were found to increase tax payments by taxpayers earning easily concealed income, but the effect disappeared within a few years (Advani, Elming, and Shaw 2017; DeBacker et al. 2018). The compliance-enhancing effects of letters from the tax authority did not last long either (Bérgolo et al. 2019). These findings suggest that individuals’ misperceptions are not the only challenge faced by law enforcement agencies. Beliefs about the particulars of the penal system may be not only incorrect but also resistant to change. Recent experimental evidence supports these conjectures (Zimmerman 2020). Given this complexity, criminal deterrence scholars investigating perceptual deterrence and other issues addressed in this section would be wise to take advantage of the findings in related literature discussed here.

5. THREE OPEN QUESTIONS

This review’s last major section takes on three open questions. The unifying theme here is a disappointing one: important as these questions
are, the criminal deterrence literature addressing these questions is lacking or entirely absent on both the theoretical and the empirical sides.

5.1. Celerity: Missing in Action?

“The swifter and closer to the crime a punishment is, the juster and more useful it will be,” said one of the founders of the deterrence theory centuries ago (Beccaria [1767] 1995, 48). Yet celerity (or swiftness) of punishment does not appear in Becker’s (1968) model. In fact, that model is static—it does not reflect time-related considerations of any kind. So while the deterrence theory rested on the foundations of certainty, severity, and celerity at its inception (Chalfin and McCrary 2017), celerity has been largely missing from modern criminal deterrence research.

Beccaria’s argument about the importance of celerity rests on his intuition that when the punishment follows the crime almost immediately, it leads to “the stronger and more lasting... association in the human mind between” the two (Beccaria [1767] 1995, 49). Centuries later, psychologists confirmed this intuition in laboratory experiments “using both rats and college students as research subjects” (Pratt and Turanovic 2018, 192). The results of all these experiments “ended up being rather similar: punishment is more effective when it is immediate, and even brief delays [10–20 seconds] can significantly compromise the effectiveness of the punishment” (Pratt and Turanovic 2018, 192, citing multiple sources).

Perhaps potential offenders are capable of associating the benefit of crime with the cost of future punishment that comes more than seconds later after all. Even if they can, scholars are skeptical. “Implementing celerity of punishment into the criminal justice system in a meaningful way is a practical impossibility. The criminal justice system is not built for speed” (Pratt and Turanovic 2018, 193).

But this conclusion seems overstated. It surely reflects the prosecution of major crimes (or just index crimes) in an idealized criminal justice system. In the real world—and conditional on detection—punishment is swift indeed, especially for low-income suspects. They are arrested and imprisoned upon detection, and they have no resources to post bail, so their punishment could not be more immediate.26 In fact,

26 On January 1, 2020, the State of New York joined New Jersey and California in implementing a bail reform that prohibits judges to set bail for misdemeanors and nonviolent felonies. The reform is controversial, and whether the new rules survive in their current form is in doubt (Asgarian 2020).
for many low-level offenses, the process of arrest is itself stressful and humiliating. One may or may not call it punishment because it does not reflect a judicial verdict, but it surely is costly for the accused (Feeley 1979). In contrast, prosecution for many white-collar offenses does reflect the idealized picture of our justice system where no punishment takes place until the final trial verdict and the exhaustion of appeals.

Given that the celerity of punishment may be relevant as a practical matter—at least no less relevant than either its certainty or severity—what have the deterrence scholars learned about it? The only attempt to embed celerity into Becker’s model appears to be Nagin and Pogarsky (2001). They add a simple discount factor to the basic deterrence formula, with the rate representing the individual’s discount rate and the number of periods representing the extent to which the punishment is delayed. They then empirically estimate both values by giving undergraduate students a hypothetical scenario involving drunk driving. What they discover foreshadows complexity in the study of celerity: although some respondents prefer to delay punishment as rational agents would be expected to do, others prefer to accelerate it instead, possibly out of the desire to get it over with.

Interest in real-world effects of celerity grew after Judge Steven Alm decided to experiment with the consequences of probation violations in Hawaii. Instead of harsh but unlikely sanctions, Hawaii’s Opportunity Probation with Enforcement (HOPE) program introduced light but immediate punishment ranging from a warning to a week in jail. The initial evaluation of the HOPE program showed dramatic effects. A randomly selected group of individuals assigned to the new regime was 55% less likely to be arrested for a new crime and 72% less likely to use drugs (Hawken and Kleiman 2009). Positive results from similar programs came from Texas, Alaska, Kentucky, and Michigan (Hawken 2016, summarizing studies). Kilmer et al. (2013) found similar effects studying a South Dakota program targeting alcohol-addicted offenders. Chalfin and McCrory (2017, 40) concluded that “swift-and-certain sanctions regimes such as that motivated by HOPE . . . seem especially promising.” By 2018, 31 states were implementing the HOPE model in 160 locations (Cullen et al. 2018).

But significant doubts about the efficacy of these programs emerged before long. And some of these doubts relate to the role that celerity plays in program outcomes. An RCT of HOPE-like programs implemented in four separate locations in Arkansas, Massachusetts, Oregon, and Texas with significant involvement of the program’s architect (Judge Alm) and its principal analysts (Angela Hawken and Mark Kleiman) showed no advantage of the HOPE regime over business
as usual (Cullen et al. 2018; Lattimore et al. 2016). Another study of a similar program that tested the effectiveness of swift-and-certain sanctions in a different institutional setting also showed no benefits (O’Connell, Brent, and Visher 2016).

Hawken (2016) and Kleiman (2016) offered several explanations for the absence of positive results in randomized controlled experiments, as did Judge Alm (2016). These included a failure to replicate HOPE’s “caring and therapeutic” nature (Alm 2016, 1202), the complexity and multimodality of the HOPE intervention (Hawken 2016, 1234; Kleiman 2016, 1187–88), the failure to adjust the program to variable local conditions (Hawken 2016, 1232), the choice of locations that already had a successful probation regimes in place (Hawken 2016, 1232), and lack of attention to perceptions of fairness (as opposed to the focus on certainty and celerity, Hawken 2016, 1235), among others. These explanations suggest that uncovering the deterrence effect of celerity (if any) in the real world is a very challenging task. Because earlier studies looking for the same effect mostly failed to find it (Dušek 2015; Pratt and Turanovic 2018, 189–90, reviewing studies), support for Bec- caria’s intuition about the importance of celerity remains to be discovered.

5.2. The Missing Misdemeanors

As the earlier discussion emphasized, misdemeanors are absent from the cost-of-crime estimates. This omission is just one facet of the deterrence scholarship’s failure to address misdemeanor offenses. Over the past decade, a burgeoning literature has highlighted the unique role of misdemeanor enforcement in our criminal justice system (Kohler-Hausmann 2018; Mayson and Stevenson 2020; Natapoff 2018; Stevenson and Mayson 2018). But economists have barely joined the effort. Both the theoretical and empirical sides of the criminal deterrence literature are missing when it comes to misdemeanors.

On the theory side, the optimal deterrence model is a poor fit for many misdemeanors. Granted, some misdemeanors such as simple assault, battery, and burglary are just less harmful versions of related felonies and may be studied using the same theoretical framework. But many misdemeanor prosecutions are pretextual, and they result in guilty pleas by innocent defendants in a mass processing system that has little in common with adversarial adjudication (Natapoff 2019). In these cases, neither the private gain nor the external harm arises from the (nonexistent) offense as the standard model assumes. Nor can these cases be treated as erroneous convictions—something that the basic model readily accommodates. Detailed studies of misdemeanor
enforcement reveal that hauling disadvantaged youth and others (guilty or not) through misdemeanor courts is a feature, not a bug (Kohler-Hausmann 2018). Even when external harms do exist, harms from misdemeanor prosecutions to the accused, their families, and their communities are of major importance as well. Yet these harms are absent from the optimal deterrence model, at least for now.

On the empirical front, there appear to be no studies of the relationship between standard deterrence variables and enforcement outcomes for misdemeanor offenses. Sanctions for misdemeanors vary a great deal from one jurisdiction to the next, and the same is true of the detection probabilities (Mayson and Stevenson 2020). Yet whether these variations affect behavior—and, if so, in what ways—remains to be discovered.

This conclusion may appear stunningly wrong to a scholar of criminal deterrence. And in fact, in one particular context, misdemeanors have been studied a great deal. That context is order-maintenance policing. This term is not clearly defined in the literature, and the underlying approach has been also called by many other names such as broken-windows policing, proactive policing, aggressive policing, and so on. In spite of the different names, the empirical literature on order-maintenance policing “seeks to understand if the intensity of arrests for minor infractions has an effect on the incidence of more serious crimes” (Chalfin and McCrary 2017, 19). Early efforts operationalized order-maintenance policing as the number of driving-under-the-influence and disorderly conduct arrests per police officer (Sampson and Cohen 1988, and later Kubrin et al. 2010; MacDonald 2002 following the same strategy). More recent studies consider all misdemeanor arrests (Corman and Mocan 2005; Harcourt and Ludwig 2006; Kelling and Sousa 2001; Rosenfeld, Fornango, and Rengifo 2007). The effectiveness of order-maintenance policing, however measured, is hotly contested and may well be very low. But what is important for our purposes is that no study that I am aware of investigates the relationship between misdemeanor enforcement and misdemeanors themselves. Rather, all these studies attempt to discover whether misdemeanor enforcement reduces felonies (typically some or all index crimes). These studies and the real-world policing strategies that they evaluate treat misdemeanor enforcement as a means to achieve some other, separate end. This is certainly not the approach taken in other empirical studies of criminal deterrence.

In the past several years, legal scholars have demonstrated convincingly that misdemeanors are an important social problem that should be the subject of a focused, rigorous study (Kohler-Hausmann 2018; Mayson and Stevenson 2020; Natapoff 2018). So when it comes to
misdemeanors, the criminal deterrence literature has some catching up to do.

5.3. The Missing Analysis of Distribution

This review’s final subject is missing from the criminal deterrence literature most decisively and, perhaps, most problematically. Although the state’s regulation of crime (however defined) surely has distributional effects, the criminal deterrence literature ignores these effects almost completely.

Becker’s (1968) model of deterrence excludes distributional considerations. “If the goal is to minimize the social loss in income from offenses, and not to take vengeance or to inflict harm on offenders, then fines should depend on the total harm done by offenders, and not directly on their income, race, sex, etc.” (Becker 1968, 195). The quoted passage refers to fines, not imprisonment. Still, it is noteworthy that at the time of Becker’s writing, the rate of incarceration in the United States was 161 per 100,000; in 2007 that rate peaked at 767 per 100,000 (National Research Council 2014). One may have a different view of what to include in the “social loss” from offenses given this change. Yet Polinsky and Shavell’s (2007) comprehensive review of the deterrence theory does not address distributional issues, despite including a brief discussion of fairness considerations. None of the recent empirical surveys, whether authored by economists or criminologists, talk about distribution. The failure to grapple with the distributional impacts of criminal law enforcement is one of the literature’s greatest failings.27

Many of the issues raised in this review may account for this unfortunate state of affairs. Empirical economists focus on index crimes. Criminals committing them elicit little sympathy, so it may be easier to miss their humanity and that of their families and communities. Misdemeanors are ignored, except as a means of achieving separate ends, as is the state machinery that leads to criminal sanctions for misdemeanor offenders (real or not) without any determination of their guilt (Kohler-Hausmann 2018; Natapoff 2018). The knowledge of the social costs of various offenses ranges from poor to nonexistent. Without knowing the costs, it is impossible to estimate their distribution. In addition, pervasive legal and factual uncertainty is not in the model

27 Research on the relationship between inequality and crime does exist. That research, however, does not investigate the distributional effect of deterrence policies. Rather, it focuses on the theoretical and empirical link between existing inequality and crime (Burdett, Lagos, and Wright 2004; Doyle, Ahmed, and Horn 1999).
because including it leads to indeterminate results [Baker and Raskolnikov 2017; Craswell and Calfee 1986]. So the unequal ability of defendants of different wealth to take advantage of that uncertainty is absent as well.28 And even though the racial disparity in criminal law enforcement is staggering, long-standing, and widely known [Natapoff 2012, 134], race has found only a limited reflection in the criminal deterrence research.

There are ongoing debates about whether police target racial minorities in fatal shootings (Cesario and Johnson 2020; Johnson et al. 2019; Knox and Mummolo 2020; Schimmack and Carlsson 2020), vehicle searches, and pedestrian stops (Antonovics and Knight 2009; Dharmapala and Ross 2004; Knowles, Persico, and Todd 2001; MacDonald and Fagan 2019; Pierson et al. 2020). Resolving these debates is important but hardly sufficient. Yet it appears that the literature has not started to grapple with evaluating the criminal justice system that even without racial targeting likely imposes a crushing burden on the disadvantaged members of the society.

For example, the oft cited estimate is that 1 in 3 male black Americans born today will end up in jail. Although this estimate is speculative [Kessler 2015], even if the true probability is 1 in 6—and even if it would be 1 in 10 if discriminatory policing ceased—it would still be shockingly high and of monumental significance for the distributional analysis of criminal law and law enforcement. Moreover, the fact that we can only speculate about the magnitude of these numbers is itself a failure of academic inquiry. It is also an another example, along with the earlier discussion of the cost of crime, that establishing basic facts about the operation of the US criminal justice system is of utmost importance.

Another likely reason for the literature’s inattention to distributional issues is that an influential theoretical argument supports this position. Distribution, this argument states, should be addressed through the tax-and-transfer system alone. Legal rules, in contrast, should be designed only to maximize efficiency [Kaplow and Shavell 1994]. Criminal law fits squarely under the rubric of “legal rules,” freeing those persuaded by this efficiency-only argument from addressing distributive questions.29

28 More precisely, uncertainty is commonly reflected in deterrence models but only as a binary error rate. Unfortunately, such binary representation fails to reflect the essential aspects of uncertainty as it exists in actual legal disputes [Baker and Raskolnikov 2017].

29 To be clear, this argument does not engage racial discrimination, racial profiling, racism in general, and policy responses to any of these phenomena. The argument does, however, engage all questions of distribution, including those that may arise due to interaction of racism and government policies [e.g., order-maintenance policing].
Kaplow and Shavell’s (1994) claim has been long contested on various grounds. But the most recent objection may be both the simplest and the clearest. Raskolnikov (forthcoming-b) shows that the essential assumption of the efficiency-only argument failed to hold for some of the most consequential social and economic policies of the past several decades. During this time, US trade policy, competition policy, labor policy, immigration policy, and social welfare policy, among others, gave rise to large, unintended distributional burdens. These burdens fell on low-skill, low-education, preretirement age workers who were not well positioned to absorb or deflect them. Yet no offsetting distributional adjustments materialized.

Shavell (1981, 417) recognized early on that the force of the efficiency-only argument depends on one’s “expectation that the income tax would be (or could be) altered in response to changes in legal rules whenever these changes result in a ‘sufficiently important’ shift in the distribution of income.” In 4 decades since these words were published, many important distributional shifts have taken place, yet the tax-and-transfer system has ignored these shifts again and again. Given this repeated failure, Raskolnikov (forthcoming-b) argues, the logic of the efficiency-only argument leads to a conclusion that is the exact opposite of the one originally advanced. If the tax system fails to adequately reflect distributional considerations, the legal system should take them into account at least in some cases.

Even if one is not prepared to discard the efficiency argument as a general matter, it is important to recognize the argument’s particular weakness when it comes to the criminal justice system. Trade economists, industrial organization economists, and labor economists point out that they did worry about the unintended distributional burdens of US trade, competition, immigration, and labor policies decades ago. Their analysis at the time led them to conclude that no significant negative effects existed. Although these conclusions are being contested or abandoned today, they were reached after a serious inquiry (Raskolnikov, forthcoming-b, reviewing the literature).

In stark contrast, there has been no serious effort at any point to analyze the distributional consequences of the dramatic shifts in the administration of criminal justice in the United States. It is no secret that policing, prosecutions, trials, sentencing, and crime itself all disproportionately burden the poor and the minorities (Mayson and Stevenson 2020; Stuntz 2006). But the vast criminal deterrence literature tells us next to nothing about the magnitude of these burdens, their precise location, and their change over time. Without this information, we cannot begin to devise policies aimed at offsetting these burdens through the tax-and-transfer system or the legal system.
Obviously, this review cannot remedy the long-standing inattention to distributional consequences of the sentencing reforms, order-maintenance policing, cash bail, mass incarceration, and other criminal justice policies of the past decades. But if the criminal deterrence research program were to pick one piece identified as missing in this review for a major remedial effort, distributional analysis may well be the one.

6. CONCLUSION

The conclusion that emerges from this review of the missing literature is that the missing parts are far from trivial. Moreover, some of the omissions are more important than others. The following four appear to be particularly consequential.

First, the literature’s subfield most visible outside of the academe is the empirical one, and the focus of the empirical work is skewed heavily toward street crime. This skew is not supported by data. There is no evidence—and surely no consensus—that street crime is a much greater social problem than white-collar offenses. We simply do not know enough about the cost of both kinds of crime to draw conclusions about their relative social importance. The literature’s disregard of white-collar offenses without a basis for doing so creates an appearance that here, as elsewhere, the haves get a pass and the have-nots get the short end of the stick.

Second, the literature’s inattention to misdemeanors and misdemeanor enforcement is a missed opportunity to enrich the theory, expand the empirical results, and connect academic research to a vexing social problem. More importantly, this inattention creates an impression that the literature does not regard a major part of the criminal justice system that affects many millions of mostly poor and nonwhite Americans as a subject worthy of a serious study.

Third, the literature would benefit a great deal from building on the recent efforts to evaluate the costs and benefits of an increasingly broad range of crime-reducing policies. Importantly, there is no reason to limit the benefit-cost analysis to measures reflecting the variables of the optimal deterrence model. Early childhood intervention, for example, is not in that model. Yet it may turn out to be the most cost-effective way of reducing violent crime.

Finally, the impacts of both crime and the criminal justice system vary so obviously, so starkly, and so disturbingly by income and race that a research program that ignores these variations runs a risk of becoming detached from reality.

By disregarding white-collar offenses, by ignoring misdemeanors and misdemeanor enforcement, by forgoing the benefit-cost analysis
of various crime-reducing strategies (especially those not reflected in
the optimal deterrence model), and by failing to address distributional
consequences of crime and criminal law enforcement, the criminal
deterrence literature may well be contributing to the overwhelming,
singular, and unjustified focus of American society and enforcement
apparatus on the forceful deterrence of street crime. At the same time,
the omissions highlighted here present a clear opportunity. Address-
ing them would enrich the literature, expand its appeal and policy rel-
evance, and enable academics to contribute to the effort of setting
the US criminal justice system on the path of long-overdue structural
reforms.

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