RESEARCH ARTICLE

DIRECTIONS IN DETERRENCE THEORY AND POLICY

Arrested Development

Misguided Directions in Deterrence Theory and Policy

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Research Summary

Deterrence theory assumes that objective and subjective sanction risk are positively related. If this assumption holds true, then the theory is useful for guiding criminal justice policy and practice. However, prior research has failed to support the assumption. Prominent review articles have dismissed this literature on the basis of methodological critiques, and they have presented certain requirements for future deterrence research to be considered credible. Informed by these reviews, Nagin, Solow, and Lum's (NSL, 2015) new deterrence theory of policing rests on the assumption of a strong positive relationship between objective and subjective sanction risk. NSL have asserted that their theory has significant policy implications; indeed, they have contended it reveals a need to alter American policing fundamentally. We elaborate why the critiques of the discredited literature are premature, and we suggest that the plausibility of NSL's theory is called into question by this literature, as well as by other logical inconsistencies. We conclude by emphasizing how much remains unknown about sanction perception updating, hot spots policing, and heuristics and biases in offender decision making.

Policy Implications

Our central argument is that NSL's (2015) policy recommendations are premature given that their theoretical model is inconsistent with existing evidence. Pending a better understanding of sanction perceptions, we suggest putting aside dictates for how

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police should best deter offenders and about what constitutes credible deterrence research. Rather, both policy makers and researchers should prioritize efforts to identify the sources of sanction perceptions, while taking seriously the possibility that such perceptions may, in part, be intuitive judgments influenced by well-known cognitive heuristics.

Keywords

Policing, rational choice, sanction perceptions, perceived risk, decision making

Deterrence and rational choice theories of crime, and all social control efforts informed by this theoretical tradition, are premised on two crucial assumptions. The first assumption is that individuals weigh the perceived costs and benefits of crime before offending, and then they choose to offend after calculating a net benefit of crime. The second is that there is a correlation between the actual (or the objective) risk of apprehension and punishment and individuals' subjective beliefs about the risk of apprehension and punishment. Nagin (1998: 5) stressed the policy importance of this second assumption: "[T]he conclusion that crime decisions are affected by sanction risk perceptions is not a sufficient condition for concluding that policy can deter crime. *Unless the perceptions themselves are manipulable by policy*, the desired deterrent effect will not be achieved" (emphasis added).

Several prior studies have tested deterrence theory's second assumption, and none have found any evidence that objective and subjective sanction risk are related (Kleck and Barnes, 2013, 2014; Kleck, Sever, Li, and Gertz, 2005; Lochner, 2007). Paternoster (2010: 804–805) explained that this is "one of the 'dirty little secrets' of deterrence." Logically, the next step for deterrence scholars should be to reconsider the theoretical conceptualization of sanction perceptions and to focus theoretical and empirical attention on identifying potential alternative sources of such perceptions (Pickett and Bushway, 2015). Instead, a series of prominent review articles by Apel and Nagin dismissed these null findings on the basis of questionable methodological critiques (Apel, 2013; Apel and Nagin, 2011; Nagin, 2013a, 2013b). We suggest that such a dismissal is premature.

The methodological critiques outlined in recent reviews of the deterrence literature have erected barriers to testing deterrence theory, and they have legitimized additional theorizing and policy making that takes the validity of deterrence theory's second assumption for granted. The reviews have basically suggested that because of said flaws in prior studies, we can ignore their findings and safely assume that, as postulated by deterrence theory, law enforcement activities exert a causal effect on individuals' sanction perceptions. Nagin, Solow, and Lum (NSL) (2015) recently put forward a new policy theory of deterrence that is informed by these reviews and rests on this assumption. They stated that their theory "has broad implications for crime prevention" (p. 92), suggests "a fundamental shift must occur in the way police operate" (pp. 93–94), and "provides the basis for analyzing the

potential crime prevention effects of formal legal sanctions, informal sanctions," as well as other crime control policies and practices (p. 75).

We contend that there are several concerns with NSL's theoretical model, which we outline in the second half of this article. Most notably, we suggest that their theory cannot be valid unless deterrence theory's second assumption holds true. Our article concludes with a candid discussion of the limits of current knowledge about sanction perception updating and hot spots policing. Drawing on dual-process theories of reasoning, we then suggest promising directions for future deterrence research. First, though, we explain why the methodological critiques used to discredit prior research are misguided.

Why Prior Studies Should Not Be Dismissed

Except for the work on arrest-based updating (Anwar and Loughran, 2011), to which we will return shortly, there is unfortunately no evidence that criminal justice policies or police activities exert any influence on individuals' perceptions of arrest risk. All prior studies examining the correlation between objective and perceived arrest risk have yielded null results (Kleck and Barnes, 2013, 2014; Kleck et al., 2005; Lochner, 2007). However, Apel and Nagin have dismissed this research as deeply flawed and, thus, uninformative about the validity of deterrence theory (Apel, 2013; Apel and Nagin, 2011; Nagin, 2013a, 2013b). They have levied three principal criticisms at these studies, arguing the null findings reflect the use of (1) inappropriate samples, (2) invalid measures of objective arrest risk, and (3) error-prone measures of risk perceptions.

Before addressing these criticisms, it is important to clarify the distinction between *calibration* and *correlational* analyses (Apel, 2013: 73). A calibration analysis examines the accuracy of sanction perceptions by descriptively comparing the levels of subjective and objective punishment risk. A correlational analysis explores whether, even if not perfectly accurate, sanction perceptions are at least positively correlated with objective punishment risk, such that individuals tend to perceive greater punishment risk in places (e.g., counties) that have higher risk. Rational choice and deterrence theories do not necessitate that sanction perceptions are perfectly calibrated to reality; they do, however, assume that such perceptions are at least positively correlated with objective sanction risk. This is a less stringent assumption, but it is nonetheless critical for policy. Unless there is some correlation between objective and subjective risk, there is no basis for assuming that policies or practices that increase actual punishment levels will in any way influence perceptions. All of the studies in question found no *correlation* between objective and subjective sanction risk. This was true regardless of whether the focus was on individuals' perceptions of others' arrest risk (Kleck and Barnes, 2013, 2014; Kleck et al., 2005) or on their perceptions of their personal arrest risk (Lochner, 2007).

Sampling Criticism

The first criticism is that prior research has drawn on general population samples, rather than on samples of offenders (Apel and Nagin, 2011: 424). Apel (2013) contended that this is "surely the most salient" limitation of previous investigations (p. 93) because "[p]opulation-based studies are likely to be composed largely of committed law abiders ... and consequently there is no a priori reason to expect that their risk perceptions will be the least bit accurate" (p. 79). Referring specifically to the null findings in Kleck et al. (2005) and Kleck and Barnes (2014), Nagin (2013b: 247–248) argued that these results "are not surprising ... [because] most of the general public has no intention of committing the types of crimes surveyed in these studies. Thus, there is no reason for them to be aware of the sanction regimes for these types of crime."[1]

This line of criticism is not persuasive for a simple reason: All of the studies in question disaggregated their samples by offending status. Kleck at al. (2005: 650) separately estimated models for arrestees and nonarrestees and found "no evidence that the correspondence [of objective and perceived punishment levels] is any closer among criminals than among noncriminals." Kleck and Barnes (2014: 730) did likewise, and they found that "even among arrestees, perceptions of arrest risk had no significant association with police manpower levels." Lochner (2007: 449) found no relationship between the county arrest rate and perceived arrest risk for auto theft, and he reported that similar results emerged when the sample was restricted to offenders. Readers might question whether an arrest record or prior offending is a good proxy for being "on the margin" for crime. Nagin (2013a: 98), however, was clear on this issue: "[O]ne important segment of the relevant population is people with criminal records. The correlation between past and future offending is among the best-documented empirical regularities in criminology."

Before proceeding, it bears emphasizing that the claim underlying the sampling argument—namely, that "population-based studies are likely to be composed largely of committed law abiders" (Apel, 2013: 79)—is probably incorrect. A large proportion of the public engages in crime; indeed, by the age of 23, between 25% and 41% of the public experiences an arrest (Brame, Bushway, Paternoster, and Turner, 2014; Brame, Turner, Paternoster, and Bushway, 2012). Certainly, an even larger proportion of the public commits arrestable offenses but escapes apprehension. Nonetheless, let us assume that Apel and Nagin are right that only a small proportion of the general public is "on the margin" for the types of serious crimes that have been the focus of prior research (e.g., aggravated assault and burglary [Kleck et al. 2005]) and, thus, is sensitive to objective risk. Even in this case,

1. It seems inadvisable to consider criminal intentions to be the only possible motivation for caring about or becoming aware of the sanction regimes for serious offenses. Other plausible motivations, which may be especially salient for law-abiding citizens, include utilitarian and just deserts concerns about crime control, criminal liability, and punishment. Relative to minor crimes, the public may also have greater access to information about arrests and punishments for serious offenses because they are more likely to be publicized by the media (Beale, 2006; Beckett and Sasson, 2004).

there should still be a correlation between objective and perceived arrest risk unless persons who are on versus off the margin *both respond* systematically and in *opposite* ways to changes in objective arrest risk (Cook, 1980: 220). This seems highly unlikely. As important, as Clarke and Cornish (2001: 33) emphasized, "the rational choice perspective makes no hard-and-fast distinction between offenders and the law abiding," but rather assumes that even "people who have generally avoided criminal choices might cease to do so in the face of overwhelming need or temptation."

Even if we put aside these issues, the evidence provided by Apel and Nagin to support their sampling argument is unconvincing. First, Apel (2013: 86) argued that "When fairly conventional samples are studied, R-square is in the neighborhood of 0.03–0.06 [Pogarsky, Piquero, and Paternoster, 2004; Pogarsky, Kim, and Paternoster, 2005]. When high-risk samples are studied, on the other hand, model R-squared's can be as high as 0.26 [Anwar and Loughran, 2011; Matsueda, Kreager, and Huizinga, 2006]." Unfortunately, closer inspection of the cited studies reveals that Anwar and Loughran (2011) did not report R^2 values and that Matsueda et al. (2006) reported "pseudo-R^2" values for tobit models, which cannot be compared with the ordinary least squares (OLS) models in Pogarsky et al. (2004, 2005). The studies also have very different dependent and independent variables. Therefore, even if the studies all included R^2 values from OLS models, it would still be impossible to determine whether differences in sample composition or model specification accounted for any differences in explained variance.

Second, Nagin (2013a, 2013b) and Apel (2013) identified support for the sampling critique in research showing that offenders and inmates are more aware than the public of the *severity* of criminal sanctions and that the public is at least somewhat aware of the *severity* of available sanctions for common minor crimes (e.g., marijuana possession and DUI). The cited studies of public knowledge focused on geographic or temporal variation in levels of knowledge about sanction *severity* after large-scale legal publicity campaigns or changes in legal statues (e.g., decriminalization).[2] For example, Apel (2013) cited MacCoun, Pacula, Chriqui, Harris, and Reuter's (2009: 363) study of residents of states with and without a decriminalization policy, which found that 37% and 40% of the states' populations, respectively, believe that a possible or mandatory prison sentence is the maximum penalty for first-time possession and that 30% and 32%, respectively, report not knowing the penalty.

However, it is a mistake to focus only on knowledge about sanction *severity* because it provides an inaccurate picture of sanction perceptions. First, there are important differences between knowledge about the *severity* of available sanctions and perceptions of the *certainty* of arrest. As Apel (2013: 73) observed, "perceived certainty of punishment is a much stronger

2. The one exception is the quasi-experimental study by Ross and Voas (1990: 65), which found modest differences in perceived arrest risk for DUI between two convenience samples ($N = 332$ and 275) of drivers in two different cities.

deterrent than perceived severity. Often, perceived punishment severity is uncorrelated with individual criminal behavior." Second, information about the *severity* of available sanctions, unlike that for the *certainty* of arrest, is widely available. It can be found in legal statutes and sentencing guidelines; is occasionally provided in public service announcements and media coverage; can be gleaned from the graduated nature of sanctions; and is less contingent on the time, place, and persons involved (e.g., the statutory penalty for a given offense does not vary across local neighborhoods and does not depend on offenders' skill/intelligence, the presence of witnesses, victim characteristics or reporting tendencies, or police patrolling behaviors). Information about sanction *severity* is especially available and accurate for sanctioned offenders. After offenders are arrested and punished, they know exactly what sentence they received, but they still do not know the precise probability of arrest for the crime they committed.

Third, the extant evidence on the *certainty* of arrest is inconsistent with the argument that being "in the market," or "on the margin," for a given type of crime increases individuals' motivation to seek out information about its arrest risk and, in turn, improves the accuracy of their risk perceptions. For example, the public's perceptions of arrest risk are not accurate for minor offenses despite many citizens being "in the market" for such crimes. The objective arrest risk for DUI is *far* less than 1%, ranging from 1/200 to less than 1/1,000 (Beitel, Sharp, and Glauz, 2000; Ross, 1992); yet the average American estimates the arrest risk to be 35% (Piquero, Piquero, Gertz, Bratton, and Loughran, 2012). Likewise, studies using samples of college students, many of whom are clearly in the market for DUI (Loughran, Paternoster, and Thomas, 2014), have found that the average respondent perceives the arrest risk for DUI to be 28% to 33% even when provided incentives for accurate responding (Loughran et al., 2014; Piquero and Pogarsky, 2002; Pogarsky and Piquero, 2003). Research examining perceived arrest risk for smoking marijuana, texting while driving, petty theft (stealing less than $10), and vandalism, among samples of high-school and college students, has found that the average respondent perceives the risk of arrest for these common crimes to be between 18% and 31%, 16% and 21%, 29% and 40%, and 27% and 34%, respectively, depending on school year and question wording (Loughran, Paternoster, Piquero, and Fagan, 2013; Loughran et al., 2014; Pogarsky et al., 2004). These risk perceptions are clearly overestimations of arrest risk: "[T]he risk of detection of any single criminal act is quite low—*practically zero*" (Apel, 2013: 79, emphasis added).

There is also no evidence that offenders are more motivated, by dint of being in the market for crime, to seek out accurate information about arrest risk. To be clear, the distinction here is between intentionally acquiring knowledge through deliberate information-seeking behavior (proactive learning) versus inadvertently acquiring knowledge through experience (reactive learning). Apel (2013: 93) specifically argued that the sanction perceptions of offenders and nonoffenders will differ because of *proactive learning*: "[A]cute conformists . . . have no incentive to invest time and energy into calibrating their subjective probabilities against punishment actualities" (p. 93). Nagin (2013b: 247–248) made a similar argument.

Studies have found that prior experiences with offending and arrest do influence individuals' perceptions of arrest risk (Anwar and Loughran, 2011; Pogarsky et al., 2004, 2005). However, acquiring information from one's past experiences represents reactive rather than proactive learning. If, as Apel and Nagin have suggested, being in the market for crime motivates individuals to seek out information about arrest risk proactively, then the accuracy of risk perceptions should depend more on offender status per se than on personal rates of offending or arrest. There is very little evidence that offenders are proactive learners. Matsueda and colleagues (2006), for example, found that high-rate offenders (i.e., those who had committed ten or more offenses) report significantly lower risk perceptions than nonoffenders (about ten percentage points lower). By contrast, there were much smaller differences between nonoffenders and both low- and medium-rate offenders, and most of these differences were not significant. Thus, perceived arrest risk varies primarily by offending *rates* and not by offender *status*, which suggests reactive rather than proactive learning. In addition, even criminals who are experienced with a particular type of crime often report very high overestimates of arrest risk (e.g., average perceived arrest risk for auto theft among those who have stolen a car = 45%, objective risk = 10%; Lochner, 2007: 447–448), which does not support the argument that offenders seek out accurate information about arrest risk.

Clearance Rate Criticism

The second criticism of extant research on the link between objective and perceived arrest risk is that researchers have used invalid measures of objective arrest risk. Typically, objective arrest risk is measured as the county-level ratio of arrests to offenses known to police, often called a "clearance rate" (Kleck et al., 2005; Lochner, 2007). However, similar to NSL (2015), Nagin (2013b: 248) argued that (1) "as first pointed out by Cook (1979), the ratio of arrest per crime is not a valid measure of the risk of apprehension for criminal opportunities that are not acted on," and (2) such "county or city level [statistics] may be very poor indicators of risk at specific locations where would-be offenders are plying their trade" (also Apel, 2013; Pogarsky, 2009). Both of these criticisms are questionable. Before proceeding, it is important to point out that Kleck et al. (2005) also included measures of the objective severity and swiftness of sanctions, *none of which are susceptible to either of the previous measurement critiques.* They found that these measures are not correlated with perceptions of sanction severity and swiftness among either arrestees or the public. It is hard to dispute the validity of county-level measures of the number of "adults sentenced to prison per 100 adults convicted," the "average maximum sentence imposed," and the "average number of days from arrest to sentencing" (Kleck et al., 2005: 633–634). Nor, for a given crime, should these aspects of punishment risk vary substantially across micro-places within a county. Unfortunately, Apel and Nagin overlooked this fact.

NSL (2015) asserted that "Cook demonstrated that the clearance rate is not a valid measure of the risk of apprehension posed by the police" (p. 79). However, Cook (1979,

1980) did not demonstrate this point; he only argued it. And his argument hinges on a key assumption that has yet to accumulate empirical support. He assumed that there is a relationship between offenders' perceptions of sanction risk and the clearance rate that is mediated by their "adaptive choices" (Cook, 1979: 141). To date, there is no evidence that such a relationship exists (see earlier discussion). Endogeneity necessitates correlation. The observed clearance rate is unlikely to be an outcome of offenders' sanction perceptions if it is not correlated with those perceptions.[3] Cook (1979: 141) recognized this fact: "There is only one circumstance in which an increase in CJS effectiveness is reliably signaled by a change in the clearance rate ... [when] the increase in effectiveness is *not perceived*" (emphasis added).

Cook (1979, 1980) and others (Apel, 2013; Nagin, 2013a, 2013b) raised three other points about the clearance rate: (1) It can "at best ... be viewed as a measure of the *average* probability of punishment for crimes committed" (Cook, 1980: 241); (2) it overestimates arrest risk because it excludes offenses not reported to police; and (3) it underestimates arrest risk because it excludes "deterred" crimes. None of these points, even if accurate, necessarily have implications for whether the clearance rate (or arrest ratio) can be used to study the correlation between objective and perceived arrest risk. Even if the clearance rate overestimates or underestimates objective arrest risk, this would only bias estimates of the correlation between objective and perceived arrest risk if the degree of over- or underestimation varied significantly across the geographic areas being examined (e.g., cities and counties). Even then, the impact of such variation on the correlation between objective and perceived arrest risk would depend on whether it was random or systematic; in the case of systematic variation, the resulting bias might inflate the correlation between objective and perceived arrest risk (e.g., if there is a positive relationship between city- or county-level reporting tendencies and perceptions of arrest risk). The key point is that, given the current lack of evidence about these issues, it is speculative to suggest that the use of the clearance rate as an indicator of objective arrest risk results in measurement error that biases the correlation between objective and perceived arrest risk, much less that it biases the correlation *toward zero*.

Finally, it is misguided to suggest that the null findings for the correlation between objective and perceived arrest risk can be dismissed because "county or city level [statistics] may be very poor indicators of risk at specific locations where would-be offenders are plying their trade" (Nagin, 2013b: 248). The clearance rate for a county or city necessarily reflects the weighted average of clearance rates for all micro-places within that county or city. Micro-places within counties/cities with high clearance rates should, on average,

3. It is possible that offenders' perceptions of arrest risk may have an indirect effect (via their choice behavior) on the clearance rate even in the absence of a zero-order effect (e.g., in the case of competitive mediation; Zhao, Lynch, and Chen, 2010). In this situation, however, it would be unlikely that the relationship between offenders' perceptions and the clearance rate would substantially bias the latter as a measure of objective arrest risk.

have higher clearance rates than equivalent micro-places within counties/cities with lower clearance rates. In turn, if there is a relationship between objective and perceived arrest risk, then residents of counties/cities with high clearance rates should be more likely than those in counties/cities with low clearance rates to perceive a high arrest risk for the micro-places within their county/city.[4] County- or city-level measures of arrest risk should also be far more reliable than indicators for micro-places because the former are based on a much larger sample of crimes than the latter.

Measurement Error Criticism

Finally, Apel and Nagin (2011: 424) argued that "articulations of risk perceptions in survey instruments are likely subject to a large degree of measurement error that will attenuate the strength of the correlation between perceptions and actual punishment risk." Their argument is that the null findings in prior studies may simply reflect error in measuring perceived arrest risk. There are two important points to make about this argument. First, although it is certainly true that self-reported risk perceptions are measured with error (Fischhoff and Bruine de Bruin, 1999; Manski and Molinari, 2010), there is considerable evidence supporting the validity of such perceptions and showing that respondents are willing and able to provide meaningful probability estimates (Dominitz and Manski, 1997; Manski, 2004). Apel (2013: 94) even emphasized this point, noting that although not without limitations, "the probabilistic measures of risk perceptions that have been used in perceptual deterrence research would seem to be *the most desirable* relative to other response formats, [in part] because ... they can be directly compared or calibrated to objective punishment risks" (emphasis added).

Second, there is no evidence that the measurement error in sanction perceptions is so extensive as to attenuate relationships fully when they exist and are of a nontrivial size. Prior studies have been able to identify correlations between sanction perceptions and many other factors, such as respondents' demographics, personality traits, prior offending and arrest, peer offending, and criminal intentions or future offending (Anwar and Loughran, 2011; Matsueda et al., 2006; Piquero and Pogarsky, 2002; Pogarsky et al., 2004, 2005; Van Gelder and De Vries, 2012). For example, although Lochner (2007) found no relationship between objective and perceived arrest risk, he was able to observe relationships between risk perceptions and personal and vicarious offending experiences. Similarly, researchers have had no difficultly identifying both the individual and contextual correlates of probabilistic risk perceptions for other events (e.g., perceived victimization risk and survival expectations; Quillian and Pager, 2010; Warner and Swisher, 2014).

4. The same reasoning applies to individual differences in objective arrest risk. Individual-level factors that increase or decrease arrest risk (e.g., IQ) would normally be expected to do so consistently across counties with different arrest rates, such that a person with a low IQ in a county with a high arrest rate should have a higher arrest risk than a comparable person with a low IQ in a county with a low arrest rate.

Critically Assessing NSL's (2015) Deterrence Theory of Policing

In the preceding pages, we reviewed Apel and Nagin's three most prominent criticisms of the collection of studies that has consistently found no relationship between objective and subjective sanction risk. We explained why these three critiques are unfounded. Unfortunately, because of the stature of their authors, these critiques have cast very long shadows and have in effect "disappeared" the respective studies from the discourse. The best evidence of this is the fact that NSL (2015) did not discuss the studies' findings, thus, treating them as if they did not exist.[5] As we will explain, this is unfortunate because the findings of this literature have important implications for the validity of NSL's theory.

NSL (2015) theorized that "being a sentinel is the key to an officer's and agency's ability to reduce crime."[6] Their central proposition is that the crime rate will decline (even though the clearance rate may also decline) if police, through their sentinel role, increase arrest risk (p_a) for attractive criminal opportunities above offenders' maximum level of acceptable arrest risk for the given opportunity (p_a^*). NSL argued that their theoretical model should guide police activities. Given the potential policy importance of their model, we believe a serious evaluation of its key arguments is warranted. In this section, we outline several limitations of NSL's theory, which call into question both its plausibility and its policy implications.

Before proceeding, however, it is important to explain the theoretical and policy significance of aggregate sanction perceptions. One anonymous reviewer suggested that Bayesian learning theory allows everyone "to have their own mean [perceived arrest risk]," and thus, "the notion that there should be one meaningful, global mean in the population which we care about is simply not credible." Although Bayesian learning theory does allow for individual means, it does not diminish the importance of the global mean. Individual means always aggregate into a global mean, and thus, their properties (accuracy and correlation with objective risk) necessarily have implications for the properties of the global mean. Additionally, as Cook (1980: 220) made clear, "deterrence theory is concerned with making predictions about aggregate behavior." The effectiveness of police policies and practices for deterring crime depends on their ability to increase aggregate sanction perceptions. NSL (2015) were also foremost concerned with aggregate sanction perceptions and aggregate behavior. They specified equations for the crime rate and clearance rate as functions of the global level of maximum acceptable arrest risk (p_a^*). Likewise, their call to shift the focus of policing presupposed that such a shift could have a meaningful net effect on arrest risk perceptions and crime at the aggregate level.

5. NSL (2015) also did not discuss meta-analytic evidence showing that sanction perceptions, including perceptions of the certainty of arrest, have modest-to-negligible relationships with offending when compared with other predictors of crime (Pratt, Cullen, Blevins, Daigle, and Madensen, 2006).

6. NSL (2015) conceptualized police sentinels as officers who proactively patrol the streets to prevent crime through the projection of a high objective arrest risk (p. 78).

NSL Conflated Objective and Perceived Arrest Risk

NSL (2015) were vague in conceptualizing apprehension (or arrest) risk (i.e., p_a), and they did not distinguish conceptually between objective and perceived arrest risk. NSL's model focuses on the role of *perceived* apprehension risk (p_a) in criminal decision making (p. 80) but "formally ... requires that the long-run rate of apprehension of persons who *actually victimize* potential targets of crime with the same p_a equals p_a for those targets" (p. 84).[7] To be clear, this is an assumption of perfect calibration between objective and subjective arrest risk. Their equations for the offending decision (p. 80), the crime rate (p. 83), and the clearance rate (p. 83) all include a single (and identical) p_a term, thereby treating objective and perceived arrest risk as interchangeable. They discussed this limitation only briefly and indirectly: "[O]ur model abstracts from how police presence actually influences both actual and perceived apprehension risk" (p. 95). As noted, however, this is the one issue from which deterrence theorists cannot abstract. Deterrence theory's policy relevance hinges on the relationship between objective and perceived arrest risk (Kleck et al., 2005). Even Nagin (2013a: 97) has emphasized this point repeatedly in his work: "*Establishing the linkage between risk perceptions and actual sanction regimes is imperative* ... Unless perceptions adjust, however crudely to changes in the sanction regime, the desired deterrent effect will not be achieved" (emphasis added, see also Nagin, 2013b: 204, and Nagin, 1998: 5).

Because NSL (2015) conflated objective and perceived arrest risk, both in their equations and in their text, several of their specific arguments are of questionable validity. For example, they asserted that the clearance rate "is always less than p_a^* because it is averaging only over all $p_a \leq p_a^*$" (p. 83). Here, as in their equation for the crime rate, their focus was on the global p_a^*—that is, the aggregate maximum acceptable level of arrest risk. Their contention was that the clearance rate necessarily underestimates arrest risk because it only measures the average arrest risk across criminal opportunities where the arrest risk was too low to deter offenders. This argument is problematic for three simple reasons. First, many offenders do not even consider arrest risk before offending. For example, in one study, between 25% and 64% of inmates, depending on the crime, reported not thinking about the risk of arrest when they committed their offense (Anderson, 2002: 303). Such offenders clearly may victimize targets where $p_a > p_a^*$ or overlook targets where $p_a \leq p_a^*$.

Second, criminal decision making is responsive to *perceived* arrest risk, not to *objective* arrest risk. Therefore, NSL's argument that the clearance rate "is always less than p_a^*" hinges on the strong assumption that criminals tend to perceive a high arrest risk in situations characterized by a high objective arrest risk and, in turn, decline to commit crime. Unless perceived arrest risk is correlated with objective arrest risk, there is no basis

7. NSL's (2015) conflation of objective and perceived risk is clear in their suggestion that the extent to which a target is perceived as profitable is necessarily affected by police performance: "If police slip in their effectiveness as apprehension agents ... this will have the effect of reducing p_a below .3, which in turn will make it an even more attractive target for victimization" (p. 84).

for estimating the level of *objective* arrest risk in uncommitted versus committed crimes.[8] As noted, in specifying equations for the objective crime and clearance rates as functions of sanction perceptions, NSL made an even stronger assumption; they assumed that subjective arrest risk is not only correlated with, but is also accurately calibrated to, objective arrest risk. Third, but not least, beyond point estimates of arrest risk, *ambiguity* in perceived risk also influences offending decisions (Loughran, Paternoster, Piquero, and Pogarsky, 2011). However, NSL failed to account for the effects of ambiguity in any of their equations.

Likewise, NSL (2015: 80) argued that "p_a widely varies across criminal opportunities ... [spanning] the full range of allowable values of a probability—0 to 1."[9] Again, the validity of their argument is questionable because they did not distinguish between objective and perceived arrest risk. Three types of contradictory evidence are instructive. First, although objective arrest risk may vary as widely as NSL suggested, extant evidence shows that *perceived* arrest exhibits less variance across offenses.[10] Kleck et al. (2005: 640), for instance, found that although there is a very large difference in the objective arrest rates for homicide and robbery (90% vs. 27%, respectively), the perceived arrest rates for these offenses are much closer (52% vs. 43%, respectively). Erickson and Gibbs (1978: 260) similarly reported that "whereas the *objective* certainty of arrest for vehicle theft is over four times that for grand theft (.181 to .042), the two scarcely differ as to the *perceived* certainty of arrest (.394 and .383)." They explained that "once the objective certainty reaches a fairly low level (say below .200), respondents persist in estimating the certainty of arrest for those types of crimes as fairly high (above .300)" (pp. 259–60). Some respondents do estimate arrest risk to be 0% or 100%, but such responses tend to reflect measurement error from rounding (Manski and Molinari, 2010), imprecise probability beliefs (Hudomiet and Willis, 2013), or stylistic responding resulting from a desire for security or certainty (Brim, 1955; Pickett, Loughran, and Bushway, 2015).

Second, NSL overlooked the fact that subjective arrest risk, unlike objective arrest risk, is not simply a function of the characteristics of the criminal opportunity and context. For example, personality traits influence both the formation and the updating of sanction

8. Extant evidence suggests that individuals tend to underestimate arrest risk for offenses and in situations where the risk of arrest is greater than 50%, but they overestimate arrest risk when arrest risk is less than 50% (Erickson and Gibbs, 1978; Kleck et al., 2005). This may reflect a tendency for individuals to anchor off of the 50% mark when estimating probabilities (Brim, 1955; Hurd, 2009).

9. NSL's (2015) argument that sanction perceptions are opportunity specific runs directly counter to Apel and Nagin's (2011: 424) suggestion that "rather than forming crime-specific estimates of the certainty, severity, and celerity of punishment, the average individual (but not necessarily the average offender) might instead rely on an omnibus assessment or some generalized conception of punishment."

10. To our knowledge, no published study of either offenders or nonoffenders has ever found a mean perceived apprehension risk for any offense described in any scenario greater than 72% to 73% (e.g., attack someone to hurt or kill them or drug violation during an intensive supervision program) or less than 12% to 15% (e.g., underage drinking or texting while driving) even when incentives are provided for truthful responding (Anwar and Loughran, 2011; Lochner, 2007; Loughran et al., 2013, 2014; Pogarsky, 2007). Rather, for most crimes in most scenarios, mean perceived arrest risk falls between 35% and 65%.

perceptions (Pickett and Bushway, 2015; Schulz, 2014; Thomas, Loughran, and Piquero, 2013; Van Gelder and de Vries, 2012). Third, the characteristics of criminal opportunities often have counterintuitive effects on arrest risk perceptions. For example, Loughran, Reid, Collins, and Mulvey (2016) found that when serious adolescent offenders, most of whom were felons, carried a gun, it actually *reduced* their perceived arrest risk. Yet, gun carrying increases objective arrest risk—felony offenders are susceptible to arrest simply for possessing a gun, and the presence of a gun during crimes greatly increases the likelihood of police notification by victims (Baumer and Lauritsen, 2010).

NSL Overlooked Contradictory Evidence

If valid, NSL's (2015) theoretical model suggests that there should be a sizable inverse correlation between perceived sanction *severity* and offending because the former should influence potential offenders' maximum acceptable level of apprehension risk (p_a*). Although NSL primarily focused on apprehension risk, their equation for p_a* (equation 2, on p. 80) indicates that offenders' maximum level of acceptable arrest risk "can be reduced by . . . increasing sanction cost" (p. 80). They were clear on this issue: "The third way to reduce crime in the context of this model is to create incentives that would cause would-be criminals to reduce the maximum risk of apprehension that they are willing to accept, namely to cause them to reduce p_a*" (p. 87). NSL then explained that "in the context of our model," one approach to doing this would be to "increase perceived sanction cost (S)" (p. 87). So, when holding other factors constant, perceived sanction severity should be negatively associated with offending through its effect on p_a*.

Yet, studies often have found that "perceived punishment severity is uncorrelated with individual criminal behavior" (Apel, 2013: 73), especially when multivariate models are used to control for other relevant factors (Pratt et al., 2006). Nagin (2013a: 75) observed that "the certainty of apprehension, not the severity of ensuing consequences is the more effective deterrent." Additionally, in what is arguably the most comprehensive test of rational choice theory to date, Loughran, Paternoster, Chalfin, and Wilson (2016) found that the perceived severity of *informal* sanctions was not significantly associated with offending for *any* type of crime (e.g., drug or violent) (p. 100) or among *any* subgroup of offenders (e.g., males, females, Blacks, and Whites) (p. 103). This evidence contradicts NSL's theoretical model, which mathematically (equation 1 on p. 80), gives *equal* weight to the perceived certainty and severity of sanctions in criminal decision making, as well as suggests that both should be inversely correlated with offending.

NSL (2015) also overlooked the fact that the only direct evidence that police can influence offenders' sanction perceptions suggests that they do so by acting in their enforcement rather than their sentinel role. As emphasized earlier, NSL provided a limited discussion of the extant perceptual deterrence research. They did not discuss studies examining the correlation between objective and perceived arrest risk. Rather, NSL only cited studies showing that offenders update their perceptions of arrest risk in response to being *arrested* (Anwar and

Loughran, 2011). This is surprising because arrest is a specific mechanism through which policy may affect sanction perceptions. Sentinel activity is an entirely different mechanism, and arrest-based updating cannot occur through such activity. Indeed, according to NSL (2015), when police arrest an offender it "is an outgrowth of a failure in their role as sentinels to have successfully prevented crime from happening in the first place" (p. 84). Arrest-based updating is thus an outcome of police acting in their enforcement role, not as sentinels. Ironically, NSL reviewed the evidence on updating through enforcement, but then they argued that police should shift their focus *away* from enforcement and *to* their sentinel role in order to have the greatest influence on offenders' perceptions of arrest risk. This puzzling argument also appears in earlier versions of the theory, where Nagin provided an even more detailed review of the literature on arrest-based updating, only to argue "that the evidence suggests that the deterrent effect of the police stems primarily from their role as sentinels" (2013a: 99; see also 2013b). Despite his argument, there is, in fact, *not a single existing study* that has found that police exert any effect on potential offenders' perceptions of arrest risk through any activity *other than making arrests*. And such an effect cannot simply be inferred from evidence of a relationship between police activities and crime rates (Apel, 2015).

NSL Overlooked the Implications of Offender Satisficing and Agency

NSL (2015) conceptualized offenders as optimizing but passive calculators. This is clear in their assertion that if "in their sentinel role the police were successful in increasing p_a to a level, say .4, where $p_a > p_a^*$. It would now be the case that the target would *never* be victimized" (pp. 84–85, emphasis added). For now, let us put aside the fact that NSL's argument that a high enough arrest risk can cause a target to "never be victimized" is far too strong given that many offenders do not even think about arrest risk before offending (Anderson, 2002). NSL (2015) provided an example of a "case of $\beta = 2$ and $p_a^* = .4$ ... [where] a tactic could be identified that moved the entire set of opportunities with $.3 \leq p_a \leq .4$ to the region of $f(p_a)$ above p_a^*" (p. 86). They argued that "if this were possible, then the crime rate would decline by 57.0 percent. The clearance rate A also would fall from 30.0 percent to 22.5 percent. The clearance rate declines because the only remaining attractive targets have $p_a < .3$" (p. 86). It should be clear to readers that such calculations were premised on the assumption that perceived arrest risk is closely calibrated to objective arrest risk and cannot be accurate otherwise (see earlier discussion). However, NSL made several other assumptions—namely, that (a) there is a finite set of criminal opportunities, (b) offenders are optimizers and always choose the lowest risk opportunity available, (c) motivated offenders outnumber low-risk opportunities, and as a result (d) offenders without access to low-risk opportunities will always choose to avoid committing offenses of the given type if police sentinels are able to increase the arrest risk for medium-risk opportunities over p_a^*. These assumptions must be true for the crime rate and clearance rate to decline simultaneously as a result of police sentinel activity.

However, criminals are satisficing, not optimizing—they make choices that seem to yield acceptable rather than best-case outcomes (Clarke and Cornish, 2001: 25). Therefore, there is no reason to suppose that the fact that targets with $p_a \geq .30$ are victimized necessarily reflects a shortage of lower risk targets (i.e., $p_a < .30$). Furthermore, if low-risk opportunities are abundant, then police sentinel activity may have no effect on the number of offenders committing offenses of a given crime type but just lead them to select (or displace to) lower risk opportunities (e.g., choose $p_a < .3$ targets). Here, in contrast to NSL's (2015) model, the clearance rate would decline but the crime rate would remain unchanged.

Similarly, NSL (2015) granted offenders the agency to weigh the costs and benefits of crime (p. 80) but they denied them the agency to decide how to respond to police-initiated changes in these costs and benefits. By contrast, if we assume that offenders also have the agency to decide how to respond to police activities, then we must accept the possibility that offenders may simply choose to update their maximum level of acceptable risk (p_a^*) in response to police sentinel activity. To illustrate, let us assume that NSL were correct that there is a limited number of low-risk criminal opportunities. If police sentinels increase the arrest risk for all opportunities of borderline attractiveness ($.3 \leq p_a \leq .4$ for $p_a^* = .4$), then offenders may simply decide that a higher p_a^* must be tolerated. This seems especially likely given the sunk-cost fallacy—decision makers tend to prefer taking gambles with greater risk rather than suffer a sure, but smaller, loss of prior investments (Arkes and Blumer, 1985; Kahneman, 2011). Therefore, offenders who have invested their time, effort, resources, and identity in criminal activities (e.g., drug dealing) or associations (e.g., gangs) may endure increased arrest risk instead of abandoning their investments. Supporting this possibility, prior survey research has found that individuals with previous offending experience are emboldened by increases in enforcement levels (Depoorter and Vanneste, 2005).

Conclusion

The trajectory of deterrence scholarship over the past few years is curious. Empirical evidence contradicting a theory typically warrants a serious reconsideration and revision of the theoretical model. Considerable evidence shows that objective and subjective sanction risk are not related (Kleck and Barnes, 2013, 2014; Kleck et al., 2005; Lochner, 2007) and that the various other factors identified by deterrence theorists as key sources of sanction perceptions collectively explain only a small amount of the variance in such perceptions (Paternoster, 2010). Despite these "dirty little secrets of deterrence" (Paternoster, 2010: 804–805), NSL (2015) put forward a deterrence theory of policing that suggests a need to change American policing fundamentally. Their theory largely rests on an assumption that has yet to accumulate any empirical support—namely, that there is a strong positive relationship between objective and subjective sanction risk. NSL jumped the theoretical gun, we submit, because recent reviews of the deterrence literature by Nagin and Apel

(Apel, 2013; Apel and Nagin, 2011; Nagin, 2013a, 2013b) have discredited the evidence contradicting this assumption.

In reviewing prior research, Nagin and Apel (Apel, 2013; Apel and Nagin, 2011; Nagin, 2013a, 2013b) criticized the use of general population samples, the clearance rate, and "error-prone" measures of risk perceptions in perceptual deterrence research. This is problematic not only because it is speculative, but also because it may inadvertently undercut the validity of future research findings. As but one example, consider the sampling criticism. Nagin (2013a: 98) suggested that "a major issue for studies [on sanction perceptions] is obtaining data from the relevant population"; he argued for studying "people with criminal records" (see also Apel, 2013). Echoing this view, NSL (2015) contended that "the methodological challenge" for researchers studying sanction perceptions is to survey "active offenders" (p. 95). Yet, this approach creates the potential for endogenous selection bias. Offending status is endogenous to sanction perceptions, and therefore it is a descendant of the outcome variable in any model predicting such perceptions (Elwert and Winship, 2014). This is true even if models are estimated that predict sanction perceptions at time t, and condition on offending status at time $t - 1$, because both are descendants of sanction perceptions at time $t - 1$. Offending status is thus a "collider variable" between sanction perceptions and any other independent predictor of criminal behavior (Elwert and Winship, 2014). The implication is that restricting analyses to samples of offenders (i.e., conditioning on the "collider variable") may induce spurious associations between sanction perceptions and other correlates of offending.

Nonetheless, let us assume that we are mistaken and that all criticisms of the extant null findings for the relationship between objective and perceived arrest risk are accurate. Even then, the perceptual deterrence literature would still be too limited, we believe, to justify advocating a fundamental change in policing on the basis of any policy theory of deterrence. Specifically, there is no direct evidence that police size, patrolling behaviors, or any other police activities are positively related to either the public's or offenders' perceptions of arrest risk.

There is evidence that individuals update their perceptions of arrest risk in response to personal and vicarious experiences with arrest (Apel, 2013). It is too soon, however, to know whether this means that police policies and practices can have any effect, much less a meaningful long-term effect, on sanction perceptions through arrest-based updating. First, updating studies have shown that the effect of an arrest is small, is dwarfed by the effects of prior perceived risk and unobserved factors, and depends on the total number of crimes committed (Anwar and Loughran, 2011). Second, the effect of an arrest may be temporary. Lochner (2007: 455), for example, documented that "there is little persistence in the effects of new information on reported beliefs [about arrest risk]." Third, the effect of an arrest varies significantly across persons. Thomas et al. (2013: 17), for example, showed that "individuals of average verbal IQ or higher were not affected by an arrest when they updated

their risk perceptions."[11] Anwar and Loughran (2011: 689) reported that "the level of risk updating in response to an arrest ... decreases as offenders accumulate more offending experience." Schulz (2014) showed that the impact of arrest experiences is conditional on risk affinity, but she found that such experiences did not have a significant main effect on sanction perceptions in the full sample of offenders. Collectively, these findings make it extremely difficult to estimate the net effect of arrest experiences in the general offender population.[12]

As important, there is no evidence that any police policy or intervention (X) has an indirect effect on perceptions of arrest risk (Y) through arrest experiences (M). First, for most offenses, it remains unclear whether it is even within the power of the police to increase substantially the proportion of offenders who are arrested (i.e., $X \rightarrow M$) (Braga, Flynn, Kelling, and Cole, 2011; Eck, 1992; Puckett and Lundman, 2003). Second, to establish that a policy or intervention has an indirect effect on sanction perceptions through arrest-based updating (i.e., $X \rightarrow M \rightarrow Y$) would require a formal mediation analysis (Hayes, 2013; Zhao et al., 2010). Establishing $M \rightarrow Y$ is not sufficient. Such a mediation analysis, however, has never been carried out.

As with the perceptual deterrence scholarship, the policing literature also lacks sufficient robustness to support a call to change the nature of policing on the basis of deterrence theory. Findings from the broader literature examining whether police force size, police per capita, and police expenditures affect crime rates remain equivocal at best (Kovandzic, Schaffer, Vieraitis, Orrick, and Piquero, 2016; Pratt and Cullen, 2005). Furthermore, studies that have found an effect of police interventions on crime generally have shown that this effect is short-lived—a process Sherman (1990: 10) characterized as "initial deterrence decay."

NSL (2015), however, argued that "the most persuasive evidence of the deterrent effect of policing pertains to ... targeting"; they cited as an example the "good evidence of the effectiveness [of] 'hot spots' policing" (p. 78). Yet, this literature is more ambiguous than it initially seems. First, although not specifically evaluating sanction perceptions, there is experimental evidence that hot spots policing has no impact on residents' views about police performance, safety, or crime (Ratcliffe, Groff, Sorg, and Haberman, 2015; Weisburd, Hinkle, Famega, and Ready, 2011). It seems unlikely that hot spots policing would affect sanction perceptions but have no effect on any of these other outcomes. Second, recent meta-analyses of the literature on hot spots policing (Braga, Papachristos, and Hureau, 2012) and targeted deterrence (Braga and Weisburd, 2011) have shown that in both literatures,

11. Thomas et al. (2013) found that the direction of the effect of arrest among those with average verbal IQ or higher, although nonsignificant, was *opposite* of what would be expected on the basis of deterrence theory.

12. This is especially true given that many updating studies have relied on youth samples (Matsueda et al., 2006; Pogarsky et al., 2004) and have conditioned on offending status by focusing only on youth convicted of crimes and by dropping observations from time periods where the respondents abstained from offending (Anwar and Loughran, 2011; Thomas et al., 2013).

(a) the studies with stronger methodologies have tended to report *much* smaller effects (e.g., .196 vs. .766), and (b) there is evidence of publication bias.[13] In both meta-analyses, the authors attempted to correct for publication bias by using the trim-and-fill procedure (Duval and Tweedi, 2000). And in both meta-analyses, the mean random effect decreased by 11% and 28%, respectively, after using this procedure. However, even these corrected estimates are likely large overestimates of the true effect sizes. Research has shown that the trim-and-fill correction fails to adjust adequately for publication bias, such that "corrected" estimates often continue to overestimate true effect sizes *dramatically* (Simonsohn, Nelson, and Simmons, 2014).[14]

We want to emphasize that we are not suggesting that hot spots policing and focused deterrence strategies are ineffective. There is evidence supporting the effectiveness of these policing approaches, at least when implemented using particular techniques and directed at certain types of offenders (Groff et al., 2015). We are simply suggesting that, as with the perceptual deterrence literature, the policing literature is currently too limited to support a call for a fundamental shift in the focus of policing. Far more research is needed before making such strong suggestions as is a full consideration of all available evidence, including studies of the link between objective and perceived arrest risk.[15] Extant research findings and methodological approaches should only be discredited on the basis of strong evidence, not speculation. And neither NSL's (2015) theory, nor the review articles that inform their theory (Nagin, 2013a, 2013b), should dictate which types of studies are considered credible. Rather, scholars should simply encourage efforts to bring new methods and ideas to bear on the questions of (a) how sanction perceptions are formed and updated, and (b) whether different policing approaches are effective.

We close by providing some specific suggestions about promising directions for future research on the sources of sanction perceptions. We openly admit that these avenues of inquiry represent low-hanging fruit; indeed, it is perplexing that they remain virtually unexplored in our field. More than 40 years of social psychological work on dual-process models of reasoning has demonstrated that people often use several specific mental shortcuts (or cognitive heuristics) to estimate the probability of future events and to judge the costs and benefits of different lines of action (Johnson and Tversky, 1983; Tversky and Kahneman,

13. Publication bias occurs in both published and unpublished studies, and it stems primarily from a preference for significant results among authors, rather than among journal editors/reviewers (Cooper, DeNeve, and Charlton, 1997; Franco, Malhotra, and Simonovits, 2014; Song et al., 2009, 2010). Franco et al. (2014) found that most null findings are never even written up.

14. Simonsohn and colleagues (2014: 670) showed that the trim-and-fill procedure fails to correct for publication bias regardless of whether the meta-analysis included nonsignificant results.

15. Some consideration of the net crime prevention effect of sentinel policing *across* crime types is also warranted. Even if a shift to sentinel policing deters some types of crime (e.g., robbery), it might also reduce enforcement resources that could go toward other types of crime (e.g., theft and domestic violence) that are less likely to be affected by police presence on the streets.

1973, 1974). People estimate the probability of outcomes, such as arrest, by using the *availability heuristic*—their probability estimate is a positive function of their speed and fluency in either (a) recalling relevant exemplars or (b) imagining a plausible scenario where the event would occur (MacLeod and Campbell, 1992; Tversky and Kahneman, 1973). Likewise, individuals simultaneously judge the costs and benefits of actions by using the *affect heuristic* (Finucane, Alhakami, Slovic, and Johnson, 2000; Slovic, 2010), so that if their emotional "feelings toward an activity are favorable, they are moved toward judging the risks as low and the benefits as high" (Slovic, Finucane, Peters, and MacGregor, 2004: 315). In turn, "whereas risk and benefit tend to be positively correlated in the world, they are negatively correlated in people's minds (and judgments)" (Slovic et al., 2004: 315). Some evidence suggests this holds for perceptions of the costs and benefits of crime, which tend to be either significantly negatively correlated or uncorrelated, but not positively correlated (Kroneberg, Heintze, and Mehlkop, 2010; McCarthy and Hagan, 2005; Piquero and Tibbetts, 1996; Tittle and Botchkovar, 2005).

The availability and affect heuristics, as well as other mental shortcuts (e.g., the representativeness heuristic), have considerable relevance to deterrence theory, and to efforts to understand the sources of sanction perceptions (Pickett and Bushway, 2015; Piquero, Paternoster, Pogarsky, and Loughran, 2011). They may also help to account for differences across individuals in their confidence (or lack of ambiguity—Loughran et al., 2011) in sanction perceptions (Pickett and Bushway, 2015; Pickett et al., 2015). As Kahneman (2011) explained, "our understanding of cognitive ease and associative coherence locates subjective confidence firmly in System 1 [the intuitive system]" (p. 217), such that "intuitive predictions tend to be overconfident and overly extreme" (p. 192).

For reasons that remain elusive, criminologists have largely overlooked the potential role of cognitive heuristics in the formation of sanction perceptions and, more generally, judgments about the costs and benefits of offending. We believe this is an important area for future exploration. Indeed, it is one that holds considerable promise for illuminating why objective and subjective risk may not be strongly correlated (Kahneman and Klein, 2009), as well as ways to strengthen this correlation. Furthermore, even if policy makers are unable to influence the objective risk of arrest, they may be able to affect *perceived* arrest risk through interventions (or advertisements) that target intuitive reasoning processes (Pickett, Loughran, and Bushway, 2016).

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