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Consequences of legal risk communication for sanction perception updating and white-collar criminality

Justin T. Pickett[1] **· Thomas A. Loughran**[2] **·**
Shawn Bushway[3]

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Abstract
Objectives Scholars have long emphasized that communicating, or "advertising", information about legal sanction risk is necessary for the success of deterrence-based crime policies. However, scant research has evaluated whether direct communications about legal risk can cause sanction perception updating, the updating of ambiguity in sanction perceptions, or changes in persons' willingness to offend. No prior studies have evaluated sanction perception updating for white-collar crimes.
Methods To address this research void, the current study analyzes data from an experiment embedded in a recent national survey ($N = 878$). Multivariate regression models estimate the effect of providing participants with information about the "objective" arrest risk for white-collar offenses on their sanction perceptions.
Results The findings provide the first evidence that such information, when it is inconsistent with individuals' prior beliefs, causes them to update: (1) their perceptions of the certainty of arrest; (2) their ambiguity about arrest risk; and, indirectly, (3) their willingness to commit white-collar crimes.
Conclusions The results imply that individuals are willing to incorporate relevant information into their subjective beliefs about sanction risks. Importantly, however, they also make meaningful distinctions about the value of new information for understanding criminal risks.

✉ Justin T. Pickett
jpickett@albany.edu

[1] School of Criminal Justice, University at Albany, SUNY, 135 Western Avenue, Albany, NY 12222, USA

[2] Department of Criminology and Criminal Justice, University of Maryland, College Park, MD, USA

[3] Rockefeller College of Public Affairs and Policy, University at Albany, SUNY, Albany, NY, USA

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Introduction

Deterrence theory is a "credible policy theory" (see Mears 2010) only to the extent that legal authorities have the ability to influence potential offenders' sanction perceptions (Apel 2013; Nagin 2013). Deterrence theorists have long recognized this fact, and have emphasized that the deterrent value of formal punishments depends wholly on the existence of mechanisms for effectively communicating information about sanction threats to what Beccaria (1872 [1764]: 75) characterized as "the rude minds of the multitude."[1] The central role of legal risk communication for deterrence was underscored early on by Zimring and Hawkins (1973: 149), who argued that the burden of the threat communication was a vital duty of the sanctioning agency. Indeed, those authors likened crime deterrence to "a form of advertising" (p. 142).

Although legal risk communication is important theoretically, and is the crux of debates about whether deterrence models of offending have practical value for crime control (Kleck and Barnes 2013; Kleck et al. 2005), it remains the most understudied deterrence process (Apel 2013). Kennedy (2009) stressed this point in his seminal review of the deterrence literature. He observed that "perhaps most dramatically, we err in neglecting the role of *information* and *communication*," and he explained that "[t]here is virtually no systematic exploration of … the deliberate use of communication to create deterrence" (p. 41). He called for deterrence studies that, in the tradition of commercial marketing research, evaluate audience reception of direct deterrence messages (or advertisements), and noted that "[i]t is not, perhaps, unreasonable that we should be as systematic about public safety as we are about soap" (p. 141).

Legal risk information can be communicated to the public through media campaigns and advertisements, or, alternatively, it can be disseminated through experiential mechanisms, such as personal and vicarious experience with arrest (Apel 2013; Cook 1980). With few exceptions (Holsinger and Novak 2004; Singer and Cooper 2009), previous studies have not evaluated how exposure to non-experiential information, such as that contained in criminal justice advertisements or publicity, influences sanction perceptions. However, in the past decade, micro-level research on deterrence perceptions has begun studying what Pogarsky et al. (2004) referred to as the "neglected linkage" in deterrence theory: the role of experiences with punishment and offending in the subsequent revision of individual perceptions of risk. This work, while correlational in nature and focused almost exclusively on street offending by youths, has yielded important findings. For example, results from multiple longitudinal studies, including the National Longitudinal Study of Youth (Lochner 2007), the Denver Youth Study (Matsueda et al. 2006), and the Pathways to Desistance Study (Anwar and Loughran 2011), have produced evidence that offenders do incorporate new information into their subjective beliefs about offending and revise them

[1] Beccaria (1872 [1764]: 26) argued, for example, that the deterrent value of punishments would increase proportionally "as the code of laws is more universally read, and understood."

accordingly through a process consistent with rational Bayesian updating.[2] Collectively, these investigations suggest that policy information may be disseminated to offenders through punishment experiences.

Importantly, however, each of these updating studies has only considered the effects of experiential information—information gleaned from personal or peer experiences with offending and arrest. From a policy standpoint, reliance on experiential mechanisms for communicating legal risk information is disadvantageous for several reasons. First, by definition, such information is largely private and, thus, highly asymmetric. Second, experiential information, such as that conveyed by an arrest, can only be transmitted after a criminal justice policy or initiative is implemented *and* enforced. Thus, employing only experience-based public communication strategies would eliminate the possibility of "anticipatory" deterrence, or crime deterrence that occurs before a policy is put into effect (Smith et al. 2002). It would also limit the potential for preventive (or general) deterrence, which is greatest when sanction threats can be communicated independently of the administration of punishment (Kennedy 2009). Not least, experienced-based communication is inefficient. As Kennedy (2009: 138) explains, "[w]here the point of sanctions is deterrence, and deterrence can be obtained through information rather than through their actual exercise, their actual exercise is wasteful and costly."

The current research builds on prior studies of sanction perception updating to advance knowledge about how individuals modify their assessments of legal risk in response to newly acquired information from non-experiential sources. We provide the first experimental test of whether deliberately communicating information about arrest risk can influence individuals' sanction perceptions, ambiguity in their sanction perceptions, and willingness to offend. Our analyses focus on sanction perception updating for white-collar offenses using a sample of adults in the United States. Before proceeding, it bears emphasizing that there is an ongoing and intense scholarly debate about how best to define white-collar crimes, which Simpson (2013: 313) suggests is "a distraction to moving the field forward." For the present study, we rely on Wheeler and colleagues' (1982: 642) definition of white-collar crimes as "economic offenses committed through the use of some combination of fraud, deception, or collusion" (see also Weisburd et al. 1991). This definition is consistent with that used by the Federal Bureau of Investigation (FBI) (Barnett 2000).

Our focus on white-collar offenses is important because "white-collar crimes may be the least understood but most consequential crime type" (Simpson 2013: 310). Indeed, Simpson (2013: 324) observes that "[n]ewer developments in the deterrence literature that focus on" factors such as "the roles that updating, [and] ambiguity … play in setting (or changing) risk preferences have yet to emerge in the white-collar crime literature." She argues that "[t]his is an important gap in the literature for future researchers to address" (p. 324). The lack of research is particularly surprising given that a meta-analysis of perceptual deterrence studies suggests that sanction perceptions may be most relevant for deterring white-collar crimes (Pratt et al. 2006).

[2] The extant evidence is not unequivocal. Some studies find no correlation between prior arrests and sanction perceptions (Kleck et al. 2005; Saridakis and Sookram 2014).

Bayesian learning theory and sanction perception updating

Bayesian learning theory suggests that sanction perception updating should occur when individuals acquire new information, and that the outcome of the updating process should be conditional on the nature of their subjective priors (Anwar and Loughran 2011; Nagin 2013). In the case of the certainty of arrest, an individual's subjective prior is his preexisting point estimate of the probability of arrest (Apel 2013). Theoretically, that point estimate of arrest risk should constitute a measure of central tendency from the person's underlying subjective distribution for the probability of arrest, which is simply a cognitive plot of his information about the possible values of arrest risk (Manski 2004). The variance of this subjective distribution represents ambiguity around one's subjective belief, with the larger variances representing more ambiguity. The person's level of confidence in his risk estimate should be an inverse function of the variance of his subjective probability distribution for arrest risk (Loughran et al. 2011). Specifically, the economic conceptualization of risk and ambiguity predicts that increases in the amount or consistency of a person's information should lead to less ambiguity, and, thus, stronger faith in his own belief, regardless of the accuracy of that belief (Camerer and Weber 1992).

To illustrate, if a person has only two values equal to 0 and 100 % in his subjective distribution, he should, if he relies on the mean of that distribution, estimate the certainty of arrest to be 50 %. Additionally, the high variance in this person's subjective distribution should cause him to have very little confidence in his risk estimate. Stated differently, for this person, the probability of arrest would be ambiguous. By contrast, if another person has four values equal to 48, 50, 50, and 52 % in his subjective distribution, he should have a higher level of confidence in his risk estimate, because there is less variation in his subjective distribution. Even still, this person should also estimate the probability of arrest to be 50 %.

According to Bayesian learning theory, an individual should incorporate newly acquired information into his subjective distribution. This process should change the central tendency of the distribution, thereby causing him to update his prior belief (Anwar and Loughran 2011). His updated estimate of arrest risk is his posterior belief. The magnitude and direction of the updating effect should vary depending on whether the new information is consistent with the individual's subjective prior. New information should have a larger effect on risk estimates, and should also increase ambiguity (i.e., reduce confidence in perceived risk), when it is inconsistent with the individual's subjective prior. By contrast, it should have little effect on risk estimates, and should reduce ambiguity (i.e., increase confidence in perceived risk), when it is consistent with the person's subjective prior (Loughran et al. 2011).

Risk, ambiguity, and crime deterrence

Deterrence and rational choice theories of offending emphasize the role of offender decision-making in crime causation (Nagin 1998, 2013). This theoretical scholarship argues that individuals decide to commit crime after they consider the costs and benefits of offending, and judge that the risk of punishment is insufficient to outweigh the benefits of the criminal act. The central hypothesis is that individuals' *perceptions* of the certainty and severity (and perhaps celerity, but see Nagin and Pogarsky 2001) of punishment (informal or formal) should influence their willingness to engage in crime (Apel 2013). Extant evidence demonstrates that the perceived certainty of detection is a far more salient consideration in offender decision-making than the perceived severity of punishments (Apel 2013; Nagin and Pogarsky 2001).

Prior research generally supports rational choice theory in the context of white-collar and corporate offending (Klepper and Nagin 1989a; Simpson 2013; but see Ariel 2012; Makkai and Braithwaite 1994). In a seminal study, Paternoster and Simpson (1996: 574) found that "the perceived costs of punishment, be they formal, informal, or based on self-imposed shame, that are directed against the individual effectively deter corporate crime." Klepper and Nagin (1989b) showed that both perceived detection risk and perceived prosecution risk were inversely related to the respondents' willingness to engage in tax non-compliance. However, they found that perceived prosecution risk had a threshold effect, such that an increase in perceived risk from zero to non-zero yielded greater deterrence, after which further increases in perceived risk were inconsequential. Piquero et al. (2005) found that the perceived certainty of informal sanctions was negatively associated with intentions to commit corporate crimes, but the perceived severity of formal sanctions was actually positively associated with corporate criminality. Smith et al. (2007) showed that, among business managers, perceived formal sanction risk exerted an indirect effect on intentions to offend through ethical evaluations and positive/negative outcome expectancies. Kroneberg and colleagues (2010) found that the perceived probability of detection interacted with the perceived sanction severity and moral norms to influence intentions to commit tax fraud. Indeed, a meta-analysis of perceptual deterrence studies revealed that the deterrent effect of perceived sanction certainty was largest in the case of "'white-collar' types of offenses" (Pratt et al. 2006: 384).

One limitation of the extant perceptual deterrence research examining white-collar offending is that it has focused only on the deterrent value of point estimates of arrest risk, thereby ignoring the inherent property of subjective beliefs, namely that they involve uncertainty or *ambiguity* (Pickett et al. 2015a). As noted above, individuals with identical point estimates of punishment risk may differ in how much confidence they place in their risk perceptions. This would be expected to occur, for example, if the amount or consistency of the information possessed by the individuals (i.e., contained in their subjective probability distributions) is not the same. Ambiguity in arrest risk perceptions is of theoretical importance for understanding criminal decision-making because, in some circumstances, people tend to be "ambiguity averse", favoring unambiguous situations and outcomes (see Ellsberg 1961). For this reason, ambiguity in arrest risk may itself deter offending (Nagin 1998; Sherman 1990). Yet, the sources of ambiguity in perceived risk and of the risk perceptions themselves may not be identical (Pickett and Bushway 2015).

An important study by Loughran and colleagues (2011), which focused on street offending by young felons, found that ambiguity in perceived arrest risk did, in fact, have its own unique influence on behavior, on top of that exerted by risk estimates. Specifically, those authors found that ambiguity deterred certain types of offending—what they termed "no one around" crimes (e.g., theft)—when individuals' point estimates of arrest risk were low. Unfortunately, however, Loughran et al. (2011: 1045) were limited in their ability to measure ambiguity in perceived risk, as they did not have direct measures of the construct. Rather, Loughran and colleagues attempted to measure ambiguity using the variance of an individual's arrest risk perceptions for similar crimes. Two experimental studies by Casey and Scholz (1991a, b), which examined tax evasion among college students, also found evidence that ambiguity in punishment risk may deter offending, though neither study directly measured risk perceptions or self-reported ambiguity levels. Two recent studies by Pickett and colleagues directly measured ambiguity, but only investigated its sources, not potential deterrent effects (Pickett and Bushway 2015; Pickett et al. 2015a).

Deterrence theory in practice: the importance of sanction perception updating

Deterrence models of offending cannot inform crime control efforts unless individuals update their sanction perceptions in response to new information about objective punishment risks (Apel 2013; Nagin 1998). Indeed, the idea that risk perceptions are susceptible to manipulation by policy intervention is a key assumption underlying both deterrence theory and agendas of criminal justice policymakers, yet, the existing research is very mixed (Kleck et al. 2005). Nagin's (2013: 204) recent review of the state of what we know about deterrence directly stresses this fact: "Establishing the link between risk perceptions and sanction regimes is imperative … Unless perceptions adjust, however crudely, to changes in the sanction regime, the desired deterrent effect will not be achieved."[3]

Perhaps the most promising evidence that criminal justice policies and activities can influence sanction perceptions has derived from evaluation studies of efforts to reduce drunk driving. Several studies have shown that these interventions are successful in reducing alcohol-related traffic accidents, at least initially (Ross 1973, 1981). Criminal justice interventions aimed at other forms of offending, at least when well publicized, can have similar effects (Johnson and Bowers 2003, but see Ariel 2012). Indeed, studies indicate that such policies and practices may even have anticipatory benefits (Smith et al. 2002). Unfortunately, the weight of the evidence suggests that the crime-prevention benefits of such publicized interventions diminish quickly over time (Kennedy 2009; Nagin 1998)—a process Sherman (1990: 10) characterizes as "initial deterrence decay". This presumably results because initial increases in sanction perceptions are not sustained. Regrettably, however, most of the research to date examining criminal justice interventions and policies has not actually measured sanction perceptions.

Two important exceptions are studies by Grube and Kearney (1983) and Ross and Voas (1990). The former found that most drivers (60 %) were aware of a new policy in their county entailing a 2-day jail sentence for driving under the influence (DUI), although the "policy had no demonstrable effect on alcohol related accidents" (p. 242). The latter found significant differences in both the perceived certainty and severity of punishment for DUI among drivers in two cities that differed objectively in enforcement levels. By contrast, more recent research (Kleck et al. 2005; Kleck and Barnes 2013; Pickett et al. 2015a, b) suggests that macro-level policies may not be able to manipulate sanction perceptions, particularly perceptions of arrest risk. For instance, Kleck et al. (2005), using a sample of individuals nested in 54 large urban counties in the United States, found no association between perceptions and actual punishment

[3] This is in contrast to the more traditional economic concept of perfect information, whereby individuals have all the necessary and relevant information with which to make a decision.

levels when aggregated to the county level. Apel's (2013: 79) review of the deterrence literature arrives at a similarly grim conclusion, noting that "the apparent lack of correspondence between subjective probabilities and punishment actualities is discouraging for the deterrence doctrine."

One potential explanation for this apparent lack of correspondence between perceived and objective sanction risk is that it may simply reflect the fact that, as Kennedy (2009: 27) explains, "legal authorities make next to no effort to inform offenders and potential offenders about penalties and risks." Put simply, it is unreasonable to expect that a relationship between subjective and objective punishment levels will exist in the absence of deliberate efforts by policymakers and practitioners to communicate legal risk information to the public. The crux of the issue, then, involves three related questions. First, do individuals update their sanction perceptions in response to new information? Second, do updated sanction perceptions have deterrent value? Third, can the type of information (e.g., experiential vs. non-experiential) that triggers updating be effectively communicated to the public by policymakers and criminal justice practitioners? As noted from the outset, there is evidence that individuals update their point estimates of arrest risk for street crimes in response to personal and vicarious experiences with arrest (Anwar and Loughran 2011; Lochner 2007; Matsueda et al. 2006; Pogarsky et al. 2004). However, because arrest is infrequent and can only convey information about a new policy after the policy is implemented *and* enforced, experiential mechanisms may be less effective than non-experiential mechanisms for quickly and accurately communicating policy information to the public (Kennedy 2009).

Very little is known about whether sanction perceptions are updated in response to non-experiential information. Only a small number of prior studies have directly examined whether exposure to criminal justice facts—either through media publicity, seminars, or information booklets—affects sanction perceptions (Chapman et al. 2002; Holsinger and Novak 2004; Pickett et al. 2015a, b; Salisbury 2004; Singer and Cooper 2009). In measuring sanction perceptions, this work has focused exclusively on perceptions of the severity of punishments for street offenses, such as rape. Overall, the results from this research, although not totally consistent (see Salisbury 2004), have supported the idea that providing individuals with criminal justice information can improve knowledge about sentencing *severity*.

Recall, however, that there is strong evidence that the perceived severity of punishment is far less important for deterring crime than the perceived certainty of arrest (Apel 2013). Unfortunately, the only existing evidence about whether individuals update their perceptions of the certainty (or probability) of arrest in response to non-experiential information comes from studies analyzing the effects of general exposure to television news. This work reveals no correlation between television news consumption and perceptions of arrest risk, at least for street crimes (Kleck and Barnes 2013; Kleck et al. 2005). Of course, television news consumption does not guarantee exposure to criminal justice information (Pickett et al. 2015a, b).

No prior studies have evaluated whether, as Bayesian learning theory would suggest, individuals update their levels of ambiguity in perceived risk after acquiring new information, either from arrest experiences or from non-experiential sources. In addition, no previous research has explored sanction perception updating for white-collar crimes (Apel 2013; Simpson 2013). Finally, because most previous studies of sanction perception updating have focused only on risk perceptions as the dependent variable (Anwar and Loughran 2011; Lochner 2007; Pogarsky et al. 2004, 2005; Schulz 2014), it is not currently known whether or how updated sanction perceptions affect persons' subsequent decisions about engaging in crime.

The current study

In the remainder of this study, we provide the first experimental test of whether individuals incorporate objective information about the rate of detection into their own subjective beliefs about the arrest risk for white-collar crimes. We focus on white-collar crimes because: (1) there is evidence that deterrence models may be particularly well suited for explaining white-collar offending (Pratt et al. 2006); (2) there is a lack of prior research examining either sanction perception updating or ambiguity in relation to these offenses (Apel 2013; Simpson 2013); and (3) white-collar crimes inflict especially high social costs on society (Simpson 2013). In addition, the conditions of offender decision-making for white-collar crimes can be better approximated in our survey-based experiment than those for other offenses, such as DUI or theft, which involve altered mental states (i.e., intoxication) and/or criminal opportunities with immediate benefits.

Our analyses test five hypotheses about sanction perception updating, which are informed by both Bayesian learning theory and deterrence theory. First, we hypothesize that irrelevant information should have no impact on individuals' subjective risk perceptions or confidence in them. We test this by examining the effect that information provided about the detection probabilities for white-collar crimes has on respondents' subjective beliefs about a non-crime event. Second, we hypothesize that newly acquired information should cause individuals to update their point estimates of arrest risk, such that those who receive the new information should, on average, report risk beliefs that are closer to the objective values than those who do not receive the information. Third, we hypothesize that the magnitude of this updating for risk estimates should be larger when the information is inconsistent with respondents' subjective priors. Fourth, we hypothesize that newly acquired information will reduce ambiguity when the information is consistent with individuals' prior beliefs, but will actually increase ambiguity when it is inconsistent with their subjective priors.

Finally, we hypothesize that the information treatment should have an indirect effect on respondents' willingness to commit white-collar offenses through its effects on both risk estimates and ambiguity (or confidence) levels. In particular, there is strong evidence that individuals, including experienced offenders (see Lochner 2007), tend to overestimate arrest risk for most crimes (Tittle 1980). Jensen (1969: 189) identified this "naive misunderstanding"—the "firm belief that most people who break the law are caught and punished"—more than 40 years ago and asked whether "if it is learned that the belief is incorrect, is one then more likely to commit offenses?" Our study provides the first experimental test of this research question.

Methods

To examine our hypotheses, we conducted a randomized experiment in which a randomly selected subset of respondents were exposed to objective information about arrest risk. The data for our study come from a nationwide sample of Internet panelists. Prior research has found that surveys with non-probability Internet samples commonly produce both experimental results and multivariate findings that are similar to those obtained from nationally representative samples (Ansolabehere and Schaffner 2014; Bhutta 2012; Sanders et al. 2007; Simmons and Bobo 2015; Weinberg et al. 2014). The experiment was embedded in a survey fielded during August 2013 with a random sample of 878 adult (18 years and older) members of SurveyMonkey's Audience panel. Several published studies have used samples from the Audience panel (see, e.g., Blodorn and O'Brien 2013; Bregman et al. 2015; Pickett and Baker 2014). Brandon et al. (2014) provide a detailed discussion of the strengths of the Audience panel for academic research, and the procedures used by SurveyMonkey to ensure data quality. The opt-in panel has more than 400,000 active members, and includes individuals from every U.S. state. Most members of the panel are recruited after participating in user-administered surveys on the SurveyMonkey website. These surveys are conducted daily by persons with SurveyMonkey accounts and target diverse groups of respondents, such as employees, coworkers, customers, and members of social clubs. More than 30 million individuals participate in such surveys each month. Those who go on to join the Audience panel receive two different types of incentives for participating in future surveys: (1) entry into a weekly drawing for \$100 and (2) a 50¢ donation to a charity of their choice. For quality control purposes, panelists are invited to no more than two surveys per week.

To develop the sample for our survey, SurveyMonkey sent generic email invitations to 2772 randomly selected panelists. Of the sampled panelists, 878 completed the questionnaire, yielding an overall participation rate of 32 %, which is relatively high for online surveys.[4] We provided a randomly selected subset of respondents, constituting slightly more than one-third of the overall sample, with information about arrest risk (see below). In the experiment, this group of respondents constitutes the treatment group, while those who did not receive the information are the control group.[5]

The descriptive statistics for both experimental groups are provided in Table 1. As shown in the table, the demographic characteristics of the groups are statistically identical. The groups are also statistically identical in relation to levels of prior offending, prior arrest, and self-control. This is supportive of successful randomization. Relative to the general public, Whites, persons with a college degree, and those with higher incomes are overrepresented in both experimental groups, as they are among white-collar offenders (see Weisburd et al. 1991; Wheeler et al. 1988). Indeed, roughly

[4] Ideally, we would have achieved a higher response rate. Future studies should consider providing monetary incentives to respondents, which have been shown to increase response rates in both traditional and online surveys (Tourangeau et al. 2013). At the same time, it bears emphasizing that studies consistently find that, across modes of data collection, response rates are a bad indicator of non-response bias (Curtin et al. 2000; Holbrook et al. 2008; Keeter et al. 2000, 2006). Meta-analyses confirm this point (Groves 2006; Groves and Peytcheva 2008). As Krosnick et al. (2015: 6) explain in their recent report on survey research to the National Science Foundation, "nonresponse bias is rarely notably related to [the] nonresponse rate."

[5] Specifically, our study used a posttest-only experimental design (Campbell and Stanley 1963: 25). We avoided using pretests—measuring risk perceptions before, as well as after, the information treatment—to reduce the risk of testing effects. Our key assumption is that, because of random assignment, the subjective priors of the treatment and control groups were equal prior to the information treatment, and, thus, any observed differences between the groups will reflect the causal effect of the information on the treatment groups' sanction perceptions.

Table 1 Descriptive statistics

Variables	Treatment group		Control group		Difference between groups		
	Mean	S.D.	Mean	S.D.	Diff.	*t*/*z*	*p*
Risk estimates							
Higher unemployment	49.48	26.56	48.85	28.17	0.63	0.32	0.75
Arrest for insider trading	41.31	31.87	51.39	33.63	−10.08	−4.29	0.00
Arrest for tax fraud	55.62	30.45	62.27	29.80	−6.65	−3.13	0.00
Arrest for insurance fraud	40.49	29.98	47.22	32.05	−6.73	−3.03	0.00
Ambiguity levels							
Unsure higher unemployment	3.12	1.25	3.02	1.15	0.11	1.25	0.21
Unsure arrest insider trading	2.95	1.28	2.89	1.37	0.06	0.64	0.52
Unsure arrest tax fraud	2.93	1.29	2.78	1.31	0.15	1.61	0.11
Unsure arrest insurance fraud	3.10	1.22	2.91	1.27	0.20	2.23	0.03
Offending probability							
Non-zero insider trading	0.37	0.48	0.38	0.49	–0.01	–0.37	0.71
Non-zero tax fraud	0.29	0.45	0.24	0.43	0.05	1.53	0.13
Non-zero insurance fraud	0.35	0.48	0.30	0.46	0.05	1.58	0.12
Control variables							
Prior offending	0.10	0.30	0.13	0.33	–0.02	−1.08	0.28
Prior arrest	0.19	0.39	0.21	0.41	–0.02	–0.79	0.43
Family/peer arrest	0.46	0.50	0.47	0.50	–0.01	–0.38	0.71
Low self-control	2.56	0.82	2.55	0.76	0.01	0.11	0.92
White	0.78	0.42	0.81	0.39	–0.03	−1.19	0.23
Female	0.52	0.50	0.51	0.50	0.01	0.29	0.77
Age	46.78	16.15	46.42	16.22	0.36	0.31	0.75
Education	3.75	1.01	3.80	0.98	–0.05	–0.69	0.49
Income	2.87	1.35	2.86	1.24	0.02	0.17	0.86
Employed full-time	0.49	0.50	0.48	0.50	0.01	0.15	0.88
Married	0.50	0.50	0.51	0.50	–0.01	–0.33	0.74
Parent	0.22	0.41	0.24	0.43	–0.02	–0.73	0.47
N	309		569				

p two-tailed *p*-value; *S.D.* standard deviation; *t*/*z* *t* statistic (for test of the equality of means) or *z* statistic (for test of the equality of proportions)

65 % of the respondents had either a college or graduate degree, 30 % had annual incomes of $100,000 or higher, and the vast majority of respondents reported being employed on either a full- or part-time basis. Demographically, then, the respondents would appear to constitute persons with ample opportunities for white-collar offending. The Internet sample also does not appear to be limited to committed conformists: 21 % of respondents reported prior arrests, 12 % reported committing at least one of five different property or white-collar offenses in just the past year, and 38 % indicated that there was a 10 % or greater chance they would commit at least one of the crimes described in the scenarios.

Experimental procedure and measurement of variables

At the outset of the survey, respondents in the treatment group received information about the risk of arrest for several white-collar and property offenses (see Table 2). The presented arrest statistics for the three white-collar crimes (i.e., fraud, embezzlement, and forgery) were taken from the data on clearances specifically by arrest provided in Barnett (2000: 6), while those for the two property crimes (i.e., theft, burglary) were derived from the 2011 Uniform Crime Reports (UCR) (U.S. Department of Justice 2012).[6] As discussed below, scholars have outlined several potential reasons why clearance rates may be invalid measures of objective arrest risk (Apel 2013; Cook 1979, 1980; Nagin et al. 2015).[7] However, in this study, our principal interest is in how individuals respond to official information, rather than whether that information is itself valid. For our purposes, then, what is most important is that the respondents perceive that the presented information is credible, and not necessarily that the information is in fact credible. Nonetheless, as we discuss in the conclusion, our reliance on clearance rates represents an important limitation of our study. It bears emphasizing that experimental manipulation has direct relevance to crime policy, as policymakers have, at times, attempted to directly communicate information about sanction risk to potential offenders through public service announcements and media advertisements (see, e.g., Flexon and Guerette 2009; Holsinger and Novak 2004).

After they read the arrest statistics, respondents in the treatment group were asked "Does it surprise you that the risk of arrest is this low for these types of crimes?" (0 = no, 1 = yes).[8] We assume that respondents will be surprised by the information if it is inconsistent with their prior beliefs. Responses to this question are, thus, of theoretical importance because new information should have the largest effect on individuals' risk estimates and ambiguity levels if it is inconsistent with their subjective priors—that is, if it surprises them. Nearly half (48 %) of the respondents in the treatment group reported being surprised by the arrest statistics. In the analyses, we included two binary variables that contrasted those who received the treatment and were either surprised (coded "1"), or not surprised (coded "1"), against the controls (coded "0").

Respondents in both experimental groups answered several questions about the percent chance (0–100 %) of different events occurring. The exact wording for these questions is provided in Table 2. One item asked about the risk of an increase in unemployment, and three items asked about the risk of arrest in hypothetical scenarios involving white-collar offenses. The scenarios focused on three forms of white-collar

[6] We included the clearance rates for property crimes to help reduce non-substantive anchoring. Specifically, if respondents are provided with only a few clearance rates, which are all similar, they may unconsciously anchor their risk perceptions on those rates, regardless of the relevance of the rates (see Kahneman 2011).

[7] As Cook (1980: 241, emphasis in original) has explained, "at best, [the clearance rate] can be viewed as a measure of the *average* probability of punishment for crimes committed."

[8] Although most citizens overestimate arrest risk (Tittle 1980), it is possible that a minority of respondents may have been surprised that the clearance rates were not lower. Theoretically, such respondents should increase their risk estimates in response to the information treatment. Thus, if these respondents classified themselves as being surprised, the findings would underestimate the differences in the effect of the treatment on those who were surprised vs. not surprised, whereas the opposite would occur if they classified themselves as being not surprised.

Table 2 Experimental manipulation and survey items measuring risk perceptions

Experimental Manipulation
"Through both the Uniform Crime Reporting Program and the National-Incidence Based Reporting System, the Federal Bureau of Investigation (FBI) collects information on property crimes and white-collar crimes. The FBI's data reveal the number of cases out of 100 (or the percentage of cases) that result in an arrest for each specific type of crime. The data show that police are able to identify and arrest an offender in: 26 out of 100 (or 26 %) of FRAUD cases 21 out of 100 (or 21 %) of THEFT cases 13 out of 100 (or 13 %) of BURGLARY cases 33 out of 100 (or 33 %) of EMBEZZLEMENT cases 26 out of 100 (or 26 %) of FORGERY cases These arrest percentages are highly stable across years."
Higher Unemployment
"Next, we would like to ask your opinion about how likely you think it is that various events will happen. Some of the questions will ask you about the PERCENT CHANCE of something happening. The percent chance must be a number from 0 to 100. Here numbers like 2 or 5 % would mean 'almost no chance,' and numbers like 95 or 98 % would mean 'almost certain.' The percent chance can also be thought of as a number of chances out of 100. Using this scale from 0 to 100, what do you think is the PERCENT CHANCE (or CHANCES OUT OF 100) that the unemployment rate in the U.S. will be higher at this time next year than it is now?"
Insider Trading
"Now, please imagine that you have $5,000 that you want to invest in the stock market. Also imagine that one of your close friends is on the board of directors at a large computer company. He tells you that if you invest in the company, he will provide you with the company's financial information before it is released to the public. He estimates that you will double your investment within a year. This practice is illegal and is known as insider trading. In your best judgment, what is the PERCENT CHANCE (or CHANCES OUT OF 100) that you would get caught by law enforcement if you invest in the company and receive insider information?"
Tax Fraud
"Now, please imagine that your close friend is an experienced accountant. He tells you that he knows a way that you can reduce your yearly taxes by about half. He also tells you that you will have to provide false information on your tax forms to do so. This is illegal and is known as tax fraud. In your best judgment, what is the PERCENT CHANCE (or CHANCES OUT OF 100) that you would get caught by law enforcement if you decided to report false information on your tax forms?"
Insurance Fraud
"Now, imagine that you are in a car accident where your car is damaged. Your close friend does auto repair work. He tells you that if you bring your car to him to fix, he will overcharge the insurance company by $2,000, and you two can split that $2,000 to keep for yourselves. This is illegal and is known as insurance fraud. In your best judgment, what is the PERCENT CHANCE (or CHANCES OUT OF 100) that you would get caught by law enforcement if you decided to take your friend up on his offer and split the $2,000?"

criminality: insider trading, tax fraud, and insurance fraud.[9] We selected these offenses because they are relatively common forms of white-collar offending (Weisburd et al. 1991), and we believed that they sufficiently represented the types of white-collar crimes (e.g., compared to offenses such as forgery, price-fixing, or environmental pollution) that would likely be relevant for the Internet panelists in our sample. The ordering of the items was identical for both groups of respondents. We asked the

[9] It is possible that, because the scenarios did not include descriptive details about all situational factors that could potentially be relevant to the crimes described therein, respondents may have differentially imputed the circumstances of the crimes, and, thus, that their estimates of arrest risk may not be comparable in every instance. If this occurred, it would result in random measurement error that biases our study toward null results.

question about unemployment before those about arrest risk. This was done as a check against non-substantive anchoring, which occurs when a person's estimate of a numerical quantity is shaped by his or her earlier exposure to an unrelated number (see Kahneman 2011). If the treatment has a substantive effect, it should influence views about arrest risk but not about unemployment.

In the questionnaire, each of the four questions measuring risk perceptions was followed by an item gauging ambiguity in the given risk estimate. The specific survey question was: "How sure are you about this answer?" The response options ranged from 1 = very sure to 6 = very unsure. Loughran et al. (2011: 1045) recommended this approach as "[t]he ideal measure for ambiguity." The method has also recently been validated against an alternative approach suggested by Manski (2004), which involves asking respondents to report numerical ranges around their risk estimates to indicate ambiguity (Pickett et al. 2015a).

Consistent with previous studies (Kamerdze et al. 2014; Kroneberg et al. 2010; Nagin and Pogarsky 2001; Paternoster and Simpson 1996; Piquero 2012; Piquero and Piquero 2006; Piquero et al. 2005; Pogarsky and Piquero 2003), we used respondents' intentions to offend as a proxy for criminal behavior. Prior research supports the validity of this approach (Kim and Hunter 1993; Pogarsky 2004). The central motivation for using this method is that it remedies both the temporal order and non-contemporaneousness problems that commonly arise in deterrence studies (Pogarsky 2004). First, in cross-sectional studies, like ours, analyzing intentions to offend helps to establish the causal order of the relationship between sanction perceptions and criminality, thereby allowing researchers to distinguish "deterrent effects" from "experiential effects" (see Paternoster 1987). Second, it ensures that criminality is measured at the same time as, but directly after, sanction perceptions, which captures the situational dependence of offender decision-making (Grasmick and Bursik 1990: 844).

For each of the three crime scenarios, we asked respondents to estimate the percent chance that they would commit the offense if they were actually in that situation. In all three cases, the resulting measures were highly skewed, because most respondents (62–76 %, depending on the offense and experimental group) reported that there was no chance they would offend. Accordingly, we generated three binary measures coded "1" if the respondent reported a non-zero probability of committing the given offense, and coded "0" if he or she reported a probability of zero.

In the multivariate models, to minimize the risk of omitted variable bias, we include controls for key factors known to be correlated with adult criminal offending. First, we include a binary measure (*Prior Offending* = 1) indicating whether the respondent has committed at least one of five different offenses in the past 12 months: (1) snuck out without paying for movies or food; (2) stolen something worth \$50 or less; (3) stolen something worth more than \$50; (4) fraudulently claimed government benefits; and (5) cheated on taxes. We also control for whether the respondent has ever been arrested (*Prior Arrest* = 1), and whether he or she has any family members or close friends who have been arrested (*Family/Peer Arrest* = 1). The models also control for *Low Self-Control*, which is measured with an index ($\alpha = 0.71$) equal to the average across responses (1 = strongly disagree, 6 = strongly agree) to six questions from the Grasmick et al. (1993) scale. Two examples of the included items are: "I often act on the spur of the moment without stopping to think" and "I lose my temper pretty easily". Prior research suggests that low self-control is positively associated with white-collar offending, at least in the case of non-corporate offenses, such as check or credit card fraud (Holtfreter et al. 2010a, b).

Finally, we incorporate controls for respondents' race (*White* = 1), gender (*Female* = 1), *Age* in years, *Education*, *Income*, employment status (*Employed Full-Time* = 1), and marital status (*Married* = 1). We also control for whether the respondent has a young child (*Parent* = 1). *Education* is measured as follows: 1 = no high school degree; 2 = high school degree; 3 = some college; 4 = college degree; and 5 = postgraduate degree. *Income* is an ordinal measure: 1 = $24.9K or less; 2 = $25–49.9K; 3 = $50–99.9K; 4 = $100–149.9K; and 5 = $150+K.[10]

Findings

In our analyses, we first investigate the influence of information exposure on respondents' point estimates of risk. The four variables measuring risk estimates are continuous indicators with approximately normal distributions. Thus, the respective models are estimated using ordinary least squares (OLS) regression. There is not a problematic level of multicollinearity in any of the models estimated in this study—none of the variance inflation factors exceed 1.40. In several models, however, the Breusch–Pagan/Cook–Weisberg test for heteroskedasticity is significant or marginally significant ($p < 0.10$). Accordingly, we estimate all of the models using robust standard errors.

Table 3 presents the results of regressing each of the four measures of risk estimates on the experimental manipulation and the control variables. Recall that exposure to arrest statistics should not influence the perceived risk of an increase in unemployment. Model 1 confirms that the manipulation has no effect on views about unemployment, which supports our first hypothesis. By contrast, exposure to arrest statistics has a significant and substantial negative effect on respondents' estimates of arrest risk for insider trading (Model 2), tax fraud (Model 3), and insurance fraud (Model 4). The results suggest that, depending on the offense, exposure to arrest statistics *reduces* respondents' perceived probability of arrest for the specific white-collar crimes described in the three scenarios by an average of 5 to 11 percentage points. This reduction is in line with our second hypothesis, as the arrest statistics provided suggest that, in each case, the objective rate of detection is much lower than the mean reported perception for the control group. In every case, the effect is larger among respondents who are surprised by the arrest statistics than among those who are not surprised. For the scenario involving tax fraud (Model 3), the effect of exposure to arrest statistics is only marginally significant ($p = 0.061$) among respondents who are not surprised.

Next, we turn to the question of whether exposure to arrest statistics influences ambiguity in risk estimates. Because the measures of ambiguity are six-point ordinal variables, we use ordinal logistic regression to estimate the models.[11] The results are presented in Table 4, and show that exposure to arrest statistics does not influence the ambiguousness of the risk of increasing unemployment. This is consistent with our first hypothesis. However, among respondents who are surprised, the arrest statistics increase ambiguity in two of the three crime scenarios—tax fraud (Model 3) and insurance fraud

[10] To preserve the sample size, missing values ($N = 56$) on *Income* were imputed based on the values of the other variables in the analyses. This did not appreciably alter the results.

[11] The parallel lines assumption is met in all of the models for the effect of the experiment on the measures of ambiguity. We also estimated supplementary models using OLS regression with the six-point measures of ambiguity. Regardless of our treatment of the dependent variable, the substantive conclusions were identical.

Table 3 Ordinary least squares (OLS) regression models predicting risk estimates

Variables	Model 1: higher unemployment	Model 2: arrest for insider trading	Model 3: arrest for tax fraud	Model 4: arrest for insurance fraud
Experimental manipulation				
Received information—surprised	–0.541	−10.729***	−8.117**	−8.314**
Received information—not surprised	0.552	−8.555**	−5.229	−7.151**
Control variables				
Prior offending	0.076	−1.017	−4.101	−4.439
Prior arrest	0.553	−3.730	−3.981	−4.070
Family/peer arrest	–0.446	2.405	2.947	−3.304
Low self-control	–0.256	−1.237	–0.373	–0.945
White	1.729	−6.371*	−6.906*	−10.937***
Female	0.561	2.571	1.587	4.864*
Age	0.098	0.188*	0.109	0.188*
Education	−3.971***	–4.410***	−3.417**	−2.944*
Income	−2.070*	–2.625**	−3.190**	−3.031**
Employed full-time	1.998	0.246	0.867	−2.972
Married	−1.849	0.789	–0.635	1.140
Parent	5.364*	1.530	2.240	1.221
Intercept	62.818***	72.667***	83.997***	70.494***
R^2	0.040	0.065	0.065	0.092
N	811	808	816	815

Presented are unstandardized coefficients

$^*p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$ (two-tailed)

(Model 4).[12] By contrast, among respondents who are not surprised, the arrest statistics have no effect on ambiguity. The results thus provide partial support for the hypothesis that newly acquired information will increase ambiguity when it is inconsistent with individuals' subjective priors—that is, when it surprises them. However, we find no support for the hypothesis that newly acquired information will reduce ambiguity when it is consistent with individuals' prior beliefs—that is, when it does not surprise them.

The final portion of the analyses examines whether exposure to arrest statistics exerts an indirect effect, through risk estimates and ambiguity, on respondents' self-reported probability of white-collar offending. Again, it bears emphasizing that, because we analyze intentions to offend as proxy for criminality, readers should exercise caution when drawing inferences about criminal behavior from our findings. Before examining the indirect effects of the treatment, we first evaluate whether sanction perceptions are associated with intentions to offend, net of the experimental treatment and the control

[12] It is possible that the differential effect of the treatment on ambiguity across the three white-collar offenses may reflect differences in respondents' familiarity with the respective behaviors. For example, respondents may have had an especially low level of familiarity with the stock market.

Table 4 Ordinal logistic regression models predicting ambiguity levels

Variables	Model 1: unsure higher unemployment	Model 2: unsure arrest insider trading	Model 3: unsure arrest tax fraud	Model 4: unsure arrest insurance fraud
Experimental manipulation				
Received information—surprised	1.364	1.322	1.533*	1.581*
Received information—not surprised	1.015	1.001	1.076	1.233
Control variables				
Prior offending	0.953	1.277	1.161	0.932
Prior arrest	0.857	0.755	0.895	1.072
Family/peer arrest	0.833	0.912	0.660**	0.710**
Low self-control	1.213*	1.018	1.079	1.090
White	0.835	1.647**	1.665**	2.040***
Female	1.323*	1.178	0.993	1.202
Age	0.986**	0.976***	0.982***	0.982***
Education	1.135	1.254**	1.333***	1.311***
Income	1.083	1.048	1.098	1.028
Employed full-time	0.972	0.807	1.016	0.806
Married	1.281	1.145	0.916	1.053
Parent	0.661*	0.771	0.886	0.672*
Nagelkerke R^2	0.046	0.073	0.084	0.089
N	810	806	816	812

Presented are odds ratios

$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$ (two-tailed)

variables. Because the three outcome variables of interest are binary measures, we use logistic regression to estimate the models. Table 5 presents the relevant regression results.

The findings reveal that, for all three white-collar offenses, respondents who perceive a higher risk of arrest are less likely to report a non-zero probability of offending.[13] This is consistent with perceptual deterrence theory. On the other hand, we find little evidence that ambiguity in risk perceptions is consequential for offending decisions. Only one of the three coefficients for the effect of ambiguity on the probability of offending is significant. Specifically, ambiguity in arrest risk for tax fraud is positively associated with the likelihood of reporting a non-zero probability of committing the offense.[14] This finding

[13] We estimated a series of supplementary models including power polynomials—quadratic, cubic, and quartic—for the measures of perceived arrest risk. We did not observe consistent evidence of a non-linear relationship between perceived arrest risk and intentions to offend. The quadratic and quartic terms were significant in the models for insurance fraud, but these effects did not emerge for the other two offenses.

[14] When negative binomial regression models are estimated with continuous versions of the offending variables, the coefficients for the effects of perceived arrest risk and ambiguity on tax fraud offending (Model 2) are not significant. The results for the other two white-collar offenses remain unchanged.

Table 5 Logistic regression models predicting whether respondents reported a non-zero probability of white-collar offending

Variables	Model 1: insider trading	Model 2: tax fraud	Model 3: insurance fraud
Experimental manipulation			
Received information—surprised	–0.409	0.147	0.065
Received information—not surprised	–0.040	0.293	0.319
Risk estimates			
Arrest for insider information	–0.007**	—	—
Arrest for tax fraud	—	–0.009**	—
Arrest for insurance fraud	—	—	–0.011***
Ambiguity levels			
Unsure arrest insider information	0.101	—	—
Unsure arrest tax fraud	—	0.128*	—
Unsure arrest insurance fraud	—	—	0.046
Control variables			
Prior offending	1.134***	1.240***	1.318***
Prior arrest	0.214	0.254	0.207
Family/peer arrest	–0.013	–0.227	0.030
Low self-control	0.491***	0.467***	0.437***
White	–0.745***	–0.608**	–0.667**
Female	–0.160	–0.340	–0.111
Age	–0.014*	–0.003	–0.023***
Education	–0.131	–0.113	–0.133
Income	0.016	0.078	0.076
Employed full-time	–0.140	–0.051	–0.094
Married	–0.400*	–0.314	–0.192
Parent	–0.110	–0.172	–0.180
Intercept	0.167	−1.103	0.170
Nagelkerke R^2	0.192	0.170	0.207
N	806	816	812

Presented are unstandardized logistic regression coefficients

*$p<0.05$; **$p<0.01$; ***$p<0.001$ (two-tailed)

contrasts the results of Loughran et al.'s (2011) study, which is the only other investigation to date to evaluate the deterrent effects of ambiguity in perceived arrest risk.

Across all of the crime scenarios, the control variables are related to the probability of offending in ways that are highly consistent with what would be expected on the basis of prior research. This supports the construct validity of our intentions to offend measures (see Thornberry and Krohn 2000). For example, in each of the models in Table 5, *Prior Offending* and *Low Self-Control* are positively associated with the self-reported probability of offending. The coefficients for these two variables are highly significant ($p<0.001$) and sizable. In addition, White respondents are consistently less likely than non-White respondents to report a non-zero probability of offending.

Finally, older respondents are less likely than their younger counterparts to report a non-zero probability of offending in the scenarios for insider trading (Model 1) and insurance fraud (Model 3).

We next assess whether the experimental treatment exerted an indirect effect on intentions to offend through risk perceptions or ambiguity. The classic approach to mediation analysis is Baron and Kenny's (1986) causal steps procedure, which involves first establishing a zero-order relationship between the independent and dependent variables before testing for indirect effects. However, recent scholarship has demonstrated that this approach has several serious flaws (Hayes 2009, 2013; Zhao et al. 2010). Most notably, in Hayes's (2013: 170) words, "it is a mistake to condition the hunt for indirect effects on evidence of a total effect of X."[15] Specifically, as Zhao et al. (2010) emphasize, "there need not be a significant zero-order effect of X on Y, r_{xy}, to establish mediation" (p. 199), but, rather, "all that matters is that the indirect effect is significant" (p. 204).[16]

For this reason, we computed the specific indirect effects through both mediators using the product of the coefficient approach (see Hayes 2009, 2013). This involved multiplying the regression coefficients for the effect of the two treatment variables on the two mediators, net of the controls, with the regression coefficients for the effects of the mediators on the binary outcomes, net of both the treatment variables and the controls.[17] All coefficients were rescaled (standardized) before multiplication (see MacKinnon and Dwyer 1993).[18] Although, as noted above, we found no evidence that ambiguity is related to intentions to offend for insider trading or insurance fraud, we, nonetheless, tested for possible indirect effects on these outcomes through this mediator. This was done because, as Hayes (2009: 410–411) has explained, the test for an indirect effect should not be preconditioned on the significance of the constituent relationship between the mediator and outcome. Consistent with the best practices in mediation analysis (Hayes 2009; Zhao et al. 2010), we used percentile-based bootstrap ($k = 5{,}000$) confidence intervals to determine the significance of the indirect effects.

The standardized indirect effects of the treatment and the associated bootstrap confidence intervals are presented in Table 6. For all three white-collar offenses, among those respondents who were surprised by the information, the information treatment exerted a significant *positive* indirect effect on intentions to offend through arrest risk estimates. Likewise, even among those respondents who were not surprised by the information, the treatment exerted a significant positive indirect effect on intentions to offend via risk perceptions for two of the three offenses (i.e., insider trading and

[15] Other problems with the Baron and Kenny (1986) method include that (1) it infers rather than quantifies the indirect effect and (2) it does so using three separate hypothesis tests, which renders it "one of the least powerful approaches to testing mediation" (Hayes 2013: 168).

[16] In our study, for all three white-collar offenses, there is not a significant total effect of the treatment on intentions to offend. However, despite common wisdom, this fact is not informative about whether an indirect effect of the treatment exists. Hayes (2009: 413) stresses this point in his seminal discussion of mediation analysis: "it is easy to show that the claim that X can't affect Y in the absence of a detectable total effect is false."

[17] Although structural equation modeling (SEM) can be used in lieu of OLS or logistic regression for estimating indirect effects, "doing so is neither necessary nor better" (Hayes 2013: 159).

[18] We used the Stata command "binary_mediation" to perform the mediation analyses.

Table 6 Standardized indirect effects of the information treatment on self-reported non-zero probability of white-collar offending

Indirect effects of experiment	DV = insider trading			DV = tax fraud			DV = insurance fraud		
	Std. Coef.	95 % CI		Std. Coef.	95 % CI		Std. Coef.	95 % CI	
		Lower	Upper		Lower	Upper		Lower	Upper
Received information—surprised									
→ Risk estimate	0.015*	0.003	0.033	0.016*	0.003	0.033	0.017*	0.004	0.035
→ Ambiguity	0.004	–0.002	0.013	0.007	–0.001	0.020	0.003	–0.006	0.013
Received information—not surprised									
→ Risk estimate	0.013*	0.002	0.028	0.010	–0.000	0.026	0.016*	0.003	0.034
→ Ambiguity	0.000	–0.006	0.006	0.001	–0.006	0.009	0.001	–0.003	0.007
N	806			816			812		

Presented are standardized regression coefficients

CI bootstrap confidence interval, *DV* dependent variable, *Std. Coef.* standardized regression coefficient

*$p < 0.05$ (two-tailed)

insurance fraud). The direction of these indirect effects is consistent with the evidence presented earlier that the information treatment reduced respondents' estimates of arrest risk. Although the magnitude of the standardized coefficients may seem relatively modest, it is important to keep in mind that these effects reflect the impact of a single exposure to objective information. In practice, any public communication campaign about legal risk is likely to result in repeated exposure to information over an extended period of time. It is, thus, likely that the effect sizes observed in our one-shot study would be amplified considerably in the context of an actual public communication campaign. We found no evidence that the treatment had an indirect effect on intentions to offend through ambiguity in perceived risk.

Supplementary analyses

At the request of a reviewer, we estimated supplementary models using indices that combined across the three offense types to measure perceived arrest risk, ambiguity, and intentions to offend. To measure *Arrest Risk* (alpha = 0.780, range = 0–300) and *Ambiguity* (alpha = 0.832, range = 3–18), respectively, we summed across the respective responses for each of the three offense types. These two indices are both normally distributed continuous measures, and, thus, we used OLS to model both of them. *Intention to Offend* is a binary variable coded "1" if the respondent reported a non-zero intention to offend in any of the three crime scenarios, and coded "0" otherwise. We used logistic regression for this variable. The results are presented in Table 7 of the Appendix. Consistent with our previous findings, they show that the experiment significantly reduced perceived arrest risk (Model 1), and increased ambiguity among those who were surprised (model 2), but did not have a direct effect on offending intentions (Model 3). Perceived arrest risk was again negatively correlated with offending intentions. In turn, the experiment had a positive indirect effect on offending intentions through perceived arrest risk among both those who were surprised ($b=0.013$, $p<0.05$) and those who were not surprised ($b=0.012$, $p<0.05$).

Discussion

In every decade since the 1970s, a new review of the deterrence literature has emphasized that insufficient scientific knowledge exists about how individuals form and modify their sanction perceptions (Apel 2013; Cook 1980; Kennedy 2009; Nagin 1998; Zimring and Hawkins 1973). Scholars have also stressed that, for crime policies and criminal justice activities to have deterrent value, (1) legal authorities must communicate information about sanction threats to the public and (2) citizens must use this information to update their sanction perceptions (Kennedy 2009; Nagin 2013). Yet, to date, scant research has evaluated whether legal risk information can be effectively communicated to the public, or how such information affects individuals' sanction perceptions. More generally, as Kennedy (2009: 134) explains, "communication has only sometimes been recognized as important in deterrence theory, virtually never in deterrence practice, and very little theoretical or practical development of the idea has occurred." This is particularly true in the case of sanctions for white-collar crime, which is surprising given the social impact of such offenses (Simpson 2013).

In this study, we sought to address this research void by more deeply exploring the link between objective level information and individuals' risk perceptions. We tested a series of logical propositions gleaned from Bayesian updating models of perception formation. Several notable findings emerged from our analyses. First, supporting our first hypothesis, we found that providing respondents with objective information about the detection probabilities for white-collar offenses had no effect on either their risk estimates or ambiguity levels for a non-crime-related event (i.e., changes in unemployment). This finding is important because it suggests that individuals make meaningful judgments about the value of new information for understanding different risks, rather than haphazardly updating their risk perceptions on the basis of any new information, regardless of its relevance to the specific risks in question.

Second, for the three white-collar crime scenarios, the objective information did significantly and substantially reduce point estimates of arrest risk in the treatment group. The effects were larger among those respondents who were surprised by the information. This finding supports Bayesian learning theory, and is consistent with the evidence deriving from a previous study assessing perceptions of punishment severity. Specifically, Chapman and colleagues (2002) evaluated whether providing respondents with information either in the form of a booklet, video, or seminar influenced knowledge about sentence severity for street crimes, such as burglary and rape. They found that the information improved knowledge about sentencing severity, particularly among those respondents who reported being surprised by it.

That, in our study, the provided information reduced estimates of arrest risk is logical, as the control group revealed point estimates of arrest risk that were much higher, on average, than the corresponding objective rates. More generally, this finding is consistent with prior research showing that most members of the public, particularly those who are not heavily involved in crime, are naïve about sanction risk, and overestimate the probability of arrest (Matsueda et al. 2006; Piquero et al. 2012). As we explain below, this naïveté, which Jensen (1969: 189) describes as a "shared misunderstanding" and Tittle (1980: 67) refers to as the "shell of illusion", suggests that deliberate communications about legal risk may have deterrent value only among persons who initially underestimate sanction risk.

Third, as would be expected on the basis of the economic conceptualization of ambiguity (Camerer and Weber 1992), in cases where individuals in the treatment group revealed that they were surprised by objective rates, we observed increased levels of ambiguity. Fourth, and conversely, we observed no reduction in ambiguity among individuals who were not surprised. In other words, among individuals who received objective information that was consistent with their prior beliefs, and, thus, that should have reinforced their prior beliefs, we did not observe an associated increase in confidence. The findings are, thus, only partially consistent with our fourth hypothesis, and provide mixed support for Bayesian learning theory.

Fifth, we found that there was a significant indirect positive relationship between receiving objective information and intentions to commit white-collar crimes, which was larger among respondents who were surprised by the information. This indirect relationship was mediated by estimates of arrest risk, which were negatively correlated with both the treatment and intentions to commit white-collar criminality. Stated differently, because providing respondents with objective information about the probability of arrest for white-collar crimes decreased their perceptions of arrest risk, it also, in turn, appears to have increased their intentions to commit the offenses described in the scenarios.

Prior theoretical work, as well as some limited correlational and qualitative evidence, suggests that there may be deterrent value in the "shared misunderstanding" that sanction risk is high, and that if individuals learn about the true state of affairs, they may become more likely to commit crime (see Jensen 1969). Our experimental findings support this idea. The key policy implication is that the effect of directly communicating factual information about the certainty of arrest to the public may be conditional on the nature of the public's preexisting sanction perceptions. Specifically, such communication may have a deterrent effect for crimes, or among groups, where arrest risk is initially underestimated, but may have a criminogenic effect when arrest risk is initially overestimated. The key assumption, though, is that sanction perception updating is symmetrical—that is, individuals are equally willing to both increase or decrease their arrest risk perceptions in response to new information. If updating is asymmetrical, such that persons are less willing to increase (rather than decrease) their sanction perceptions, legal risk communications may be less effective for deterrence. Prior research suggests that updating is symmetrical, at least in the case of experiential learning (Anwar and Loughran 2011; Matsueda et al. 2006). Our study, however, which focuses on non-experiential learning, can only provide evidence of downward updating, because respondents overestimated the arrest risk for all of the crimes we examined. Future research is, thus, needed that explores whether persons are willing to increase their sanction perceptions when exposed to legal risk communications revealing that punishment risk is higher than they initially believed.

Note also that our findings should not simply be interpreted as suggesting that legal risk communications will have a deterrent effect among actual offenders, but not among the general public, because even experienced offenders drastically *overestimate* arrest risk for many crimes. Lochner (2007: 448), for example, found that offenders who had stolen a car perceived the arrest risk for auto theft to be 45 %, when the objective risk was actually only 10 %. By contrast, for some offenses, such as murder and aggravated assault, the general public actually *underestimates* arrest risk (Erickson and Gibbs 1978; Kleck et al. 2005). The important point, then, is not that public communication campaigns about legal risk cannot deter offending. Rather, it is that before engaging in any such campaign, policymakers should take steps to accurately assess the target population's subjective priors about the specific crimes in question. Information campaigns based on communicating objective arrest risk may hold promise for reducing crime if, and only if, the target population's subjective priors for a given offense are found to be lower, on average, than the objective arrest risk.

Taken together, our findings imply that individuals are willing to incorporate relevant objective information into their subjective beliefs about sanction risks, though the degree to which this occurs may be limited. The observed reduction in risk estimates in the treatment group was both large and statistically significant for each of the white-collar crime outcomes, though the averages for the treatment group did not nearly reach the level of any of the objective rates.[19] Our results also show that, by considering ambiguity in perceived risk, objective information can be linked to individual perceptions. Interestingly, we did not observe a hypothesized reduction in ambiguity for individuals who were not surprised by the objective information. This finding may reflect our somewhat coarse measure of surprise meant to serve as a proxy for prior beliefs. However, it could also indicate that ambiguity in risk perceptions is less of a factor derived from economic concepts of Bayesian updating (i.e., second order probabilistic; Camerer and Weber 1992) and perhaps related to more individual-specific factors (see Pickett and Bushway 2015; Pickett et al. 2015a). For example, Thomas et al. (2013) found that individuals with higher levels of anxiety tended to place greater weight on unobservable factors when updating their risk perceptions. If ambiguity is not a pure rational concept linked to Bayesian updating but, instead, a function of individual-level traits (e.g., anxiety), then this too has implication for threat communication. The implication here is that, if individuals are not sensitive to new information in that they are not becoming more or less confident of their sanction beliefs through exposure to indicators of objective sanction risk, then the general deterrent effects of macro-level policy shifts might be less than anticipated.

Our findings also contribute to efforts to understand how sanction perceptions influence white-collar criminality. First, we find very little evidence that ambiguity (or lack of confidence) in risk perceptions is associated with intentions to offend, which contrasts with Loughran et al.'s (2011) results for street offending. This may indicate that the deterrent value of ambiguity in perceived arrest risk varies by crime type.

[19] There is no definitive reason why risk perceptions should directly converge to objective rates. Individuals exercise bounded rather than perfect rationality (Clarke and Cornish 2001). Additionally, perceptual errors in estimating arrest risk are not random, in which case they would cancel each other out at the aggregate level, but, instead, tend to be biased toward overestimating arrest risk. The convergence of levels of perceived and objective arrest risk is also not a requisite assumption for the standard economic model of crime, as individual-specific detection probabilities can vary due to multiple factors, including different levels of skill and experience, offense mix, and presence of self-serving bias in self-evaluation. This is why models of Bayesian updating, which are harmonious with rational choice models of offending, allow for each individual to have their own mean.

Second, our study was able to address a key limitation of the extant research examining perceptual deterrence processes for white-collar crimes. Specifically, prior studies have generally been unable to control for prior offending, prior arrest, or low self-control (e.g., Kroneberg et al. 2010; Paternoster and Simpson 1996; Piquero et al. 2005; Smith et al. 2007), all of which are potential sources of omitted variable bias. For example, there is evidence that low self-control affects risk perceptions as well as intentions to offend (Van Gelder and de Vries 2012). In addition, prior offending and prior arrest may both impact perceived arrest risk (Anwar and Loughran 2011; Matsueda et al. 2006), and may also, if they are correlated with unmeasured criminogenic influences (e.g., low social capital or a deviant self-identity due to labeling), be associated with intentions to offend. Our models show that, net of prior offending, prior personal and vicarious experiences with arrest, and low self-control, the perceived certainty of arrest is negatively correlated with intentions to commit white-collar crime. Finally, for all three of the white-collar offenses that we examine (i.e., insider trading, tax fraud, and insurance fraud), we find a strong positive association between low self-control and intentions to offend, which supports Holtfreter and colleagues' (2010a, b) findings for other types of fraud (e.g., credit card fraud and driver's license fraud).

There are several important limitations to the current study worth noting, which we hope will be addressed in future research. First, our information treatment presented clearance rates as objective measures of arrest risk. Yet, scholars (Cook 1977, 1979; Nagin et al. 2015) have long argued that the clearance rate may be an invalid measure of objective arrest risk for several reasons, such as its endogeneity with criminal decision-making, its inability to measure arrest risk for crimes not committed, and its exclusion of unreported crimes. Our models, however, make no assumptions about the accuracy of the presented clearance rates as measures of objective arrest risk. Rather, our only assumption is that respondents *perceive* that the presented clearance rates are informative about the generalized arrest risk for the relevant crime types. In this sense, the key limitation of using clearance rates in our experiment is likely that the rates overestimate the actual arrest risk, because they exclude unreported crimes (Apel 2013; Cook 1977). It is probable, then, that if we had presented respondents with the ratio of arrests to *all* crimes committed, both reported and unreported, we would have observed even larger reductions in perceived arrest risk. Future studies should, thus, replicate our analyses using alternative measures of objective arrest risk.

Related to the above point, the clearance rates that we presented to respondents were for broad categories of white-collar offending (e.g., fraud). They, thus, lumped together many different offenses (e.g., tax fraud, securities fraud, mortgage fraud), and did not overlap exactly with either the crimes or situations described in the offending scenarios. However, we do not believe that this is a cause for concern in our analyses for two reasons. First, it is unlikely that "perfect information" about one's *own* arrest risk in a specific criminal situation will normally exist, except in the minority of cases where the arrest risk is 100 % (e.g., a police officer is present). Consider, for example, the infinite number of different personal (e.g., skill) and environmental (e.g., the presence of witnesses) factors that could influence the probability of apprehension for a specific crime. It is implausible that there is a knowable objective arrest risk for every combination of these factors. Therefore, the type of objective arrest information that is likely most relevant to potential offenders is the arrest risk for a given category of similar crimes (see, e.g., Anwar and Loughran 2011; Loughran et al. 2011), aggregated across offenders, victims, and situational contexts. This is because, in specific criminal situations, potential offenders likely use their perceived generalized arrest risk, the perceptual counterpart of the objective generalized arrest risk (or clearance rate), as an anchor, adjusting the level of assessed situational arrest risk to account for personal and environmental factors that increase or decrease the probability of apprehension.[20] In the absence of relevant situational factors, the generalized arrest risk would provide the best estimate of situational arrest risk. Second, and supporting the above argument, we observed relatively large effects despite having presented clearance rates for broad categories of offenses, which suggests that respondents did, in fact, use the information about the objective generalized arrest risk to estimate their situational arrest risk. Nonetheless, it would be helpful if future studies examined whether similar effects emerge when arrest rates are provided for specific types of white-collar offenses, and for specific types of criminal situations.

Additionally, we were not able to observe how individual perceptions track or change over time. Longitudinal data would allow for a more dynamic measure of updating that could incorporate both experiential factors (such as private information acquired over time) and objective factors into the updating process. It would also permit researchers to examine sanction perception updating using direct measures of prior beliefs, rather than relying, as we did, on a proxy measure of prior beliefs (i.e., whether the respondent was surprised). Indeed, it is possible that it was actually our proxy measure of prior beliefs, rather than the provided arrest statistics, that caused the observed reductions in perceived arrest risk. Specifically, the respective question asked if respondents were surprised "that the risk of arrest is this low", which could have primed respondents to perceive a lower level of arrest risk. We doubt this explanation for the findings, however, because it would not explain the observed differences in the magnitude of the treatment effects across respondents who were surprised versus those who were not surprised. These respondents responded to the same survey question, and, thus, should have been similarly primed if the question had a priming effect. Nonetheless, it is important for future research to examine information-based updating in the context of a longitudinal experiment in which sanction perceptions are measured both before and after the provision of information.

Third, our investigation, like most cross-sectional studies of perceptual deterrence processes, relied on respondents' intentions to offend as a proxy for criminality. While this approach has strengths—it establishes both the appropriate temporal ordering and concurrent measurement of sanction perceptions and criminality (Grasmick and Bursik 1990; Pogarsky 2004)—it also has a key weakness. Specifically, its use raises the possibility that observed relationships may not hold when criminal behavior is analyzed. There is, thus, a need for subsequent research that replicates our study using measures of actual offending.

Finally, and equally as important, we evaluated only one type of information treatment. Specifically, we provided respondents' with base-rate statistical information about arrest risk. However, the risk communication literature suggests that some individuals are better able to learn from personal narratives or exemplars, particularly emotional ones, than from base-rate information (Betsch et al. 2011; Zillmann 2006). For this reason, additional studies are needed that evaluate the potential effects of other

[20] By definition, the mean of an offender's subjective probability distribution necessarily provides a point estimate of generalized arrest risk, not of situational arrest risk, because the subjective distribution contains information amassed over time from his or her full set of relevant personal, vicarious, and mass-mediated experiences. This point estimate must then be adjusted based on the situational context.

forms of criminal justice "advertisements" on sanction perception updating as well as criminal behavior. Equally important, researchers should compare the relative effectiveness of deterrence communications to other forms of crime-prevention communications (e.g., moral persuasion) (e.g., Ariel 2012), and also consider whether there is preventative value to a combining different approaches.

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Appendix

Table 7 Supplementary models using indices combining measures of risk, ambiguity, and offending across all three offenses

Variables	Model 1: arrest risk[a]	Model 2: ambiguity[a]	Model 3: intention to offend[b]
Experimental manipulation			
Received information—surprised	−25.469**	0.738*	–0.332
Received information—not surprised	−22.143**	0.116	0.067
Sanction perceptions			
Arrest risk	—	—	–0.003*
Ambiguity	—	—	0.040
Control variables			
Prior offending	−9.558	0.230	1.398***
Prior arrest	−11.890	–0.178	0.266
Family/peer arrest	2.264	–0.564*	0.165
Low self-control	−2.649	0.032	0.421***
White	−25.620**	0.948**	–0.637**
Female	10.070	0.247	0.020
Age	0.502**	–0.041***	–0.018***
Education	−11.233***	0.500***	–0.195*
Income	−8.468***	0.107	0.022
Employed full-time	−2.078	–0.309	0.058
Married	0.807	0.133	–0.335
Parent	4.162	–0.436	–0.317
Intercept	228.146***	7.858***	0.883
R^2	0.101	0.080	—
Nagelkerke R^2	—	—	0.203
N	802	802	802

Presented are unstandardized regression coefficients

[a] OLS regression

[b] logistic regression

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$ (two-tailed)

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Justin T. Pickett, Ph.D. is an assistant professor in the School of Criminal Justice at the University at Albany, SUNY. He received his Ph.D. in criminology in 2011 from the College of Criminology and Criminal Justice at Florida State University. Justin is the 2015 recipient of the American Society of Criminology's Ruth Shonle Cavan Young Scholar Award. His research interests include survey research methods and public opinion about crime and punishment. He is currently examining cognitive heuristics and sanction perceptions. His recent work has appeared in *Criminology*, the *Journal of Research in Crime and Delinquency*, *Sociological Methods & Research*, and other leading criminology and sociology journals.

Thomas A. Loughran, Ph.D. is an associate professor in the Department of Criminology and Criminal Justice at the University of Maryland. He received his Ph.D. in public policy in 2007 from the H. John Heinz III School of Public Policy and Management at Carnegie Mellon University. His research interests include deterrence and offender decision-making, individuals' responses to sanctioning, and quantitative methods. His work has appeared in the *American Sociological Review*, *Sociological Methods & Research*, *Criminology*, and other leading sociology and criminology journals.

Shawn Bushway, Ph.D. is a professor of criminal justice, and public administration and policy at the University at Albany, SUNY. He received his Ph.D. in public policy analysis and political economy in 1996 from the H. John Heinz III School of Public Policy and Management at Carnegie Mellon University. His current research focuses on the process of desistance, the impact of a criminal history on subsequent outcomes, and the distribution of discretion in the criminal justice sentencing process. He has published in *Criminology*, *Justice Quarterly*, *Criminology and Public Policy*, and other leading crime and policy journals.