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UNIVERSITY OF CALIFORNIA,

IRVINE

Juror Perceptions of Incentivized Informant Testimony

DISSERTATION

submitted in partial satisfaction of the requirements  
for the degree of

DOCTOR OF PHILOSOPHY

in Psychological Science

by

Emily Victoria Shaw

Dissertation Committee:  
Associate Professor Nicholas Scurich, Chair  
Distinguished Professor Elizabeth Loftus  
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2021



## **DEDICATION**

This dissertation is dedicated to my grandfather, Robert MacGregor Shaw.

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**Shaw, E. V.**, Scurich, N., & Faigman, D. L. (2018). Intellectual disability, the death penalty, and jurors. *Jurimetrics*, 58, 437-458.

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## **SELECTED GRANTS, AWARDS & HONORS**

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## **ABSTRACT OF THE DISSERTATION**

Juror Perceptions of Incentivized Informant Testimony

by

Emily Shaw

Doctor of Philosophy in Psychological Science

University of California, Irvine

Associate Professor Nicholas Scurich, Chair

Informants are an integral part of the American criminal justice system. However, relatively little research has been done on nearly all aspects of informant use, from how they are recruited and how they reach agreements to cooperate with law enforcement to how jurors evaluate their testimony in court. The present study focuses on this last area – juror perceptions of informants who testify. The limited research that exists on this topic has presented troubling conclusions: jurors may not be appropriately responsive to cues that could signal informant unreliability. In particular, jurors may fail to account for and properly weigh evidence that an informant is testifying for an incentive (such as a reduced prison sentence) when reaching a verdict. However, thus far, studies in this area have some limitations in case type, materials used, and statistical power. The objective of the current study is to advance in each of these areas and provide new evidence about the impact of juror perceptions of informant incentives. This study used a novel fact pattern, video stimuli manipulations, and a sample of 886 online participants to test the impact of informant incentives on juror judgments. It featured four conditions that vary the nature and size of the incentive reported by a jailhouse informant (i.e., no incentive, a vague leniency incentive, and small and large sentence reduction incentives), plus a control condition

with no informant testimony. The results speak to the conflicted literature on this topic – finding a significant impact of incentive presence on verdicts. Participants who observed an incentivized informant rated him as less credible and were less likely to convict the defendant based on his testimony – compared to participants who viewed an otherwise identical non-incentivized informant. However, participants’ verdicts in this study were not sensitive to changes in the size of an informant’s incentive, showing no significant difference in conviction rates when the incentive was large (10 years off of a 20-year sentence) compared to small (1 year off of a 20-year sentence). This study also included a test of a novel hope-for-leniency condition, which prompted verdict responses most similar to that of the small incentive condition. These results suggest that jurors may be more capable than previously thought at accounting for informant incentives in reaching a verdict. It also suggests that reforms that aim to preserve evidence of jailhouse informant incentives and ensure that such information is able to reach a jury at trial have potential to meaningfully influence verdict outcomes.

## **CHAPTER 1: AN INTRODUCTION TO INFORMANT TESTIMONY**

### **Background: Defining an Informant**

Informants play a crucial role in the American criminal justice system. They are used by police and prosecutors across the country in the prosecution of criminal defendants. The term “informant” broadly refers to any lay individual who provides law enforcement with details about a defendant’s crime. This umbrella term includes several sub-categories of witnesses: jailhouse informants, accomplices/co-defendants and cooperating witnesses. Jailhouse informants are not direct participants in the crime, but claim to have information about it, often in the form of a “secondary confession” from the defendant while in jail or prison (see Neuschatz et al., 2008; Neuschatz et al., 2020). Informants can also be co-defendants or accomplices in a crime, such as a getaway driver or a friend called to assist in a criminal cover-up after the fact; these informants may provide evidence against their co-defendant, often in exchange for potential leniency in sentencing. Some informants can also be classified as cooperating or “civic duty” witnesses – individuals who are not incarcerated or implicated in a crime, but who obtain knowledge of the defendant’s criminal activities.

Informants may provide law enforcement with several different kinds of evidence. The most widely recognized form is that of a secondary confession, in which the informant claims the defendant admitted or confessed to participating in criminal activities, either directly to the informant or to a third party. Informants may also assert that the defendant revealed (on purpose or by accident) other incriminating details that fall short of full admissions of responsibility, such as revealing an alibi was falsified or making statements that contradict the defendant’s earlier accounts of the crime. Informants outside of prison may also personally assist law enforcement

in gathering further evidence of additional criminal activities (e.g., participating a sting operation) to build a stronger case against a defendant.

One factor that distinguishes informants from other types of lay witnesses is the frequent presence of incentives to testify. While some informants do approach law enforcement without the hope or suggestion of an incentive, many cases featuring informants involve some potential benefit to the informer (see Cassidy, 2004; Roth, 2016). These incentives can be related to reductions of negative experiences (such as reduced prison time) or presentations of positive rewards (such as monetary benefits). For example, informants facing criminal charges of their own may receive reduced prison sentences or have charges against them dropped in exchange for their cooperation with the government (Natapoff, 2009). Informants who are already sentenced (i.e., jailhouse informants) may receive perks within prison, such as better cell accommodations, sources of entertainment or improved access to visitors (L.A. Grand Jury Report, 1990). Other informants not facing charges or prison time may receive external incentives, such as money, access to favors or other valued resources (see Cassidy, 2004).

### **The Use of Informants in Criminal Cases**

The use of informants in criminal justice predates the United States by thousands of years, with some of the earliest records of informant use coming from ancient Athens around 400 BCE (Lanni, 2010; Bloom, 2005). Within the United States, one of the earliest documented cases of informant testimony occurred in Vermont in 1819. Stephan and Jesse Boorn were two brothers accused of murdering Russell Colvin, their brother-in-law. At trial, the prosecution called a jailhouse informant, Silas Merrill, who testified that Jesse Boorn confessed to the murder

while in jail. The brothers were convicted and sentenced to death – but were spared when Mr. Colvin turned up alive in New Jersey (see Surratt, 2018).

Today, informants are an integral part of the criminal justice system, used by prosecutors in a variety of ways (see Roth, 2016; Natapoff, 2009). Before a defendant is arrested or charged, the statement of an informant can be presented to a judge to obtain a warrant for further investigation. Informants who are still on the street may help law enforcement collect further evidence against a suspect; they may be asked to wear a wire, to participate in criminal activities or help lure a suspect to a particular location. Once a suspect is in custody, prosecutors can use the threat of informant testimony as leverage to pressure defendants to confess to crimes or make a plea agreement. For defendants kept in police custody, jailhouse informants may provide statements to prosecutors detailing alleged confessions made by the defendant. Finally, if a defendant does go to trial, an informant may be called as a witness to testify against him or her.

Despite being an integral part of the justice system, the use of informants by law enforcement in the United States is largely unregulated. Prosecutors across the country rely on informants without being required to report to the courts or any regulatory body when and how they use them in their cases, especially in cases that do not go to trial or where the informant is not called (Cassidy, 2004). Prosecutors have wide discretion in recruiting informants and deciding the incentive they will be offered in exchange for cooperation (Roth, 2016). They often do not publicly release the identity of informants (for safety reasons or to protect an ongoing investigation), so criminal defendants may not even know the identities of accusers offering information against them until close to the start of trial (Cassidy, 2004).

Even major jailhouse testimony reforms, such as Texas House Bill 34, have notable limitations. Texas House Bill 34 went into effect in 2017 and requires prosecutors to track the



use of jailhouse informant testimony and any benefits offered in exchange for that testimony (see Allen, 2020). It specifically allows evidence of an informant's prior offenses to be admitted for impeachment purposes if the informant received a benefit (e.g., charges were dropped in exchange for cooperation). It also requires the state to disclose to the defense information such as a jailhouse informant's criminal history; offers of immunity, sentencing reduction or other benefits; and information about other criminal cases where the informant has testified. However, this reform still has major limitations in that it depends on prosecutors to track and disclose information. Prosecutors face pressure to secure convictions, and this reform does not create new oversight requirements (see Allen, 2020). It also did not establish or require a database for sharing information, making it challenging for prosecutors and defense attorneys to share and access this information. Finally, this reform assumes that if information about informant incentives and prior history are successfully tracked and consistently presented to juries, juries would be sensitive to that information and use it when reaching their verdicts. However, whether or not jurors actually are influenced by such information is an empirical question in need of additional study (see Chapter 3).

### **The Dangers of False Informant Testimony and Protections Against It**

The absence of regulation or accountability leaves room for the misuse of informants. In the last few decades, evidence has started to emerge that links false informant testimony to wrongful convictions. One study of wrongful capital convictions found that informant testimony was used in 46% of cases (Warden, 2004). In another study, Garrett (2011) reviewed 250 cases of DNA exonerations and found that 21% of those cases included informant witness testimony, including 28 cases that featured at least one jailhouse informant who testified at trial. The

Innocence Project (2013) analyzed 325 cases of DNA exonerations and determined that 15% of wrongful convictions featured informant testimony. Together, these findings suggest that false informant testimony is at the very least associated with a meaningful number of wrongful convictions.

These discoveries about informant testimony have added new pressure to an ongoing debate within the legal community about the appropriate use of informants. The controversy surrounding the use of informants is nothing new to the legal system; scholars, policy makers and attorneys have debated the merits and dangers of the system for decades (see Taslitz, 2008; Natapoff, 2009). Proponents of the current use of informants argue that informants are an irreplaceable part of gathering information and bringing criminals to justice. They defend the use of incentives as an unfortunate but necessary tool to prompt informants to come forward and cooperate; they assert that the removal of such incentives would curtail the ability of the government to prosecute offenders (see Cassidy, 2004). Critics of this system argue that the use of tools like incentives may inspire informants to fabricate evidence for personal gain, at the expense of innocent defendants (see Roth, 2016). They also point out that the personal cost an informant faces from being caught fabricating evidence are small (i.e., the chances of further criminal prosecution are perceived as low) and the benefits of cooperation are large, sometimes amounting to years of freedom, a reduced criminal record, or valued monetary gains – and that this combination of factors may dangerously inflate the chances of informants falsifying evidence (see Roth, 2016).

Also, despite the fact that jailhouse informants often have strong incentives to testify for the prosecution, this information is not always made known to juries. Neuschatz et al. (2020) conducted an archival analysis using real trial transcripts from 53 cases of exonerated defendants

identified by the Innocence Project that featured jailhouse informants. The researchers found that 72% of prosecution jailhouse informants denied receiving a benefit for their testimony. Also, 78% of prosecution jailhouse informants gave a dispositional motivation for why they were testifying, such as wanting to do the morally right thing.

The courts have long been cognizant of the threat of unreliable informant testimony. Interest in the topic from the courts goes back decades (see *Lee v. United States*, 1952), and there have been a variety of cases at the state and federal levels that offer insight into the use of criminal informants in relation to defendant rights, culminating in the case of *Giglio v. United States* (1972). In *Giglio*, the Supreme Court ruled that the prosecution had a duty to disclose to the defendant that an informant testifying against him had been promised immunity from prosecution in exchange for cooperation. Because the prosecution's case against Mr. Giglio had relied heavily on the informant's testimony, yet the prosecution had not disclosed relevant evidence that affected the credibility of the informant, the Supreme Court ruled Mr. Giglio was entitled to a new trial.

Subsequent cases have reinforced the ability of the government to offer cooperation incentives to informants (see *United States v. Singleton*, 1999), but have also reiterated the rights of defendants to receive information about informant incentives (*Hoffa v. United States*, 1966). Through these cases and others, the courts have converged on the position that existing defendant protections against false informant testimony are adequate as long as they are enforced (see Cassidy, 2004). In other words, as long as a defendant is notified about informant incentives and allowed to cross-examine on them, and appropriate judicial instructions are given pertaining to informant credibility, the jury should be sufficiently equipped to appropriately weigh the evidence (*Giglio v. United States*, 1972; *Hoffa v. United States*, 1966).

Unfortunately, evidence has mounted that these protections are inadequate. Setting aside the many cases of wrongful convictions on the basis of false informant testimony, there is evidence to suggest that the standards set in cases like *Giglio* have largely just changed how prosecutors reach agreements with informants. *Giglio* formally requires that any promises of leniency made by the government to an informant be shared with the defendant prior to trial, specifically so that the witness can be cross-examined on them. Unfortunately, in practice, prosecutors have readily found ways around this mandate (Cassidy, 2004). As long as prosecutors are sufficiently vague about the incentives available to an informant for cooperating, they can avoid reaching terms clear enough that they require disclosure to a defendant. Loose, non-binding assurances of leniency in exchange for cooperation, often made orally behind closed doors, do not need to be relayed to defendants (Cassidy, 2004). This allows informants to testify in trial, honestly, that they were never promised anything in exchange for their testimony, even when they are aware of the kinds of benefits typically granted and the approximate size of the reduction they may be getting. It is only later, after the defendant has been convicted, that the informant in the case may face sentencing and reap the benefits of cooperation. As a result, information about an informant's likely incentive may be concealed from a jury without any violations of the law. Moreover, even when the defendant is made aware of an informant's incentive to testify, it is still an empirical question as to whether cross-examination is an effective technique for making jurors aware of the impact of informant incentives.

## **CHAPTER 2: THE PSYCHOLOGY OF DECEPTION DETECTION**

In the simplest terms, jurors presented with informant testimony (as well as other forms of witness testimony) are faced with a lie-detection task. The process of lie detection is a combination of two abilities: the ability to correctly recognize a lie and the ability to correctly recognize a truth. Jurors are called to do both when evaluating informant testimony – to recognize when a witness is telling the truth and when a witness is fabricating evidence. Substantial research has been conducted to better understand how accurate laypeople are when it comes to distinguishing lies from truth, as well as the factors they use to make these judgments. In the following chapter, I will briefly review this literature and highlight its major findings.

### **How Capable are Laypeople at Detecting Lies?**

Overwhelmingly, research on lie detection indicates that laypeople are not talented lie detectors; they are right around chance at detecting lies. The most comprehensive meta-analysis on lie detection to date was done by Bond and DePaulo (2006). In their paper, the authors analyzed 206 studies featuring over 24,000 lay participants and found that the average accuracy for lie detection was 54%. When judging lies, participants identified 47% of statements correctly; when judging truths, they identified 61% correctly. A different, independent meta-analysis on the topic published that same year confirmed the overall finding of 54% accuracy for lay judgments (Aamodt & Custer, 2006).

Even if laypeople are poor lie detectors in aggregate, it could theoretically still be possible for some individuals to be meaningfully better at the task of lie detection than others. However, numerous studies have been conducted with the assumption that individual differences

in lie-detecting abilities exist and have sought to measure them, with no consistent results. Individual differences like age, gender, confidence, education or expertise all seem to have no detectable bearing on whether or not an individual will be a capable lie detector (Aamodt & Custer, 2006).

Bond and DePaulo (2008) empirically tested the possibility of individual differences in lie-detecting ability, conducting a meta-analysis of 247 studies on lie detection. They examined the variance among lay judges in percentage of correct responses across studies and found that individuals did not differ meaningfully from one another in detection ability. In other words, the ability to detect lies accurately is not a reliable individual difference. These results were consistent with other prior studies finding that the ability to judge truth was not positively correlated with the ability to judge lies (Levine et al., 1999) and an individual's ability to judge the truthfulness of one person is independent of judgments of another second person (Kraut, 1978). Together, this body of work indicates that laypeople are not especially accurate when it comes to detecting lies, both individually and in aggregate.

### **What Causes Laypeople to be Poor Lie Detectors?**

The literature has offered a variety of potential explanations to account for laypeople's poor performance on lie-detection tasks. One commonly cited possibility is that laypeople are misguided and are considering unreliable cues for lying when assessing accuracy; instead of using behaviors or signals that actually predict lying, laypeople may be relying on stereotypes or false information about how liars behave.

To understand the cues that laypeople are using in making accuracy judgments, many researchers have employed self-report survey measures. For example, the Global Deception

Research Team (2006) surveyed over 2,000 residents of 58 countries, asking them: “How can you tell when people are lying?” They found that the most common beliefs about liars were that liars tend to avoid eye contact, they show signs of nervousness, they tend to be incoherent, and they have signs in body motion. Another more recent study done by Bogaard et al. (2016) on American students ( $n = 104$ ) and police officers ( $n = 94$ ) also examined beliefs about cues for deception. Both groups expressed high belief that avoiding eye contact and increased movement were signals for lying.

Unfortunately for lay lie detectors, the cues cited by laypeople in self-report studies are not actually strong predictors of lying behavior. DePaulo et al. (2003) performed a meta-analysis on 150 samples examining 158 cues to deception. These samples all compared behavior of speakers telling the truth to speakers who were lying. The meta-analysis included a review of behavioral measures (e.g., eye contact, posture shifts, etc.) as well as perceptions from observers (e.g., subjective judgments about a speaker’s evasiveness). The authors found that behaviors like gaze aversion, body movement and most forms of fidgeting were not predictive of lying behavior. Therefore, if laypeople are truly relying on unreliable cues like gaze aversion to detect liars, then that error could help explain their poor performance on lie-detection tasks.

However, there are reasons to question this explanation for poor lie detection. In particular, it is possible that laypeople are not actually using the cues for lie detection that they self-report. People may be repeating cultural scripts about what cues reveal a liar, but when presented with an actual lie-detection task, they may use entirely different cues. This possibility was directly tested in one of several meta-analyses conducted by Hartwig and Bond (2011). In one meta-analysis, Hartwig and Bond (2011) looked at 66 cues across 153 samples and aimed to

predict the truth/lie judgments made by observers, irrespective of their accuracy. They found that participants seemed to be using cues that differed from popularly self-reported cues.

For example, even though self-report studies indicate people think gaze aversion signals lying, Hartwig and Bond (2011) found that judgments of honesty did not meaningfully differ as a function of gaze. In other words, when participants judged people who did not make eye contact, they did not tend to label those people as liars, nor did they consistently label people with sustained eye contact as truth-tellers. Gaze aversion was not a cue that was used in practice by participants across studies. The authors found a similar disconnect for fidgeting and body movements: the cues were not predictive of judgments. In contrast, the authors found that measures of competence and ambivalence, as well as content plausibility, were all stronger predictors of judgments. This suggests that laypeople may claim they use one set of cues for lie-detection, but in reality, use another.

Another possible explanation for why laypeople are poor lie detectors is that there may not be strong, reliable cues for deception available to detect. Some supporting evidence for this possibility comes from the previously cited meta-analysis by DePaulo et al. (2003). After looking at the effects of 158 potential cues for deception, the authors described the cues of deceit as “faint” (p. 104), with the overwhelming majority of cues having very small effect sizes, if any at all. This suggests that there may not be strong cues for lying present in the first place.

These two potential explanations for the challenging nature of lie detection are not necessarily mutually exclusive. For example, Vrij et al. (2010) argues that poor lie-detection performance is likely caused by multiple factors – including both the absence of cues uniquely indicative of lying *and* the fact that differences between liars and truth-tellers seem to be small. In sum, the literature on the causes of poor lie-detection performance is mixed, but a few themes



have emerged. It appears that, to the extent that liars give cues that signal deception, these signals are often weak and unreliable. Also, the most promising cues for deception are often not aligned with what laypeople self-report as signals for lying.

### **How Can Additional Context Impact Lie Detection?**

An important critique of the studies done on lie detection is the fact that they often look at lies removed from context (Blair et al., 2010). Participants are asked to assess whether a statement made is true or false, usually based on behavioral or verbal cues alone, often without much additional content or context cues that could assist them in evaluating plausibility. These context cues, such as motivation to lie or standards for normative behavior, are likely to be present in legally relevant situations where lie detection takes place, such as criminal trials.

In response to this critique, several studies in the past decade have attempted to explore the impact of context on lie detection experimentally. For example, Blair et al. (2010) conducted a series of 10 studies featuring several hundred lay judges, which collectively suggested that laypeople could achieve accuracy levels well above chance (i.e., 75% mean accuracy) when they were motivated and given contextual information. For example, in study 3, 45 participants were asked to judge 12 videos of speakers who did or did not cheat on a trivia task (with the ground truth known by the researchers); once finished with the first round of judgments, the participants were given additional context that the trivia test was “extremely difficult” and that people in the videos “were rarely able to have more than two correct if they did not cheat” (p. 432). When given the additional context in the second round of evaluations, accuracy increased by 16%.

Another important contextual cue for deception could be the presence of an incentive to lie; it is possible that when laypeople are told that a speaker has a motivation to lie, they may

reach more accurate judgments of truthfulness. This possibility was examined by Bond et al. (2013). First, the authors induced student speakers to lie on video about their favorite or least favorite high school class by offering them an incentive (i.e., the ability to help the experiment and to avoid doing an additional boring task). This inducement prompted all speakers to lie in the presence of the incentive, but when no incentive was given, all speakers were truthful.

Crucially, when the authors asked another set of undergraduate participants to judge the accuracy of the speakers from a videotape – and provided context by telling participants about the incentives the speakers were offered – they found that these lay judges were over 99% accurate; this was a significant improvement from the 51% to 58% accuracy rate for participants who were not exposed to the incentive information. This high accuracy rate was further supported by two additional experiments also presented within Bond et al. (2013), each of which produced an accuracy rate of 97%.

These results suggest that participants were able to recognize the influence the incentive would have on speaker truthfulness and make accurate honesty judgments. Participants were able to use the context information (i.e., the details about the incentive speakers were offered to lie) to correctly identify false statements. However, one limitation of the design was that it did not have any speakers who chose not to lie even in the presence of the incentive, and it is not clear if participants would have been as accurate in their judgments if some speakers had refused to lie. Additional experiments by Bond et al. (2013) ruled out the possibility that lay judges were using speaker behavior and indicated that exposure to the behavioral videos (in addition to the incentive details) actually made participants worse than they were with the incentive details alone. This set of studies suggests that providing information to lay judges about context (such as a speaker's incentive to lie) may meaningfully improve lie-detection accuracy.

## **How Does the Lie-Detection Literature Inform Understandings of Informant Testimony?**

It is important to note that while this literature is relevant for understanding laypeople's lie-detection abilities, it has some limitations. There are many ways in which the lie-detection studies conducted in controlled laboratory settings differ from the lie detection that real jurors are asked to do. For example, research subjects in lie-detection studies may be asked to judge the accuracy of statements that are presented as written transcripts or exclusively audio recordings (see Bond & DePaulo, 2006), whereas jurors observe live testimony. Jurors in real legal trials are also exposed to a wide array of potential information sources – including the judge, attorneys, and witnesses – any of whom could provide additional context for evaluating informant testimony. Jurors are also exposed to cross-examination of informant witnesses, in which a defense attorney will typically question the credibility of the witness and may do more to clearly amplify cues for dishonesty when such cues are present (see Natapoff, 2009).

However, even with all of the above limitations, there is still important information about jurors' expected lie-detection capabilities that can be gained from lab studies of deception detection. The evidence gathered thus far suggests that laypeople (including jurors) seem to struggle to assess speaker accuracy at levels above chance. However, accuracy may be improved through additional contextual content and signals of credibility (or challenges to it) – which is exactly the kind of information typically provided in a criminal trial. While jurors seem likely to be less adept at lie detection than the courts might hope, there is reason to suspect that with additional context, they are more accurate than the bleak reports of 54% accuracy Bond and DePaulo (2006) suggest.

### **CHAPTER 3: JUROR PERCEPTIONS OF INFORMANTS**

Research on juror perceptions of informant testimony is still in its infancy. From the literature that exists on this topic, three findings have emerged that are relevant to the current study of incentives. Each of these findings requires additional investigation by researchers, and some are still controversial, but they offer crucial starting points for understanding the literature relevant to juror perceptions of informant incentives.

The first relevant finding is that juror verdicts may not be consistently responsive to the presence of informant incentives. Multiple studies have manipulated the presence or absence of an incentive in the testimony of an informant. Most of these studies have reported that jurors fail to respond to informant incentives as measured by verdicts (Neuschatz et al., 2008; Neuschatz et al., 2012; Robertson & Winkelman, 2017; Jenkins et al., 2021). However, at least two studies to date have diverged from this finding and found evidence of an impact of informant incentives on juror verdicts (Maeder & Pica, 2014; Maeder & Yamamoto, 2017).

Second, jurors do not seem to respond to a variety of other commonsense factors that we might expect to influence verdicts in an informant case. They do not appear to consistently be more persuaded by one type of informant (i.e., civic duty, jailhouse, accomplice) compared to others (Neuschatz et al., 2008; Neuschatz et al., 2012). Jurors also do not seem to discount the testimony of the informant even when presented with expert testimony warning about informant evidence (Neuschatz et al., 2012; Maeder & Pica, 2014) or evidence of the informant's past history of testimony (Neuschatz et al., 2012).

The third major emergent claim of this literature is that jurors may be impacted by the fundamental attribution error (Ross, 1977) when considering informant testimony. It is possible that jurors evaluating the causes of informant behavior are more inclined to identify dispositional

causes (e.g., the informant is an honest person) rather than situational causes (e.g., the informant wanted to get a sentence reduction for himself), which may cause jurors to underestimate the influence of incentives on an informant and, in turn, disregard incentive evidence when reaching a verdict (Neuschatz et al. 2008, Neuschatz et al., 2012, Wetmore et al., 2014). However, because some studies do find evidence of an impact of incentive (see Maeder & Pica, 2014; Maeder & Yamamoto, 2017), it is not clear if the lack of impact of incentive is truly driven by the fundamental attribution error or some other psychological cause, or if the cause of the null results is linked to design challenges, such as limitations in statistical power.

In the following chapter, I will review the papers published to date that have examined jurors' perceptions of informant testimony in relation to verdicts. Each of the papers assessed here has added value to the field and brought greater attention to this important but understudied area. However, there are several important opportunities for advancement in the study of perceptions of informant incentives. First, there is a need to examine a variety of case types, especially serious but less heinous ones than what has most frequently been used to date. Second, there is a need to expand the use of engaging audio/visual materials that enhance the realism and ecological validity of the experience of jurors. Third, there is a need for increasing sample sizes to allow for better-powered tests of the potential impact of informant incentives on jury verdicts.

### **Neuschatz, Lawson, Swanner, Meissner and Neuschatz (2008)**

The first published set of empirical studies on the impact of informant testimony on jurors was conducted by Neuschatz et al. (2008). The authors sought to examine whether and how juror verdicts would change when presented with evidence that an informant had an

incentive to testify. The authors also looked for changes in juror verdicts based on the type of informant witness provided.

In study 1, the authors recruited a combination of college students ( $N = 168$ ) and community members ( $N = 177$ ) as participants. On average, there were 24 college participants and 25 community participants per cell. The study used a 3 (witness type: accomplice, jailhouse or community member) x 2 (incentive stated or not) design, plus a control condition with no informant testimony.

Participants read a 6- to 8-page trial transcript based on the case of *State of Arkansas vs. Echols and Baldwin* (as cited by Neuschatz et al., 2008, at p. 140). The case was about a 22-year-old male defendant accused of murdering three young boys, and the government's case included testimony from two expert witnesses (in fiber evidence and knife evidence). The transcript also included a secondary confession from a jailhouse informant, criminal accomplice, or community member, or no confession at all (control). The authors also varied whether or not participants were told about an informant's incentive. To create this incentive manipulation in the jailhouse and accomplice witness conditions, the informant testified to receiving a 5-year sentence reduction; for the civic duty witness, it was a \$10,000 monetary reward.

The authors found differences across sample types; college participants were more conviction-prone than community members (i.e., conviction rates of approximately 74% and 56% respectively). The authors also found that jurors were sensitive to the informant witness type. Specifically, jurors were more likely to convict the defendant when presented with the civic duty witness (90% conviction rate for college sample, 58% conviction rate for community sample) compared to either the accomplice (65% college, 57% community) or jailhouse informant (67% college, 52% community). Also, jurors exposed to *any* informant testimony

were more likely to find the defendant guilty (with a conviction rate of approximately 65% collapsed across conditions and samples) than those in the informant-absent control conditions (approximately 31%). The authors found no impact of incentive information on juror verdicts, nor did they find any interactions between the incentive manipulation and witness type.

One potential confound in study 1 was the ambiguity of the incentive-absent conditions; jurors were left to make their own inferences about whether or not the informant may have had incentive to lie, and were not explicitly told that no incentive was present. To address this issue, in study 2, all conditions remained the same, but one additional manipulation was added to test for an impact of an explicit assurance that no incentive was given. The manipulation was created by having the informant in the explicit conditions be asked if he received an incentive for his testimony, to which he said, “No sir, I did not.” The resulting design was a 3 (witness type: accomplice, jailhouse or community member) x 3 (incentive present, incentive explicitly not present, or no incentive mentioned) between-subjects, using only college participants this time. Jurors were also asked to state in an open-ended measure “why the cooperating witness would come forward with the secondary confession evidence.” There were 254 participants in study 2, averaging 25 participants per cell.

Once again, the authors found that the presence of an informant of any kind increased convictions, but that incentive information did not impact verdicts. Additional measures of witness credibility were collected, and the authors found no significant impact of incentive presence on ratings of informant trustworthiness or truthfulness; however, witnesses who received an incentive were rated as more self-interested and less interested in justice. These witness attributions did not significantly predict ratings of guilt or culpability. Also, the authors did not find differences in verdict responses based on informant witness type in study 2,

contrasting with the results of study 1, where they found that the civic duty witness produced more convictions compared to either the jailhouse informant or accomplice.

The authors identified one potential explanation for their null results for incentives on verdict: the fundamental attribution error (FAE). First proposed by Ross (1977), FAE is the tendency to attribute the behavior of others to internal dispositional factors, and to fail to give appropriate weight to the impact of situational factors. Because jurors in these studies did not seem responsive to the presence of incentives, the authors suggested that jurors may be making the FAE and not recognizing the powerful influence an incentive could have on informant testimony (see the subsection A Null Effect of Incentive and the Fundamental Attribution Error for additional discussion of FAE in this context).

To assess the potential role of FAE, in study 2, the authors had raters code jurors' open-ended responses to the question of informant motivation. Responses were coded as pertaining to situational factors (e.g., getting an incentive), "personal" (i.e., dispositional) factors (e.g., he felt responsible), neither or both. Overall, 76% of participants cited only dispositional factors, 15% cited only situational, and 9% cited both. The frequency of dispositional attributions did not vary significantly as a function of the incentive manipulation (and does not appear to have been tested as a function of witness type manipulation); jurors were no more likely to use situational attributions when there was an incentive present compared to when none was given; the authors suggested that this finding (coupled with the absence of an impact of incentive on verdicts) was consistent with FAE. The authors did not comment on any patterns in attributions in relation to verdicts. The authors concluded that, while jurors are able to identify and recall the presence of incentives, they were not significantly impacted by incentive evidence when forming their



verdicts. See the Critical Analysis section for a discussion of this possibility in relation to cell size and reported statistical power.

### **Neuschatz, Wilkinson, Goodsell, Wetmore, Quinlivan and Jones (2012)**

Building on the 2008 findings, Neuschatz et al. (2012) conducted additional research to understand the influence of informant testimony and its potential moderators. Specifically, the authors aimed to examine how both expert testimony and evidence of prior informant testimony might impact juror verdicts.

In study 1, the authors tested the impact of an informant's testimony history (i.e., the number of times an informant previously testified) on verdict preference. The basic fact pattern was the same as that used in Neuschatz et al. (2008). The study used 172 undergraduate psychology students and had an average of 24 to 25 students per cell. The authors had a 2 (incentive present or absent) x 3 (testified 0, 5 or 20 times before) between-subjects factorial design, plus a control condition with no informant testimony. The incentive manipulation was created by having the jailhouse informant testify that he either "received 5 years off his jail sentence in exchange for his testimony" (incentive present condition), or that he "is not currently receiving any incentive for his testimony" (no incentive condition). To create the testimony history manipulation, the informant testifies that he has "testified against defendants in separate cases," either 5 or 20 times, "each time for an incentive." In the no-prior-testimony condition, the informant testifies he has not previously testified against any defendant. A manipulation check was used to confirm that jurors noticed the testimony history.

The authors found no significant impact of testimony history or incentive presence on verdicts; in terms of verdicts, jurors did not respond any differently when presented with

evidence of frequent past testimony or a 5-year sentence reduction incentive to testify, compared to when this evidence was not given. There were also no significant interactions reported between the incentive manipulation and the history of testifying manipulation. However, the authors did find that exposure to informant testimony was persuasive to jurors; just as in Neuschatz et al. (2008), jurors were more likely to support conviction when there was some informant testimony compared to the informant-absent control.

The authors also collected open-ended attributional data about why the informant testified, and coded responses into the same four categories that were used in Neuschatz et al. (2008): dispositional, situational, neither or both. The authors stated that these attributions were not significantly related to verdicts, as they did not find any significant correlations between their dichotomous measures of dispositional or situational attributions and verdicts. However, in contrast to their previous findings, the authors found that there was a significant impact of incentive condition on attributions; participants in the incentive condition were more likely to report the informant came forward due to *situational* factors rather than dispositional ones. The authors interpreted their results as consistent with participants making the FAE, because participants were still making more attributions that were entirely dispositional (68%) compared to situational (14% or less) across conditions, and because the incentive manipulation did not produce a significant impact on verdicts (p. 189).

In study 2, the authors examined how jury verdicts were impacted by both (1) expert testimony pertaining to informant testimony and (2) the informant type (accomplice or jailhouse informant). In this case, the expert was a former offender who had testified many times in the past as an informant or accomplice. This expert explained how he did not need to talk to the defendant in order to acquire information about the defendant's alleged crime.

The authors used a 2 (incentive present or absent) x 2 (accomplice or jailhouse informant) x 2 (expert present or absent) between-subjects design and had a control group with no informant or expert testimony. The same case and trial transcripts from study 1 were used, but new content was included to create the expert testimony manipulation. A group of 229 undergraduate students participated in the study, providing an average of 25 to 26 participants per cell.

The authors found no significant impact of expert testimony on verdicts, no significant differences between informant types (accomplice or jailhouse) on verdicts, and no interactions between the manipulations. The only significant effect on verdicts was in the presence or absence of the informant; just as prior studies found (i.e., Neuschatz et al., 2008), the presence of any informant testimony significantly increased guilty verdicts relative to the informant-absent control.

When reviewing attributional responses, the authors found that across conditions, jurors favored dispositional explanations for the informants' decision to testify (63% of respondents offering only dispositional causes), which the authors again cited as evidence consistent with FAE. However, jurors were significantly more likely to attribute informant testimony to situational factors when an incentive was present compared to when it was absent; in other words, the presence of an incentive increased situational attributions. The authors did not provide a comparison of the frequency of situational attributions across incentive condition types. The authors stated that attributions were not correlated with verdicts.

### **Wetmore, Neuschatz and Gronlund (2014)**

Wetmore, Neuschatz and Gronlund (2014) is somewhat unique among the juror perception literature reviewed here. It differs in three major ways: it uses a less common set of

case facts (i.e., not *Echols and Baldwin v. State of Arkansas*), it forgoes a between-subjects experimental design, and it does not examine the impact of incentives or informant characteristics in any way. Instead, the goal of Wetmore et al. (2014) is to examine the weight that jurors assign to secondary confession evidence relative to other forms of evidence. The study was modeled off of an experiment done by Kassin and Neumann (1997), which compares primary confession evidence to other types of witness testimony to illustrate the impact such confessions can have on verdicts.

Because this study did not involve between-subjects manipulations and did not examine incentive information, I will not provide a detailed account of the study design. The primary conclusion of the study (i.e., that secondary confession evidence is persuasive to jurors) is now supported by the other studies in this area (i.e., Neuschatz et al., 2008; Neuschatz et al., 2012; Maeder & Pica, 2014; Maeder & Yamamoto, 2017; Robertson & Winkelman, 2017; Golding et al., 2020), which consistently show that the addition of informant testimony significantly increases guilty verdicts – at least in cases with limited prosecution evidence. However, there is one study – study 2 – within Wetmore et al. (2014) that pertains to FAE, so it is worth reviewing in more detail here.

In study 2, the authors recruited 65 undergraduate students from a psychology course at the University of Oklahoma. The authors used the same fact pattern as Kassin and Neumann (1997); the case was a first-degree murder trial of a defendant accused of killing his wife and her male friend, allegedly after discovering the two at home together. The trial summary consisted of 17 paragraphs of text, including openings statements, direct and cross-examinations, closing arguments and instructions. Three witnesses testified for the prosecution, including an eyewitness neighbor, a character witness against the defendant, and a jailhouse informant. The

jailhouse informant presented a secondary confession from the defendant. During cross-examination, he indicated that when he heard this confession, he was in a different jail cell from the defendant and the jail had been loud. He also said he “thought it was a bit strange that [the defendant] would confess only after a few hours.” For the defense, the defendant testified and denied ever confessing.

The authors included a variety of measures, but I will highlight just one of consequence: they included the same open-ended attribution question that was used in Neuschatz et al. (2008; 2012), which asked participants to provide reasons why the informant came forward. The authors had these responses coded as situational (e.g., “he was getting an incentive”), dispositional (e.g., “he felt guilty”), both or neither. Across conditions, participants were significantly more likely to give dispositional attributions (57%) in explaining the informant’s actions compared to situational attributions (29%). However, in contrast to prior findings (Neuschatz et al., 2008; Neuschatz et al., 2012), the authors found a relationship between these attributions and verdicts. Jurors who reached a guilty verdict gave significantly more dispositional attributions for the informants’ behavior; jurors who reached a verdict of not guilty showed no significant differences in situational versus dispositional attributions. The authors concluded based on this result that “when participants are in favor of a guilty verdict, it appears they commit the FAE” (p. 349).

### **Maeder and Pica (2014)**

Maeder and Pica (2014) sought to extend the findings of the earlier Neuschatz studies by examining the impact of incentive size and social science expert testimony in moderating informant impact on verdicts. Their study was conducted using 258 community members

recruited via MTurk and used a 3 (incentive size small/medium/large) x 2 (expert testimony present or absent) between-subjects design, plus two control groups (one with informant testimony but no incentive or expert testimony; the other with no informant testimony, incentive or expert testimony). There were 30 to 37 participants per cell, with an average of 32 to 33.

The authors used a modified version of the same trial transcript employed by Neuschatz et al. (2008; 2012), adding in an expert testimony manipulation. Unlike the expert used in Neuschatz et al. (2012), this expert was a social science researcher rather than a former offender; the expert warned jurors about their potential susceptibility to FAE when evaluating the impact of incentives on informant testimony. The expert also discussed the Innocence Project and the notion that informant testimony is a leading cause of wrongful convictions (apparently making several claims that have yet to be conclusively established in this literature). To create the incentive size manipulation, the jailhouse informant admitted to receiving a sentence reduction of either 6 months (small), 1 year (medium) or 2 years (large).

Consistent with other studies, the authors found a “marginally significant” effect of informant testimony on verdicts ( $p = .06$ ), such that participants were more likely to find the defendant guilty when the jailhouse informant testified (without any expert testimony) compared to when no informant was presented. However, in contrast to prior Neuschatz et al. (2008; 2012) findings, Maeder and Pica (2014) did find an effect of incentives; jurors were significantly more likely to find the defendant guilty when the informant had no incentive (63% conviction rate), compared to when there was some sentence reduction provided (39%). The authors went on to examine the impact of incentive size and found their incentive-present conditions were not significantly different from each other; the only differences were that the medium and large incentive conditions both differed from the incentive-absent control.

To assess the potential impact of FAE, the authors coded and examined the response patterns to the open-ended informant motivation question. They found that, in contrast to prior studies, the majority of participants offered only situational attributions (55%), while close to a quarter gave only dispositional attributions (26%). They also found that these attributions significantly varied with incentive presence, such that jurors made more situational attributions when there was an incentive present (78%) and fewer situational attributions when no incentive was given (30%); a similar pattern was found for dispositional attributions in the opposite direction, with jurors more likely to make a dispositional attribution when no incentive was present (73%) compared to when incentive was stated (34%). This response pattern is consistent with jurors recognizing the informant was testifying due to situational pressures (i.e., the incentive).

The authors also found a significant difference in attributions by verdict. Jurors who offered only dispositional explanations were more likely to reach a guilty verdict (90% conviction rate) than those who offered only situational explanations (18%). The authors did not comment specifically on whether the reverse was also true; they did not comment on whether jurors who offered only situational explanations were more likely to support a verdict of not guilty. If they did find that effect, it would be evidence against the presence of FAE; it would instead be consistent with jurors recognizing the impact of the situational motivator (i.e., the incentive) and taking that fact into account in reaching their verdicts.

### **Maeder and Yamamoto (2017)**

Maeder and Yamamoto (2017) aimed to further extend research on informant incentives by exploring it in the context of informant and defendant race. They used a 2 (defendant: Black

or White) x 2 (informant: Black or White) x 2 (civic duty or jailhouse informant) x 2 (incentive: present or absent) between-subjects design. There were also two control conditions with no informant testimony that only manipulated defendant race, for a total of 18 conditions. After screening out 86 participants for failing manipulation checks, the authors analyzed the remaining sample of 569 jury-eligible MTurk workers from the United States. Cell sizes ranged from 29 to 36, with an average of 32. The authors used the same *Echols and Baldwin v. State of Arkansas* case facts as was used in several other studies in this area, and trial transcripts included opening and closing statements, testimony from three witnesses (informant, defendant, detective) and jury instructions.

The authors created their race manipulation using names and photographs which were pilot-tested with 30 participants to match on non-race-related traits like age, attractiveness and likability. They manipulated incentive by having the informant testify that he had received time off his sentence (for the jailhouse informant condition) or a monetary reward (for civic duty witness). In all cases, the informant provided a secondary confession in which the defendant allegedly admitted to the murders.

The authors compared their no-informant control conditions to check for the effect of defendant race and found a significant effect; jurors voted guilty more often when the defendant was White (21% guilt) compared to when Black (6% guilt). Consistent with prior studies, they also found a main effect of the presence of informant testimony, with participants more likely to reach a verdict of guilty when the informant testified (39% guilt) compared to when he did not (13% guilt). Most importantly, the authors found a main effect of incentive presence, such that participants were more likely to vote guilty when there was no incentive (44%) compared to when an incentive was present (35%); this finding is consistent with the results of Maeder and



Pica (2014) but differs from Neuschatz et al. (2008; 2012). The authors also found an interaction that “fell short of statistical significance” ( $p = .08$ ) between defendant race, informant race and incentive in predicting verdicts; incentive interacted with verdicts when the defendant was White (i.e., fewer convictions when incentive was present), but not when the defendant was Black.

Maeder and Yamamoto (2017) also examined patterns in open-ended explanations of informant behavior, using the same methodology as previously described to look at FAE. They found that, when the informant was a jailhouse witness, participants were more likely to attribute his choice to come forward to situational causes (58%) compared to when he was a civic duty witness (47%). Participants were also more likely to attribute his choice to testify to situational factors when there was an incentive present (68%) compared to when none was provided (29%). The opposite pattern was observed for dispositional attributions, with participants more likely to make dispositional attributions for the civic duty witness, and for the incentive-absent conditions. Participants were also more likely to vote guilty when they made dispositional attributions, and more likely to vote not guilty when they made situational attributions.

### **Robertson and Winkelman (2017)**

Like Wetmore et al. (2014), Robertson and Winkelman (2017) is another study that departed from prior literature on this topic. This paper was published in a law journal rather than a social-science journal and it uses a unique fact pattern based on criminal bribery. The focus of the paper across multiple studies was to assess the impact of incentives on the potential *willingness of jailhouse witnesses (or laypeople) to testify*, which is outside the scope of this review as it pertains to the causes of false informant confessions, rather than juror perceptions of

informant testimony. However, there is one portion of the paper – study 3 – that included a juror evaluation component, making it relevant for the review here.

In study 3, the authors used 351 MTurk workers. There were five conditions, two involving participants imagining themselves as potential jailhouse witnesses, and the other three conditions with participants acting as mock jurors. Participants assigned to be potential jailhouse witnesses were given case details about the accusations against two defendants; they were then told they were offered immunity or not (i.e., incentive manipulation), and would choose to either refuse or agree to give false testimony against the defendants. Participants assigned to be jurors would evaluate the evidence with or without the presence of informant testimony.

Participants were presented with written case materials and measures that took an average of 9 minutes to complete. The case involved criminal bribery of a state governor, and involved multiple people, including the informant, the FBI, the governor, a CEO and a lobbyist. It is not entirely clear whether these materials were presented at an easy reading level for the average juror, nor whether juror comprehension of case facts was adequate; a superficial review of the quotes provided by the authors suggests they may feature relatively complex sentence structure.

The authors sought to manipulate informant incentive, but they did so in a manner that diverges from other studies. Specifically, there were no conditions in which the informant explicitly testified not receiving an incentive, nor were any conditions where incentive was not mentioned by the testifying informant. For participants assigned to be mock jurors, the authors used three conditions: a control condition (with no informant testimony), a conditional-incentive condition (with immunity to the informant dependent on testimony favorable to the government), and an unconditional-incentive condition (with immunity given to the informant before she gave the prosecutor her statement). It is important to note that in both cases where the informant

testified, she was given immunity; there were no conditions where the informant testified without any mention of potential incentives or a government guarantee of immunity. In this sense, there was not a condition where incentive was truly *absent* analogous to the other studies in this area.

The intention of the authors was to create two different immunity conditions – one that explicitly would motivate the informant to testify for the prosecution, and another that, from a legal perspective, freed the informant to testify however she pleased without consequence. The authors introduced their incentive manipulation later in the trial summary by varying content in the overview of the closing arguments. The two incentive conditions differed by several sentences aiming to explain *why* the immunity grant supported either the prosecution (i.e., informant only had an incentive to be truthful, with no threat of jail time either way) or the defense (i.e., informant had incentive to fabricate testimony to avoid jail time).

With this design, the authors found no significant difference between the conditional (44% conviction rate) and unconditional incentive conditions (50%). Also, consistent with prior findings (e.g., Neuschatz et al., 2008; 2012), the authors found an impact of the presence of informant testimony relative to the control group without an informant. Jurors were significantly more likely to convict when informant testimony was present (44% conviction rate for the conditional immunity informant) compared to the control condition without any informant testimony (25% conviction rate).

Robertson and Winkelman (2017) conclude that they found no impact of incentive. However, I would argue that conclusions about the impact of incentive presence cannot be drawn from this study, because of a confound: in all informant-present conditions, the informant received the benefit of immunity. In footnotes on page 72, the authors mention that the following content is shown to jurors in both of the immunity conditions: “On cross examination, you learn

that the Aide was granted immunity from prosecution in exchange for her testimony.” This statement could potentially lead jurors to think that the informant was incentivized by the government to testify and infer that that assistance could be directional (i.e., in support of the government) – in both of the informant-present conditions. There was also no mention of any manipulation checks in the study, so it is difficult to determine whether or not jurors recognized the unconditional immunity as equivalent to receiving no incentive, or if they were viewing it as a pro-government incentive.

It appears that the authors are in fact manipulating the *qualities of the incentive* – such that it would plausibly free the witness to be truthful or motivate the witness to give testimony favorable to the prosecution. But this is different from the presence or absence of an incentive as tested in prior literature. Because there was no condition in which the informant testified and was given *nothing* in exchange for doing so, we cannot draw conclusions from this study about the impact of incentive *presence*.

### **DeLoach, Neuschatz, Wetmore and Bornstein (2020)**

DeLoach et al. (2020) examined multiple features of jailhouse informant testimony presumed to impact witness credibility and verdicts. The authors tested the impact of (1) inconsistencies in jailhouse informant testimony, (2) the presence of alternative explanations for knowing non-public crime facts, and (3) the presence of an ulterior motive for the informant to testify.

The study featured a 2 (inconsistency present or absent) x 2 (alternative explanation present or absent) x 2 (ulterior motives present or absent) between-subjects fully crossed design. To manipulate the consistency of the informant’s testimony, in the inconsistency-present

conditions, the informant was cross-examined about two inconsistencies in his claims regarding the type of weapon used. The defense attorney identified these inconsistencies by comparing the informant's current testimony at trial to the informant's previous statements made to a detective.

To manipulate the presence of an alternative explanation, in the explanation-present conditions, the defense attorney presented evidence for how the informant could have learned a non-public crime fact. The defense attorney raised the possibility that the informant was told the non-public fact about the crime from a friend who visited the informant in jail; the friend had a girlfriend in the police department. Finally, to manipulate the presence of an ulterior motive, the defense attorney presented evidence that the informant only came forward after learning about a potential sentence reduction from another inmate.

In this study, participants listened to an audio recording of a trial transcript. The case was adapted from that of Neuschatz et al. (2008) and described the trial of a man accused of murdering three young boys. It included opening statements, testimony from three prosecution witnesses (an expert, police officer and the jailhouse informant). There was a cross-examination of the informant, and he testified that he was receiving a sentence reduction of 5 years for his testimony, but said he came forward because he felt bad for the families of the victims.

It is important to note that this study did not directly manipulate the presence or absence of an incentive to testify because in all conditions the informant testified he was receiving a 5-year sentence reduction. Instead, the manipulation of the ulterior motive had the potential to highlight the extent to which the informant seemed to be motivated by the sentence reduction when coming forward to testify – compared to his stated desire to help the families of the victims.

The final analyzed sample for this study was 384 college undergraduates who participated for course credit. The authors reported that each condition had between 44 and 48 participants per cell. The authors found no main effect of ulterior motive and verdicts and did not find any significant interactions among the manipulations. They did find a main effect of inconsistency, such that jurors were less likely to convict the defendant when the informant was inconsistent (24% conviction rate) compared to when no inconsistencies were presented (39%). The authors also found a main effect of the alternative explanation, such that jurors were more likely to convict the defendant when the alternative explanation was absent (37% conviction rate) compared to when it was present (27%).

As is common to studies in this area, attributions were assessed using an open-ended question that asked them why they believed the jailhouse informant decided to testify. This was then coded as situational-only, dispositional-only, or both. The authors describe dispositional attributions in the context of this study as “positive, pro-social motivations such as a desire to do the right thing” (p. 674).

The authors reported that the ulterior motive manipulation influenced attributions, such that jurors made more situational-only attributions when the ulterior motive was present (59%) compared to the “control” condition which had informant testimony but no motive, no inconsistency and no alternative explanation provided (46%). Because of the greater frequency of situational-only attributions when the ulterior motive was present, the authors stated that the presence of the ulterior motive had reduced the likelihood that participants committed the FAE,

The authors also examined the relationship between attributions and verdicts. They combined the “dispositional-only” and “both situational and dispositional” responses into one category and compared it to the “situational-only” attributions, and they found that attributions

predicted verdicts. They found that 15% of participants who gave only a situational attribution voted guilty, while 81% of participants who made a dispositional attribution voted guilty, consistent with their hypothesis that situational attributions would be significantly related to “not guilty” verdicts.

### **Wetmore, Neuschatz, Fessinger, Bornstein and Golding (2020)**

The objective of Wetmore, Neuschatz, Fessinger, Bornstein and Golding (2020) was to examine how verdicts were impacted by the relationship between different types of judicial instructions and jailhouse informant witness reliability. The case scenario used in this study was the same as that used in Neuschatz et al. (2008), involving a defendant accused of murdering three young boys. The trial materials were written and the provided transcript was 6 to 8 pages long. In all conditions, the trial included opening statements, closing arguments, and testimony from at least two government witnesses.

The study used a 2 (informant reliable or unreliable) x 3 (instructions: standard, Connecticut [defined below], or enhanced) between-subjects design, along with a control condition with no informant. In the reliable informant condition, the informant testified that he was not receiving an incentive, stated that he still had 15 years left on his sentence, and provided non-public details about the crime. In the unreliable informant conditions, the informant provided vague details about the crime, testified to his prior criminal history, admitted to seeing the case on TV, and said he was receiving 5 years off of his 15-year sentence. Regarding the instructions, all participants were provided with the standard instructions for the case (e.g., the need to decide based on evidence presented in court, the reasonable doubt standard of proof, the need to ignore personal opinions). Jurors in the standard condition received those instructions

exclusively. Jurors in the Connecticut condition received additional instructions currently used in Connecticut regarding informant testimony (see Wetmore et al., 2020), which included a warning about jailhouse informant testimony and the need to consider motives for testifying falsely and additional jailhouse-informant specific warnings. Jurors in the enhanced instruction condition were told the standard instructions along with the author's additional enhancements, including an explanation of how specific factors should shape the reliability or unreliability of an informant.

The analyzed sample featured 454 participants recruited via Qualtrics Survey Panels. The authors found no significant impact of the informant reliability manipulation, with the reliable informant producing a 59% conviction rate and the unreliable informant producing a 52% conviction rate. This study is one of the best-powered versions of its kind in the informant literature, with a large pool of responses for both the reliable ( $n = 207$ ) and unreliable ( $n = 192$ ) conditions. However, the authors did report that participants rated the reliable informant as significantly more honest, trustworthy and interested in justice, and less self-interested. There was no significant impact of the instruction manipulations on verdicts, nor were there any significant interactions between the instruction and reliability manipulations.

As with the studies in this area, attributions were assessed using an open-ended question asking participants why they believed the informant came forward. Responses were coded as situational, dispositional, both or unable to be classified. Of the 454 responses, 337 responses were both in an informant-present condition and usable. From this group, 55% of respondents gave a situational motivation, while 35% gave a dispositional motivation and 10% gave both. When looking only at situational or dispositional attributions (i.e., excluding the "both" category), the authors also found a significant relationship between informant reliability and



attributions, such that when the informant was unreliable, participants cited more situational attributions (80%); when the informant was reliable, situational attributions were less common (46%). This was true even though the reliability manipulation did not significantly influence verdicts.

The authors went on to conduct a mediation analysis to see if attributions mediated the relationship between informant reliability and verdicts. They reported that there was a significant direct effect of informant reliability on attributions (i.e., more situational attributions when the informant was unreliable), and there was also a significant direct effect of attributions on verdict (i.e., jurors who made situational attributions were less likely to convict). They also found a significant indirect effect of informant reliability on verdict through attributions: participants in the reliable informant condition were more likely to make dispositional attributions, which in turn predicted verdicts (i.e., more convictions), while participants in the unreliable informant condition were more likely to make situational attributions, which in turn predicted verdicts (i.e., more acquittals). The authors stated that the fact that attributions varied as a function of informant reliability is “not necessarily discrepant from past research,” but it suggests that unreliable informant testimony “may be enough to overcome juror’s commission of the fundamental attribution error.”

### **Jenkins, Grand, Neuschatz, Golding, Wetmore and Price (2021)**

Jenkins et al. (2021) explored the possibility that jailhouse informant evidence may bias juror evaluations of forensic evidence, violating evidentiary independence. Evidentiary independence is the presumption within the legal system that evidence from different sources presented at trial are independent from each other and have not influenced each other (see Hasel

& Kassin, 2009). In this study, the authors specifically tested the potential of jailhouse informant reliability and incentives to impact how jurors interpret forensic handwriting evidence. Their primary dependent variables were (1) whether or not participants would believe that the handwriting samples matched and (2) participants' rating of sample similarity. The authors predicted that these measures could vary as a function of informant reliability, informant incentive, and the factual truth of whether or not the handwriting samples actually were a match.

The study used a 2 (reliable or unreliable informant) x 2 (incentive present or absent) x 2 (handwriting match or mismatch) between-subjects design. For the informant reliability manipulation, the jailhouse informant either provided accurate details of the crime that were consistent with the facts ("reliable") or gave inaccurate and inconsistent details of the crime ("unreliable"). To create the incentive manipulation, the informant either admitted to getting a sentence reduction (4 years off of a 10-year sentence), or else testified that he was not receiving a reward. The study also included two additional conditions: an informant-absent control condition and primary confession condition, in which the defendant was subjected to a lengthy interrogation and confessed to police.

Jenkins et al. (2021) provides an additional unique contribution to this literature by examining a case type that does not involve murder. The authors used a one-and-a-half page trial summary from Kukucka and Kassin (2004), which was based on the case of *U.S. v Hines* (1999), in which there was an armed bank robbery involving a suspect who disguised himself with Vaseline. The defendant was identified in a photograph lineup by the bank teller. He was charged with robbery. While in police custody, he allegedly signed a handwritten contract to waive his *Miranda Rights* and was then interrogated by police. The use of this case scenario provides an important novel contribution in a literature that has thus far almost exclusively relied

on the *Echols* fact pattern, which features a defendant accused of the heinous crime of murdering three young boys.

Participants were recruited through MTurk, producing a final usable sample of 296 participants. Spread across ten conditions, this produces an average cell count of approximately 30 responses per condition. The overall conviction rate across conditions in the study was 30%. The central finding of the paper was a main effect of informant reliability on the handwriting similarity ratings. Participants exposed to the testimony of a reliable informant went on to rate two handwriting samples as more similar to each other, compared to participants who were exposed to the unreliable informant. This suggests that exposure to the informant evidence (reliable vs. unreliable testimony) improperly influenced participants' interpretations of evidence that should be considered independent.

The authors also used a logistic regression to test for the impact of their manipulated variables on verdicts, but they reported that the overall model was not significant. They stated that there were no significant interactions between their manipulated variables and the incentive manipulation did not predict verdicts. No statistics were included regarding the relative conviction rates for the incentive-present condition compared to the incentive-absent condition.

The authors also examined the relationship between incentive presence and informant credibility. To create their measure of credibility, participants were asked to rate on a 10-point scale how "truthful, confident, credible, convincing, believable, consistent and sincere" they considered the informant to be (p. 6). These items were averaged together to produce the credibility scale. The authors found that incentive information significantly impacted informant credibility ratings, with the informant being rated as less credible when there was an incentive to testify.

There were no direct measures of attributions in this study. In the discussion, the authors suggest that the lack of effect of incentives could be caused by confirmation bias, because the reliability manipulation occurred first and may have prompted jurors to seek confirming evidence consistent with the reliability condition. The authors state that, “In searching for evidence of culpability, participants may have committed the FAE and ignored the incentive as a result.”

### **A Critical Analysis of the Informant Literature**

To date, research on the impact of informant testimony is still new and ongoing. All of the studies reviewed in detail here make important contributions to this rapidly growing area of research. However, there are a few methodological challenges and limitations common to these studies that are worth addressing, because those issues threaten to undermine some of the claims of the literature. There are three major areas for improvement in studies of juror perceptions of informant incentives: (1) the reliance on the same fact pattern from one particular case, (2) the exclusive use of written stimuli, and (3) the concerning small sample sizes used for detecting complex interactions and even main effects.

The first major limitation of existing research in this area is the ubiquity of one specific fact pattern, the “*Echols*” case. Of the studies reviewed here that include a direct manipulation of the presence or absence of an incentive (see Table 1), only one (Jenkins et al., 2021) uses a fact pattern that is not based on *Echols*. This is notable in part because the original *Echols* case appears to be especially heinous and unusual, involving the murder and genital mutilation of three 8-year-old boys. It is not clear the genital mutilation detail was ever included in the facts provided to participants, but at the very least, the defendant in these studies is accused of

murdering three boys. There is evidence to suggest jurors treat sex crimes differently from other kinds of crimes and may be less impartial (see Vidmar, 1997). It is theoretically possible that the inflammatory nature of these crimes may change juror standards of proof or desire to convict; for example, some studies of juror exposure to crime scene images have found an impact on juror standards of proof and the proportion of guilty verdicts (i.e., Grady et al., 2018; Kassin & Garfield, 1991; Douglas et al., 1997). Jurors may respond differently to a heinous crime like the murder of three boys than they would to other kinds of violent offenses, such as killings that occur during robberies, domestic disputes or in drug transactions.

**Table 1**

***Summary of published between-subjects experiments on juror verdicts in response to informant incentives***

| Paper  | Case Basis    | Final Sample   | # of Cells, Average Cell Count & Design                                | Incentive Results      | Collapsed Incentive-Absent: Sample & Conviction Rate   | Collapsed Incentive-Present: Sample & Conviction Rate  |
|--|---------------|--|--|------------------------|--|--|
| Neuschatz et al. (2008) – Study 1 <sup>a</sup> | <i>Echols</i> | <i>N</i> = 168 undergrads & <i>N</i> = 177 community members | 7 cells; <i>n</i> = 24 (undergrad), <i>n</i> = 25 (comm.)<br>3 x 2 + 1 | No effect of incentive | <i>n</i> = 72 undergrad (estimated) 76% <sup>b</sup><br><i>n</i> = 76 comm. (estimated) 53% <sup>b</sup> | <i>n</i> = 72 undergrad (estimated) 71% <sup>b</sup><br><i>n</i> = 76 comm. (estimated) 58% <sup>b</sup> |
| Neuschatz et al. (2008) – Study 2              | <i>Echols</i> | <i>N</i> = 254 undergrads                                    | 10 cells; <i>n</i> = 25<br>3 x 3 + 1                                   | No effect of incentive | <i>n</i> = 76 (estimated) 73% <sup>b</sup>   | <i>n</i> = 76 (estimated) 80% <sup>b</sup>   |
| Neuschatz et al. (2012) – Study 1              | <i>Echols</i> | <i>N</i> = 172 undergrads                                    | 7 cells; <i>n</i> = 25<br>2 x 3 + 1                                    | No effect of incentive | <i>n</i> = 74 (estimated) 71% <sup>b</sup>   | <i>n</i> = 74 (estimated) 70% <sup>b</sup>   |
| Neuschatz et al. (2012) – Study 2              | <i>Echols</i> | <i>N</i> = 229 undergrads                                    | 9 cells; <i>n</i> = 25<br>2 x 2 x 2 + 1                                | No effect of incentive | <i>n</i> = 102 (estimated) 66%   | <i>n</i> = 102 (estimated) 70%   |

|                          |               |   |   |  |  |   |
|--------------------------|---------------|---|---|--|--|---|
| Maeder & Pica (2014)     | <i>Echols</i> | <i>N</i> = 258<br>MTurk workers from U.S. | 8 cells;<br><i>n</i> = 32<br>3 x 2 + 2          | Significant effect of incentive presence, not size | <i>n</i> = 30 (reported)<br>63%                            | <i>n</i> = 98 (reported – no expert only) <sup>c</sup><br>39% |
| Maeder & Yamamoto (2017) | <i>Echols</i> | <i>N</i> = 569<br>MTurk workers from U.S. | 18 cells;<br><i>n</i> = 32<br>2 x 2 x 2 x 2 + 2 | Significant effect of incentive                    | <i>n</i> = 254 (reported)<br>44%                           | <i>n</i> = 246 (reported)<br>35%                              |
| Jenkins et al. (2021)    | <i>Hines</i>  | <i>N</i> = 296<br>MTurk workers from U.S. | 10 cells;<br><i>n</i> = 30<br>2 x 2 x 2 + 2     | No effect of incentive                             | <i>n</i> = 118 (estimated)<br>Conviction rate not provided | <i>n</i> = 118 (estimated)<br>Conviction rate not provided    |

NOTE: Wetmore et al. (2014), Robertson & Winkelmann (2017), DeLoach et al. (2020) and Wetmore et al. (2020) are not included in Table 1 because they did not feature a direct incentive present / absent experimental manipulation.

<sup>a</sup> Neuschatz et al. (2008) Study 1 is unique in that its “no incentive” condition does not include an explicit assertion about the lack of incentive. Incentives are not mentioned at all in this study’s “no incentive” condition.

<sup>b</sup> Conviction rates for these studies were estimated by averaging rates across collapsed condition categories.

<sup>c</sup> A sample of 98 is listed here because the authors explicitly noted that they used only participants from the no-expert conditions for their chi-square analysis where they observed the effect they reported. All other sample estimates presume a collapse across other categories for the test of incentives.

However, there is no direct evidence that the heinousness of the *Echols* case caused the results observed regarding incentives. Condition conviction rates across studies using *Echols* show wide variability and typically do not exceed 80%, indicating that the crime scenario is not producing a ceiling effect. In other words, jurors do not seem to immediately and persistently favor conviction in this case scenario in a manner consistent with a clear anti-defendant bias. Also, the two published studies to date that found an impact of incentives (Maeder & Pica, 2014; Maeder & Yamamoto, 2017) both relied on *Echols* as well, suggesting that the case scenario is not inherently biasing in ways that reduce the impact of informant incentives.

Yet, even if heinousness is not driving results in these studies, there are still reasons why varying the case scenarios within the published literature would strengthen the science. For

example, the inclusion of more varied case types would improve the generalizability of findings; it seems risky for courts to extrapolate patterns of behavior among jailhouse informants from a pool of studies where only one case type could be examined. This case scenario may have attributes other than heinousness that interact with incentives (e.g., informant salience, incentive saliency). It is therefore important to test an array of case scenarios to have a complete picture of how informant incentives are perceived and applied by jurors in reaching a verdict.

The second collective limitation of the literature is its exclusive reliance on written materials. Only one study reviewed here included an audio presentation of materials (DeLoach et al., 2020) and none of them used video. Only one study (Maeder & Yamamoto, 2017) reported using images and did so to produce race manipulations. The use of written materials in published jury research is very common (see Bornstein et al., 2017). However, among researchers in the area of psychology and law, the use of video simulations to present trial materials is considered the ideal methodological choice; a published survey of 180 jury researchers (including corresponding authors and editors of top jury research journals) found that video simulations are considered the most acceptable method of trial presentation format among these experts in the field, followed by audio recordings and then written transcripts (Lieberman et al., 2016; see also, Scurich, 2018).

One potential consequence of the reliance on written materials is that it may artificially limit the behavioral cues for deception that jurors might ordinarily use when judging informant testimony in an actual trial. The tools jurors may want in order to assess witness credibility (e.g., gaze, expressions, hand movement, speed, pauses, etc.) – as described in Chapter 2 – could not be applied to the informant across any of the studies that exclusively used written materials (see Global Deception Research Team, 2006; Bogaard et al., 2016; Gongola et al., 2017). However,

as previously described in Chapter 2, there is evidence to suggest that laypeople are not skilled lie detectors (Bond & DePaulo, 2006; 2008) and often report relying on visual or auditory cues to deception that do not actually improve the accuracy of their judgments, such as gaze aversion or fidgeting (DePaulo et al., 2003). Given this fact, while jurors may judge informant credibility differently when presented with video testimony compared to a transcript, it is unlikely that jurors would be systematically more discerning.

The third limitation with existing research in this area concerns small sample sizes and statistical power. The statistical power of a test is the probability that the test will correctly reject the null hypothesis and detect an effect if the effect exists (see Cohen, 1992). All of the between-subjects studies reviewed here that feature a manipulation of incentive presence do so in the context of a crossed factorial design, featuring between two and four experimental manipulations and anywhere from 7 to 18 distinct cells. However, the average participant count per cell in these studies is relatively low, often between 24 and 32 (see Table 1). Unfortunately, such small cell counts are underpowered for detecting the kinds of higher-order interaction effects that are presumably being tested in these studies (see Maxwell et al., 2004). If there were to be interactions between two or more manipulations, many of these designs would lack the power to detect it.

The issue of sample size and power also extends to examining main effects. When testing for a main effect of incentive size, the other manipulated cells are functionally combined to compare all participants in the incentive-present conditions to all those in the incentive-absent condition. This produces samples that typically vary from 72 to 118 per incentive-condition but can still be underpowered. For example, Neuschatz et al. (2008) reports that for study 1, for detecting a small effect of incentive presence, they have a power of 10% (p. 141). In other



words, if a true but small effect existed for incentive presence, their design would only be able to detect it 10% of the time. More power is needed in future studies to test for effects of incentive presence, either through study design choices or through larger sample sizes (see Maxwell, 2004).

### **A Null Effect of Incentive and the Fundamental Attribution Error**

Multiple studies reviewed here have reported no significant impact of informant incentive on juror verdicts. One possibility is that there is an effect of incentive, but that it is small and that existing studies have been underpowered and unable to detect it. However, it is also possible that jurors do genuinely struggle to discount questionable informant testimony, even when given clear evidence that an informant is strongly motivated to lie. If jurors do fail to respond to evidence of informant incentives, it is important to identify some potential underlying causes of such a bias.

One of the most commonly cited explanations for the lack of incentive effect in this literature is FAE. As discussed previously, FAE is the tendency to underestimate the impact of situational factors on the behavior of others, while giving undue weight to dispositional factors internal to the person (see Ross, 1977). One classic example of FAE comes from Jones and Harris (1967). The authors had participants write essays supporting or opposing Fidel Castro; when other participants were presented with these statements, they were either told the writers freely chose their position, or that the writers were assigned to the topic by coin flip. The authors were surprised to find that even when participants were told about the situational pressures on participants (i.e., the assigned topic given by coin flip), they still tended to believe the pro-Castro writers genuinely supported Castro. This result suggested that participants were failing to

account for the influence of the situation when evaluating the beliefs and motives of other people. When Ross (1977) commented on this phenomena and similar patterns in other psychological studies (i.e., Milgram, 1963), he conceptualized this as a representation of a deeper bias – an ingrained tendency for humans to fail to perceive powerful situational influences.

Since Jones and Harris (1967) concluded their seminal study, hundreds of studies have been conducted to examine FAE, which have collectively yielded mixed results for the effect. For example, Malle (2006) conducted a meta-analysis on 173 published studies of a concept related to FAE called the *actor-observer effect* – the idea that people tend to attribute their own behavior to situational factors and the behavior of others to dispositional factors. Malle (2006) found this effect was overall quite small and sensitive to a variety of moderators, including whether the effect was measured through close-ended rating scales (which did not show the effect) or open-ended response items later coded as situational or dispositional (which did find an effect). Malle (2006) argued that this difference due to measurement could reflect variation in language – people may describe behavior differently when explaining their own actions compared to others – without that linguistic difference necessarily reflecting a meaningful bias in psychological attributions.

FAE is cited repeatedly in the literature on juror perceptions of incentives and informant testimony. Of course, two of the reviewed studies actually did find an impact of incentives on verdicts, which suggests that FAE was not a contributing cause; jurors in those studies were clearly capable of accounting for the impact of the situational pressure (i.e., the incentive), because they did so in their verdicts (i.e., Maeder & Pica, 2004; Maeder & Yamamoto, 2017). But – even when we only consider the studies that did *not* find an impact of incentive on verdicts (i.e., Neuschatz et al., 2008; 2012) – there are reasons to be skeptical of the explanation that FAE

was the causal mechanism. Across the papers reviewed here, when FAE is measured, it tends to be assessed in the same way: jurors are asked in an open-ended question to comment on the motivation of the informant in coming forward. These responses are then coded as either situational, dispositional, both or neither, and the authors look for patterns in these responses by incentive condition and in predicting verdicts. Some authors suggest that the generally high frequency of *dispositional* informant attributions across conditions is consistent with FAE, because it may reflect a biased preference for dispositional attributions and an underweighting of situational motivators like the incentive (e.g., Neuschatz et al., 2008). However, even if *all* of the studies in this area found a high rate of dispositional attributions (which was not the case – e.g., Maeder & Pica, 2014), that trend would not be particularly informative because there are strong alternative explanations for it besides FAE.

For example, one possibility is that the high frequency of dispositional attributions observed in some of these studies is actually an artifact of the coding scheme. The authors in the reviewed studies are a bit sparse in their details about coding, and do not cite measures of FAE used by other attribution researchers. Without a validated coding scheme, there can be more room for researcher preconceptions to shape the instructions given to coders and the results those coders produce. As just one example, Neuschatz et al. (2008) apparently had coders make assignments based on examples of each one provided by the researchers. The authors did not include the instructions given to coders or a list of examples of actual participant responses to the items. However, just from the provided text, there is evidence of a potential bias in the coding scheme. The authors state: “All the raters were provided with a sheet that classified types of answers with designations for situational (e.g., incentive, time off sentence, monetary reward) and personal (e.g., he felt guilty, he was a good person, he believed it was the right thing to do in

the situation)” (p. 146). It is striking that even in the examples provided here, all examples of *situational* attributions appear to be logically associated with *not-guilty* verdicts (e.g., a juror thinks: the informant lied for an incentive, so I did not believe him and voted “not guilty”), and the examples of *dispositional* attributions are linked to *guilty* verdicts (e.g., a juror thinks: the informant testified because he was a good person, so I believe him and voted guilty). While Neuschatz et al. (2008) reported no significant relationship between these attributions and verdicts, multiple studies since then have found a significant association between situational motive attributions and “not guilty” verdicts (Neuschatz et al., 2012; Maeder & Pica, 2014; DeLoach et al., 2020).

Another possible cause of the relatively large frequency of dispositional attributions across studies (besides FAE) is that the pattern is an artifact of the English language itself. The line separating a dispositional attribution from a situational one can be thin. It is easy to imagine how some attributions – even those sorted as only one kind of attribution in these studies – could actually be a combination of both. Take, for example, a participant who asserts the informant came forward because “he wanted to help.” This would be coded as purely dispositional in the studies described, but I would argue the statement could also reflect situational factors in the mind of a participant. The statement “he wanted to help” could mean that the informant is a fundamentally helpful person who always helps when he can (dispositional), or it could mean that given the dire circumstances of young boys being murdered, he felt pressure to help police and reveal what he knew (situational). Saying the informant “wanted to help” in this way could arguably be evidence of either one.

An even stronger example of this comes from Neuschatz et al. (2008), in which the authors seem to have provided coders with the statement “he believed it was the right thing to do

in the situation” as an example of something that is purely a *dispositional* attribution. However, it is unclear why the verb “believed” (implying his disposition) should take greater precedence over the word “situation” (implying situational pressures) to classify this as a purely dispositional attribution. The room for ambiguity in language alone could potentially explain the high frequency of dispositional attributions in the sample. Together, this suggests that the attributional data reported across these studies is not especially informative on its own, and the apparent high frequency of dispositional attributions is not necessarily indicative of FAE.

Moreover, if jurors really are failing to account for the impact of incentives, it might not be for lack of caring about incentives. The limited survey evidence collected from lay people about informant testimony suggests that they actually do care about incentive information. For example, Key et al. (2018) conducted a large-scale opinion survey on students ( $n = 582$ ), MTurk workers ( $n = 125$ ), and defense attorneys ( $n = 47$ ), using a survey that probed beliefs about secondary confession evidence. The authors found that laypeople described secondary confessions as less credible than other evidence, such as forensics or eyewitness testimony. Laypeople were also aware of factors that could theoretically impact secondary confessions, such as incentives or a past history of testimony. All groups showed low agreement with the statement “offering incentives does not affect the accuracy of secondary confessions” – with only 18% of students and 14% of MTurk community members agreeing. Moreover, 80% of laypeople surveyed agreed that jurors should be informed about whether the informant is receiving an incentive in exchange for testimony. This suggests that, at least superficially, jurors believe incentives matter and should be considered by a jury.

Together, this evidence suggests that it is unlikely that FAE specifically is a major factor determining juror perceptions of informant incentives. There is reason to suspect that jurors are

influenced by incentive information, as was shown in Maeder and Pica (2014) and Maeder and Yamamoto (2017), such that there is no evidence FAE is present as measured by verdicts. And, even in studies where no impact of incentive is found, there are alternative explanations for why dispositional attributions may appear to be popular with jurors (i.e., artifact of coding or language).

## **CHAPTER 4: THE PRESENT STUDY – METHOD AND RESULTS**

### **Overview and Purpose**

The goal of the present study is to build upon prior research to understand juror perceptions of informant incentives. This study improves the literature in this area through (1) the use of a new fact pattern not used in any prior studies – one that does not feature a defendant accused of serial child murder, (2) trial materials presented as a video rather than as a short, written summary, and (3) the inclusion of a new test of the effect of an informant’s hope for leniency. The materials used in this study are novel and should help to improve the generalizability of research findings regarding incentives.

The results of this study could potentially speak to a conflicted literature about the impact of informant incentives. Given the mixed findings on incentive testimony, high-powered research with a large sample is needed to address this question. This study is among the largest of its kind, with at least 174 participants per incentive condition cell, and it exclusively manipulates aspects of informant incentives, both in terms of presence and size.

This study also has the potential to speak to the mixed results regarding attributions for the informant’s motivations for testifying. Multiple measures are included for assessing perceptions of informant motives. This allows for an improved test of the potential impact of FAE, building upon what has been done thus far in the literature.

Finally, this study examines a new way of manipulating the presentation of incentive evidence through the inclusion of a new “hope for leniency” condition. Many informants may testify in a case without necessarily having concrete agreements made with the government regarding sentence reductions. This study is the first of its kind to include a condition where the

informant asserts that, while he is hoping for leniency, he was not promised any specific benefits from the government. The inclusion of this condition allows for novel comparisons between this relatively ambiguous “hope” condition and more specific manipulations of incentive information.

## **Method**

**Participants and procedure.** Participants were recruited via the website Prolific.co in September 2020. Participants could only complete the study if they were over 18 years old, a citizen and current U.S. resident, and had not been to prison. Participants were paid \$2.17 to finish the 20-minute study and were randomly assigned to one experimental condition. Each incentive condition (including the control) has its own cell, and cell sizes varied from 174 to 181 participants per cell.

A total of 886 participants completed the survey. Of these, 51% identified as male, 48% as female and 1% as “Other” (a total of 12 participants). When asked about race/ethnicity, 64% identified as White, 18% as Asian, 9% as Hispanic / Latinx, 6% as Black, and 3% as some other option. The age of the sample ranged from 18 to 77 years old, with a mean of 31 years. In terms of education, 1% of the sample had less than a high school diploma, 13% earned a high school diploma, 23% had some college but no degree, 9% had an associate’s degree, 32% had a bachelor’s degree, 17% had a master’s degree and 5% had some other professional degree. Politically, 51% identified as Democrat, 15% as Republican, 27% as Independent, and 7% as either other or none. For political perspective, 13% identified as conservative, 31% as moderate, 54% as liberal, and 2% as other. When asked about death penalty attitudes, 28% said they were



“Strongly opposed,” 33% said they were “Somewhat opposed,” 29% were “Somewhat in favor” and 10% said they were “Strongly in favor.”

The fact pattern used in this study was based on a real case of murder in Long Beach California against defendant Thomas Goldstein in 1980 (see Possley, 2012). In this case, the prosecution relied heavily on the testimony of a jailhouse informant, Edward Fink, who testified at trial that Goldstein had “confessed” to shooting the victim; on cross-examination, Mr. Fink lied about the benefits he was receiving from the police in exchange for his cooperation, and the jury ultimately convicted Mr. Goldstein. Over two decades later, in 2002, Mr. Goldstein’s conviction was overturned when it was discovered that the prosecution had failed to disclose the benefits Mr. Fink had received for his testimony. Mr. Goldstein went on to receive a settlement of \$8 million from the city of Long Beach.

In the case scenario presented to participants, the fact pattern remained the same, but with alterations in the names of all individuals to prevent participants from looking up the case. All participants were told that the victim in the case was a 25-year-old man who was seen selling drugs in the area earlier. Eyewitnesses reported hearing gunshots and seeing a man fleeing from an alley; the witnesses gave conflicting descriptions of the suspect’s race, and the murder weapon was never recovered. The suspect was observed running into an apartment building. Police presented photos of six male residents who lived in the complex to the eyewitnesses, and one witness identified one of the photos as the suspect he saw fleeing the scene. The suspect, “Thomas Gabert,” asserted he was at home during the shooting, but police could not corroborate his account.

For participants who saw the informant testify, additional material was added beyond these details. Some participants were told that Mr. Gabert shared a jail cell with Mr. Felton, and

Mr. Felton later reported that Mr. Gabert had confessed to shooting a man in a dispute over drugs. Then participants were shown the video of Mr. Felton's testimony, which featured a manipulated section of cross-examination that varied the incentive Mr. Felton had to testify.

All video materials used here were created by me, specifically for this study. I recruited actors to play the roles of the informant (see Figure 1), the prosecutor and defense attorney. Only the informant was presented on camera due to limitations of the physical space. All condition videos presented the same footage of a direct examination and the first part of a cross-examination. The final section of the cross-examination featured the manipulated content. Each video segment with manipulated content was scripted to be identical, except for the scripted manipulated content. See Appendix A for the script of the videos. All videos were approximately 7 minutes in length.

**Figure 1**

*Screenshot of trial video*



*Note:* This is an image of the actor who portrayed the informant.

At the end of the case scenario, all participants received standard instructions from the judge, explaining the need to set aside personal biases, to only convict if there is proof beyond a reasonable doubt, to consider both direct and circumstantial evidence, to consider witness credibility and not to make any inferences from the fact that the defendant did not testify. Instructions were adapted from those used in Shaw et al. (in press), see Appendix B.

**Design.** The study featured five experimental conditions (see Table 2). There was a control condition with no informant testimony presented, which allows for a baseline comparison point in understanding what impact the informant adds above and beyond the evidence already presented in the trial. There were also two conditions with no explicit incentive; one had the informant state he is not receiving an incentive, and the other had the informant assert he hopes his cooperation will lead to leniency when he is sentenced in the future. The remaining two conditions both had an explicit incentive in the form of a sentence reduction; the incentive was either small (i.e., 1 year) or large (i.e., 10 years) from a 20-year sentence. While a 10-year sentence reduction may appear extreme, there are documented cases of sentences being functionally reduced by this much, either through lowered sentences from a judge or through a dropped charge (see Walker, 2013; Purpura, 2012).

**Table 2**

*Experimental conditions for the current study*

| Condition # | Label           | Description   |
|-------------|-----------------|---|
| 1           | Control         | Control condition with no informant testimony           |
| 2           | No Incentive    | Informant testifies: explicitly not receiving incentive |
| 3           | Hope Incentive  | Informant testifies: “hope for leniency”                |
| 4           | Small Incentive | Informant testifies: receiving 1 year off of sentence   |
| 5           | Large Incentive | Informant testifies: receiving 10 years off of sentence |

**Power and effect size.** I used G\*Power 3.1 to calculate the required sample size for the analysis – a binary logistic regression. I assumed an alpha of .05, statistical power at 80% and a two-tailed test. Given these conditions, the sample needed for the main test (i.e., the difference between incentive present / absent conditions) varies as a function of the expected distribution of verdicts. The estimate requires using expected proportions of guilty verdicts found when an incentive is present compared to when it is absent – based on existing literature.

Maeder and Pica (2014) found that the incentive-absent condition yielded a 63% conviction rate, while the incentive-present condition produced a 39% conviction rate. This results in an odds ratio of 2.66, which is a “small” effect by some standards (e.g., Chen et al., 2010) and a “medium” effect by others (e.g., Olivier et al., 2017). If the proportions seen by Maeder and Pica (2014) reflect the true effect of incentive on verdicts, then a sample size of 69 participants per cell is required, or 345 participants for a five-condition study like this one. With a final sample size of 886 and at least 174 responses per condition, this design has a far larger sample size per cell relative to previously published studies in this area (which often use 24 to 32 participants per cell). For the results of the pilot study, see Appendix C.

**Measures.** The primary dependent variable of interest was binary verdict decision (guilty / not guilty). Additional questions included a measure of confidence in verdict on a scale from 0 (not at all confident) to 100 (completely confident). Participants were given open-ended essay measures asking about the evidence they found most persuasive. An attention check question was included (discussed in the Results). Following that question, participants who were presented with informant testimony were given an additional array of questions about the informant on a 10-point scale, including items about his credibility, trustworthiness, honesty, self-interest and interest in justice, adapted from Neuschatz et al. (2008).

To gather information about perceptions of the informant's motives, an open-ended question asking: "In your opinion, why did the jailhouse informant (Edward Felton) come forward to testify?" was given to all non-control-condition participants. Next, participants were asked to respond on a sliding scale from 0 to 100 to indicate how much Mr. Felton was driven to testify because of who he is as a person (e.g., he is a kind person, he wants to do what is right), compared to something in his own situation (e.g., he feels pressured, he hopes to gain something). Then, participants were asked to select from a list of potential reasons why the jailhouse informant chose to testify, which they considered the most likely cause. A follow-up question asked them to pick from the remaining options the second strongest factor in causing him to testify.

A manipulation check question was provided (see Results) which asked participants to report the actual testimony of what the informant said he was receiving in the case. Participants were also asked if, regardless of what the informant expected, whether they thought he would receive a reduced sentence in his own case in exchange for his testimony / cooperation with the government. The section concluded with a block of general opinion measures about the jailhouse informant and how much participants agreed with the statements (ex: "The jailhouse informant is a liar" or "The jailhouse informant is a sophisticated thinker").

Finally, participants across conditions were asked basic demographic and opinion questions. Demographic questions included measures of gender, age, race, education, political party affiliation (Democrat, Republican, etc.) and political perspective (conservative, liberal, etc.). Another question measured opinion on the death penalty. Finally, questions about past experiences were asked, including whether or not the participant had previously served on a jury, participated in online jury research, or been arrested and been to jail.

## Hypotheses

*Hypothesis 1:* Exposure to any informant testimony will produce more guilty verdicts compared to the no-informant control condition. If this hypothesis is supported, then the results would be consistent with prior literature and would indicate that informant testimony has probative value to jurors (e.g., Neuschatz et al., 2008; Neuschatz et al., 2012).

*Hypothesis 2:* There will be a significant difference between the no incentive condition and both the small and large incentive conditions, with the incentive conditions producing fewer guilty verdicts than the no incentive condition. If this hypothesis is supported, then it would suggest jurors are sensitive to the presence of a sentence-reduction incentive in reaching verdicts, consistent with prior work (Maeder & Pica, 2014; Maeder & Yamamoto, 2017).

*Hypothesis 3:* In contrast to prior research (Maeder & Pica, 2014), I predict there will be a significant difference between the small and large incentive conditions, with the large incentive condition producing fewer guilty verdicts than the small incentive condition. If this hypothesis is supported, then it would indicate jurors are sensitive to differences in incentive size in reaching verdicts.

*Hypothesis 4:* There will be no significant difference between the no incentive and hope conditions. However, there will be a significant difference between the hope condition and both the small and large incentive conditions, with the hope condition producing more guilty verdicts than the small or large incentive conditions. If this hypothesis is supported, it would imply that the hope incentive that is often presented to jurors in real cases does not register as a meaningful incentive. It would suggest that jurors see the hope incentive as unlikely to motivate the informant to lie, relative to the concrete incentives (small + large conditions). It would suggest that the offer of a vague “hope for leniency” obscures the true motive informants may have to lie.

*Hypothesis 5:* The large incentive condition will not differ significantly from the informant-absent control condition. If this hypothesis is supported, then it would suggest jurors are capable of fully disregarding informant evidence when exposed to a sufficiently large cue that the informant is getting an incentive. However, if the hypothesis is not supported because the large incentive produces *more* guilty verdicts, then that result would suggest that even when jurors are presented with a very strong incentive for an informant to lie, they do not disregard the evidence entirely (consistent with Neuschatz et al., 2008). In contrast, if the hypothesis is not supported because large incentive produces *fewer* guilty verdicts, then that would suggest that exposure to a sufficiently large incentive to testify can taint the rest of the government's case and inspire jurors to doubt other, unrelated prosecution evidence.

## Results

**Manipulation checks and attention checks.** All participants, except for those in the informant-absent control condition, were given a manipulation check question about the informant's incentive. The question was: "Based ONLY on the actual testimony of the jailhouse informant (Edward Felton), was Mr. Felton receiving anything in exchange for his testimony and/or cooperation with the government?" and the answer array listed four options consistent with each of the four informant-present conditions (e.g., "Yes, he said he was receiving a 1-year sentence reduction in his own case"). Participants in the control condition would not be able to answer this item because they were not shown the testimony of the informant, and it was therefore not presented to them. Of the 709 participants who were asked about the informant's incentive (i.e., not including the 177 from the control condition), 600 participants (85%) accurately identified the incentive they were shown.

All participants were also given an attention check question, which read: “To show you are paying attention, please select the option that says, “Strongly disagree” below.” The options included Strongly agree, Somewhat agree, Somewhat disagree, and Strongly disagree. 96% correctly responded to the check. A total of 11 respondents selected “Strongly agree” and 28 selected “Somewhat disagree,” while no one selected “Somewhat agree.”

Table 3 shows the breakdown of participants’ errors in responding to the attention check question (“Failed AC”) and in the manipulation check question (“Failed MC”), which was only given to participants who saw the informant. The final column, “Fail Overlap” highlights how little overlap there was between participants who failed the attention check and those who failed the manipulation check (see Discussion for an exploration of the implications of this trend).

I performed a chi-square test to examine the distribution of the MC failures across the four informant-present conditions. I found a significant relationship between incentive condition and manipulation check failure,  $\chi^2(3, N = 709) = 10.34, p = .016$ . Participants were more likely to fail the MC in the large incentive condition compared to the incentive-absent condition.

**Table 3**

*Error type and frequency by condition for attention and manipulation check questions*

| <b>Condition</b> | <b>Conviction Rate</b> | <b>Total N</b> | <b>Passed Check(s)</b> | <b>Failed AC</b> | <b>Failed MC</b> | <b>Fail Overlap</b> |
|------------------|------------------------|----------------|------------------------|------------------|------------------|---------------------|
| No Incentive     | 49.4%                  | 178            | 155 (87.1%)            | 8 (4.5%)         | 15 (8.4%)        | 0 (0%)              |
| Hope Incentive   | 40.3%                  | 181            | 138 (76.2%)            | 12 (6.6%)        | 32 (17.7%)       | 1 (0.5%)            |
| Small Incentive  | 42.6%                  | 176            | 145 (82.4%)            | 5 (2.8%)         | 27 (15.3%)       | 1 (0.5%)            |
| Large Incentive  | 34.5%                  | 174            | 137 (78.7%)            | 4 (2.3%)         | 35 (20.1%)       | 2 (1.1%)            |
| Control          | 26.6%                  | 177            | 167 (94.4%)            | 10 (5.6%)        | N/A              | N/A                 |
| <b>Total</b>     | <b>38.7%</b>           | <b>886</b>     | <b>742 (83.7%)</b>     | <b>39 (4.4%)</b> | <b>N/A</b>       | <b>N/A</b>          |

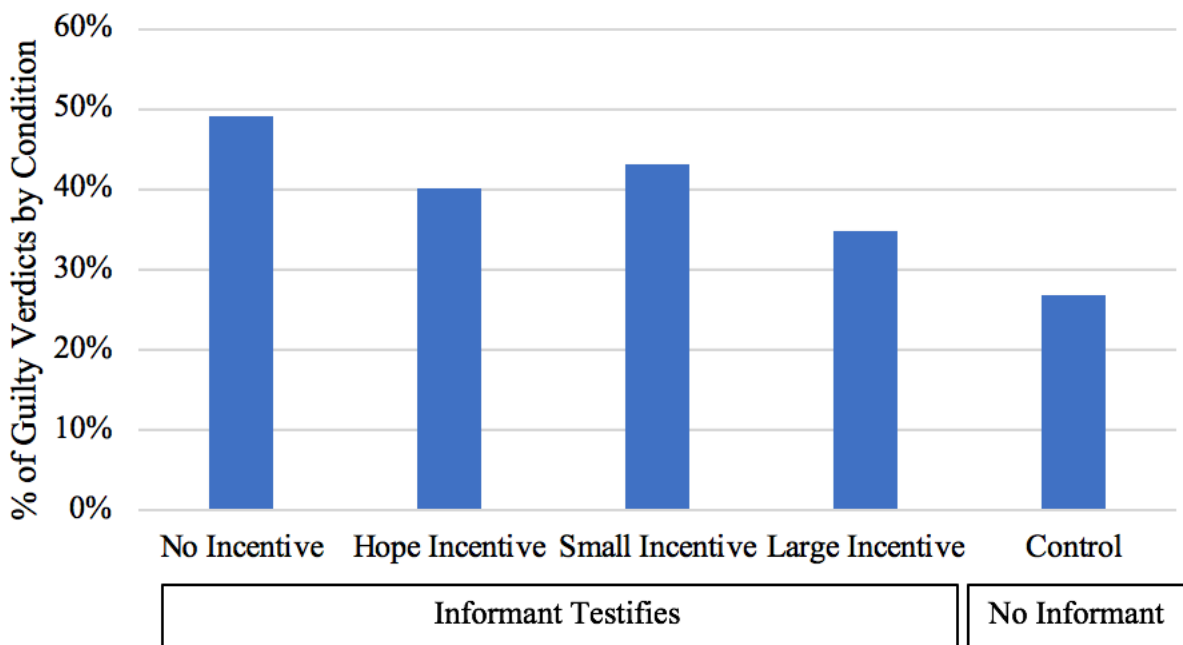


For the hypothesis testing presented below, the full sample without any exclusions was used. Given that jurors in real life may at times fail to pay attention or to recall some portions of testimony, I felt it would be most ecologically valid to include the full sample for this analysis. All results remain the same in terms of statistical significance, even when the attention check failures and manipulation check failures are excluded. For a full presentation of those results, see Appendix D.

**Conviction rates across conditions.** Within the full sample of 886 responses, 39% of respondents found the defendant guilty. See Figure 2 for the conviction rate across conditions.

**Figure 2**

*Verdict responses by condition*



**Hypothesis 1: Impact of the informant.** Per Hypothesis 1, I predicted that exposure to any informant testimony would produce more guilty verdicts compared to the no-informant

control condition. Overall, just 27% of jurors in the control condition voted for a conviction, while 42% of jurors who saw the informant testify voted to convict. To test this hypothesis, I performed a binary logistic regression, comparing verdict responses in the control condition ( $n = 177$ ) to the other four informant-present conditions combined ( $n = 709$ ). The overall model was significant,  $\chi^2(1, N = 886) = 14.35, p < .001$ . Consistent with the hypothesis, there was a significant difference in conviction rates when the informant was present compared to absent. Jurors were almost twice as likely to convict the defendant when exposed to informant testimony, compared to when no such testimony was presented ( $\text{Exp}(B) = 1.98, 95\% \text{ CI } [1.38, 2.86], \text{Wald} = 13.47, p < .001$ ).

**Hypothesis 2: Sensitivity to the absence of an incentive.** Per Hypothesis 2, I predicted that there would be a significant difference between the no incentive condition and both the small and large incentive conditions, with the clear incentive conditions (small + large) producing fewer guilty verdicts than the no incentive condition. In the no incentive condition, the informant testifies on cross-examination that he was not offered any deals in exchange for his testimony. This condition ( $n = 178$ ) had an overall conviction rate of 49%. In contrast, in the small and large incentive conditions, the informant admits to being offered an incentive to testify, which he identifies as either a 1-year reduction (small) or a 10-year reduction (large) on his 20-year sentence. The conviction rate was 43% in the small incentive condition, but 35% in the large incentive condition, averaging to 39% when merged as a group ( $n = 350$ ).

To test the significance of this apparent difference, I performed a binary logistic regression. The overall model was significant,  $\chi^2(1, N = 528) = 5.68, p = .017$ . The difference between the no incentive condition and the two explicit incentive conditions combined was significant ( $\text{Exp}(B) = .64, 95\% \text{ CI } [0.45, 0.92], \text{Wald} = 5.68, p = .017$ ), consistent with the

hypothesis. Notably, this effect seems to be primarily driven by the difference between the large incentive condition and the no incentive condition. When two separate tests are done to compare the difference between the no incentive condition and the small and large conditions separately, the overall model is not significant ( $p = .198$ ) for the no/small incentive comparison, but it is significant ( $\chi^2(1, N = 352) = 8.11, p = .004$ ) for the no/large incentive comparison (Exp(B) = .538, 95% CI [0.35, 0.83], Wald = 8.01,  $p = .005$ ).

**Hypothesis 3: Sensitivity to incentive size.** In Hypothesis 3, I predicted that there would be a significant difference between the small (1 year off of a 20-year sentence) and large incentive (10 years off of a 20-year sentence) conditions, with the large incentive condition producing fewer guilty verdicts than the small incentive condition. While the large condition produced a conviction rate of 35% compared to the small incentive conviction rate of 43%, when testing for differences in conviction rate between these two conditions using a logistic regression, the overall model was not significant,  $p = .118$ . Based on this test, there was no evidence that that jurors responded differently in their verdicts to an incentive that was large compared to one that was small.

**Hypothesis 4: Perception of the hope-for-leniency condition.** In Hypothesis 4, I predicted that there would be no significant difference between the no incentive and hope conditions; however, there would be a difference between the hope condition and both the small and large incentive conditions, with the hope condition producing more guilty verdicts than either the small or large incentive conditions.

The overall conviction rate for the hope incentive condition was 40%, indicating that, if anything, it was *less* persuasive to jurors than the assertion of a small incentive, which produced a 43% conviction rate. Contrary to Hypothesis 4, the conviction rate for the hope incentive

condition was most similar to that of the small incentive condition, rather than to the no incentive condition. When the hope incentive condition was compared individually to each of the other three informant-present conditions, no significant differences were observed between conviction rates.

**Hypothesis 5: Impact of a large incentive.** In Hypothesis 5, I predicted that jurors in the large incentive condition would be able to completely disregard the testimony of the informant after hearing about the large incentive, producing a pattern of results with no significant difference between the large incentive condition and the control condition without the informant testifying. The large incentive condition produced a conviction rate of 35%, which is 9% higher than the conviction rate of the control condition (in which the informant evidence was not included). To see if this difference was significant, I performed a binary logistic regression, but the overall model was not significant,  $p = .11$ .

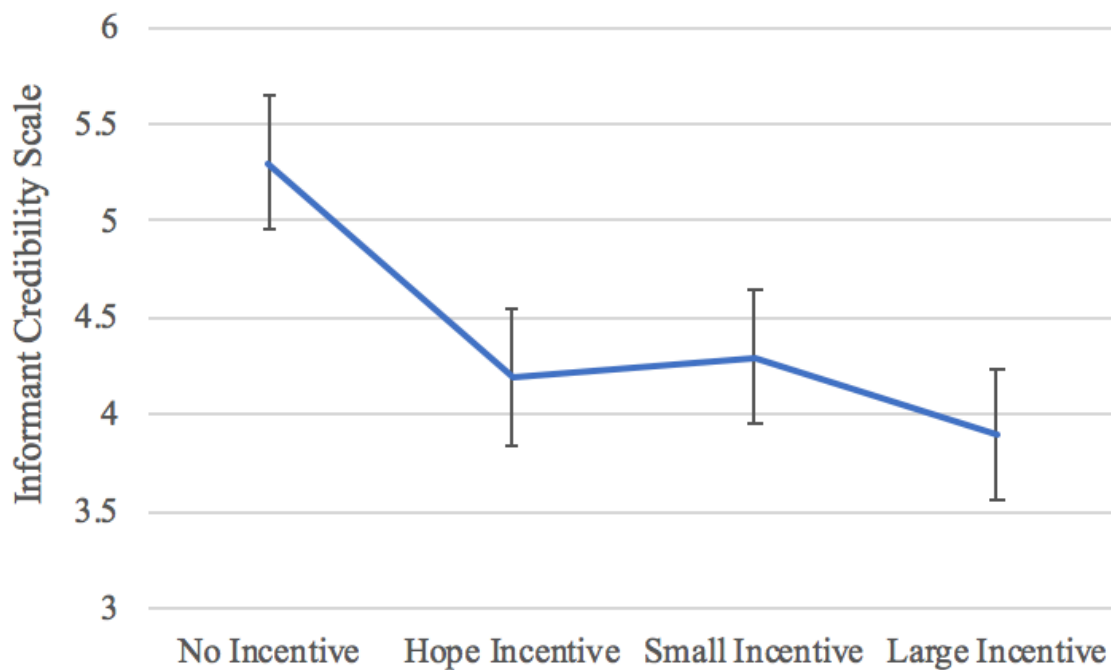
**Verdict confidence.** Immediately after providing a verdict, participants were asked to indicate how confident they felt in their verdict on a scale from 0 (not at all confident) to 100 (completely confident). Jurors gave a mean score of 67.7 ( $SD = 21.2$ ). A one-way ANOVA of verdict confidence identified no significant differences in confidence scores across conditions ( $p = .999$ ). The mean confidence scores for all conditions fell between 67.3 and 68.0, suggesting virtually no variability in mean confidence dependent on condition.

**Informant credibility by condition.** To analyze the credibility of the informant, five items were combined to create the credibility scale in which larger numbers reflect greater credibility. Four items concerned the witness's credibility, honesty, trustworthiness and interest in justice. All items were measured on a scale from 0 to 10, with 10 being a strong representation of the quality (e.g., "How trustworthy is the informant witness, Edward Felton?" with a scale

from 0 [“Not at all trustworthy”] to 10 [“Extremely trustworthy”]). The measure assessing how much the informant was “serving his own interests” was reverse-coded. The four original items and the reverse-coded item were then entered into a principal components analysis with a varimax rotation, which yielded a single factor solution with an Eigenvalue of 3.76 (all other values less than 1). This model explained 75% of the cumulative variance, indicating that the five items tap a unified construct. The resulting scale had a Cronbach’s  $\alpha = 0.92$ .

**Figure 3**

***Informant credibility scale by experimental condition***



The final credibility scale ranged from 0 to 9.6, with a mean of 4.4 ( $SD = 2.3$ ). The means in each experimental condition are plotted in Figure 3 above. A one-way ANOVA found an effect of condition on informant credibility scores,  $F(3, 677) = 11.54, p < .001$ . A post-hoc Tukey test was run to look for differences among the groups, and it detected that the no incentive

condition significantly differed from the hope incentive condition ( $p < .001$ ), small incentive condition ( $p = .001$ ) and large incentive condition ( $p < .001$ ). All other comparisons were not statistically significant ( $ps > .33$ ). Figure 3 depicts the mean credibility ratings given to the informant, split by condition, with error bars reflecting the 95% confidence intervals.

**Informant credibility and verdict outcomes.** To explore the relationship between informant credibility and verdict outcome, I performed a binary logistic regression. Only participants who were exposed to the informant's testimony (and thus provided credibility ratings about him) were included in the analysis. The overall model was significant,  $\chi^2(1, N = 709) = 396.7, p < .001$ . Credibility scores significantly predicted verdict responses, with larger informant credibility scores reflecting a greater likelihood of a guilty verdict ( $\text{Exp}(B) = 2.85$ , 95% CI [2.44, 3.32], Wald = 178.4,  $p < .001$ ).

Next, I examined whether credibility would account for the variability in verdicts created by the manipulated informant conditions. To do this, I performed the same binary logistic regression, but included dummy-coded variables to reflect the impact of the no incentive condition, hope incentive condition and small incentive condition. (Note that no dummy-coded measure for the large incentive condition was included, as this would create a redundancy in the analysis). The informant credibility scale was also included. The overall model was significant,  $\chi^2(4, N = 709) = 400.96, p < .001$ . Each condition variable was nonsignificant (all  $ps > .16$ ). However, credibility still significantly predicted verdict outcomes ( $\text{Exp}(B) = 2.93$ , 95% CI [2.93, 2.50], Wald = 175.32,  $p < .001$ ), suggesting that informant credibility mediates the relationship between incentive condition and verdict.

Finally, using PROCESS (Hayes, 2013) plug-in for SPSS, I tested the model for mediation. Comparing the no incentive and large incentive conditions, I found that the indirect

effect of the incentive manipulation on verdicts through credibility was significant (95% CI [0.87, 2.13]) while the direct effect of incentive manipulation on verdicts was no longer significant (95% CI [-1.03, 0.21]). This indicates that informant credibility fully mediated the relationship between incentive presence and verdicts.

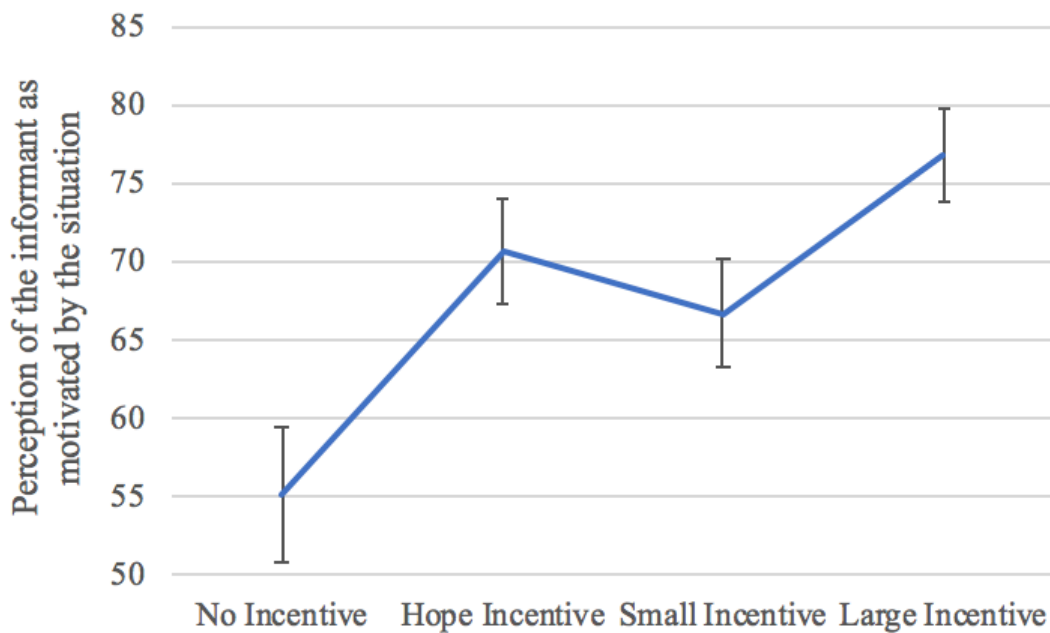
**Personal versus situational attributions.** As described in the Method, jurors who saw the informant testify were asked to rate the extent to which the informant was driven to testify because of who he is as a person compared to wanting to change his situation. Participants responded on a sliding scale from 0 (“Who he is as a person”) to 100 (“Wanted to change his situation”). Figure 4 depicts the situational attribution scale (“SAS”) ratings given to the informant, split by condition, with error bars reflecting the 95% confidence intervals. The overall average across all informant-present conditions was 67.3 ( $SD = 25.0$ ), consistent with more participants making situational rather than dispositional attributions for the informant’s choice to testify.

To examine the impact of the four informant-present conditions on perceptions of the informant’s motivation, I performed a one-way ANOVA using the SAS. For this data set, the assumption of homogeneity was violated, and *Welsh’s* adjusted  $F$  ratio was used instead. A significant difference was found, *Welsh’s*  $F(3, 389.6) = 23.68, p < .001$ . Because the assumption of homogeneity of variances was violated, a post-hoc Games-Howell test was run to look for differences among the conditions in attributions. The test showed one nonsignificant pairwise comparison; the difference between the small incentive ( $M = 66.7, SD = 23.1$ ) and hope incentive conditions ( $M = 70.7, SD = 22.7$ ) was not statistically significant ( $p = .345$ ). However, there was a significant difference between the hope incentive condition and the large incentive condition ( $M = 76.8, SD = 19.9, p = .039$ ). All other pair-wise comparisons were also significant

( $p < .001$ ). Participants in the large incentive condition gave the highest SAS scores, with a mean of 76.8, consistent with a stronger endorsement of situational causes for the informant's testimony. In contrast, participants in the no incentive condition gave the lowest SAS scores ( $M = 55.1, SD = 28.6$ ), consistent with a weaker endorsement of situational causes for the informant's testimony. Notably, no conditions produced mean scores that were below 50 and therefore indicative of more dispositional attributions on the scale.

**Figure 4**

***Juror perceptions of the informant as motivated by the situation***



To test the relationship between the situational attribution scale (SAS) and verdicts, I performed two binary logistic regressions. With SAS predicting verdicts, the overall model was significant,  $\chi^2 (1, N = 709) = 129.12, p < .001$ , and the SAS scale significantly predicted verdicts (Exp(B) = 0.96, 95% CI [0.96, 0.97], Wald = 103.96,  $p < .001$ ). For the second logistic



regression, SAS and the no incentive manipulation were added to the regression to predict verdicts, and the overall model was significant,  $\chi^2(2, N = 709) = 129.80, p < .001$ . The SAS continued to significantly predict verdicts ( $\text{Exp}(B) = 0.96, 95\% \text{ CI } [0.95, 0.97], \text{Wald} = 100.64, p < .001$ ), but the impact of incentive presence was no longer significant ( $p = .412$ ), consistent with the possibility of mediation.

**Identifying informant motivations.** Participants who saw the informant testify were asked to provide the first and second most likely reasons that informant testified. They were given a list of eight options to select their top choices from, and the options were presented in a randomized order. After selecting their single top choice reason, participants were directed to a new question with the list reduced (minus their top choice option) and told to pick the second strongest factor causing the informant to testify. See Table 4.

**Table 4**

*Frequency table for causes of the informant's testimony*

| Motivation Options        | Overall #1 Reason | Overall #2 Reason | Combined Top 2 |
|---------------------------|-------------------|-------------------|----------------|
| Get a break               | 444 (63%)         | 117 (16%)         | 561            |
| Will to lie for gain      | 40 (6%)           | 151 (21%)         | 191            |
| Tell the truth            | 88 (12%)          | 93 (13%)          | 181            |
| Police pressure           | 26 (4%)           | 148 (21%)         | 174            |
| Lacked care for defendant | 8 (1%)            | 47 (7%)           | 55             |
| Drug addiction            | 10 (1%)           | 45 (6%)           | 55             |
| Fear for safety           | 17 (2%)           | 33 (5%)           | 50             |

The most popular identified cause of the informant’s behavior was his hope of getting a break or sentence reduction in his own case; 79% of respondents who saw the informant testify gave this as one of the top two reasons for his testimony. Endorsement of particular motivations tended to vary as a function of verdict preference. Among the 296 participants who saw the informant and convicted the defendant, 186 (or 63%) thought the informant was testifying for a sentence reduction. Meanwhile, for the 413 participants who favored a verdict of not guilty, 375 (or 90%) said he was testifying for a reduction. For the most commonly selected reasons for the informant’s behavior, split by condition and verdict preference, see Table 5.

**Table 5**

*Most common reasons for informant behavior by condition and verdict*

| Condition              | Top 3 Most Commonly Selected Reasons for Behavior |   |   |
|------------------------|---|---|---|
| <b>No incentive</b>    | <b>Get a break</b><br><b>58.4% (104/178)</b>      | <b>Tell the truth</b><br><b>38.8% (69/178)</b>  | <b>Keep killer off streets</b><br><b>33.7% (60/178)</b> |
| Convictions            | Tell the truth<br>60.2% (53/88)                   | Keep killer off streets<br>54.5% (48/88)        | Get a break<br>40.9% (36/88)                            |
| Acquittals             | Get a break<br>75.6% (68/90)                      | Will to lie for gain<br>43.3% (39/90)           | Police pressure<br>25.6% (23/90)                        |
| <b>Hope incentive</b>  | <b>Get a break</b><br><b>85.6% (155/181)</b>      | <b>Police pressure</b><br><b>27.6% (50/181)</b> | <b>Will to lie for gain</b><br><b>27.6% (50/181)</b>    |
| Convictions            | Get a break<br>71.2% (52/73)                      | Tell the truth<br>41.1% (30/73)                 | Keep killer off streets<br>32.9% (24/73)                |
| Acquittals             | Get a break<br>95.4% (103/108)                    | Will to lie for gain<br>40.7% (44/108)          | Police pressure<br>30.6% (33/108)                       |
| <b>Small Incentive</b> | <b>Get a break</b><br><b>86.9% (153/176)</b>      | <b>Police pressure</b><br><b>27.8% (49/176)</b> | <b>Will to lie for gain</b><br><b>27.3% (48/176)</b>    |
| Convictions            | Get a break<br>77.3% (58/75)                      | Keep killer off streets<br>37.3% (28/75)        | Tell the truth<br>34.7% (26/75)                         |

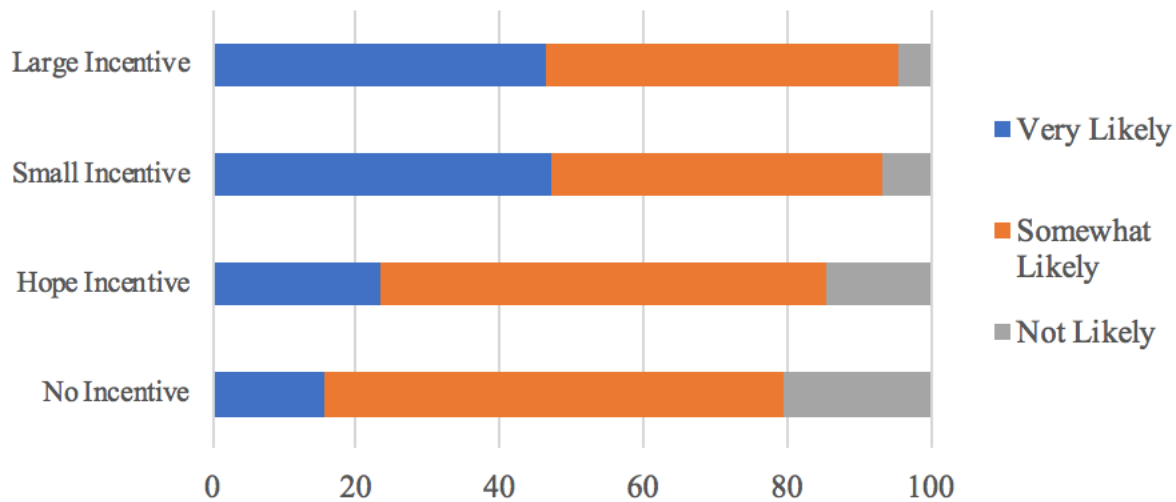
|                        |  |  |   |
|------------------------|--|--|---|
| Acquittals             | Get a break<br>94.1% (95/101)          | Will to lie for gain<br>41.6% (42/101)         | Police pressure<br>32.7% (33/101)         |
| <b>Large Incentive</b> | <b>Get a break<br/>85.6% (149/174)</b> | <b>Will to lie for gain<br/>29.3% (51/174)</b> | <b>Police pressure<br/>24.7% (43/174)</b> |
| Convictions            | Get a break<br>66.7% (40/60)           | Tell the truth<br>35.0% (21/60)                | Keep killer off streets<br>30.0% (18/60)  |
| Acquittals             | Get a break<br>95.6% (109/114)         | Will to lie for gain<br>40.4% (46/114)         | Police pressure<br>27.2% (31/114)         |

*Note:* This table displays the most common responses listed by participants for the causes of the informant's behavior, split by condition and verdict choice. For example, out of the 178 participants in the no incentive condition, 104 listed "His hope of getting a break or sentence reduction in his own case" as the most likely or second most likely cause of the informant's testimony. This was a more common response for participants who acquitted in the no incentive condition (68 out of 90) compared to those who convicted (36 out of 88).

**Expected reward for testifying.** As described in the Method, participants were asked if they thought the informant would receive a reduced sentence in exchange for his testimony / cooperation with the government, regardless of what he personally expected. Among those who saw the informant testify, 33% said it is very likely the informant would get a reduced sentence, 55% said it was somewhat likely, and 12% said it was not likely the informant would get a reduced sentence. A chi-square test was performed to see if these responses varied as a function of verdict preference, but the result was nonsignificant ( $p = .085$ ). However, when a chi-square test was done to see if sentence expectations varied as a function of the condition, the model was significant,  $\chi^2(6, N = 709) = 73.37, p < .001$ . See Figure 5 for the distribution of responses across conditions and see Appendix E for additional figures displaying responses by verdict preference.

**Figure 5**

*Distribution of beliefs about actual informant incentives*



Distribution of responses to the question:  
Will the informant actually receive a sentence reduction?

Participants in the incentive-absent condition showed the highest percentage of “not likely” responses (20.2%) and the lowest percentage of “very likely” responses (15.7%). The distribution of responses for the large and small incentive condition were similar, with a high selection rate of “very likely” (46.6% and 47.2% respectively) and a low rate of “not likely” responses (4.6% and 6.8% respectively). Responses for the hope incentive condition fell between the incentive-absent and the large/small incentive conditions, with 23.2% giving a response of “very likely” and 14.4% giving a response of “not likely.”

## **CHAPTER 5: DISCUSSION AND CONCLUSION**

This study tested five main hypotheses along with multiple exploratory tests. The five main hypotheses concerned relationships between the manipulated incentive conditions and juror verdicts. Additional tests were run to examine the relationships among incentive conditions, verdicts, and measures of informant credibility, attributions and motives. The following chapter highlights the major findings of this study and contextualizes the results within the prior literature. It also identifies limitations in the current study and opportunities for additional research in the area of juror perceptions of informant testimony.

### **Is Informant Testimony Persuasive to Jurors?**

Consistent with Hypothesis 1, the presence of informant testimony in this study made a significant impact on verdicts. Jurors were more likely to convict the defendant after hearing the testimony of the informant witness, compared to when no such testimony was provided (i.e., in the control condition). The conviction rate in the control condition without any informant testimony was 26.6%, compared to an average of 41.7% across the other four informant-present conditions. This outcome is consistent with existing literature on informant testimony, which has repeatedly shown that informant testimony can be persuasive to jurors and significantly increases the conviction rate compared to control conditions with no informant testimony (e.g., Neuschatz et al., 2008; Neuschatz et al., 2012; Maeder & Pica, 2014; Maeder & Yamamoto, 2017; Golding et al., 2020).

Interestingly, the difference between the control condition and other informant-present conditions in this study was relatively smaller than that observed in other studies. In the present

study, there was on average a 15% increase in the conviction rate with the addition of the informant's testimony relative to the control group. In contrast, study 1 of Neuschatz et al. (2008) showed a conviction rate increase of approximately 34% (i.e., 65% conviction rate with an informant versus 31% without an informant) and Maeder and Yamamoto (2017) found a difference of 26% (i.e., 39% conviction rate with an informant versus 13% without an informant).

It is not clear what features of this study prompted this apparent decrease in the impact of the informant relative to the control group. There are a variety of ways in which this study diverges from past literature, such as in the fact pattern used or the video stimuli presented, but it is not clear why those differences would diminish the impact of the informant on conviction rates compared to the control group. One potential contributing factor is the presence of an unusually large incentive manipulation in this study; the fact that this study uses a condition with a 10-year reduction from a 20-year prison sentence may have contributed to the relatively reduced average conviction rate across the incentivized conditions. Future research should explore the size of this effect and how it varies with case type, sample, and other potential moderators.

When weighing the impact of informant testimony – in this study and other similar studies – it is also important to consider the relative impact of presenting any added material at all. In this study, the control condition did not feature an informant testimony video and was therefore shorter than the four informant-present conditions. It is possible that the mere addition of material in this study, regardless of factual content, could increase the conviction rate. Future studies could expand on this possibility by (1) matching length across all conditions including the control, (2) including a control condition with neutral filler material, or (3) comparing conviction rates driven by informant testimony against that of other kinds of evidence.

## What Is the Impact of Incentive Presence on Verdicts?

This study diverged from some of the prior literature with the finding of a significant impact of incentive presence. Consistent with Hypothesis 2, jurors showed sensitivity to the nature of the incentive testimony given by the informant. Jurors convicted the defendant at a significantly higher rate when the informant claimed to have no incentive to testify (49.4% conviction rate) compared to when he admitted to receiving a large sentence reduction benefit (i.e., 10 years off of a 20-year sentence; 34.5% conviction rate). The fact that jurors reached different verdicts depending on that incentive manipulation demonstrates a clear sensitivity to the presence of incentive information. This contrasts with prior studies finding no impact of incentive evidence (e.g., Neuschatz et al., 2008; Neuschatz et al., 2012), and aligns with studies demonstrating incentive responsiveness (e.g., Maeder & Pica, 2014; Maeder & Yamamoto, 2017).

There are several possible explanations for why this particular study found an impact of incentives, while several other prior studies did not. One potential contributing factor is statistical power. A number of the studies reviewed here have cell sizes that, even when collapsed across other non-incentivized conditions (see Table 1), would not be adequately powered to detect a small effect if one existed (see remarks on power from Neuschatz et al., 2008, in the Critical Analysis in Chapter 3). The issue of power is especially crucial when asserting that an intuitively expected effect does not exist, because traditional significance testing is oriented toward rejecting a null hypothesis of sameness (see Cohen, 1992); in other words, studies should be especially well-powered to detect effects when making claims about the absence of an effect.

However, limitations in statistical power alone would not explain why several prior studies have showed trends opposite of the expected direction (e.g., Neuschatz et al. (2012) study 2 finding a 7% higher conviction rate when the informant was incentivized compared to not).

Another potential factor that could moderate the impact of incentives on verdicts (in a manner that distinguishes this study from others) is the overall salience of the informant testimony. It is possible that when the informant evidence is more salient to jurors, they will be more influenced by incentive evidence. There are at least two ways that the informant in this particular study may have been more salient to jurors than the informant evidence in prior research studies. Informant salience may have been increased in this study due to (1) aspects of the case scenario, or (2) aspects of the video materials.

One potential way the informant in the current study may have been more salient is through aspects of the case scenario. Some case scenarios may rely more heavily on the testimony of the informant than others. It is possible that studies that did not find an effect of incentive also used case materials where the informant's testimony was less central to the government's case. If that were true, then we would expect to see a relatively higher conviction rate in the no-informant control condition for studies that did not find an effect of incentive, compared to those that did. There is some evidence consistent with this possibility. For example, the two published studies reviewed here that found an effect of incentive have a control condition conviction rate of 13% (Maeder & Yamamoto; 2017) and 26% (Maeder & Pica, 2014). The present study is similar, with a control conviction rate of 27%. In contrast, the first five published samples on this topic, each of which did not show an impact of incentive, had control condition conviction rates of 38%, 24% and 40% (Neuschatz et al., 2008; study 1 college sample, study 1 community sample and study 2 respectively) and then 32% twice (Neuschatz et



al. 2012; study 1 and study 2). While the case materials used by Maeder and Pica (2014) and Maeder and Yamamoto (2017) were based on the *Echols* fact pattern, just like the studies by Neuschatz et al. (2008; 2012), it is at least possible that something about the presentation of information within those studies made the informant appear more central and salient.

Another potential way the salience of the informant could be more pronounced in this particular study relative to others is through the use of video materials (Chaiken & Eagly, 1983). It is possible that the use of visual and/or auditory elements that focused on the informant in this study contributed to the perceived importance of the informant to the case, which may have made jurors more sensitive to the informant. This study presents video testimony from the informant, and no other witness testimony is given on video. Maeder and Yamamoto (2017), one of the other studies to find an effect of incentives, included a visual manipulation that involved showing an image of the informant (and the defendant), which may also have increased the perceived importance of the informant testimony. These visual aids may have increased the salience of the informant's testimony to jurors and induced them to be more sensitive to incentive evidence. However, one important caveat to that possibility is that even in the pilot test for this particular study, a significant impact of incentive presence was found (see Appendix C). So even without any visuals, and just relying on the transcript of the informant's testimony, an impact of informant incentive presence was observed.

One additional consideration for the impact of incentive presence is the type of sample used (i.e., undergraduate compared to online samples). Concerns about research that relies on college students has been longstanding and pervasive in psychological research (see Henrich et al., 2010), and studies in the literature on sexually violent predators has found evidence of differences between college samples and community samples (e.g., McCabe et al., 2010). There

is evidence from the prior studies reviewed in Chapter 3 that the source of sample used in prior incentive studies may influence verdict patterns. Specifically, it appears that undergraduates may be more conviction-prone than online MTurk samples. As shown in Table 1, when comparing the conviction rates across collapsed incentive-present and incentive-absent conditions, we see that conviction rates generally range from 66% to 80% for the studies done with undergraduate participants. However, for studies done with MTurk samples, conviction rates are lower, varying from 35% to 63% across the collapsed conditions. Both of the published studies to report an effect of incentives on verdicts did so using an online sample (Maeder & Pica, 2014; Maeder & Yamamoto, 2017), and the present study uses an online sample as well. While Jenkins et al. (2021) was conducted on a sample of MTurk participants and did not find a significant impact of incentives on verdicts, it is difficult to interpret that outcome without reported conviction rates by condition. Future research (ideally a meta-analysis) should investigate whether or not the source of the sample changes juror sensitivity to incentive presence, or even their tendency to convict defendants in the frequently used *Echols* case scenario.

Yet another potential explanation for the finding of an impact of incentive presence comes from the size of the incentive manipulation itself. In identifying future directions for research, Jenkins et al. (2021) specifically calls for studies that test the impact of a sentence reduction that is at least 50% of an original sentence, on the grounds that a large and salient incentive may be needed “for participants to become sensitive to the motivation an incentive provides to lie” (p. 10). No prior studies reviewed here have provided an explicit incentive of that size, but the present study does answer that call through the large incentive manipulation (i.e., 10 years off of a 20-year sentence). It is possible that the effect of informant incentives on verdicts is shaped in part by the size of the incentive, such that prior studies that featured

incentives such as getting 5 years off a sentence (e.g., Neuschatz et al., 2008) without reference to the original sentence might not make the incentive adequately powerful or salient to detect an effect. However, it is important to note that Maeder and Pica (2014) found an impact of incentive presence even when the largest incentive manipulation was a 2-year sentence reduction, so the observed impact of incentive presence in this study is unlikely to be attributable exclusively to the large size of the incentive.

### **Are Jurors Sensitive to Differences in Incentive Size?**

In Hypothesis 3 of this study, I predicted an impact of incentive size, such that there would be a significant difference in the conviction rate between the small (1-year reduction from a 20-year sentence) and large (10-year reduction from a 20-year sentence) incentive conditions, with the large incentive condition producing fewer guilty verdicts than the small incentive condition. That hypothesis was not supported with the data from this study. While the small incentive produced a conviction rate of 42.6% compared to 34.5% in the large incentive condition, consistent with Hypothesis 3, the overall model failed to achieve statistical significance ( $p = .118$ ).

The failure to find a significant effect of incentive size for this sample is consistent with prior research. To date, Maeder and Pica (2014) has provided the only attempted test of incentive size in published literature and reported no significant differences in conviction rates between size conditions (and did not report exact conviction rates across incentive-present conditions). This is consistent with the possibility that jurors may not show a high degree of sensitivity to the size of an informant's incentive to testify, beyond recognizing an impact of any concrete incentive.

However, I performed an additional set of analyses (listed in Appendix F) in which I combined the data for the present study and the pilot study. With a combined sample size of 504 for the combined large and small conditions, the overall model was significant ( $p = .039$ ), and I found a statistically significant effect of incentive size, with the large incentive producing fewer convictions than the small incentive (32.3% versus 41.1% conviction rates respectively), consistent with Hypothesis 3.

One possibility is that incentives need to be perceived by jurors as sufficiently large before jurors will consider it biasing enough to disregard the evidence. For example, Maeder and Pica (2014) manipulated incentives by having the jailhouse informant admit to receiving a sentence reduction of either 6-months (small), 1 year (medium) or 2 years (large). It is not clear whether participants in that study were given a frame of reference for the original size of the jailhouse informant's sentence when considering those adjustments, but even in absolute terms, both size-related manipulations in this study were larger (i.e., a "small" incentive was a 1-year reduction to a 20-year sentence, while a "large" incentive was a 10-year reduction to a 20-year sentence).

Therefore, while this study provides clear evidence that incentive *presence* can impact verdicts, the evidence for an impact of incentive *size* is weaker. The fact that the results for the main study and the pilot study both found a nonsignificant difference between the large and small conditions in shaping verdict, and only achieved statistically significant results when combined, suggests that any effect of incentive *size* on verdicts is not especially pronounced.

However, this does not mean that differences in incentive size have no possible practical significance. The observed difference of 8% in conviction rates between the small and large incentive conditions in the combined sample has the potential for practical impact when applied

to a national context with thousands of criminal trials across the country. This result may also be useful as a cautionary tale to other researchers. A sample of 504 participants split across just two incentive conditions was used to reach statistical significance in the combined sample (see Appendix F). It is possible that a study that featured an even larger incentive manipulation would achieve stronger results, but that poses a potential threat to ecological validity, as a larger incentive than a 50% sentence reduction may be implausible in real legal cases.

### **How Do Jurors Respond to an Informant's Hope for Leniency?**

This paper provides a novel contribution to the literature in this area through its test of the hope-for-leniency condition. This condition has not been used in any prior studies and provides a first attempt to examine a more ambiguous statement by an informant as to his incentives for testifying. Because there was no specific prior literature from which to generate a hypothesis about the impact of the hope condition, I reasoned that perhaps the hope incentive would operate similar to the no incentive condition, given that it does not involve testimony about any concrete benefits given to the informant. My Hypothesis 4 was that the hope condition would be most similar to the incentive-absent condition in its effects on convictions. I intuitively expected that the hope incentive condition would not differ significantly from the incentive-absent condition – but would differ from both the small and large incentive conditions.

Hypothesis 4 was not supported in the present study. Instead, the conviction rate produced by the hope incentive (40.3%) was closer to the small incentive condition (42.6%) than to the incentive-absent condition (49.4%). The hope condition did not significantly differ from any of the other incentive conditions, with the exception of the control group with no informant testimony. However, when the pilot data was combined with the data for the present study (see

Appendix F), the difference between the hope and the incentive-absent conditions was statistically significant, with the incentive-absent condition producing more convictions (50.0%) than the hope incentive condition (40.4%), further contrasting with expectations stated in Hypothesis 4.

That result suggests that, while jurors may recognize that a hope for leniency can be motivating for an informant to lie in ways that threaten credibility (and differentiate responses from the incentive-absent condition), the effect does not appear to be substantially more damaging to credibility than an informant admitting to receiving a relatively small incentive to testify (i.e., 1 year off of a 20-year sentence). It is also notable that, even with a large sample with at least 174 responses per condition cell, in a study with highly salient informant testimony, this study did not detect a significant impact of the hope manipulation on verdicts without an additional 100 participants per cell added through the pilot study. Future researchers should take care to obtain large samples when attempting to test for the impact of informant incentives on verdicts.

### **Can Jurors Fully Disregard a Highly Incentivized Informant?**

In Hypothesis 5, I predicted that jurors would disregard the testimony of the informant in the large incentive condition and would convict the defendant at a similar rate to the control condition with no informant testimony. Limitations in the past literature on incentive size provided little basis for grounding a hypothesis on the impact of such a large incentive. No prior study reviewed here had a sentence reduction as large as 10 years off of a 20-year sentence. For comparison, in Maeder and Pica (2014), a 2-year sentence reduction was considered “large” and was sufficient (along with other incentive-present conditions) to produce an effect of incentive

*presence* – although it did not contribute to any impact of incentive *size*. Maeder and Pica (2014) also did not report any tests analogous to what was done for Hypothesis 5 of this study; specifically, they did not report whether there were significant differences in conviction rates produced by the largest incentive condition relative to the informant-absent control condition. It was therefore not clear in identifying predictions for this study how damaging such a large incentive would be to an informant’s impact on verdicts. No prior literature had indicated whether a large incentive manipulation of 10 years off a 20-year sentence would be damaging enough to an informant’s credibility that it would produce a conviction rate similar to the no-informant control condition – but I predicted that it might be strong enough to produce such an effect.

As discussed previously, traditional hypothesis testing is oriented around the rejection of a null hypothesis that assumes no effect. Failing to reject a null hypothesis is not on its own sufficient to demonstrate an effect does not exist. In this study, when testing for differences between the large incentive and the control condition, the samples for each condition were large (i.e., 174 and 177 respectively), but the overall model was not statistically significant ( $p = .11$ ) and therefore the null hypothesis was not rejected. This result is consistent with Hypothesis 5 and consistent with the possibility of jurors disregarding the testimony of a highly incentivized informant.

However, there are some crucial caveats to this finding. First, while the model did not achieve statistical significance, the results still trended in a direction consistent with a potential difference between the conditions. Specifically, the large incentive condition produced a conviction rate of 34.5%, while the control had a conviction rate of 26.6%, a difference of almost 8%. Second, as described in Appendix F, when the present study was merged with data from the

pilot, the overall model was significant ( $p = .012$ ) and there was a significant difference in conviction rates between the large incentive condition (32.3%) and control condition (22.3%).

Because of these caveats, this study cannot offer compelling evidence to suggest that jurors are able to successfully discard the testimony of a highly incentivized informant. The combined pilot and current study sample indicates that jurors are more likely to convict the defendant after hearing an informant's testimony, even a highly incentivized informant. In other words, participants in the combined sample for this study (described in Appendix F) continued to be influenced by the testimony of the informant, even when it was shown that the informant had an exceptionally large incentive to lie. This echoes the implications for Hypothesis 1 by suggesting that informants can be quite persuasive to jurors; if an informant's testimony can increase conviction rates even when the testimony is highly incentivized (and therefore dubious), that fact speaks to the powerful influence of informant secondary confession evidence.

### **What Is the Relationship between Incentivized Informant Credibility and Verdicts?**

The five formal hypotheses presented in this study use verdict as the dependent variable. However, additional analyses were performed on the measure of informant credibility, and the results mirrored expected trends in verdicts. Consistent with past literature (e.g., Jenkins et al., 2021), participants gave the informant significantly higher credibility scores when he claimed to have no incentive to testify, relative to all other incentive conditions.

Those credibility scores also predicted verdict responses. Participants who convicted the defendant rated the informant as more credible than participants who acquitted, consistent with past literature (e.g., Jenkins et al., 2021). Credibility also fully mediated the relationship between incentive presence and verdicts, consistent with the possibility that the incentive manipulation



impacted how credible jurors perceived the informant, which in turn shaped their verdict preference.

This observed relationship between incentives, credibility and verdicts is promising for future research. To the extent that credibility measures capture the effect of incentive manipulations on verdicts, such measures can be helpful to researchers exploring this relationship. Measures of credibility may offer a more sensitive measure of the impact of incentive manipulations (and other types of credibility-related manipulations) on informant testimony. However, it is crucial not to overlook a limitation of this potential relationship between informant credibility and verdicts, which is the possibility of credibility scores being the product of demand characteristics from verdicts. Participants in this study were asked to provide a verdict in the case before responding to any measures of informant credibility. It is therefore possible that the experience of reporting a specific verdict prompted participants to respond to measures of informant credibility in an internally consistent way.

### **Did Participants Make the Fundamental Attribution Error?**

Multiple prior studies in this area have tested the impact of informant incentives and found no significant effect (e.g., Neuschatz et al., 2008; 2012; Jenkins et al., 2021). As described in Chapter 3, one of the most commonly cited possible explanations of the lack of incentive effect is fundamental attribution error (FAE). However, this particular study did not find evidence that participants made the fundamental attribution error. To the contrary, and consistent with hypothesized results, this study found evidence that jurors considered the incentivized informant less credible than a non-incentivized one and adjusted their verdicts accordingly. In other words, there is no evidence in this study that participants were making the FAE.

In prior studies where no effect of incentives has been observed, evidence for FAE has come from relatively high frequencies of dispositional attributions. As detailed in Chapter 3, multiple studies in this area have collected attribution data by asking participants to identify the motive of the informant in coming forward to testify and then coding the open-ended essay responses as dispositional, situational, both or neither (e.g., Neuschatz et al., 2008; 2012; Wetmore et al., 2014). Using this attribution data, some researchers have reported a high frequency of dispositional attributions and offered it as potential evidence of FAE; in study 2 of Neuschatz et al. (2008) for example, 76% of participants made exclusively dispositional attributions, which the authors linked to the possibility of FAE as a means to explain the lack of impact of incentive presence on verdicts.

Because of potential challenges with interpretation associated with the open-ended coding approach (see Critical Analysis, Chapter 3), in the present study, I opted to focus the analysis on a single bipolar 100-point scale that assessed attributions as dispositional (scores closer to 0) or situational (scores closer to 100) for the informant. While this forces a somewhat artificial dichotomy between dispositional and situational attributions within a single bipolar scale (see Miller et al., 1981), it is not clear that such an approach would be any more or less valid than an open-ended coding approach used in current literature. The process of analyzing the open-ended coded data described in other similar studies has also involved dichotomizing the attributions for purposes of analysis (e.g., DeLoach et al., 2020). My decision to use a bipolar scale approach arguably avoids the complications of interpreting ambiguous open-ended responses and obtains an opinion measure given directly by participants, but there is no reason to consider it any more valid than the open-ended approach used previously in this area.

The bipolar attribution scale used in this study is one I developed myself in order to compare attribution patterns across conditions. Although the scale is bipolar, I refer to it as the situational attribution scale or “SAS” because larger scores reflect a stronger situational attribution made by participants. While this scale appears promising, it still needs a proper psychometric evaluation and cross-validation with other studies.

In this study, I found that the average SAS score across conditions mirrored expectations absent any evidence of FAE. Participants gave higher SAS ratings (consistent with more situational attributions) when the informant was in the incentivized conditions (with an explicit external source of motivation to testify), compared to the incentive-absent condition. Not only did SAS vary by incentive *presence*; it was even sensitive enough that it varied by incentive *size*. Jurors exposed to the large incentive condition made even higher SAS ratings (consistent with more situational attributions) than they did when presented with the small incentive condition. In other words, jurors showed ratings on this scale consistent with a responsiveness to the presence and size of the informant’s incentive, and not in a way that provides evidence of FAE.

Participants in this study did not show evidence of making the fundamental attribution error (FAE) as measured by verdict or by the situational attribution scale (SAS). Instead, participants showed sensitivity to the situational pressures to lie that were present for the informant when he was incentivized. This study also provides evidence of a lack of effect of FAE through the results from a third measure: the selection of the top two most likely motives for the informant from an option list.

All participants who were exposed to informant testimony were presented with a list of seven potential options for why the informant chose to testify (see Table 4). Participants were asked to select the most likely motive from the list. Then, they were asked to select a second

most likely motive (with their previously selected choice removed). Responses are shown in Table 4 and Table 5.

Among the list of seven provided motive options (see Table 4), the most commonly selected motive for the informant to testify was for him to “get a break” in his own sentence. This was the most commonly identified motivation for the informant within almost all conditions and for jurors of either verdict preference. The only exception was for jurors who convicted the defendant in the incentive-absent condition, who were most likely to assert the informant wanted to tell the truth (see Table 5). In other words, participants in this study overwhelmingly indicated that the informant was likely motivated by his desire for a sentencing break in his own case. This pattern is aligned with the results of the SAS and inconsistent with the influence of FAE, because this is a frequent endorsement of a situational cause of behavior rather than any dispositional causes.

### **What Do Jurors Believe about the Informant’s Future Sentencing Outcome?**

In addition to being asked about the informant’s motives, participants in this study were asked about their expectations for the informant’s actual sentencing outcome. All participants who saw the informant testify were asked if they thought he would receive a reduced sentence in exchange for his testimony / cooperation with the government, regardless of what he personally expected. Participants had three possible response options: “very likely,” “somewhat likely,” and “not likely” that the informant would get a reduced sentence.

Across all conditions, a relatively high number of participants expressed that the informant was likely motivated by, and had a chance of getting, a benefit in exchange for his testimony. Responses to this question varied significantly by condition, with 46.6% of

participants in the large condition and 47.2% participants in the small condition expressing that it was “very likely” the informant would actually receive an incentive, compared to just 23.2% in the hope condition and 15.7% in the no incentive condition (see Figure 5). This outcome is not particularly surprising, because in the small and large incentive conditions, the informant directly indicated that he expected a sentencing break in exchange for his cooperation.

However, using this same measure of expected outcomes, participants also expressed a high general endorsement of the belief that the informant would still get a sentence reduction for his cooperation. Just 20.2% of participants in the no incentive condition said it was “not likely” that the informant would actually get a sentence reduction, indicating that, even when the informant testified to expecting no benefits, many participants (79.8%) still thought it was at least somewhat likely that he would receive a sentence reduction of some kind.

I suspect this result is most likely an artifact of a likelihood scale that, in hindsight, may have been too restrictive. Participants only had three options when responding to the question about expected outcomes. While this was intended to help simplify the task of responding to a likelihood measure for participants, it may have had unintended effects. For example, it may have inadvertently prompted participants who seriously doubted that the informant would get an incentive to still select the “somewhat likely” option, if they perceived the “not at all likely” response to be reflective of a 0% chance of the informant receiving a benefit. This limitation makes it challenging to interpret the results for this particular measure.

It is also possible that this pattern of results is not just an artifact of the question, and that participants in this study did believe that the jailhouse informant would be somewhat likely to receive an incentive – even when he said he was not hoping to receive any benefits. If so, this presents an interesting starting point for future research on juror beliefs about informant

testimony. Prior survey research has examined juror beliefs about the appropriateness of informant-related measures (e.g., “Jailhouse informants should be permitted to testify only if they are NOT getting an incentive (i.e., reduced sentencing, better prison accommodations or privileges)”, Key et al., 2008; p. 6) and whether jurors should be informed about various threats to an informant’s credibility. However, there are opportunities for future research to assess jurors’ actual understanding of the use of informants in criminal cases. It is possible that increasing media coverage on issues related to informant testimony in criminal cases may impact laypeople’s beliefs and expectations about informant rewards for testifying (see Key et al., 2008), which may in turn shape how they use informant testimony when reaching a verdict.

### **What Are the Implications of the Attention and Manipulation Check Failures?**

This study included an attention check (“AC”) given to all participants and a manipulation check (“MC”) given to participants in the four informant-present conditions. The AC question came first and asked participants to select the option “Strongly disagree” from a list of four similar options. The MC was presented with questions about the informant and it asked if the informant was receiving anything in exchange for his testimony and/or cooperation with the government – and provided four answer options corresponding to the four informant-present manipulations. Out of 886 responses, 144 participants (16%) failed at least one of the check questions. All results presented for this study were done with the full sample of 886, without excluding participants for failing the attention or manipulation check. Outcomes for the five main hypotheses were the same regardless of whether or not they were excluded. See Appendix D for verification of all five main hypotheses with the reduced sample.

My approach to including responses in this study was influenced by Cullen and Monds (2020). In their 2020 paper, Cullen and Monds reviewed 157 jury simulation studies published in top legal psychology journals from 2009 to 2019. The purpose of their review was to assess variability in how researchers approached excluding participants who failed manipulation checks, memory checks, or other comprehension failures. The authors reported that over a third of the 157 jury studies they reviewed excluded participants based on failed manipulation, attention, or general memory checks – and the authors provided a detailed critique of that practice. They argued that, while removing these participants may be important to preserve internal validity, these exclusions present a threat to ecological validity in the jury research context. Real jurors are likely to experience failures of attention or memory without being tested or screened from a jury pool, even with trials that are much longer than simulations and therefore present more opportunities for errors in memory to occur. The authors conclude with a call to avoid exclusions on the basis of failures to recall manipulated case facts.

It is a common practice in jury simulation research to exclude participants who fail attention or manipulation check questions (Cullen & Monds, 2020). However, in this study, I opted to present the data with the full sample results. This provided increased statistical power and aligned with Cullen and Monds' (2020) call to preserve ecological validity and include manipulation check failures within the sample. I would also suggest that this choice to include all responses is suitable based on the strikingly minimal overlap between participants who failed the MC and the AC questions (see Table 3); participants who failed the AC tended to pass the MC, and vice versa. Only four out of 886 participants failed both the AC and the MC questions. It is impossible know with certainty why there was so little overlap between these categories. However, it arguably provides some evidence that the 105 participants who failed the MC (but

not the AC) question may have still provided an appropriate and genuine effort to engage in the materials – and therefore still provide reasonable approximations for the behavior and response of actual jurors.

One interesting finding to emerge from an examination of the MC failures relates to differences across incentive conditions. I found a significant difference in the frequency of MC failures across conditions, with participants most likely to fail the MC in the large incentive condition (20.1% failure rate) and least likely to fail it in the incentive-absent condition (8.4% failure rate). The hope incentive and small incentive conditions produced a similar MC failure rate (17.7% and 15.3% respectively). It is not clear why this difference across conditions exists, but one possibility is that it is harder for participants to remember the precise details of an informant's stated incentive, beyond a recognition that he is incentivized. If true, this could explain the higher failure rate for the three incentive-present conditions but the lower rate for the one incentive-absent condition. This also provides another potential explanation for the lack of observed effect of incentive size. If participants struggle to remember the specific details of an incentive that is either small or large or vague (i.e., the hope condition), but readily recall the presence compared to absence of an incentive, that could reduce the potential impact of incentive size on verdicts compared to incentive present.

### **Limitations and Future Directions**

This dissertation study provides several new contributions to the field of juror perceptions of informant incentives. However, there are also limitations in this study and opportunities for improvement through future research.



One limitation is that this study collected participant data exclusively online. While this was a practical necessity given that data collection occurred during the COVID-19 pandemic in 2020, it means the results may lack some of the ecological validity that an in-person selection of participants may have. For example, this sample appears to be relatively well-educated, with just 1% of the sample having less than a high school diploma and 17% reporting having a master's degree. It is unclear if this difference in education – or some other unique feature of the Prolific.co study sample – may impact the generalizability of the findings. However, prior research suggests that differences between online and in-person studies tend to be minimal (see Irvine et al., 2018). Participants in this study also might have some differences from jurors who would appear in person for jury duty. Participants in this study freely chose to participate and were not subjected to a mandatory jury summons; it is therefore possible for there to be some differences based on that interest and willingness to participate. There was also no voir dire in this study, so there is no screening of participants for hardship or other factors that could prevent particular individuals from ending up on a real jury.

In addition to not including voir dire, another limitation of this study is the lack of features present for a real jury, such as deliberations or consequentiality. Participants in this study were not able to deliberate as a group to reach a final verdict. Although individual verdict preferences have been shown to be a consistent predictor of group outcomes (Davis, 1973; Devine et al., 2001), it is possible that the results would vary if a group deliberation component was used (see Salerno & Diamond, 2010; see also Garcia et al., 2021). For example, prior studies have suggested that deliberations may help reduce the impact of certain sources of bias, such as bias from exposure to inadmissible evidence (London & Nunez, 2000) or prejudice related to terrorism (Tait, 2011). Additional research is therefore needed to explore whether the effects

observed in this study would be consistent, amplified or reduced with the inclusion of deliberations. Also, participants in this study were aware that they were not actually reaching a verdict that would impact the life of a real defendant, and it is possible that jurors would be more attentive to a trial scenario with actual consequences for the defendant (see Bornstein & McCabe, 2004). At the very least, this is a feature that distinguishes experimental jury research from actual trials.

Another limitation of this study comes from the use of video stimuli. While this study provides a new test of juror responses to a video of trial materials, the video that was presented exclusively featured the direct examination and cross-examination of the jailhouse informant. The directing attorney and crossing attorney were heard but not seen, the camera was focused on the seated testifying witness, and no other parts of trial were shown. It is possible that this focus on the informant heightened the salience of his testimony. This may have increased the sensitivity of participants to the informant's testimony about his incentives, by implicitly presenting him as important relative to other details via the mode of presentation. Future studies should test these results in the context of more detailed and complete trial scenarios to improve generalizability.

Another consideration for this study concerns its unique case features. The case scenario used here involved a homicide connected to a drug offense. As discussed in Chapter 3, numerous studies in this area have examined the impact of a specific case that involved a defendant accused of murdering three young boys. Jenkins et al. (2021) expands this literature with the use of a case summary involving a robbery. It is possible that these cases vary in ways that impact the influence of incentives on verdicts. Variations in heinousness, in the strength of the government's evidence, and in the plausibility of an informant's testimony being manufactured

are just a few potential moderators of this effect. Future studies should explore the impact of different kinds of case scenarios on perceptions of informants, to further diversify the generalizability of the research findings. Special attention should be paid to the salience of the informant and their importance to the government's case, as this may be a potential moderator of the impact of informant incentives.

### **Conclusion**

This study speaks to a conflicted literature about the impact of informant incentives on juror verdicts. It demonstrates that informant incentives can impact participant verdicts, with an incentivized informant producing fewer convictions than a non-incentivized informant. The results of this study suggest that jurors may be more capable than previously thought at accounting for informant incentives to testify. And, to the extent that jurors can account for informant incentives to testify when weighing verdicts, it is valuable for new reforms to ensure that incentive evidence can consistently reach juries in criminal trials.

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## APPENDICIES

### Appendix A – Video Material Transcript

#### PROSECUTOR’S DIRECT EXAMINATION:

Q: Please state your name, spelling the last.

A: Ed Felton. F-E-L-T-O-N.

Q: Mr. Felton, what area are you originally from?

A: Hyde Park, here in the L.A. area.

Q: And how far did you go in school?

A: 10th grade.

Q: Have you ever used illegal narcotics?

A: Yes, sir.

Q: And how old were you at that time?

A: 16.

Q: What was your drug of choice?

A: Heroin.

Q: And for how long did you use heroin?

A: Up until my early 20s. I had gotten arrested. That was the only reason I stopped.

Q: Did there come a time when you started using again?

A: Yes, sir, there did. I started using again as soon as I came back after getting arrested.

Q: And when’s the most recent time you have used heroin?

A: The day before I got locked up for this case.

Q: Do you remember approximately how long ago that was?

A: Yes. It was November of 2018.

Q: Have you previously been convicted of offenses related to narcotics?

A: Yes, I have.

Q: Can you tell me when those convictions took place?

A: Yes, I was convicted in 2012 for heroin possession and again in 2015.

Q: Now, I’d like to turn your attention to today’s case. Are you familiar with the defendant, Thomas Gabert?

A: Yeah I know Tom. We were in jail together, cellmates.

Q: And do you see Mr. Gabert in the courtroom today?

A: Yes.

Q: Could you identify him by where he's seated and an article of clothing he is wearing?

A: A blue dress shirt, next to the defense attorney.

Q: May the record reflect that Mr. Felton has identified the defendant, Mr. Gabert.

Q: Mr. Felton, How long have you known Mr. Gabert?

A: I knew him for about a week back in fall of 2018. Like I said, we were cellmates.

Q: Did Mr. Gabert ever discuss why he was there in jail with you?

A: Yes, he did.

Q: What did he say?

A: At first, I thought he was busted for possession. He came in a few days after I did, and that first day, I was telling him about my dope habit. And he said he was into coke.

Q: When Mr. Gabert told you he was into coke, what did you think he meant by that? What is coke?

A: Coke, like cocaine. He said that was his thing. Been on it for the last few years.

Q: Did Mr. Gabert say anything else to you about why he was in jail?

A: Yeah he did. About a week after that he confessed the whole thing to me...Told me he killed a guy.

Q: How did that conversation start?

A: It was after lights out. And I was telling him stuff about me. Like about how I once beat a guy for stealing my tar. And Tom was like, "man I get that."

Q: Did Mr. Gabert say what he meant by that?

Yeah he said his dealer tried to cheat him, so that guy got a bullet to the face. He told me that's what he's in for.

Q: Did Mr. Gabert ever mention the name of his dealer, a person named Joe McClain?

A: Could be. I don't know. I don't remember a name.

Q: What happened after Mr. Gabert told you he shot his drug dealer?

A: Next day, I made a call to the cops. Told them what Tom said.

Q: Why did you contact police about Mr. Gabert's confession?

A: I may be a dope addict, but I'm not about killing. I thought someone should know what he said to me.

Q: Thank you for your testimony, Mr. Felton. I have no further questions at this time.

DEFENSE CROSS-EXAMINATION:

Q: Good afternoon, Mr. Felton. My name is Michael Wallace, and I represent Thomas Gabert. I just want to ask you a few questions.

Q: You claim that you heard Mr. Gabert confess to shooting a man?

A: Yes, I did.

Q: You say this confession happened in your jail cell?

A: Yes.

Q: At the time of this alleged confession, there were just two people in that cell right? It was you and Mr. Gabert.

A: Yes.

Q: You don't know if anyone else, other than you, heard Mr. Gabert confess, do you?

A: No I don't.

Q: Mr. Felton, you didn't happen to have a tape recorder on you when you spoke to Mr. Gabert, right?

A: No sir.

Q: You weren't wearing a wire?

A: No.

Q: You weren't recorded on video while you were in your cell?

A: Not to my knowledge, no sir.

Q: So the only evidence we have that this confession was ever made is your word?

A: I guess so.

Q: You've been in and out of jail multiple times, right?

A: Yes.

Q: Two of your arrests have been for heroin possession?

A: Yes.

Q: But this last charge was for armed robbery, correct?

A: Yes.

Q: And that robbery charge is the one that put you in jail with Mr. Gabert?

A: Yes.

Q: Were you ever advised as to how much jail time you are facing for this armed robbery charge? In the event you were to be convicted.

A: Yes. My lawyer said based on my record I was looking at 20 years, maybe more.

Q: 20 years or more -- that's a whole lot longer than you've ever faced before, correct?

A: Yes.

Q: Is it true that you reached a plea agreement with the government for your own case, for your armed robbery charge?

A: Yes, I took a plea. I pled guilty.

Q: Have you been sentenced in your own case?

A: Not yet.

NO INCENTIVE CONDITION:

Q: Did you also enter into any sort of cooperation agreement with the government?

A: No sir.

Q: So you never made an agreement with the government to get a reduced sentence in exchange for cooperating and testifying against Mr. Gabert?

A: No sir. My plea for my own case was a separate thing.

Q: Are you hoping to receive any benefits in exchange for your cooperation in the case against Mr. Gabert?

A: No sir I am not.

Q: Thank you. Your honor, I have no further questions at this time.

HOPE CONDITION:

Q: Did you also enter into any sort of cooperation agreement with the government?

A: Yes sir.

Q: So you made an agreement with the government to get a reduced sentence in exchange for cooperating and testifying against Mr. Gabert?

A: Sir I agreed to cooperate. I was never promised anything for my own case. I'm hoping the judge will go easy on me, but I know it's not a guarantee.

Q: Are you hoping to receive any benefits in exchange for your cooperation in the case against Mr. Gabert?

A: I hope my own sentence will be lighter, but I don't know if it will be.

Q: Thank you. Your honor, I have no further questions at this time.



SMALL ONE-YEAR SENTENCE REDUCTION CONDITION:

Q: Did you also enter into any sort of cooperation agreement with the government?

A: Yes sir.

Q: So you made an agreement with the government to get a reduced sentence in exchange for cooperating and testifying against Mr. Gabert?

A: Yes sir, I agreed to cooperate. I was told I'd be looking at getting a year off my sentence.

Q: Aside from getting one year off of your 20 year prison sentence, are you hoping to receive any benefits in exchange for your cooperation in the case against Mr. Gabert?

A: Other than the year I'm getting off, no.

Q: Thank you. Your honor, I have no further questions at this time.

LARGE 10-YEAR SENTENCE REDUCTION CONDITION:

Q: Did you also enter into any sort of cooperation agreement with the government?

A: Yes sir.

Q: So you made an agreement with the government to get a reduced sentence in exchange for cooperating and testifying against Mr. Gabert?

A: Yes sir, I agreed to cooperate. I was told I'd be looking at getting 10 years off my sentence.

Q: Aside from getting 10 years off of your 20 year prison sentence, are you hoping to receive any benefits in exchange for your cooperation in the case against Mr. Gabert?

A: Other than the 10 years I'm getting off, no.

Q: Thank you. Your honor, I have no further questions at this time.

## Appendix B – Judge Instructions

**The following is the text used to provide all participants with instructions from the judge, following the presentation of the trial information.**

You have now heard the facts of this case. Next, the judge provides instructions about the law and your duty as a juror.

The judge says: *You have now heard all the evidence, and it is up to you to make a decision. It is your duty to follow the rules, even if you disagree with them.*

*You must decide the case solely on the evidence before you and must not be influenced by any personal likes or dislikes, opinions, prejudices or sympathy.*

*There are two kinds of evidence at trial. There is direct evidence, where a witness takes the stand and testifies to something they perceived with one of their five senses.*

*There is also circumstantial evidence that points in a certain direction. If someone walked into a room with a wet umbrella, we would have circumstantial evidence it was raining outside, and we might draw a fair conclusion it was raining. Circumstantial evidence can be considered but it must be viewed with care.*

*You cannot infer anything from the fact the defendant did not testify. The defendant has no burden of proof and therefore, there is no obligation for him testify. You may not infer guilt from silence or infer he is hiding something merely because he did not testify.*

*You must instead evaluate the witnesses who did testify. You must decide whether these witnesses were telling the truth or not. A person can tell the truth in whole, in part, or not at all.*

*I invite you to consider: if the witness had a good opportunity to see what they testified about, if they have an accurate memory of events, if they have something to gain or lose by the jury's decision, or if they have some bias toward one side.*

*In this case, the defendant is charged with murder in the second degree.*

*To prove this charge, the government must show you that the defendant committed an act that resulted in the death of another person, and that he did so intentionally or recklessly.*

*The government does not need to prove the defendant planned or premeditated the murder.*

*You need to decide if there is a reasonable doubt as to the defendant's guilt.*

*If you do have a reasonable doubt, you must find the defendant not guilty because the government has not met its burden.*

*But if you are convinced beyond a reasonable doubt, you must find the defendant guilty.*

## Appendix C – Pilot Study Results

**Introduction.** A pilot test for this study was conducted in October 2019. The manipulated materials provided were written as a transcript (rather than presented on video). The same five conditions were used. A total of 375 valid participant responses were collected via TurkPrime. Participants were randomly assigned to condition. See Table C1 for pilot condition sample sizes and conviction rates.

**Table C1**

*Pilot sample: Cell sizes and conviction rates by condition compared to the Prolific sample*

| <b>Condition</b> | <b>Prolific N</b> | <b>Prolific<br/>Conviction Rate</b> | <b>Pilot N</b> | <b>Pilot<br/>Conviction Rate</b> |
|------------------|-------------------|-------------------------------------|----------------|----------------------------------|
| Control          | 177               | 26.6%                               | 74             | 12.2%                            |
| No Incentive     | 178               | 49.4%                               | 78             | 51.3%                            |
| Hope Incentive   | 181               | 40.3%                               | 69             | 40.6%                            |
| Small Incentive  | 176               | 42.6%                               | 77             | 37.7%                            |
| Large Incentive  | 174               | 34.5%                               | 77             | 27.3%                            |

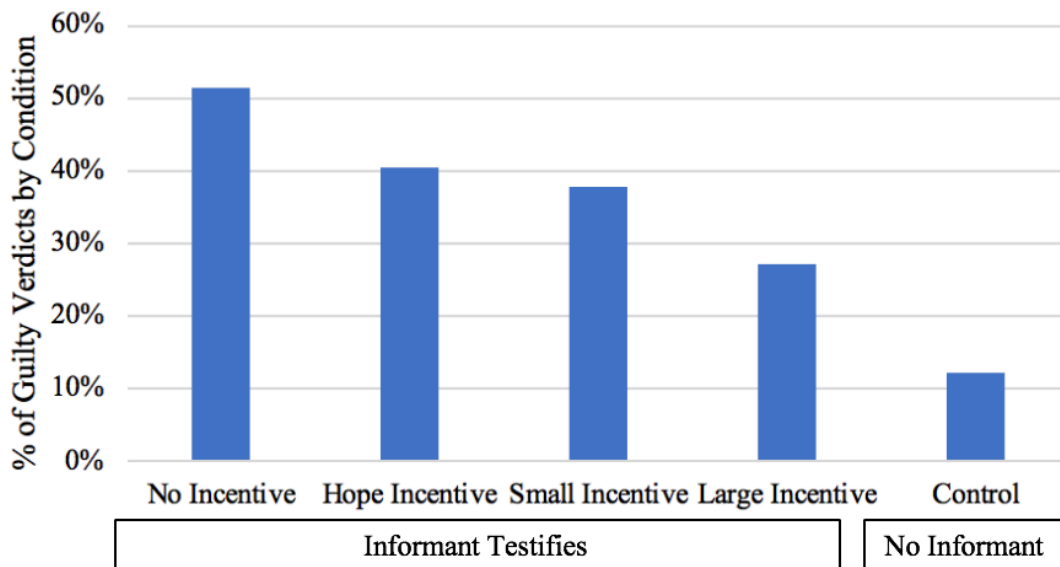
**Pilot verdict responses.** Across conditions, participants showed variability in case opinions, with 51% of participants convicting the defendant in the no incentive condition, compared to 12% convicting the defendant in the informant-absent control condition (see Figure C1). A series of binary logistic regressions were run to check for differences among the conditions in verdict outcomes. There was a statistically significant difference between the control condition (with no informant testimony) and all others ( $\text{Exp}(B) = 4.65, p < .001$ ), with jurors being the least willing to convict in the absence of informant testimony (see Hypothesis 1). There was also a significant difference between the no incentive condition and the large incentive condition ( $\text{Exp}(B) = 2.81, p = .003$ ), suggesting that jurors were more persuaded by the

informant (and therefore more likely to convict the defendant) when the informant reported no incentive to testify compared to when he had a large incentive to do so (see Hypothesis 2).

There was no significant difference in conviction rates between the small and large incentive conditions ( $p = .170$ , see Hypothesis 3). However, there was a significant difference between the large incentive and control condition (overall model  $\chi^2(1, N = 151) = 5.55, p = .019$ ), such that the large incentive produced significantly more convictions than the control condition ( $\text{Exp}(B) = 2.71, p < .001$ , see Hypothesis 5).

**Figure C1**

***Pilot study guilty verdicts by condition***



When comparing the conviction rate for the hope incentive condition to that of the control condition, the overall model was significant,  $\chi^2(1, N = 143) = 15.55, p < .001$ . The hope incentive produced significantly more convictions than the control condition,  $\text{Exp}(B) = 4.93, p <$

.001. However, the hope incentive condition did not significantly differ from any other condition (all other  $ps > .08$ , see Hypothesis 4). See the Pilot discussion section below.

**Pilot manipulation checks.** Participants who were exposed to some informant testimony (i.e., all participants besides those in the control condition) were asked to report the stated incentive of the jailhouse informant as a manipulation check. The vast majority of participants were able to correctly report the incentive information presented in the materials (no incentive: 92% correct; hope incentive 84%; small incentive 90%; large incentive 86%).

It is plausible that some kinds of informant incentive testimony may be more memorable than others. I generated a dichotomous variable to reflect whether or not the participant correctly answered the incentive manipulation check for their specific condition. I then tested for differences in manipulation check failure rate between the no incentive condition and the hope incentive condition using a binary logistic regression. However, the overall model was not significant,  $p = .117$ .

I also tested whether participants in the hope condition were significantly less likely to correctly answer the incentive manipulation check question compared to all other groups combined (excluding the control). The model was again nonsignificant,  $p = .259$ .

**Pilot informant credibility.** Unlike the scale used in the full study, informant credibility in the pilot study was measured using a single scale from 0 (not at all credible) to 100 (completely credible). I then performed an independent samples t-test including only participants in the no incentive and large incentive conditions to compare the difference in credibility scores between those two conditions. There was a significant difference between the two conditions in terms of average informant credibility scores,  $t(153) = 3.255$ , 95% CI [6.44, 26.36],  $p = .001$ ,

such that the informant was more credible in the no incentive condition than the large incentive condition. Average informant credibility scores are listed for each condition in Table C2.

I also tested for differences in informant credibility scores between the hope incentive condition and the large incentive condition. Once again, there was a significant difference between the two conditions in terms of average informant credibility scores,  $t(144) = 2.12$ , 95% CI [.69, 20.01],  $p = .036$ . The same test, when done to compare the no incentive condition with the hope incentive condition proved to be nonsignificant,  $p = .244$ .

**Table C2**

*Average informant credibility score by informant-present condition*

| Verdict    | No Incentive | Hope Incentive | Small Incentive | Large Incentive |
|------------|--------------|----------------|-----------------|-----------------|
| Guilty     | 78.5         | 74.0           | 76.8            | 72.2            |
| Not Guilty | 23.4         | 26.3           | 28.0            | 21.4            |
| Overall    | 51.7         | 45.6           | 46.4            | 35.3            |

*Note.* Scale goes from 0 (not at all credible) to 100 (completely credible).

Next, I sought to determine if informant witness credibility scores would predict verdicts. Using a simple logistic regression to predict verdicts with credibility score, the overall model is significant,  $\chi^2(1, N = 301) = 257.98$ ,  $p < .001$ . Higher credibility scores were associated with an increased likelihood of a guilty verdict,  $\text{Exp}(B) = 1.12$ , 95% CI [1.09, 1.15],  $\text{Wald} = 77.49$ ,  $p < .001$ .

**Pilot discussion.** Verdict responses across conditions broadly matched hypothesized trends. The presence of the informant did increase convictions compared to when no informant was present, consistent with Hypothesis 1. There was also evidence of sensitivity to the existence of an incentive, demonstrated by the significant difference between the no incentive and large incentive conditions, consistent with Hypothesis 2.

There was no evidence from this pilot study that jurors were sensitive to the *size* of the incentive, contrary to Hypothesis 3, based on the absence of any significant differences between the small and large incentive conditions. This is consistent with other studies that have also failed to find an effect of incentive size (e.g., Maeder & Pica, 2014).

This study found evidence that jurors did not completely disregard even highly incentivized informant testimony. Contrary to Hypothesis 5, there was a significant difference between the large incentive and control condition, with the large incentive condition producing more convictions than the informant-absent control condition. This suggests that even when the informant admits to having a very strong incentive to testify (and therefore to lie), jurors were still influenced by his testimony and more likely to convict after exposure to his claims.

When examining the hope incentive condition, the conviction rate fell between that of the no incentive and small incentive condition, which did not precisely match the predictions of Hypothesis 4 but were broadly consistent with expected results. The hope incentive did not significantly differ from any condition except for the informant-absent control condition, which is consistent with Hypothesis 1 and the notion that even incentivized informant testimony is more persuasive to jurors than the absence of such testimony.

One area of interest for this study is manipulation check failures. In this pilot study, the rate of failure of the manipulation check varied from 84% in the hope incentive condition to 92% in the no incentive condition. There were no significant differences in the rate of manipulation check failure across conditions, so there was no evidence to indicate that one condition had more memorable incentive information than the other conditions.

When assessing informant credibility in this study, results indicated that the informant was viewed as significantly more credible in the no incentive condition compared to the large

incentive condition (i.e., average score of 52 compared to 35, with a score of 100 being “completely credible”). This provides a potential mechanism for the impact of incentive on verdicts outlined in Hypothesis 2. Participants who learn the informant has a large incentive to testify may be prompted to see the informant as less credible and, in turn, be less likely to convict the defendant. Results for the hope incentive compared to the large incentive also match this pattern.



## Appendix D – Analyses with Exclusions

Analyses for the main five hypotheses are presented in the paper using the full sample with no exclusions. The following section presents the results for each hypothesis when the sample excludes participants who failed the attention check question or manipulation check question. All hypotheses show the same core pattern of results (i.e., same outcomes for statistical significance) as that presented for the full sample.

**Hypothesis 1: Impact of the informant.** With the manipulation and attention check failures removed, the result is still consistent with that found for the full sample. For the binary logistic regression comparing verdict responses to the control condition ( $n = 167$ ) to the other four informant-present conditions ( $n = 575$ ), the overall model was significant,  $\chi^2(1, N = 742) = 9.98, p = .002$ . Once again, jurors were almost twice as likely to convict the defendant when exposed to informant testimony, compared to when no such testimony was presented ( $\text{Exp}(B) = 1.82, 95\% \text{ CI } [1.24, 2.67], \text{Wald} = 9.46, p = .002$ ). See Table D1 for conviction rates in each condition, with both the original Prolific sample and the adjusted sample with attention and manipulation check failures excluded.

**Table D1**

*Reduced sample: Adjusted cell sizes and conviction rates by condition*

| Condition       | Prolific N | Prolific<br>Conviction Rate | Adjusted N | Adjusted<br>Conviction Rate |
|-----------------|------------|-----------------------------|------------|-----------------------------|
| Control         | 177        | 26.6%                       | 167        | 26.3%                       |
| No Incentive    | 178        | 49.4%                       | 155        | 49.7%                       |
| Hope Incentive  | 181        | 40.3%                       | 138        | 39.1%                       |
| Small Incentive | 176        | 42.6%                       | 145        | 37.9%                       |
| Large Incentive | 174        | 34.5%                       | 137        | 29.9%                       |

**Hypothesis 2: Sensitivity to the absence of an incentive.** With the manipulation and attention check failures removed, the result is still consistent with that found for the full sample. For the binary logistic regression comparing verdict responses for the no incentive condition ( $n = 155$ ) to the two clear incentive conditions (small + large) combined ( $n = 282$ ), the overall model was significant,  $\chi^2(1, N = 437) = 10.15, p = .001$ . Once again, the difference between the no incentive condition and the two explicit incentive conditions combined was significant (Exp(B) = .52, 95% CI [0.35, 0.78], Wald = 10.11,  $p = .001$ ), consistent with the hypothesis.

**Hypothesis 3: Sensitivity to incentive size.** With the manipulation and attention check failures removed, the result is still consistent with that found for the full sample. For the binary logistic regression comparing verdict responses for the small incentive ( $n = 145$ ) and large incentive ( $n = 137$ ) conditions, the overall model was once again not significant,  $p = .156$ . Once again, based on this test, there was no evidence that that jurors responded differently in their verdicts to an incentive that was large compared to one that was small.

**Hypothesis 4: Perception of the hope-for-leniency condition.** With the manipulation and attention check failures removed, the result is still consistent with that found for the full sample. The hope incentive and small incentive conditions continue to show very similar conviction rates (39% compared to 38%). With the removal of the attention and manipulation check failures, the gap between the conviction rates for the hope and large incentive conditions seems to have grown, from a difference of 5% with the full sample to a difference of 9% with the reduced sample. However, when tested, the overall model comparing responses for the hope incentive and large incentive conditions was still not significant,  $p = .108$ .

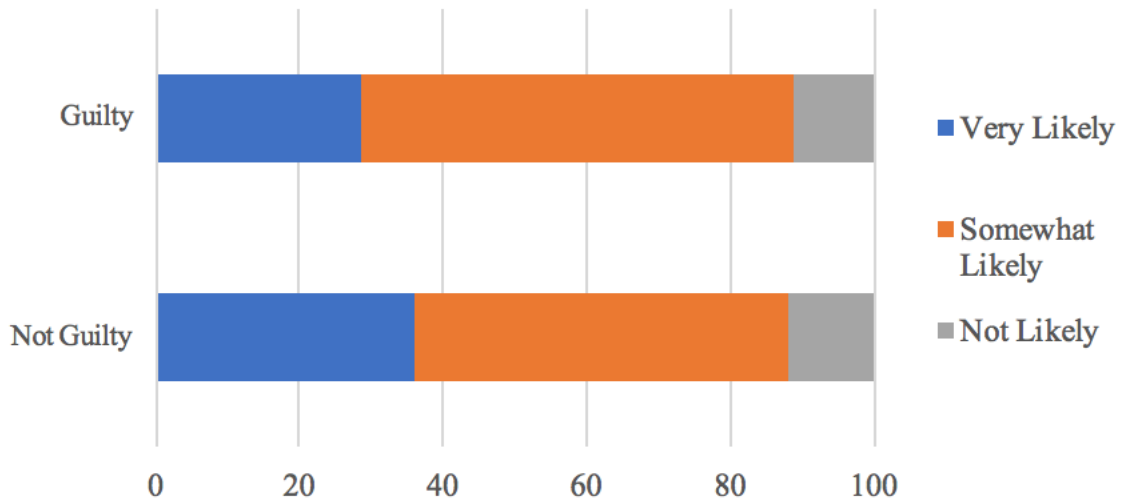
**Hypothesis 5: Impact of a large incentive.** With the manipulation and attention check failures removed, the result is still consistent with that found for the full sample. For the binary

logistic regression comparing verdict responses for the large incentive ( $n = 137$ ) and control ( $n = 167$ ) conditions, the overall model was once again not significant,  $p = .489$ .

Appendix E – Additional Figures for Beliefs about Informant Incentives

Figure E1

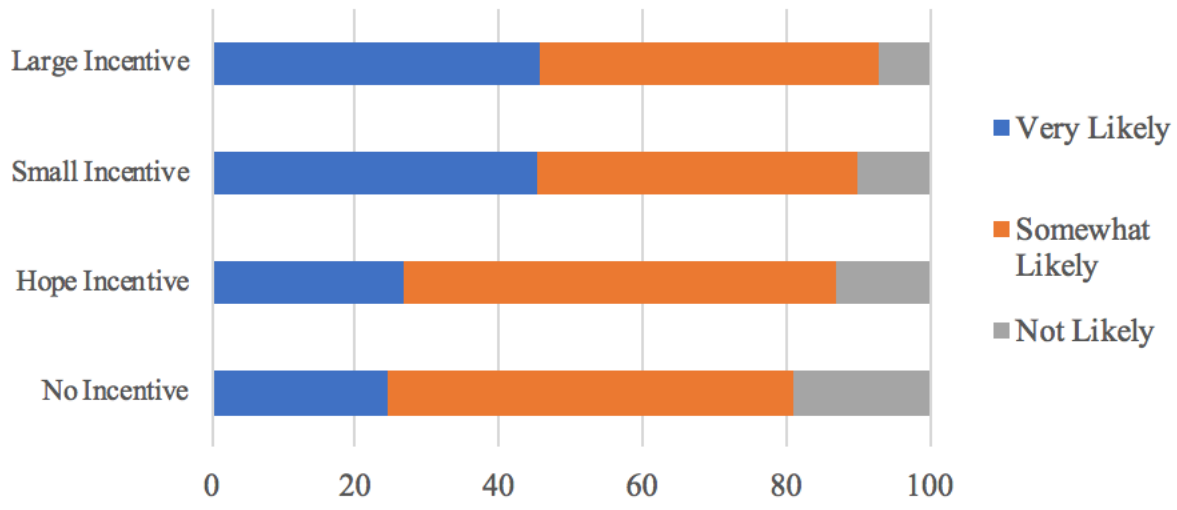
*Distribution of beliefs about actual informant incentives by verdict*



Distribution of responses to the question:  
Will the informant actually receive a sentence reduction?

**Figure E2**

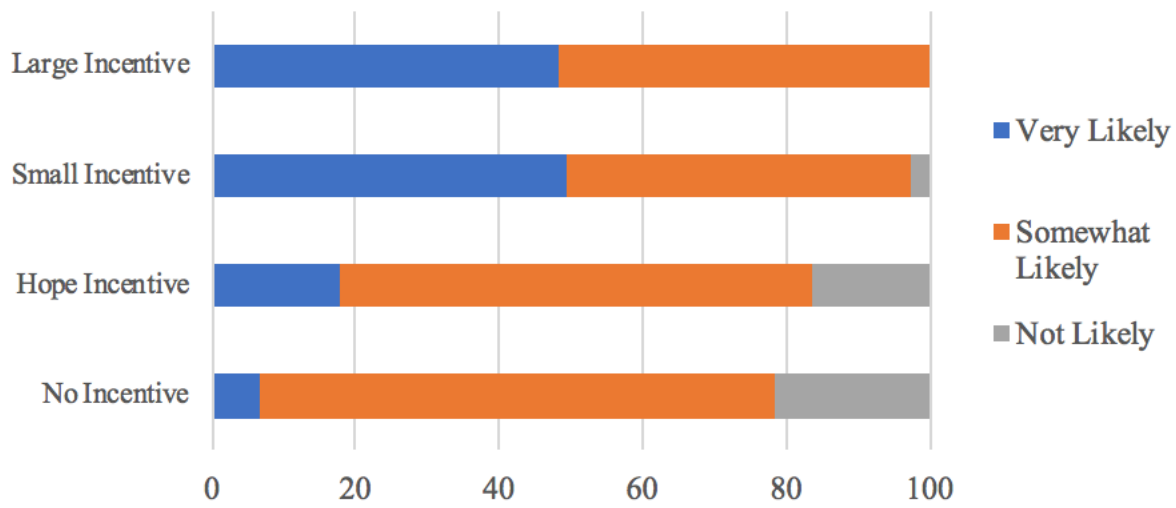
*Acquittals only — distribution of beliefs about actual informant incentives*



Distribution of responses to the question:  
Will the informant actually receive a sentence reduction?

**Figure E3**

*Convictions only — distribution of beliefs about actual informant incentives*



Distribution of responses to the question:  
Will the informant actually receive a sentence reduction?

## Appendix F – Combined Pilot and Final Study Results

In order to explore the results for an exceptionally well-powered design, the pilot data and the final study data were merged to produce a single combined sample. Due to changes made in measures of informant credibility, the scales could not be directly compared. However, the verdict question was present in the pilot and study data and was the focus of this analysis. Each of the primary five hypotheses was re-tested with this combined data set, which included 886 responses from the study data set, along with the 375 responses from the pilot, for a total of 1,261 responses. This produced a final count per cell of between 250 and 256 for all conditions. The combined sample was 47% male, 52% female, and 1% other (12 respondents). The sample was 68% White, 14% Asian, 8% Hispanic/Latinx, 7% Black, and 3% all other groups. The mean age was 34, with a minimum of 18 and a maximum of 82 ( $SD = 12.6$ ).

**Table F1**

*Reduced sample: Adjusted cell sizes and conviction rates by condition*

| Condition       | Prolific N | Prolific Conviction Rate | Combined N | Combined Conviction Rate |
|-----------------|------------|--------------------------|------------|--------------------------|
| Control         | 177        | 26.6%                    | 251        | 22.3%                    |
| No Incentive    | 178        | 49.4%                    | 256        | 50.0%                    |
| Hope Incentive  | 181        | 40.3%                    | 250        | 40.4%                    |
| Small Incentive | 176        | 42.6%                    | 253        | 41.1%                    |
| Large Incentive | 174        | 34.5%                    | 251        | 32.3%                    |

**Hypothesis 1: Impact of the informant.** With the combined data set, the result is still consistent with that found for the final study sample. For the binary logistic regression comparing verdict responses to the control condition ( $n = 251$ ) to the other four informant-present conditions ( $n = 1010$ ), the overall model was statistically significant,  $\chi^2(1, N = 1261) =$

30.0,  $p < .001$ . Once again, jurors were approximately twice as likely to convict the defendant when exposed to informant testimony, compared to when no such testimony was presented (Exp(B) = 2.42, 95% CI [1.75, 3.34], Wald = 28.8,  $p < .001$ ). See Table F1 for the conviction rates in each condition for the combined pilot and full study sample.

**Hypothesis 2: Sensitivity to the absence of an incentive.** With the combined data set, the result is still consistent with that found for the final study sample. For the binary logistic regression comparing verdict responses for the no incentive condition ( $n = 256$ ) to the two clear incentive conditions (small + large) combined ( $n = 504$ ), the overall model was significant,  $\chi^2(1, N = 760) = 12.31, p < .001$ . Once again, the difference between the no incentive condition and the two explicit incentive conditions combined was significant (Exp(B) = .58, 95% CI [0.43, 0.79], Wald = 12.28,  $p < .001$ ), consistent with the hypothesis.

**Hypothesis 3: Sensitivity to incentive size.** With the combined sample, the result diverges from that found with the final study sample. For the binary logistic regression comparing verdict responses for the small incentive ( $n = 253$ ) and large incentive ( $n = 251$ ) conditions, the overall model is now significant,  $\chi^2(1, N = 504) = 4.24, p = .039$ , where it previously was not when only the study sample was included. The difference between the small incentive condition and the large condition was significant (Exp(B) = .68, 95% CI [0.47, 0.98], Wald = 4.22,  $p = .040$ ). This result provides support for Hypothesis 3, which stated that there would be a significant difference between the small and large incentive conditions, with the large incentive condition producing fewer guilty verdicts than the small incentive condition.

**Hypothesis 4: Perception of the hope-for-leniency condition.** With the combined sample, the result diverges from that found for the final study sample. First, for the binary logistic regression comparing verdict responses for the no incentive ( $n = 256$ ) and hope incentive

( $n = 250$ ) conditions, the overall model is now significant,  $\chi^2(1, N = 506) = 4.71, p = .030$ . The difference between the no incentive condition and the hope condition was significant ( $\text{Exp}(B) = .68, 95\% \text{ CI } [0.48, 0.96], \text{Wald} = 4.69, p = .030$ ). However, when the hope condition was compared to the small and large incentive conditions (each separately), the overall models were not significant ( $p = .872$  and  $p = .058$  respectively). The hope incentive and small incentive conditions continue to show very similar conviction rates (60% compared to 59%). Together, these results do not support Hypothesis 4, in which I predicted that the hope incentive condition would be similar to the no incentive condition, but significantly different from the small and large conditions. Here, the opposite pattern is found.

**Hypothesis 5: Impact of a large incentive.** With the combined sample, the result diverges from that found for the final study sample. For the binary logistic regression comparing verdict responses for the large incentive ( $n = 251$ ) and control ( $n = 251$ ) conditions, the overall model is now significant,  $\chi^2(1, N = 502) = 6.30, p = .012$ , where it previously was not when only the study sample was included. The difference between the large incentive condition and the control condition was significant ( $\text{Exp}(B) = .61, 95\% \text{ CI } [0.41, 0.90], \text{Wald} = 6.22, p = .013$ ).

This result does not support Hypothesis 5, which stated that there would not be a significant difference between the large incentive and control conditions. Here, the opposite pattern is found, with the large incentive condition producing significantly more convictions than the control condition (32% compared to 22% respectively).

**Discussion of Combined Study Results.** The results for Hypothesis 1 and 2 are supported by the combined sample, which was also the case for the individual final study sample (see Discussion section in full manuscript for implications). However, the results for Hypotheses 3, 4 and 5 all diverge from that of the final study sample when the combined sample is used.



For Hypothesis 3, the difference between the small and large incentive condition is statistically significant, which indicates that jurors are sensitive not just to existence of an incentive, but also to the size of the incentive. This contrasts with prior research done with smaller samples that has found no evidence of sensitivity to incentive size (e.g., Maeder & Pica, 2014).

Hypothesis 4 concerned the new hope incentive condition and its conviction rate relative to other incentive conditions. This analysis of the combined sample found that the hope incentive is significantly different from the no incentive condition (contrary to Hypothesis 4), such that the hope incentive condition produced fewer convictions than the no incentive condition. However, the hope incentive did not significantly differ from either of the specific incentive conditions (also contrary to Hypothesis 4). This suggests that jurors exposed to the hope incentive condition may recognize that the informant is still incentivized to lie, even if the benefits are not described concretely.

Using the combined sample, the results were no longer consistent with Hypothesis 5. I had expected no significant difference between the large incentive and control conditions, but the combined sample found a significant difference between the two. The large incentive condition produced more convictions than the control condition. This has important implications for the powerful influence of informant testimony; even when the informant admits to receiving a major incentive to testify (10 years off of a 20-year prison sentence), his testimony is not fully disregarded by jurors, and it continues to produce significantly more convictions than the trial evidence would provide without the informant.