Snitching, lies, and computer crashes: An experimental investigation of secondary confessions

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Snitching, Lies and Computer Crashes: An Experimental Investigation of Secondary Confessions

Jessica K. Swanner · Denise R. Beike · Alexander T. Cole

Abstract Two laboratory studies with 332 student participants investigated secondary confessions (provided by an informant instead of the suspect). Participants allegedly caused or witnessed a simulated computer crash, then were asked to give primary or secondary confessions during interrogation. Study 1 replicated the false evidence effect for primary confessions. Secondary confessions were obtained at a high rate, which was increased by false evidence in combination with incentive to confess. In Study 2 a confederate either confessed to or denied crashing the computer. Incentive increased the rate of secondary confession only in the presence of a denial; that is, incentive increased the number of false secondary confessions only. Implications for the use of incentives during informant interrogation are discussed.

Keywords Confessions · Interrogations · Informants · Incentive

Testimony from cooperative witnesses, who have information about a crime but are neither suspects nor victims (Cassidy, 2004), is a critical feature of the US justice system. For example, on January 29, 1987, 25-year-old Diana Lowery was found asphyxiated in her bathroom in San Antonio, TX. The Lowery murder remained unsolved until 2005, when the San Antonio Police Department (SAPD) Cold Case Detectives filed murder charges against Larry Leroy Moore, Lowery’s landlord. Earlier in 2005, a cooperative witness had contacted SAPD implicating Moore, leading the Cold Case detectives to reopen the case. SAPD Cold Case detectives went to California to collect DNA evidence from Moore, and that DNA matched sexual assault evidence from the murder scene. On July 12, 2005, the cold case of Diana Lowery’s murder was solved. The above case demonstrates the positive effect of cooperative witnesses—the discovery of information that leads to the apprehension of perpetrators (Mazur, 2002).

Unfortunately, the effect of interrogating cooperative witnesses is not always positive. For example, on May 26, 2006, Larry Peterson was exonerated after serving over 16 years for murder and aggravated sexual assault, crimes that he did not in fact commit. The primary evidence against Peterson was testimony from cooperative witnesses. One of the informants was a jailhouse snitch who had an incentive to testify because he was awaiting charges in three counties. The other three informants were coworkers who originally claimed to be with Peterson at the time the victim was found. Initially Peterson’s coworkers claimed Peterson did not confess to them that he committed the crime; however, after multiple interrogations, the informants told the police that Peterson confessed to them while they were carpooling to work. The type of confession evidence provided by the jailhouse snitch and by Peterson’s coworkers has been termed a secondary confession (Neuschatz, Lawson, Swanner, Meissner, & Neuschatz, 2008). A secondary confession is a confession provided by someone other than the suspect (i.e., a snitch) based on information told to that person by the suspect. Based upon these secondary confessions, Peterson was sentenced to life plus 20 years in prison. In 1995, the Innocence Project began working on Peterson’s case; in 2005, the results of DNA testing of evidence from the rape concluded that the biological evidence could not have come from Peterson. This case demonstrates the potentially
biased nature of cooperative witness testimony, specifically secondary confessions, which are the focus of the present research. Moreover, the case demonstrates two potentially problematic procedures used to obtain the false secondary confessions: For the jailhouse informant, incentive to provide testimony; and for the coworkers, extended and pressuring interrogation tactics.

The type of potential secondary confessor of interest in the present research is that of an informant. An informant is any person who can provide information about a case but who is not a complainant (i.e., someone who is requesting police action regarding a crime), witness (i.e., someone who saw, at least in part, the crime taking place), victim or suspect (Bennett & Hess, 2004). An informant may testify for the prosecution against the suspect, and this testimony may occur in exchange for an incentive. In recent decades, the Supreme Court has recognized that bartered testimony may be prejudicial and unreliable; however, the Court argues that in the case of bartered testimony, there are safeguards designed to protect the rights of the accused (Giglio v. United States, 1972; United States v. Singleton, 1998). Unfortunately, either the incentive provided by prosecutors or the pressuring interrogation tactics used by police officers may induce an informant to fabricate information. According to the Northwestern School of Law Center on Wrongful Convictions, informants testified in 46% of death row exonerations (Warden, 2004). The Supreme Court has recognized that incentives provided for testimony may bias a witness and provide a motive to lie; however, they argue that safeguards are in place to protect defendants based upon the Sixth Amendment right to confrontation of witnesses (Giglio v. United States, 1972; United States v. Singleton, 1998). One safeguard is that the defense must be made aware of any incentives or deals provided to informants for their testimony (Cassidy, 2004). In other words, the court ruled in Giglio v. United States (1972) that if the prosecution has offered an incentive or deal (i.e., reduced sentence, agreement not to prosecute, or monetary reward) in exchange for testimony, this agreement must be presented to the defense (Cassidy, 2004). A defense team so informed can then cross-examine the informant to make the jury aware of the potentially biased nature of the testimony.

Unfortunately, the present safeguards have not been sufficient to prevent innocent people from being convicted due to false secondary confessions (Gershman, 2002; Scheck, 2002; Scheck, Neufeld, & Dwyer, 2000; Skurka, 2002; Warden, 2004). In a symposium on cooperating witnesses at Cardozo School of Law, legal experts and social scientists discussed the pitfalls of informant testimony. Caution in using informant testimony was advised because informants (1) can be influenced by coercive and leading interviewing techniques; (2) may provide false or misleading testimony with or without the prosecutor’s knowledge; (3) may provide false testimony via errors in memory or incomplete disclosure of facts that without any video or audio recording or documentation may remain unknown to the defense; and (4) may seem inordinately convincing to a jury (Gershman, 2002).

The following potential prophylactic methods were proposed in the symposium in order to better safeguard against false informant testimony (Gershman, 2002). First, pretrial hearings should be conducted in efforts to reveal inconsistencies in testimony from informants, as well as exploration of incentives for testimony. Second, experts should be allowed to testify in trials regarding the malleability of memory and leading interviewing techniques. Finally, interviews of informants conducted by police investigators and prosecutors should be recorded (preferably video recording) in order to help jurors accurately assess whether or not the informant has been led or was provided an incentive by the interrogator or prosecutor. The third prophylactic procedure seems to be the most effective method for exposing incentivized informants.

However, the mere knowledge that an informant was provided an incentive for her testimony does not guarantee that jurors will account for this information in their verdict. In an investigation of the effects of secondary confessions on judicial decision-making, similar to primary confessions (statements of guilt provided by the suspect directly; see Kassin & Gudjonsson, 2004 for a review of the impact of primary confessions on judicial decision-making), mock jurors were more likely to convict the defendant when there was a secondary confession present relative to no confession, regardless of the presence of an incentive (Neuschatz et al., 2008). High conviction rates were present even when the incentive was explicitly stated, suggesting that the safeguard implemented in Giglio v. United States is insufficient. Further, 85% of participants attributed the cause of the testimony to internal characteristics of the secondary confessor (i.e., felt guilty, felt sorry for the family), while only a minority attributed the testimony to external factors (i.e., the reward, leniency, etc.). Thus, jurors may not discount the role of incentive given to secondary confessors because they simply cannot imagine that incentive could potentially elicit a false secondary confession.

A similar problem is encountered with primary confessions: Most people (and therefore most jurors) find it difficult to imagine that a person would falsely confess to a crime she did not commit. When people assume a confession necessarily arises from the guilt status of the confessor, they commit the fundamental attribution error (FAE; Kassin & Gudjonsson, 2004). According to the FAE, people overattribute internal motives as the cause of others’ behaviors and do not take into account external factors that
may cause behavior (Ross, 1977). A similar error may lead people to attribute secondary confessions to the confessor’s genuine knowledge of the crime rather than to external factors such as high-pressure interrogation or incentive to provide a confession. The present research investigates conditions under which the average person may provide a false secondary confession.

We developed the research situation from experimental research on primary false confessions. These laboratory analog situations have revealed that under the right social conditions, innocent people will confess (Kassin & Kiechel, 1996). Further, innocent people can and will internalize false confessions (i.e., the participants seem to actually believe that they performed the act that they did not commit) and will confabulate details about the event (i.e., they provide false details about the act they did not commit). The first published laboratory analog (Kassin & Kiechel, 1996) demonstrating confessions from innocent people involved a computer program in which participants were required to type a set of letters as they were read by a confederate. Participants were informed that there was a flaw in the program and urged not to press the ALT key because it would cause the loss of all the data. After a simulated computer crash, the experimenter accused the participant of hitting the ALT key and asked the participant to sign a written confession admitting as much. Participants were led to believe they would face an unpleasant and lengthy discussion with the professor in charge of the experiment if they signed the statement.

Although all of the participants were in fact innocent, 69% of them confessed to hitting the ALT key. Clearly the consequences of confessing to hitting the ALT key are not equivalent to confessing to even the most minor of criminal offenses, yet even under the mildest forms of pressure, a student will follow the request of a perceived authority figure, the experimenter, and sign a confession. Moreover, participants who had been given false evidence (the confederate said that she had seen the participant hit the ALT key) internalized this confession. Of the 69% of participants who confessed, 28% of them internalized the confession, and 9% of those also confabulated details to maintain the internalization.

The higher confession and internalization rates in the presence of false evidence (an alleged eyewitness) have been replicated (Horselenberg, Merkelbach, & Josephs, 2003; Horselenberg et al., 2006; Redlich & Goodman, 2003). In addition, the plausibility of the forbidden key also affects false confession rates and internalization (Horselenberg et al., 2006). Specifically, participants are more likely to falsely confess when the forbidden key is plausible (close to the keys the participant is typing) than when it is more implausible (farther away). Replication studies using the computer crash paradigm demonstrated that increasing the severity of the consequences does not deter false confessions, and that individual characteristics of the participants and situation may make an individual more prone to signing a false confession (Candel, Merkelbach, Loyen, & Reykens, 2005; Forrest, Wadkins, & Larson, 2006; Forrest, Wadkins, & Miller, 2002; Horselenberg et al., 2003; Redlich & Goodman, 2003).

Unfortunately, no extant studies have investigated whether participants will falsely provide secondary confessions. Investigating secondary confessions in the laboratory is important because reluctant cooperative witnesses may be subject to the same interrogation techniques employed to obtain primary confessions (Inbau, Reid, Buckley, & Jayne, 2001). In fact the authors of Criminal Interrogation and Confessions (Inbau et al., 2001), recommend that witnesses reluctant to cooperate with police investigation should be treated like suspects and interrogated as such. In other words, witnesses may be provided with false evidence, or may be given incentives to confess. For example, in the Peterson case the jailhouse informant was granted a reduction in sentence for providing a secondary confession. Further, Peterson’s coworkers were subjected to lengthy and coercive interrogations where the officers discussed the details of the case outside the interrogation room, in effect providing the cooperative witnesses with the necessary information for a sound confession. Because secondary confessions are so important to criminal investigations, it is essential that investigators as well as jurors understand the circumstances that are likely to lead to true secondary confessions, and those that might lead to false secondary confessions.

Toward these ends, the present studies were designed to extend the computer crash methodology to secondary confessions. One aim of the research was to compare the rates of false secondary confessions to primary confessions. Another aim of the research was to investigate whether false evidence increases the amount of false secondary confessions as it does for primary confessions. A third aim was to investigate the role of incentive in both primary and secondary confessions. In the present two experiments, we used an explicitly stated incentive. Previous research has demonstrated that minimization techniques (i.e., interrogation techniques that make the suspect feel that confessing is the right thing to do and is safe; Kassin, 1997) imply leniency (Kassin & McNall, 1991) and have a similar effect to direct incentives of increasing the number of false primary confessions (Russano, Meissner, Narchet, & Kassin, 2005). Thus, minimization and other forms of implied incentives should work in a similar manner as the direct incentives used in the present studies.

Therefore, in Experiment 1 both the reader (secondary confessor) and the typist (primary confessor) were participants, to whom false evidence and incentive were or were not provided. Based upon previous research (Kassin &
Kassin and Kiechel (1996), we predicted that false evidence would increase both primary and secondary confessions. In addition, due to the number of wrongful convictions based upon false informants we predicted that incentive to escape punishment would increase secondary confessions. Another aim of the research was to examine true and false secondary confessions. Experiment 2 therefore investigated the effect of incentive on secondary confessions, when the informant heard either a confession or a denial from a confederate. We predicted that incentive to escape punishment will increase the number of false, but not true, secondary confessions.

EXPERIMENT 1

Experiment 1 was designed in part as a replication of the Kassin and Kiechel (1996) computer crash study, with the addition of (a) a participant who could provide a false secondary confession; and (b) incentive provided for a confession for some participants. As in the original study, one participant typed on the computer while a second person read aloud the letters to be typed. In Kassin and Kiechel’s (1996) study, the role of the reader had been played by a confederate. In our Experiment 1, the role of the reader was assigned to a second participant, as naïve to the experiment set-up as the typist. Thus, one participant was in a position to make a false primary confession under interrogation, and the other a false secondary confession.

As in the original study, half of participants received false evidence in the form of an untruthful eyewitness. But in this study the false evidence was provided by the experimenter (e.g., Horselenberg et al., 2003; Horselenberg et al., 2006), who alleged that the other participant said they had either seen the typist hit the forbidden key (provided to the typist), or that they admitted to having hit the forbidden key (provided to the reader). In addition, we manipulated whether or not the participants were informed about the possibility of an incentive for providing either a primary or secondary confession. As an incentive, each participant was told that signing a confession statement would mean that only one of the two participants would be punished for the computer crash.

The incentive that the reader received was a selfish incentive (i.e., the reader would not have to come back for a second session); whereas, the incentive for the typist was more altruistic (i.e., the typist would have to come back for a second session, but the other participant could go free). Although the incentive for the typist is not completely externally valid (see section “Procedure”??? for exact wording), we opted to offer an altruistic incentive so that the wording would closely match the incentive for the reader while still being realistic. It is unlikely that police interrogators would offer an incentive that allows the suspect to go free if they confess while they prosecute the informant. Therefore, we made the incentive an altruistic one that by confessing the nonresponsible individual would escape punishment.

Method

Participants

A total of 212 University of Arkansas students participated in this study in exchange for partial fulfillment of a course requirement. Each participant was randomly assigned to one of the eight different experimental conditions. The data from 9 participants were not used in the analyses due to these participants being computer programmers (3), non-native English speaking students (4), or not following the instructions (2), leaving a total of 203 participants (78 males and 125 females).

Materials

For each participant dyad, the experimenter completed each page of an experimental packet. The cover page allowed the experimenter to record participant information including condition and gender. The next two pages contain the script for the protocol. Before each piece of information in the script was a space in which the experimenter placed a check mark to signify that he or she accurately presented that information. Italicized parts of the script signified lines spoken by the experimenter, while normal font signified actions performed by the experimenter. The script was designed specifically to make adherence to the script easy for the experimenters. The protocol contained many entrances and exits from the room(s) with the participants, in order to allow the experimenter to look over his or her lines before delivering them to the participants. The following is an example from the script demonstrating spoken lines and entrances and exits performed by the experimenter:

“Well, first my advisor said that you will have to reschedule for a second session because of this. Let me go get the running schedule and tell the other participant that she/he will have to reschedule too.

Enter the room with the READER

Well, first my advisor said that you will have to reschedule for a second session because of this. Let me go get the running schedule and tell the other participant that she/he will have to reschedule too.”

The script contained eight interrogation questions adapted from Forrest et al. ((2002); e.g.. “Are you sure
they didn’t mention anything about hitting the TAB key?” or “The pace was pretty fast. Don’t you think it’s possible they could have hit the TAB key?”). The last pages of the experimental packet contained the debriefing script.

Procedure

During their training, experimenters were told that their job was to elicit confessions, and that a prize would be awarded to the experimenter who obtained the highest rate of confession. Before experimenters conducted any sessions they were required to fully memorize the script. In order to demonstrate their adherence to the script, all experimenters ran the first author through the protocol and then a pair of mock participants through the protocol while the first author was present. Further, experimenters were instructed to record any inconsistencies in their ability to adhere to the script.

Before participants arrived for the session, the experimenter placed an experimental packet on a clipboard to have with him or her during the session. Experimenters were instructed to make sure the participants did not see what was on their clipboard. Participants were run in pairs. Participants were led to believe they would be participating in a study on unconscious memory. Further, they were informed that the purpose of the study was whether typing in a study on unconscious memory. Further, they were informed that the participants will have to reschedule for a second session. The experimenter separately provided the experimental manipulations to and questioned the reader and the typist. The experimenters asked the eight questions in order. If the participant provided a verbal admission of guilt, then the experimenter recorded how many of the eight questions were necessary in order to obtain an admission of guilt from the participant.

If a participant gave a verbal primary confession, the experimenter gave him or her a pad of paper and told him or her to write, “I admit to hitting the TAB key and causing the computer to crash,” followed by a signature. If a participant gave a verbal secondary confession, the experimenter gave him or her a pad of paper and told him or her to write, “The other participant admitted to me that she/he hit the TAB key and caused the computer to crash,” followed by a signature. Regardless of whether participants signed a confession statement, either primary or secondary, all participants were handed a tape recorder and were instructed to record their thoughts about everything that had happened. The results from the tape recordings are not discussed further, as they did not provide any additional information beyond the written confession statements themselves.

After this, the participants were separately debriefed and thanked. Experimenters went through a lengthy debriefing script based upon the suggestions by Aronson, Wilson, and Brewer (1998). Participants were probed for suspicion, and the purpose of the study and reasons for using deception were gradually explained to them. They were informed that 2 weeks later they would receive a link to an online follow-up questionnaire regarding the experiment. The follow-up questionnaire contained the Impact of Event Scale-Revised.
(Weiss & Marmar, 1997) and the Juror Bias Scale (Kassin & Wrightsman, 1983), which revealed no lingering effects of the procedure. The only effect on the Impact of Event Scale was that those who had signed a confession statement reported marginally more intrusive thoughts and more hyperarousal symptoms during the preceding week than those who had not signed a confession statement (for intrusive thoughts, 5.0 for those who signed and 2.5 for those who did not; \( \tau(85) = -1.94, p < .06 \); for hyperarousal, 2.5 for those who signed and 0.9 for those who did not; \( \tau(85) = -2.11, p < .04 \)). However, this level of intrusive thoughts and hyperarousal symptoms among those who signed a confession statement was very low. For comparison, intrusive thoughts and hyperarousal symptoms for the same period of time were 3.3 and 2.4 (nonsignificantly different from those who signed a confession statement) among a group of participants who had completed a questionnaire study instead of the computer crash study. Three experimenters tied for the highest rate of confessions, and each was awarded a book on false confessions as a prize.

False Evidence Each typist was randomly assigned to receive either no false evidence or false evidence from the experimenter. For the false evidence, the experimenter told the typist that “the other participant told me that they are sure they saw you hit the TAB key.” For no false evidence, the typist was told that “the other participant told me that they don’t think they saw you hit the TAB key.” Each reader was also randomly assigned to receive either no false evidence or false evidence from the experimenter. For the false evidence, the experimenter told the reader that “the other participant told me that they are sure they hit the TAB key.” For no false evidence, the experimenter told the reader that “the other participant told me that they did not hit the TAB key.”

Incentives for Confessing In addition, each typist was randomly assigned to receive or not receive incentive to confess. In the incentive condition, the experimenter said,

You know, when I talked to my advisor she said that there’s no reason for you both to get in trouble for this. She said that only one of you two really has to come back for the second session, but that’s only if you tell me what happened. So if you tell me what happened, only you will have to come back.

This information was not presented in the no incentive condition. In addition, each reader was randomly assigned to receive an incentive for their testimony or no deal. For the reader in the incentive condition, the experimenter said, said the same thing as for the typist except the reader was told that, “only the other person will have to come back.” Readers in the no incentive condition did not hear this information.

Design and Measures

The study conformed to a 2 (Role: Reader vs. typist) × 2 (False evidence: False evidence vs. no false evidence) × 2 (Incentive: Incentive vs. no incentive) design. The dependent measures include whether or not the participant signed a confession statement and the number of questions required for a confession (1–8). We used the number of questions required to elicit a confession as a dependent measure because research on primary false confessions suggests that most require extended questioning (Drizin & Leo, 2004). Typically interrogations take 2 hours or less (Leo, 1996); however approximately 73% of actual interrogations that resulted in a false confession lasted over 6 h, with the average interrogation lasting 16.3 h (Drizin & Leo, 2004). Moreover, the false secondary confessions from the coworkers in the Peterson case were in part the result of a lengthy interrogation. Therefore, we concluded that the number of questions required to obtain a confession in this paradigm could serve as a proxy for the length of the interrogation, and therefore as a measure of how much pressure was necessary to produce a confession.

Further, each confession statement was coded for how much guilt the participant attributed to the typist. Guilt attributions were measured on a 7 point scale (1 = complete denial, e.g., “I did not hit the TAB key” or “The other participant did not hit the TAB key”; 7 = full confession, (e.g., “I hit the TAB key” or “The other participant did not hit the TAB key”). Four independent coders rated the confession statements for attribution of guilt. We measured guilt attribution rather than the more typical measure of internalization. Internalization is operationalized as whether or not the participant actually believed she performed the forbidden act (Kassin & Kiechel, 1996). Because secondary confessors cannot internalize performing the act in the same manner as the primary confessors, we measured how much each participant believed that the typist performed the forbidden act. Inter-rater reliability for the guilt attributions was high (\( \alpha = .82 \)); therefore, the guilt attribution score was an average of the four coders’ scores. The confession statements were also coded for whether or not the participant provided an excuse for the typist’s alleged behavior (i.e., suggesting that the typist did not hit the TAB key on purpose, or stating that it was an accident). Excuses were measured using a dichotomous scale (i.e., yes or no). Two independent coders rated each confession statement for providing an excuse. Inter-rater reliability for the excuse coding was high (Cohen’s Kappa = .92, \( p < .001 \)). Discrepancies were resolved using a third independent coder.
Results and Discussion

Signed Confession Statements

First, the effect of role on the numbers of participants who signed versus refused to sign confession statements was analyzed. Readers were more likely to sign (secondary) confessions than typists were to sign (primary) confessions, $\chi^2(1) = 15.94, p < .001$, with 79% of readers and 52% of typists signing confession statements. Not surprisingly, participants were more willing to implicate others than themselves. A single hierarchical loglinear analysis across all participants in Experiment 1 revealed only a marginal interaction among role, false evidence, and incentive ($\chi^2 = 3.45, p < .06$). Therefore, effects of false evidence and incentive were explored separately for typists (primary confessors) and readers (secondary confessors).

The numbers of typist participants who signed versus refused to sign primary confession statements were submitted to hierarchical loglinear analysis. There was a main effect of false evidence ($\chi^2(1) = 5.75, p < .03$), qualified by an interaction with incentive ($\chi^2(1) = 7.80, p < .01$). As shown in Table 1, providing false evidence was sufficient to increase the rate of signing statements but only in the presence of false evidence (see Table 1 for pairwise comparisons). In other words, the typical false evidence effect was found only in the absence of incentive. It appears the altruistic incentive provided to the participants was nearly sufficient to induce false confession even in the absence of false evidence. Finally, only one participant (2%) wrote a primary confession statement exactly as instructed by the experimenter. The remaining 98% of participants altered the wording to read that they “may have” or “accidentally” hit the TAB key, suggesting that participants spontaneously minimized their role in producing the computer crash. All other main effects and interactions were nonsignificant ($\chi^2$s < 1, $p$s > .3).

Table 1 Effects of false evidence and incentive on primary confessions in experiment 1

<table>
<thead>
<tr>
<th>Measure</th>
<th>Evidence condition</th>
<th>Incentive condition</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>No incentive</td>
</tr>
<tr>
<td>Signed statement</td>
<td>No evidence</td>
<td>26%$_a$</td>
</tr>
<tr>
<td></td>
<td>False evidence</td>
<td>65%$_b$</td>
</tr>
<tr>
<td>Pressure needed</td>
<td>No evidence</td>
<td>7.5 (1.2)</td>
</tr>
<tr>
<td></td>
<td>False evidence</td>
<td>5.6 (2.8)</td>
</tr>
<tr>
<td>Guilt attribution</td>
<td>No evidence</td>
<td>4.8$_{ab}$ (0.6)</td>
</tr>
<tr>
<td></td>
<td>False evidence</td>
<td>5.0$_b$ (0.8)</td>
</tr>
<tr>
<td>Provided excuse</td>
<td>No evidence</td>
<td>17%</td>
</tr>
<tr>
<td></td>
<td>False evidence</td>
<td>36%</td>
</tr>
</tbody>
</table>

Notes: Standard deviations are given in parentheses. Pressure ranges from 1 (agreed after 1 pressuring question) to 8 (agreed after 8 pressuring questions). Guilt attribution ranges from 1 (complete denial) to 7 (complete admission of guilt). Guilt attribution and excuses are present only for those who signed a statement. Means within a measure not sharing a subscript differ at $p < .05$

As shown in Table 1, providing false evidence was sufficient to increase the rate of signing statements but only in the presence of false evidence (see Table 1 for pairwise comparisons). In other words, the typical false evidence effect was found only in the absence of incentive. It appears the altruistic incentive provided to the participants was nearly sufficient to induce false confession even in the absence of false evidence. Finally, only one participant (2%) wrote a primary confession statement exactly as instructed by the experimenter. The remaining 98% of participants altered the wording to read that they “may have” or “accidentally” hit the TAB key, suggesting that participants spontaneously minimized their role in producing the computer crash. All other main effects and interactions were nonsignificant ($\chi^2$s < 1, $p$s > .3).

Table 2 Effects of false evidence and incentive on secondary confessions in experiment 1

<table>
<thead>
<tr>
<th>Measure</th>
<th>Evidence condition</th>
<th>Incentive condition</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>No incentive</td>
</tr>
<tr>
<td>Signed statement</td>
<td>No evidence</td>
<td>65%$_a$</td>
</tr>
<tr>
<td></td>
<td>False evidence</td>
<td>77%$_{ab}$</td>
</tr>
<tr>
<td>Pressure needed</td>
<td>No evidence</td>
<td>6.4 (2.1)</td>
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<td></td>
<td>False evidence</td>
<td>4.9 (3.1)</td>
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<tr>
<td>Guilt attribution</td>
<td>No evidence</td>
<td>4.9 (0.7)</td>
</tr>
<tr>
<td></td>
<td>False evidence</td>
<td>5.0 (0.8)</td>
</tr>
<tr>
<td>Provided excuse</td>
<td>No evidence</td>
<td>32%</td>
</tr>
<tr>
<td></td>
<td>False evidence</td>
<td>31%</td>
</tr>
</tbody>
</table>

Notes: Standard deviations are given in parentheses. Pressure ranges from 1 (agreed after 1 pressuring question) to 8 (agreed after 8 pressuring questions). Guilt attribution ranges from 1 (complete denial) to 7 (complete admission of guilt). Guilt attribution and excuses are present only for those who signed a statement. Means within a measure not sharing a subscript differ at $p < .05$

The number of interrogation questions needed to elicit a confession was submitted to a 2 (Role) $\times$ 2 (Evidence) $\times$ 2 (Incentive) ANOVA, which revealed a marginal interaction of evidence and incentive, $F (1, 125) = 3.17, p < .08$, $\eta^2 = .03$. Across both primary and secondary confessions,
providing false evidence reduced the number of questions necessary to obtain a confession in the absence of incentive ($M_s = 6.7$ and $5.2$; $t(99) = 3.19$, $p < .002$, Cohen’s $d = 0.73$), but not in the presence of incentive, where relatively few questions were required in both conditions ($M_s = 5.5$ and $5.7$; $t(100) = 0.65$, $p > .51$). The pattern suggests that the offer of incentive to confess was nearly as powerful as false evidence in reducing the number of questions needed to elicit a confession. All other main effects and interactions were nonsignificant ($\chi^2 s < 1.2$, $ps > .2$).

EXPERIMENT 2

Experiment 1 revealed that incentive (like false evidence) increases the rate of both primary and secondary false confessions. However, one cannot yet conclude that incentive is problematic in the interrogation of potential secondary confessors, because its effect on the rate of true secondary confessions is unknown. Although innocent people have been wrongfully convicted based upon false informant testimony evoked by an offer of incentive (Scheck et al., 2000; Warden, 2004), such as the testimony of the jailhouse informant in the Peterson case, this does not necessarily imply that providing an incentive for testimony is problematic. Incentive is often offered to cooperative witnesses in order to entice reluctant informants to come forward when they have incriminating evidence (Mazur, 2002). If the offer of incentives brings forward a greater number of reluctant accurate informants than inaccurate informants, its utility would be verified. Experiment 2 therefore compared the effect of incentive on the rate of true versus false secondary confessions.

In order to assess the validity of secondary confessions obtained, we modified the protocol slightly from Experiment 1. In Experiment 2 a confederate played the role of typist. After the computer apparently crashed the confederate provided either a (primary) confession or a denial. Thus a secondary confession would either confirm (in the case of a primary confession) or refute (in the case of a denial) the information provided by the typist (the alleged perpetrator). In actuality, because the confederate never hit the TAB key, any secondary confession obtained in this experiment would be a false confession. Our interest in this experiment, however, was not in the true state of affairs with respect to the computer crash, but the state of affairs with respect to the primary confession (or lack thereof) encountered by the participant. For the purposes of this experiment, then, we defined “false” secondary confessions as those resulting from the participant refuting the information provided by the alleged perpetrator. We defined “true” secondary confessions as those resulting from the participant confirming the information provided by the alleged perpetrator. Thus, if incentives entice reluctant informants, there should be an increase in the number of true (relative to false) secondary confessions. However, based upon previous research on primary confessions (Russano et al., 2005), we predicted that providing

Guilt Attribution

The degree to which the written confession showed evidence of attribution of guilt to the alleged offender was submitted to a 2 (Role) $\times$ 2 (Evidence) $\times$ 2 (Incentive) ANOVA, which revealed a main effect of role, $F (1, 125) = 7.48$, $p < .008$, $\eta^2 = .06$, a main effect of incentive, $F (1, 125) = 5.97$, $p < .02$, $\eta^2 = .05$, and an interaction of the two, $F (1, 125) = 7.52$, $p < .008$, $\eta^2 = .06$. As shown in Tables 1 and 2, (a) secondary confessors saw the perpetrator as less guilty of the alleged misdeed than primary confessors saw themselves; and (b) providing incentive decreased guilt attribution for primary confessors ($M_s = 5.0$ and $3.9$; $t(50) = 2.97$, $p < .005$, Cohen’s $d = 0.86$), but not for secondary confessors ($M_s = 4.9$ and $5.0$; $t(79) = 0.71$, $p > .71$). In other words, people indicated less responsibility for their own (but not another’s) alleged misdeed when given incentive for confessing. The incentive therefore influenced participants to implicate themselves by signing a confession statement, yet to write a confession that made themselves seem less guilty. We will return to the issue of confessions worded to deflect guilt attribution in the General Discussion. All other main effects and interactions were nonsignificant ($F s < 1, ps > .4$).

Excuses

The numbers of participants whose written confession did versus did not provide an excuse or justification for the alleged misdeed were submitted to hierarchical loglinear analysis. A single hierarchical loglinear analysis across both primary and secondary confessors revealed only a marginal interaction of role and false evidence ($\chi^2 = 2.90$, $p < .09$). False evidence increased excuse-making for primary confessors (37% vs. 23%), but decreased excuse-making for secondary confessors (27% vs. 37%; see Tables 1 and 2 for means by role condition). All other main effects and interactions were nonsignificant ($\chi^2 s < 1.2$, $ps > .2$).
an incentive will increase only false (but not true) secondary confessions.

**Method**

**Participants**

A total of 139 University of Arkansas students participated in this study in exchange for partial fulfillment of a course requirement. Each participant was randomly assigned to one of the eight different experimental conditions. The data from 10 participants were not used in the analyses due to the participants’ admission that someone told them that the computer was designed to crash or had told them the purpose of the study, leaving a total of 129 participants (56 males and 73 females).

**Procedure**

The procedure was identical to Experiment 1 with the following modifications. The first modification was that the typist in this study was a confederate. After the computer crashed, the confederate provided to the participant either a confession or a denial that they hit the TAB key. Because there were high rates of secondary confessions in Experiment 1, we opted to have the confederate provide only a weak confession (“I think I hit the TAB key”) to reduce the possibility of a ceiling effect in the confession condition, which would limit our ability to detect an effect of incentive. We opted to use a strong phrasing (“I did not hit the TAB key”) in the denial condition both to provide an unambiguous denial and to allow a powerful test of the effect of incentive. That is, if incentive increases the rate of secondary confessions even in the face of a strong denial, it suggests that caution is warranted in the use of incentivized testimony. Both experimenters and confederates were trained using the same procedure by which experimenters had been trained in Experiment 1. To ensure that experimenters would follow the script exactly, the confederate did not learn until immediately prior to each session whether he or she was to confess or to deny hitting the TAB key. This required the confederate to review his or her line (i.e., confession or denial) prior to each session.

The second modification was that the experimenter wrote the secondary confession statement (as in the original Kassin & Kiechel (1996) study) and asked the participant to sign it after the participant provided an admission of guilt. Third, instead of recording their thoughts about the computer crash on audiotape, participants wrote them on a separate piece of paper, whether or not they had signed a confession statement. Fourth, participants did not complete follow-up surveys. Finally, false evidence was not manipulated, as our focus was on the role of incentive. As in Experiment 1, all experimenters were told that their job was to elicit confessions; the experimenter with the highest rate of signed confessions was awarded a book as a prize.

**Design**

The study conformed to a 2 (Incentive: No incentive vs. incentive) × 2 (Confederate Reaction: Confession vs. denial) between participants design. The dependent measures were identical to Experiment 1—whether or not the reader signed the secondary confession, guilt attribution, and making excuses. Two independent raters coded notes with the participants’ thoughts for both guilt attribution and excuses. Inter-rater reliability was high for the coding of guilt attribution ($\kappa = .93$), so the scores were averaged for further analyses. Also, inter-rater reliability was high for the coding of excuses for the confederate (Cohen’s Kappa = .65, $p < .001$). Again, discrepancies were resolved using a third coder.

**Results and Discussion**

**Signed Confession Statements**

The numbers of participants who signed versus refused to sign secondary confession statements were submitted to hierarchical loglinear analysis. There was a main effect of confederate reaction ($\chi^2 (1) = 30.06, p < .001$), which obtained in both the no incentive condition ($\chi^2 (1) = 23.79, p < .001$) and in the incentive condition ($\chi^2 (1) = 8.10, p < .005$). This effect was qualified by an interaction with incentive ($\chi^2 (4) = 33.11, p < .001$). As shown in Table 3, hearing the alleged perpetrator confess to versus deny hitting the TAB key increased the rate of signing a statement, but incentive increased the rate of signing only after a denial and not after a confession (see Table 3 for pairwise comparisons). The main effect of incentive was nonsignificant ($\chi^2 = 1.22, p > .27$).

Finally, only 32 participants (38%) signed the secondary confession statement exactly as written by the experimenter. The remaining 62% of participants altered the written wording in a manner similar to participants in Experiment 1, to minimize the role of the alleged perpetrator in causing the computer crash; this rate did not differ across conditions.

**Number of Questions Required**

The number of pressuring interrogation questions needed to elicit a confession was submitted to a 2 (Incentive) × 2 (Confederate Reaction) ANOVA, which revealed a main effect of confederate reaction, $F (1, 80) = 32.41, p < .001$,
particularly in the no incentive condition (see Table 3 for primary confession inflated the rate of making excuses but effects were nonsignificant ($F = 2.29$). As shown in Table 3, hearing a primary confession reduced the number of questions necessary to obtain a secondary confession. That is, it was easier to elicit true than false secondary confessions. All other effects were nonsignificant ($F < 1, ps > .05$).

**Guilt Attribution**

The degree to which the participant’s thoughts about the experiment showed evidence of attribution of guilt was submitted to a $2 \times 2$ (Incentive) ANOVA, which revealed a main effect of confederate reaction. As shown in Table 3, hearing a primary confession caused participants to infer greater guilt in the alleged offender, $F(1, 125) = 56.56, p < .001, \eta^2 = .31$. Thus participants seemed to see the alleged offender’s confession as a statement of his or her guilt, as we had intended. All other effects were nonsignificant ($F < 1, ps > .4$).

**Excuses**

The numbers of participants whose thoughts about the experiment did versus did not provide an excuse or justification for the alleged misdeed of the offender were submitted to hierarchical loglinear analysis. There was a main effect of confederate reaction ($\chi^2(1) = 6.06, p < .01$), qualified by an interaction with incentive ($\chi^2(1) = 11.10, p < .01$). As shown in Table 3, hearing a primary confession inflated the rate of making excuses but particularly in the no incentive condition (see Table 3 for pairwise comparisons among conditions). In other words, participants saw alleged offenders who admitted to rather than denied guilt as having committed the misdeed unintentionally; the typist made an honest mistake rather than having malevolent intentions. But offering incentive for secondary confessions eliminated this “honest mistake” pattern. The main effect of incentive was nonsignificant ($\chi^2 = 0.02, p > .8$). In summary, Experiment 2 revealed two undesirable effects of incentive: An increased rate of false secondary confessions and a decreased tendency to see freely admitted misdeeds as unintentional.

### GENERAL DISCUSSION

The US justice system is dependent upon both admissions of guilt by offenders (primary confessions), and reports of guilt by informants (secondary confessions). The present research revealed that those who were present at the time of an alleged misdeed provided secondary confessions more often than those accused of the misdeed provided primary confessions. Unsurprisingly, it seems easier to implicate someone else rather than the self of wrongdoing. One reason for the difference in primary versus secondary confession rates is that the secondary confessors did not have personal responsibility for crashing the computer, and therefore would not be admitting guilt to the same degree as primary confessors.

A second reason for the difference is that the secondary confessor is similar to an observer in actor–observer bias research. In the actor–observer bias (Jones & Nisbett, 1971), actors tend to attribute their behavior to aspects of the situation, in part because actors know their own history of behavior and realize the strong influence of the situation upon it. However, an observer of the actor’s behavior is more likely to attribute the behavior to the actor’s personal attributes, in part because the observer is not privy to information about the actor’s behavioral history. Because the two participants were strangers, the secondary confessor could not know the accused participant’s history of good and careful behavior, and therefore would be more likely to assume the typist’s (compared to their own) guilt.

A third reason for the difference in primary versus secondary confessions is that secondary confessors were undoubtedly less certain about whether the alleged offender had indeed hit the forbidden key than the alleged offender herself was, introducing greater doubt that could be harnessed during the interrogation. Previous computer crash studies demonstrated that the more doubt and uncertainty during the event or the more likely the questioning will elicit a false primary confession (i.e., the difference between the fast and slow pace conditions in Kassin and Kiechel (1996); and the difference between the plausible WINDOWS key and implausible F12 key conditions in Horselenberg et al. (2006)), which could account for why there were more false secondary confessions than primary confessions.

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**Table 3** Effects of confederate reaction and incentive on secondary confessions in experiment 2

<table>
<thead>
<tr>
<th>Measure</th>
<th>Confederate reaction</th>
<th>Incentive condition</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>No incentive</td>
<td>Incentive</td>
</tr>
<tr>
<td>Signed statement</td>
<td>Confessed</td>
<td>90%a</td>
</tr>
<tr>
<td></td>
<td>Denied</td>
<td>34%b</td>
</tr>
<tr>
<td></td>
<td></td>
<td>87%a</td>
</tr>
<tr>
<td></td>
<td></td>
<td>55%c</td>
</tr>
<tr>
<td>Pressure needed</td>
<td>Confessed</td>
<td>3.1a (2.3)</td>
</tr>
<tr>
<td></td>
<td>Denied</td>
<td>6.4a (2.2)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>3.1a (2.4)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>6.2b (2.8)</td>
</tr>
<tr>
<td>Guilt attribution</td>
<td>Confessed</td>
<td>5.6a (1.0)</td>
</tr>
<tr>
<td></td>
<td>Denied</td>
<td>3.4a (1.6)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>5.3a (1.0)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>3.5b (1.9)</td>
</tr>
<tr>
<td>Provided excuse</td>
<td>Confessed</td>
<td>43%a</td>
</tr>
<tr>
<td></td>
<td>Denied</td>
<td>9%b</td>
</tr>
<tr>
<td></td>
<td></td>
<td>27%ab</td>
</tr>
<tr>
<td></td>
<td></td>
<td>24%ab</td>
</tr>
</tbody>
</table>

*Notes: Standard deviations are given in parentheses. Pressure ranges from 1 (agreed after 1 pressuring question) to 8 (agreed after 8 pressuring questions). Guilt attribution ranges from 1 (complete denial) to 7 (complete admission of guilt). Guilt attribution and excuses are present for all participants. Means within a measure not sharing a subscript differ at $p < .05$.*
The rate of primary and secondary confessions was also affected by the presence of false evidence and incentive to confess. Experiment 1 replicated the false evidence effect for primary confessions, in that false evidence increased the number of false primary confessions relative to no false evidence. Further, false evidence presented in conjunction with incentive increased the already-high rate of false secondary confessions to nearly 100%. Offering incentives for personal gain such as leniency have been shown to increase the rate of false primary confessions in previous studies (Russano et al., 2005), but incentive did not increase the primary confession rate in Experiment 1.

The larger impact of incentive upon secondary than primary confessors may be due to the altruistic nature of the incentive offered to primary confessors in Experiment 1: A primary confession would allow the other participant to go free and not have to come back for a second session. Thus, the incentive did not provide the primary confessor an opportunity for personal gain. Yet incentive did influence primary confessors in another way: It muted the false evidence effect, such that a relatively small number of pressuring interrogation questions were required to elicit confessions whether false evidence was provided or not. In other words, incentive did not affect the rate of false primary confessions, but did allow them to be equally easily elicited with or without false evidence.

Experiment 2 extended these results to show that offers of incentive actually reduce the truth value of secondary confessions. In Experiment 2, incentive only inflated the rate of false secondary confessions (when the confederate denied guilt) and did not have a significant effect on true secondary confessions (when the confederate admitted guilt). It is important to note that the results of Experiment 2 were not due to a ceiling effect among participants who had heard an admission of guilt. Further analyses showed that at no point in the interrogation process did incentive increase the number of true secondary confessions. Therefore, incentive only increased the number of false secondary confessions.

In both experiments participants who agreed to sign a confession statement either wrote out the statement themselves or could take the opportunity to write on a prewritten statement. The majority of participants (74% overall) who signed a statement changed the wording so as to minimize the amount of guilt or to provide an excuse for the forbidden act. Still, 26% of participants wrote or agreed to sign a confession statement that unambiguously attributed guilt to the accused, strikingly similar to the 28% of confessors in Kassin and Kiechel’s (1996) study whose descriptions of the computer crash suggested internalization of guilt.

Although participants’ insistence upon changing the wording of the confession statement might seem to provide only weak and ineffectual evidence against the accused, this problem would likely be resolved in an actual interrogation. Attempts at denial would be cut off and attention would be refocused at getting to the “truth” (Inbau et al., 2001). If a primary or secondary confessor tried to excuse the crime or minimize the involvement of the accused (particularly for a primary confessor), the interrogators would have the confessor revise the statement—advising the confessor to stick to the facts and avoid using opinions or excuses (Inbau et al., 2001). A preliminary minimized confession statement would therefore likely operate as a foot-in-the-door (Freedman & Fraser, 1966) that the interrogator could use to achieve a full and complete signed confession.

The present research tells a cautionary tale about using incentives in order to obtain secondary confessions. Prosecutors and legal scholars have argued that it is necessary to offer incentives to motivate reluctant witnesses to come forward with information (Cassidy, 2004; Martinez, 1999; Mazur, 2002). However, the results from Experiment 2 do not support this logic. The readers in the experiments were analogous to accomplices because they were present during the commission of the crime and faced the same punishment as the typists (i.e., coming back for a second session). Therefore, the incentive presented to the readers was equivalent to a “get-out-of-jail-free” card. At least for incentives of this magnitude, prosecutors, interrogators, judges and jurors should view secondary confession testimony with skepticism.

Unfortunately, in the case of jurors this necessary skepticism is not apparent. Mock jurors are more likely to convict when there is secondary confession evidence regardless of the presence of an incentive, even when that incentive is explicitly clear to the jurors (Neuschatz et al., 2008). In part as a consequence of jurors’ lack of skepticism of incentivized testimony, then, false informants are one of the leading causes of wrongful convictions (Scheck et al., 2000; Warden, 2004; http://innocenceproject.org). As previously mentioned, offering reduced sentences as an incentive is a long-standing tradition in the judicial system and does a great service by alleviating already overcrowded prisons. However, extreme incentives combined with pressuring interrogation tactics such as the ones used in the present studies may make the motivation to confess (primarily or secondarily) stronger than the motivation to tell only the truth. Therefore, the new proposed safeguards discussed in the introduction should be implemented to fully inform jurors of dubious nature of secondary confession evidence. In particular, we believe, like others (Gershman, 2002; Kassin, 2008) that interviews and interrogations of informants and suspects should be video recorded in order to allow jurors to better assess the validity of testimony. Pretrial hearings and expert
testimony may also be necessary to help jurors correct for the potentially biasing nature of incentive (and interrogation) on testimony. We hope the results of the present research can aid experts in their ability to testify on this topic.

One of the limitations of the present research is that only one form of incentive (full freedom) was offered. Future research should examine whether other forms of incentive for secondary confessions, such as a reduced sentence or monetary reward, might in fact increase the rate of true but not false secondary confessions. A second limitation of the present research was that the accused and the informant were strangers. In actual interrogations, informants usually know the accused. For example in the Peterson case, it was coworkers who were interrogated until they provided a secondary confession. The presence of a relationship between the accused and the informant might reduce the informant’s willingness to provide a true secondary confession, either due to concern for the accused (e.g., the accused is a friend or relative) or due to concern for one’s own safety (e.g., the accused has threatened the informant). In such cases, informants may be much more reluctant to come forward than they were in the present research, perhaps necessitating the offer of incentive. Future research should therefore manipulate the relationship between the two participants in the dyad to determine whether incentive may indeed be beneficial for overcoming reluctance to inform on a known other.

Several methodological limitations of both experiments should also be considered. In Experiment 1 the type of incentive provided to potential primary and secondary confessors differed. Potential secondary confessors were provided a selfish incentive of avoiding punishment, whereas potential primary confessors were provided an altruistic incentive of allowing the other participant to avoid punishment. There were two reasons for choosing to manipulate incentive in this manner. First, we wanted the wording of the incentive for both participants to be as similar as possible. There were two ways to accomplish this. The first was to tell potential primary confessors that their confession would allow them to go free but would require the other participant to come back. However, the widely used interrogation manual *Criminal Interrogations and Confessions* instructs interrogators to avoid making such statements because any obtained confession could be ruled as involuntary and therefore inadmissible in court (Inbau et al., 2001). The second option, which we chose, was to tell potential primary confessors that their confession would allow the other participant to go free but would require them to come back. Had we used a selfish rather than an altruistic incentive for the potential primary confessors, we would expect an even higher rate of confessions, as previous research has demonstrated relatively high rates of confession with self-serving incentives (Russano et al., 2005). Thus, we conclude that our decision to use an altruistic incentive in Experiment 1 most likely resulted in a weaker incentive effect than might otherwise be obtained.

A second methodological limitation was that in Experiment 2 all secondary confessions were in reality false confessions, because the confederate never actually committed the misdeed of which he or she was accused. As previously mentioned, our interest was in whether incentive would have a differential impact on potential secondary confessors who had heard a primary confession versus a denial. Because any primary confession heard by the participant was in fact deceptive, it may be premature to generalize the findings to real-world interrogations in which potential secondary confessors have actually witnessed a crime. Therefore, future research using actual misdeeds is necessary before we can conclude that incentive increases only false secondary confessions. The cheating paradigm used in Russano et al. (2005) would lend itself well to the investigation of true and false secondary as well as primary confessions.

A third methodological limitation was that the confederate in Experiment 2 provided either a weak primary confession or a strong denial. As previously mentioned, this asymmetry in the strength of denial versus confession was necessary to avoid ceiling and floor effects on the rate of confessions, which would obscure any effects of incentive. It is possible that the difference in strength (versus the content) of denials versus confessions caused incentive to increase the rate of secondary confessions after denials only. That is, participants may have been more susceptible to incentive after a strong rather than a weak statement by the confederate. Logically, however, there would seem to be greater opportunity for the effect of incentive in a situation with greater ambiguity—that is, in the case of a weak statement from the confederate. Yet it was in the situation of less ambiguity—a strong denial from the confederate—that incentive had a larger effect. Thus we conclude that the differential strength of the statement provided by the confederate in Experiment 2 worked against our hypothesis, suggesting that the biasing effect of incentive would be even greater had participants heard equally strong confessions as denials.

In the meantime, it is essential for jurors, prosecutors, and judges to be informed about the potentially biasing nature of incentives to confess. Rather than make the fundamental attribution error and assume that all confessions—primary or secondary—convey the actual guilt of the accused, we must consider the possibility that confessions might instead be a product of extensive interrogation, false evidence, or tempting incentives. Snitches may indeed lie, or come to believe a falsehood about another to
be the truth. Jurors must be able to consider this possibility as they make their verdicts.

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