

Lost Proof of Innocence: The Impact of Confessions on Alibi Witnesses

Stéphanie B. Marion
Ryerson University

Jeff Kukucka
Towson University

Carisa Collins and Saul M. Kassin
John Jay College of Criminal Justice

Tara M. Burke
Ryerson University

The present study investigated how alibi witnesses react in the face of an innocent suspect's confession. Under the pretext of a problem-solving study, a participant and confederate completed a series of tasks in the same testing room. The confederate was subsequently accused of stealing money from an adjacent office during the study session. After initially corroborating the innocent confederate's alibi that she never left the testing room, only 45% of participants maintained their support of that alibi once informed that the confederate had confessed (vs. 95% when participants believed the confederate had denied involvement). Even fewer (20%) maintained their corroboration when the experimenter insinuated that their support of the alibi might imply their complicity. The presence of a confession also decreased participants' confidence in the accuracy of the alibi and their belief in the confederate's innocence. These findings suggest that a police-induced confession can strip an innocent confessor of a vital source of exculpatory evidence. This effect may well explain the often-puzzling absence of exculpatory evidence in many cases involving wrongful conviction.

Keywords: alibi witness, false confessions, psychology and law, wrongful convictions

Since 1989, more than 1,400 wrongful convictions of innocent people have been exposed in the United States—either through DNA testing or via the discovery of other types of evidence. Two vexing questions have arisen from the study of wrongful convictions. The first concerns the question of what erroneous incriminating evidence was used to convict these innocent people. Of great relevance to psychological science, investigations into these cases have indicated that the predominant contributing factors are mistaken eyewitness identifications, flawed forensic science conclusions, coerced false confessions, and informants who were incentivized by prosecutors (Innocence Project, 2014; National Registry of Exonerations, 2014; for a review, see [Garrett, 2011](#)).

The second vexing question concerns the lack of persuasive evidence of innocence in these case files. People who are absent from the crime scene at a particular time often have alibis that are

corroborated by either physical evidence (e.g., surveillance video footage, time-stamped receipts) or witness evidence (e.g., from friends, family, strangers, or coworkers who vouch for their whereabouts). Certain types of corroborating evidence, however, are perceived as more credible than others (e.g., physical evidence vs. witness evidence, strangers vs. family members; e.g., [Dysart & Strange, 2012](#); [Hosch, Culhane, Jolly, Chavez, & Shaw, 2011](#); [Olson & Wells, 2004](#)). Thus, it is not surprising that many wrongfully convicted defendants had presented alibi witnesses at trial who were not sufficiently convincing to judges and juries. A more troublesome problem is that some wrongfully convicted defendants could not generate and validate accurate alibis for a specific time and place—and worse, that some alibi witnesses they had identified withdrew their initial support and did not appear at trial. This latter possibility is suggested by numerous cases in which an innocent defendant had confessed to police—and the confession corrupted other evidence ([Kassin, Bogart, & Kerner, 2012](#)).

Recent research has shown that a belief in a suspect's guilt can corrupt people's perceptions of whether degraded speech recordings betray incriminating remarks ([Lange, Thomas, Dana, & Dawes, 2011](#)), whether a facial composite resembles a suspect ([Charman, Gregory, & Carlucci, 2009](#)), whether a suspect's handwriting is similar to that appearing in a bank robbery note ([Kukucka & Kassin, 2014](#)), whether ambiguous polygraph charts indicate deception ([Elaad, Ginton, & Ben-Shakhar, 1994](#)), and even whether a suspect is included or excluded in a complex DNA mixture ([Dror & Hampikian, 2011](#)). Confessions can also influence eyewitness identifications. To demonstrate, [Hasel and Kassin \(2009\)](#) staged a theft and witnesses made identification decisions from a lineup that did not contain the culprit. Two days later, individual witnesses were told that the person they had identified

This article was published Online First August 24, 2015.

Stéphanie B. Marion, Department of Psychology, Ryerson University; Jeff Kukucka, Department of Psychology, Towson University; Carisa Collins and Saul M. Kassin, Department of Psychology, John Jay College of Criminal Justice; Tara M. Burke, Department of Psychology, Ryerson University.

Stéphanie B. Marion is now at the Faculty of Social Science and Humanities, University of Ontario Institute of Technology. Carisa Collins is now at the Faculty of Social Science and Humanities, University of Ontario Institute of Technology.

Correspondence concerning this article should be addressed to Stéphanie B. Marion, Faculty of Social Science and Humanities, University of Ontario Institute of Technology, Oshawa, ON, Canada, L1H 7K4. E-mail: stephanie.marion@uoit.ca

denied guilt during a subsequent interrogation, or that he confessed, or that a specific other lineup member confessed. Sixty-one percent of participants who had made an identification went on to change their selection, with confidence, when later told that another lineup member confessed. Among those who had initially made a correct nonidentification, 50% proceeded to select the confessor.

Although the aforementioned studies indicate that confessions can spawn other incriminating evidence, thereby creating an illusion of corroboration (Kassin, 2012), and although research also indicates that context can bias the judgments of forensic examiners (Kassin, Dror, & Kukucka, 2013; Saks, Risinger, Rosenthal, & Thompson, 2003), it is also possible that confessions can suppress exculpatory alibi evidence that could prevent wrongful convictions. At present, only anecdotal data are available on this point. In one case, DNA exoneree Barry Laughman was induced to confess to rape and murder. When two witnesses voluntarily approached police to insist that they had seen the victim alive after the alleged murder, the police sent them home, saying: "You must have seen a ghost." In a second case, DNA exoneree John Kogut named several alibi witnesses he was with on the night of the murder. Initially, the witnesses he named confirmed his whereabouts. These witnesses later withdrew their support, however, once informed that Kogut had confessed.

There are two bases for the expectation that confessions might suppress alibi evidence. First, research shows that confessions are powerfully incriminating—more so than other forms of evidence (Kassin & Neumann, 1997). Hence, studies have shown that mock jurors and judges do not adequately discount confessions—even when they are retracted and perceived to have been coerced (Kassin & Sukel, 1997; Wallace & Kassin, 2012). Most people believe they would never confess to a crime they did not commit, so they evaluate others accordingly and treat confessions with uncritical acceptance (Blandón-Gitlin, Sperry, & Leo, 2011; Henkel, Coffman, & Dailey, 2008; Leo & Liu, 2009). From a strictly cognitive standpoint, therefore, it stands to reason that prospective alibi witnesses might presume guilt from a suspect's confession and question their own perceptions and memories concerning that suspect's whereabouts at a particular time.

As suggested by the account of one alibi witness who was deterred in the DNA exoneration case of John Kogut, a second possibility is that prospective alibi witnesses may become motivated to distance themselves from a suspect who had confessed for self-serving reasons—so authorities would not suspect their complicity with the confessor. Across all areas of psychology, laboratory experiments have shown that people's perceptions are biased not only by expectations but by motivational influences as well (Balcetis & Dunning, 2006, 2010; Radel & Clement-Guillotin, 2012). The question is, would self-protective motives cause people to recant their accurate alibis even in the high-stakes situation of a criminal investigation, with consequences that are real?

By adapting a laboratory paradigm introduced by Marion and Burke (2013), the present research tested the hypothesis that confessions will lead people to recant rather than to reaffirm their alibis for an innocent person—and that this tendency would be exacerbated by an insinuation of complicity.

Method

Design and Overview

Under the pretext of a problem-solving study, a naïve participant and a confederate completed a series of individual and collaborative problem-solving tasks. At the end of an individual task, the experimenter informed them that a sum of cash had just been stolen from an adjacent office and asked if they had any knowledge of the alleged crime. The confederate provided a true alibi by stating that they had both been in the testing room when the cash was stolen. Participants were asked to corroborate this alibi both before and after being informed that the confederate had either denied (Denial condition) or confessed to (Confession condition) stealing the cash. In a third condition, participants were told that the confederate had confessed and that their continued corroboration of the alibi would imply their complicity in the theft (Implied Guilt condition). The current study thus utilized a single-factor between-groups design with three levels. The primary dependent variable was whether participants reaffirmed or recanted their support of the alibi.

Participants

Seventy-five college students participated for partial course credit. One participant was excluded because of her limited knowledge of the English language. Nine participants (12.16%) were later excluded because they expressed suspicions as to the true purpose of the experiment during the study itself (when they were told of the theft), or at debriefing when they were probed for suspicion. Thus, data from 65 participants (mean age of 20.45 ($SD = 5.53$); 86% female) were retained for analysis.

Procedure

Two students were present for each study session: a confederate (i.e., one of two female graduate students) and a naïve participant. Both were met by the experimenter in the laboratory waiting room and brought into a small office to sign consent forms. They were seated at a small, cluttered desk on which there was an open moneybox containing several \$20 bills. The confederate always sat on the side opposite the moneybox to ensure that the participant would sit in front of (and therefore notice) it. The experimenter explained that the goal of the study was to investigate how personality characteristics relate to how people perform on problem-solving tasks when working alone versus in pairs. Once the consent forms were signed, they were led to an adjacent testing room.

Dyadic task. The confederate and participant were seated at the same desk and given 10 minutes to work together on a series of anagram, probability, and logical reasoning problems. The experimenter was not present during this task and the testing room door was left open. The confederate was instructed to act aloof toward the participant, that is, to not initiate conversation, to provide one-word answers to questions, and to appear disinterested in the task at hand. This was done to avoid fostering feelings of liking toward the confederate, which we believed could impact rates of alibi corroboration. The two confederates acted this way with all participants and were blind to experimental condition.

Individual task. After 10 minutes of the dyadic task, the experimenter returned to the testing room and asked the confederate and participant to sit at one of two adjacent desks that were separated by a cubicle wall partition 5'6" high by 4' wide. The confederate always sat at the desk nearest the door, which forced the participant to sit at the desk on the other side of the divider, from which neither the confederate nor the door was visible (see Figure 1).

The experimenter explained that heart-rate measures would be obtained as the students worked on the next sets of problems, purportedly as part of a pilot study on the physiology of problem-solving. However, because the lab only had one heart rate monitor, which was connected to the participant's workstation, only he or she would provide this measure. A bogus monitor was attached to the participant's finger and 'turned on.' The experimenter explained that they would have 10 minutes to complete the next task, during which time she needed to run an errand on another floor, but would return before the 10 minutes were up. She then exited the testing room, leaving the door ajar.

The confederate always sat on the side of the room closest to the door, and did not leave the room. To make her presence more evident, the confederate made subtle noises (e.g., shuffling papers, clicking a pen, etc.) every 30 seconds for the first and last thirds of the 10-min duration. This was done in order to give the impression that it would have been difficult, yet not impossible, for the confederate to leave and return without the participant noticing. This methodology was also adopted to mimic cases in which a witness believes an alibi to be true despite not having confirmatory visual knowledge of a suspect's whereabouts during an entire alibi event (e.g., when two individuals attend the same party).

First alibi corroboration. Upon her return, the experimenter informed the pair that money had just been stolen from the adjacent office. In a concerned tone, she asked whether they had seen or heard anyone enter the office. The confederate responded "no," and provided an alibi by stating that they had both remained in the testing room during the task. The experimenter stated that she would have to contact the building security office. She separated the confederate and participant by escorting the confederate to a nearby testing room. The experimenter then returned to the participant, informed him or her that a heart rate measure had been recorded, proving that he or she had not left the testing room. At that point, she asked whether the confederate was also in the testing room for the entire duration of the task. The participant's

response was captured by a concealed audio-recording device, which provided our measure of alibi corroboration at Time 1. The participant was then asked to complete a filler questionnaire, which included, among other questions about their perceived level of difficulty and enjoyment of the dyadic task, a 5-point Likert scale measure of how much he or she liked the confederate. The participant completed this questionnaire while the experimenter left to further investigate the theft.

Second alibi corroboration. Ten minutes later, the experimenter returned to the testing room and told the participant that a security officer had asked her to complete an 'incident report form,' which she also needed the participant to read and sign to confirm that, as far as he or she was aware, its contents were accurate. The report consisted of a three-paragraph handwritten description of the theft, the confederate's alibi, and the participant's initial corroboration of the alibi.

The report also contained a description of either the confederate's denial of involvement in the theft (Denial condition), or of the confederate's confession (Confession and Implied Guilt conditions). In the latter conditions, it was reported that the confederate had confessed but then 'changed her mind,' refused to sign the incident report form, and was again claiming that she had not left the testing room. The experimenter read the report aloud to the participant, and in the Implied Guilt condition, the experimenter also told the participant that his or her corroboration of the alibi seemed suspicious given that the confederate had confessed. The experimenter stated that if the participant corroborated the confederate's alibi, it might suggest the two had 'made a deal,' insinuating that the participant was an accomplice. No specific or explicit consequence for corroborating the alibi was stated. If the participants asked what would happen to the confederate, the experimenter stated that the decision would be made by the security office once the investigation is complete.

Finally, participants in all conditions were once again asked whether they were sure that the confederate was in the testing room for the entire duration of the task, thus giving them the opportunity to reaffirm or retract their earlier corroboration of the alibi. The participant's response, which was again audio-recorded, provided our measure of alibi corroboration at Time 2. If the participant retracted their corroboration, the experimenter amended the report accordingly. All participants were then asked to print their name, sign, and provide a contact number under a section labeled 'Other involved individual' on the form. Participants were also asked to provide verbal ratings, on a 10-point Likert scale, of their confidence that the participant had remained in the testing room for the entire duration of the task (1 = *not at all confident*; 10 = *completely confident*), and how much they believed the confederate was guilty of the theft (1 = *belief in innocence*; 10 = *belief in guilt*). After answering these questions, participants were fully debriefed.

Dependent Measures

Participants' corroboration statements at Time 1 and Time 2 were surreptitiously audio-recorded and coded by the experimenter as well as by a second blind coder as having provided either no corroboration (when the participant reported not knowing whether or not the confederate had left the room; e.g., "I don't know," "I wasn't paying attention"), weak corroboration (when the partici-

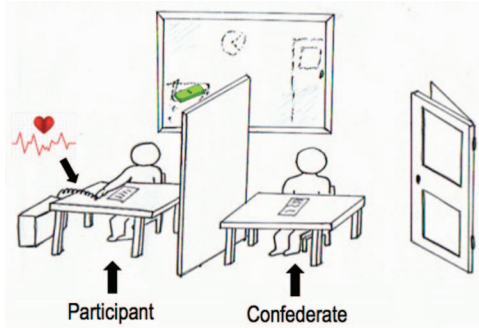


Figure 1. Experimental setup. See the online article for the color version of this figure.

participant reported that the confederate had remained in the room but used tentative language; e.g., “I think so,” “Yes, I think I would have heard her leave”), or strong corroboration (when the participant unequivocally reported that the confederate had remained in the room; e.g., “Yes, I heard her coughing,” “Yes, I’m sure”). The two coders exhibited 98.39% agreement ($p = .96$) at Time 1, and 84.13% agreement ($p = .89$) at Time 2.¹ Disagreements were resolved via discussion between coders. These data were later dichotomized to reflect whether or not the participant had corroborated the alibi, irrespective of strength. Five participants (8.33%) who did not corroborate the alibi at Time 1 were then excluded, leaving a final sample size of 60 ($n = 20$ in each experimental condition) for all analyses, unless otherwise noted.

At Time 2 only, participants were asked to verbally report how strongly they believed that (a) the confederate had not left the room, and (b) the confederate had stolen the money—each on a scale from 1 (*not at all*) to 10 (*definitely*). These confidence statements were also audiotaped and independently coded by two coders, who exhibited 93.10% agreement for each of these two items. Disagreements were again resolved via discussion, and stemmed from participants confusing the direction of the scale (e.g., for the confidence in the alibi question, “I’m a 9 – I don’t think she stole the money”, which should be coded as a “2”) and from participants who provided a verbal response instead of an end value of the scale (e.g., “I am absolutely, completely confident that she did not leave the room”). In addition, many participants were very hesitant to provide a rating at all. The experimenter prompted each participant up to two times for each question, yet some still refused to provide a quantitative answer. In total, 52 participants (86.67%) provided ratings of their confidence that the confederate had not left the room, and 46 (76.67%) provided ratings of their belief that the confederate had stolen the money.

Results

Before analyzing our results, we sought to verify that the likelihood of alibi corroboration did not depend on which of our two confederates was used. It did not: Within the full sample, the two confederates produced equivalent rates of corroboration at Time 1, $\chi^2(1) = 0.22, p = .642, OR = 1.56, 95\% CI [0.24, 10.10]$, and at Time 2, $\chi^2(1) = 0.01, p = .934, OR = 1.05, 95\% CI [0.37, 3.00]$, and thus we collapsed their data. In addition, and in alignment with the confederates’ instruction to act aloof and disinterested, participants rated our two confederates as moderately and equally likable ($M_s = 3.10$ and $3.31, SD_s = 0.73$ and 0.93), $t(58) = 0.94, p = .351, d = 0.27, 95\% CI [0.07, 0.47]$, and likability did not predict corroboration at Time 2, $\beta = 0.44, Wald's\ chi-square(1) = 1.63, p = .201, OR = 1.55, 95\% CI [0.79, 3.01]$.

Corroboration Statements

Across all conditions and irrespective of corroboration strength, 32 of the 60 participants (53.33%) who corroborated the alibi at Time 1 maintained their support at Time 2. A two-way chi-square test revealed a strong effect of our manipulation on the likelihood of corroboration at Time 2, $\chi^2(2) = 23.44, p < .001, Cramér's\ V = .63, 95\% CI [.46, .80]$ (see Figure 2). Pairwise χ^2 comparisons indicated that participants in the Denial condition were more likely to maintain their corroboration at Time 2 (95.00%) relative to

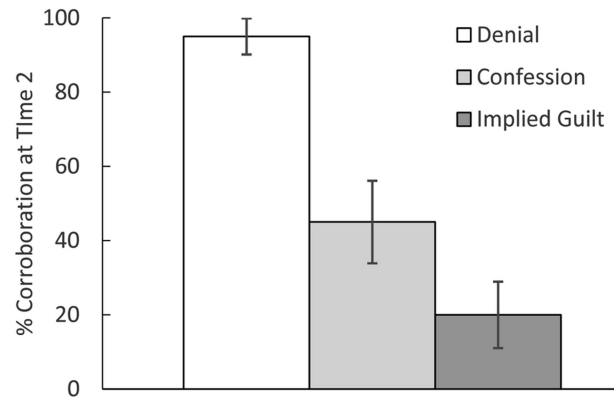


Figure 2. Percentage of Time 1 corroborators who reaffirmed their support of the alibi at Time 2, by experimental condition. Error bars represent $\pm 1 SE$.

those in the Confession (45.00%), $\chi^2(1) = 11.91, p = .001, OR = 23.22, 95\% CI [2.59, 208.59]$, and Implied Guilt (20.00%), $\chi^2(1) = 23.02, p < .001, OR = 76.00, 95\% CI [7.70, 750.75]$, conditions. Although the observed difference between the Confession and Implied Guilt conditions trended in the hypothesized direction, it did not achieve statistical significance, $\chi^2(1) = 2.85, p = .091, OR = 3.27, 95\% CI [0.80, 13.35]$.

Using only participants who gave a *strong* corroboration at Time 1 ($n = 50; 83.33\%$), we then tested corroboration strength at Time 2 as a function of our manipulation. Again, a strong overall effect emerged, $\chi^2(4) = 23.54, p < .001, V = .49, 95\% CI [.38, .63]$, such that the Denial condition differed from both the Confession, $\chi^2(2) = 11.15, p = .004, V = .58, 95\% CI [.34, .84]$, and Implied Guilt, $\chi^2(2) = 21.75, p < .001, V = .80, 95\% CI [.61, .96]$, conditions—which did not differ, $\chi^2(2) = 3.06, p = .217, V = .30, 95\% CI [.08, .62]$ (see Figure 3). Specifically, 76.47% of strong Time 1 corroborators in the Denial condition ($n = 17$) again provided a strong corroboration at Time 2, compared with only 25.00% and 5.88% of those in the Confession ($n = 16$) and Implied Guilt ($n = 17$) conditions, respectively. Conversely, only 5.88% of strong Time 1 corroborators in the Denial condition fully withdrew their corroboration at Time 2, compared to 56.25% in the Confession condition and 82.35% in the Implied Guilt condition.

Confidence Ratings

Across all conditions, participants were moderately confident that the confederate had not left the room ($M = 6.38, SD = 2.62$). A one-way ANOVA on these confidence ratings revealed a significant effect of our manipulation, $F(2, 49) = 7.28, p = .002, \eta_p^2 = .23$. Post hoc Bonferroni analyses indicated that participants in the Denial condition ($n = 19$) were more confident ($M = 8.00, SD = 1.80$) relative to those in the Confession ($n = 17; M = 5.62, SD = 2.36$) and Implied Guilt ($n = 16; M = 5.25, SD = 2.86$) conditions, which did not differ.

¹ Three of the Time 1 responses (4.62%) and two of the Time 2 responses (3.08%) were coded by only one individual because of a malfunction of the audio-recording device.

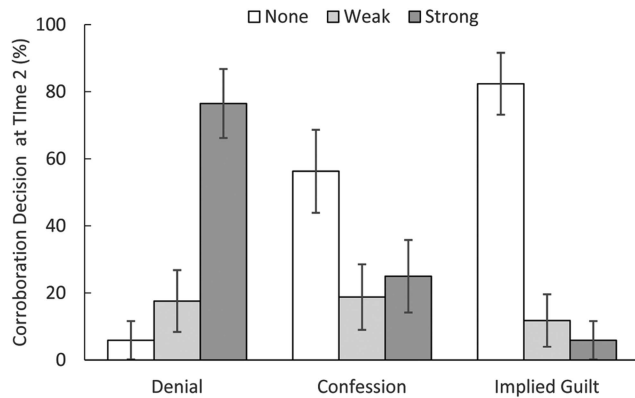


Figure 3. Percentage of strong Time 1 corroborators who withdrew their corroboration, provided a weak corroboration, and maintained a strong corroboration at Time 2, by experimental condition. Error bars represent $\pm 1 SE$.

Participants' belief that the confederate had stolen the money was generally low ($M = 3.38$, $SD = 2.43$). These ratings were marginally affected by our manipulation, $F(2, 43) = 3.22$, $p = .050$, $\eta_p^2 = .13$. Post hoc Bonferroni analyses found that those in the Denial condition ($n = 15$) were marginally less confident ($M = 2.13$, $SD = 1.55$) than those in the Confession ($n = 16$; $M = 3.97$, $SD = 2.43$) and Implied Guilt ($n = 16$; $M = 4.00$, $SD = 2.78$) conditions, which did not differ. This is unsurprising, given that confidence in the confederate's guilt was negatively correlated with confidence that she had not left the room, $r(40) = -.63$, $p < .001$.

Discussion

As illustrated by numerous recently discovered wrongful convictions, false confessions often trump factual innocence. Part of the problem is that confessions are powerfully persuasive as a matter of common sense, making it difficult for judges, juries, and others to discount a defendant's confession even when it was elicited in the presence of strong situational pressures. Conceptually, this phenomenon may be viewed as a manifestation of the *fundamental attribution error*, or *correspondence bias* (Kassin & Sukel, 1997; more generally, see Gilbert & Malone, 1995; Jones, 1990; Ross, 1977). A second and more pernicious problem, however, is that confessions—precisely because they are powerfully persuasive—can corrupt other evidence from lay witnesses and forensic examiners, producing an illusion of false support. This forensic confirmation bias has been demonstrated in laboratory experiments (Kassin et al., 2013) and also receives support in an analysis of DNA exoneration cases, where 78% of cases that involved a false confession also contained one or more other evidence errors—most of which were more likely to follow rather than precede the confession (Kassin et al., 2012).

Using a newly developed paradigm for studying alibi witnesses (Marion & Burke, 2013), the present study was designed to test the hypothesis that confessions can also trump innocence by suppressing exculpatory evidence. Our results strongly supported this hypothesis. Depending on the experimental condition, our participants were 10 to 16 times more likely to recant their support of the

innocent confederate's alibi after being informed that she had confessed—even though participants believed that she had immediately retracted that confession and even though they believed that their responses could carry real consequences (i.e., they believed their statement would be read by an investigating security officer who may contact them with further questions). Participants in the confession conditions also expressed less confidence in the accuracy of the alibi and reported more doubt about the confederate's innocence. Anecdotally, it is interesting that many participants in the confession conditions who were initially confident in the alibi went on to question what they had heard, reinterpreting in memory the “innocent” sounds made by the confederate during the experiment (e.g., moving on her chair) to suggest the possibility that she had left the room. We also tested the possibility of a second mechanism, a self-serving reason why a confession may influence alibi witnesses: That they fear being viewed as an accomplice. This possibility may present itself whenever an alibi witness vouches for someone who later confesses, indicating guilt. In the present study, we explicitly suggested this inference to participants in the Implied Guilt condition and found that they were somewhat more likely to recant their support of the alibi than those in the mere Confession condition. It is important to note that although this latter effect was in the predicted direction, it was not significant.

The implications of these results are important. Participants were in the room with the confederate who was accused of a stealing the experimenter's money—a serious and real crime. Most participants were almost certain she remained in the room through the entire session, thereby helping her to prove her innocence. Yet when told that the confederate had confessed—even though they were also told that she recanted that confession—the recanted confession caused a significant number of participants to withdraw their alibi support of her. In light of the fact that the confession was recanted and that the withdrawal of support had serious consequence, we believe that this result is not only disturbing but also profound.

The results of this study have considerable implications for criminal justice and the safety nets in place to prevent wrongful convictions. The courts in many states demand that confessions be voluntary and corroborated to gain admissibility at trial. As in research showing that confessions can spawn other incriminating evidence, artificially increasing perceptions of guilt, our finding that confessions can suppress exculpatory evidence raises a similar concern that confessions will more easily survive scrutiny when not contradicted by exculpatory information. The suppression of alibi witnesses is also relevant at the appellate level, where appeals courts may determine that even if a confession was coerced and erroneously admitted at trial, the conviction can stand if the error was “harmless” as measured by whether the sum total of the remaining evidence was sufficient to support a jury's conviction beyond a reasonable doubt.

There are some limitations to this study that should be considered. The participants were students enrolled at a criminal justice college, which could have impacted our results in two ways: First, it is possible that these students had more knowledge of the issues surrounding false confessions than the general population. They may have been more skeptical of the validity of confession evidence, and thus more likely to maintain their corroboration of the alibi, compared with populations from which potential false con-

fessors are more likely to originate. Second, this increased sensitivity to criminal justice issues, along with participants' likely general awareness of the college's research interests may explain the elevated rate of suspiciousness among our participants (12% were excluded because of their disbelief in the cover story). Future studies in this area should therefore ideally be conducted with different populations.

Another limitation may be the relative severity of our crime scenario. The mock crime we presented (the theft of a small amount of money) is relatively minor compared with, for example, known cases of wrongful convictions where the suspect is accused of murder or rape. Increases in crime severity and violence are accompanied by an increase in the severity of the consequences for both an accused person and the victim(s). These crime characteristics may impact the effects found in the current study. It is possible that serious and explicit consequences for the guilty judgment of a suspect may decrease the number of alibi retractions (i.e., as the severity or violent nature of the crime increases). At the same time, however, as the potential consequences for the victim(s) increase (e.g., trauma, loss of independence, death), so should witnesses' motivations to avoid making a mistake, which in turn could conceivably increase their confidence threshold necessary to corroborate an alibi. Thus, future research should investigate the impact of suspect, victim, and alibi witness consequences by manipulating these factors as well as obtaining measures of alibi witnesses' motivations for maintaining or withdrawing their support of a suspect's innocence.

We think several additional lines of research are also worthy of pursuit. First, it is clear that some alibi witnesses are inherently more credible than others—for example, as a function of their relationship to the defendant (Olson & Wells, 2004). In the present study, likability of the suspect was held constant at a low to moderate level. However, it is important to determine whether the confession effect that we have observed within this laboratory paradigm is moderated by naturally occurring (e.g., friends vs. strangers) or experimentally manipulated (e.g., likability) variations in that relationship.

A related implication concerns the possible effects on evidence in the form of character witnesses who would testify for the defense. Just as confessions can taint forensic examiners, eyewitnesses, and alibi witnesses, it is possible that confessions would similarly taint people's perceptions of the defendant's character. Anecdotal support for this hypothesis was apparent among some of our participants. When asked during the study whether they believed that the confederate stole the money, several participants in the Confession conditions made unflattering spontaneous remarks about the confederate (e.g., "She wasn't really friendly, kept to herself," "She seemed a little nervous," and "She's, like, weird . . . she has, no offense, but, like, a bitch attitude"). No such remarks were made in the Denial condition, despite the confederate behaving similarly and obtaining similar premanipulation measures of likability in all conditions. The possibility of a radical shift of opinion was illustrated in the wrongful conviction in Italy of Amanda Knox. Just before Knox was induced to confess to murder, she and one of her two Italian roommates, Filomena Romanelli, had spoken on the phone. Their conversation, which was recorded by police, revealed that Romanelli was friendly and empathic to Knox and that the two women were planning to look for another house so they could continue to live together. Yet

subsequent to Knox's confession, Romanelli's opinion of Knox had soured. Ultimately, she testified as a negative character witness for the prosecution.

Finally, it is important to note that a confession is not the only form of incriminating evidence with the potential to deter exculpatory alibi witnesses. Over the years the accumulation of DNA exoneration cases have shown that even more wrongful convictions had involved mistaken eyewitness identifications, often more than one per case (Garrett, 2011; Wells et al., 1998). Although mock jury research suggests that confessions are more potent than eyewitness identifications (Kassin & Neumann, 1997), research on eyewitness influences (e.g., Gabbert, Memon, & Allan, 2003; Skagerberg, 2007) suggests the possibility that exculpatory alibi witnesses would similarly be deterred by the competing memory of an eyewitness.

References

- Balcetis, E., & Dunning, D. (2006). See what you want to see: Motivational influences on visual perception. *Journal of Personality and Social Psychology, 91*, 612–625. <http://dx.doi.org/10.1037/0022-3514.91.4.612>
- Balcetis, E., & Dunning, D. (2010). Wishful seeing: More desired objects are seen as closer. *Psychological Science, 21*, 147–152. <http://dx.doi.org/10.1177/0956797609356283>
- Blandón-Gitlin, I., Sperry, K., & Leo, R. A. (2011). Jurors believe interrogation tactics are not likely to elicit false confessions: Will expert witness testimony inform them otherwise? *Psychology, Crime & Law, 17*, 239–260. <http://dx.doi.org/10.1080/10683160903113699>
- Charman, S. D., Gregory, A. H., & Carlucci, M. (2009). Exploring the diagnostic utility of facial composites: Beliefs of guilt can bias perceived similarity between composite and suspect. *Journal of Experimental Psychology: Applied, 15*, 76–90. <http://dx.doi.org/10.1037/a0014682>
- Dror, I. E., & Hampikian, G. (2011). Subjectivity and bias in forensic DNA mixture interpretation. *Science & Justice, 51*, 204–208. <http://dx.doi.org/10.1016/j.scijus.2011.08.004>
- Dysart, J. E., & Strange, D. (2012). Beliefs about alibis and alibi investigations: A survey of law enforcement. *Psychology, Crime & Law, 18*, 11–25. <http://dx.doi.org/10.1080/1068316X.2011.562867>
- Elaad, E., Ginton, A., & Ben-Shakhar, G. (1994). The effects of prior expectations and outcome knowledge on polygraph examiners' decisions. *Journal of Behavioral Decision Making, 7*, 279–292. <http://dx.doi.org/10.1002/bdm.3960070405>
- Faul, F., Erdfelder, E., Lang, A.-G., & Buchner, A. (2007). G*Power 3: A flexible statistical power analysis program for the social, behavioral, and biomedical sciences. *Behavior Research Methods, 39*, 175–191. <http://dx.doi.org/10.3758/BF03193146>
- Gabbert, F., Memon, A., & Allan, K. (2003). Memory conformity: Can eyewitnesses influence each other's memories for an event? *Applied Cognitive Psychology, 17*, 533–543. <http://dx.doi.org/10.1002/acp.885>
- Garrett, B. L. (2011). *Convicting the innocent: Where criminal prosecutions go wrong*. Cambridge, MA: Harvard University Press. <http://dx.doi.org/10.4159/harvard.9780674060982>
- Gilbert, D. T., & Malone, P. S. (1995). The correspondence bias. *Psychological Bulletin, 117*, 21–38. <http://dx.doi.org/10.1037/0033-2909.117.1.21>
- Hasel, L. E., & Kassin, S. M. (2009). On the presumption of evidentiary independence: Can confessions corrupt eyewitness identifications? *Psychological Science, 20*, 122–126. <http://dx.doi.org/10.1111/j.1467-9280.2008.02262.x>
- Henkel, L. A., Coffman, K. A. J., & Dailey, E. M. (2008). A survey of people's attitudes and beliefs about false confessions. *Behavioral Sciences & the Law, 26*, 555–584. <http://dx.doi.org/10.1002/bsl.826>

- Hosch, H. M., Culhane, S. E., Jolly, K. W., Chavez, R. M., & Shaw, L. H. (2011). Effects of an alibi witness's relationship to the defendant on mock jurors' judgments. *Law and Human Behavior, 35*, 127–142. <http://dx.doi.org/10.1007/s10979-010-9225-5>
- Jones, E. E. (1990). *Interpersonal perception*. New York, NY: WH Freeman/Times Books/Henry Holt & Co.
- Kassin, S. M. (2012). Why confessions trump innocence. *American Psychologist, 67*, 431–445. <http://dx.doi.org/10.1037/a0028212>
- Kassin, S. M., Bogart, D., & Kerner, J. (2012). Confessions that corrupt: Evidence from the DNA exoneration case files. *Psychological Science, 23*, 41–45. <http://dx.doi.org/10.1177/0956797611422918>
- Kassin, S. M., Dror, I., & Kukucka, J. (2013). The forensic confirmation bias: Problems, perspectives, and proposed solutions. *Journal of Applied Research in Memory & Cognition, 2*, 42–52. <http://dx.doi.org/10.1016/j.jarmac.2013.01.001>
- Kassin, S. M., & Neumann, K. (1997). On the power of confession evidence: An experimental test of the fundamental difference hypothesis. *Law and Human Behavior, 21*, 469–484. <http://dx.doi.org/10.1023/A:1024871622490>
- Kassin, S. M., & Sukel, H. (1997). Coerced confessions and the jury: An experimental test of the harmless error rule. *Law and Human Behavior, 21*, 27–46. <http://dx.doi.org/10.1023/A:1024814009769>
- Kukucka, J., & Kassin, S. M. (2014). Do confessions taint perceptions of handwriting evidence? An empirical test of the forensic confirmation bias. *Law and Human Behavior, 38*, 256–270. <http://dx.doi.org/10.1037/lhb0000066>
- Lange, N. D., Thomas, R. P., Dana, J., & Dawes, R. M. (2011). Contextual biases in the interpretation of auditory evidence. *Law and Human Behavior, 35*, 178–187. <http://dx.doi.org/10.1007/s10979-010-9226-4>
- Leo, R. A., & Liu, B. (2009). What do potential jurors know about police interrogation techniques and false confessions? *Behavioral Sciences & the Law, 27*, 381–399. <http://dx.doi.org/10.1002/bsl.872>
- Marion, S. B., & Burke, T. M. (2013). False alibi corroboration: Witnesses lie for suspects who seem innocent, whether they like them or not. *Law and Human Behavior, 37*, 136–143. <http://dx.doi.org/10.1037/lhb0000021>
- Olson, E. A., & Wells, G. L. (2004). What makes a good alibi? A proposed taxonomy. *Law and Human Behavior, 28*, 157–176. <http://dx.doi.org/10.1023/B:LAHU.0000022320.47112.d3>
- Radel, R., & Clément-Guillotin, C. (2012). Evidence of motivational influences in early visual perception: Hunger modulates conscious access. *Psychological Science, 23*, 232–234. <http://dx.doi.org/10.1177/0956797611427920>
- Ross, L. (1977). The intuitive psychologist and his shortcomings: Distortions in the attribution process. *Advances in Experimental Social Psychology, 10*, 173–220. [http://dx.doi.org/10.1016/S0065-2601\(08\)60357-3](http://dx.doi.org/10.1016/S0065-2601(08)60357-3)
- Saks, M. J., Risinger, D. M., Rosenthal, R., & Thompson, W. C. (2003). Context effects in forensic science: A review and application of the science of science to crime laboratory practice in the United States. *Science & Justice, 43*, 77–90. [http://dx.doi.org/10.1016/S1355-0306\(03\)71747-X](http://dx.doi.org/10.1016/S1355-0306(03)71747-X)
- Skagerberg, E. M. (2007). Co-witness feedback in line-ups. *Applied Cognitive Psychology, 21*, 489–497. <http://dx.doi.org/10.1002/acp.1285>
- Wallace, D. B., & Kassin, S. M. (2012). Harmless error analysis: How do judges respond to confession errors? *Law and Human Behavior, 36*, 151–157. <http://dx.doi.org/10.1037/h0093975>
- Wells, G. L., Small, M., Penrod, S., Malpass, R. S., Fulero, S. M., & Brimacombe, C. A. E. (1998). Eyewitness identification procedures: Recommendations for lineups and photospreads. *Law and Human Behavior, 22*, 603–647. <http://dx.doi.org/10.1023/A:1025750605807>

Received April 17, 2015

Revision received July 15, 2015

Accepted July 31, 2015 ■