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Retroactive memory interference: A potential countermeasure technique against psychophysiological knowledge detection methods

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ABSTRACT

The concealed information test is designed to detect concealed knowledge through differential physiological responses elicited by the concealed items. This study was designed to examine the role of retroactive interference (RI) as a potential countermeasure that may weaken memory traces of the concealed items and attenuate the physiological responses elicited by them. A total of 120 participants committed a mock crime and were randomly assigned to either an interference condition, where they learned and retrieved an alternative mock crime, or a control condition. Further, each group was randomly assigned to one of three "time-delay" conditions. The results revealed that both memory for the mock crime details and the skin conductance responses (SCRs) to these details were attenuated under the memory-interference condition. Time of testing affected recall, but had no effect on the SCRs. In addition, the memory-interference manipulation had no effect on the respiration measure. Theoretical and practical implications of these results are discussed.

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Research interest in psychophysiological detection of deception has significantly increased since the September 11 terror attack in the USA. In particular, the concealed information test (CIT), designed to detect memory traces that can connect suspects to a certain crime, has been extensively studied (Ben-Shakhar, 2012; Rosenfeld, Ben-Shakhar, & Ganis, 2012; Verschuere, Ben-Shakhar, & Meijer, 2011). The CIT is not a deception test, but rather aims to detect whether an examinee possesses certain crime-related information (see Verschuere et al., 2011). It can be used as an aid to law enforcement agencies because possessing crime-related information may imply involvement in the crime, while lack of such knowledge may exonerate a suspect. In the CIT, examinees are presented with a series of multiple-choice questions, each having one relevant (correct) alternative, e.g., a feature of the crime under investigation, and several neutral, control (incorrect) alternatives, chosen so that an innocent suspect would not be able to discriminate them from the relevant alternative. However, for guilty suspects who are familiar with the crime details and are able to discriminate them from the neutral items, these crime-related items are of great significance and are consequently expected to elicit enhanced physiological reactions (Lykken, 1974; Verschuere & Ben-Shakhar, 2011).

The validity of the CIT has been examined extensively under experimental laboratory conditions since the 1950s (e.g., Gustafson & Orne, 1963, 1965; Lykken, 1959, 1960; Kugelmass & Lieblich, 1966). More recently it has been subjected to several meta-analytic studies (Ben-Shakhar & Elaad, 2003; MacLaren, 2001; Meijer, Klein-Selle, Elber, & Ben-Shakhar, 2014) that revealed impressive effect size estimates. For example, Meijer et al. (2014) covered 100 laboratory studies, which used two CIT paradigms (for detection of self-referring personal information, as well as mock crime information) and utilized three ANS measures (skin conductance response—SCR, respiration line length—RLL, and heart rate—HR) as well as the P300 component of the event-related potential. The reported overall averages of Cohen's *d* effect size (Cohen, 1988), reflecting the differentiation between guilty and innocent examinees, were 1.55, 1.11, 0.89, and 1.89 for these four measures, respectively. It should be noted that although the four measures differed significantly in their effect size, even the HR that was the least effective measure yielded a large effect size.

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However, as mentioned above, these impressive validity estimates were based on laboratory experiments and it is yet unclear whether their results would generalize to realistic applications of the CIT (see Carmel, Dayan, Naveh, Raveh, & Ben-Shakhar, 2003). For example, the bulk of CIT research used the mock crime paradigm, where subjects assigned to the "guilty" condition are instructed to steal something (e.g., an envelope containing money and jewelry). Typically, the experiments are designed to optimize memory of the critical items. Thus, for instance, the CIT is conducted immediately after completion of the mock crime, and often memory of the critical items is verified before the administration of the CIT. Clearly, in reality, the situation is very different and culprits do not necessarily pay attention to all of the crime scene details. Furthermore, the test is usually delayed and may be administered weeks or months after the event. Thus, memory of the critical items, which is clearly crucial for a successful CIT, may be compromised in realistic setups but not in laboratory studies. Indeed, several studies that have examined the role of memory for critical items on the CIT's outcomes (Carmel et al., 2003; Gamer, Kosiol, & Vossel, 2010; Nahari & Ben-Shakhar, 2011) revealed that when the CIT is administered 1 or 2 weeks after the mock crime, certain critical items are not recalled and do not elicit differential responses. However, consistent with memory research (e.g., Kensinger, Garoff-Eaton, & Schacter, 2007; Loftus, 1979; Safer, Christianson, Autry, & Osterlund, 1998), memory loss occurs mostly with 'peripheral' items (features that are not directly related to the execution of the crime, such as a picture on the wall of the crime scene). 'Central' features, such as type of weapon used or the stolen item, are typically recalled and are capable of eliciting large responses even when the test is delayed (e.g., Hu & Rosenfeld, 2012).

Various additional factors may differentiate the artificial laboratory conditions from those characterizing realistic lie-detection situations. One of these factors, which is the focus of the present study, is the possibility that guilty suspects try to avoid detection by using countermeasures to distort their physiological responses. The effects of countermeasures on the outcomes of the CIT have been studied with both ANS measures (e.g., Ben-Shakhar & Dolev, 1996; Honts, Devitt, Winbush, & Kircher, 1996) and ERPs (Mertens & Allen, 2008; Rosenfeld, Soskins, Bosh, & Ryan, 2004). Most of these studies demonstrated large effects, showing a dramatic increase in false-negative outcomes when examinees performed countermeasures (for a review see Ben-Shakhar, 2011). One exception to this finding is a new CIT protocol, called the *Complex Trial Protocol*, which was proposed by Rosenfeld et al. (2008) and has been repeatedly demonstrated by Rosenfeld and his colleagues to be relatively resistant to countermeasures in CIT based on the P300 component (Meixner & Rosenfeld, 2010; Rosenfeld & Labkovsky, 2010; Winograd & Rosenfeld, 2011).

The bulk of countermeasures studies relied either on physical countermeasures (e.g., subjects can bite their tongue to inflict pain when the control items are presented) or on mental means (e.g., recalling exciting and emotional memories during presentation of control items). Both types of countermeasures involve specific actions taken during the CIT in an attempt to increase responses to the neutral items and reduce CIT effectiveness. In this study, we examined a completely different type of potential mental countermeasure that involves an attempt to learn, *prior* to the CIT, self-generated false information in order to interfere with memory of the *crime-related* details. As mentioned earlier, the CIT is based on increased physiological responses to items that match existing memory traces (e.g., of the committed crime), and therefore its success depends on the strength of these memory traces. Several studies have shown a positive correlation between explicit memory performance for crime-related information and CIT detection efficiency (Ben-Shakhar, Gronau, & Elaad, 1999; Carmel et al., 2003; Iacono, Boisvenu, & Fleming, 1984; Verschuere, Crombez, Koster, Van Bockstaele, & De Clercq, 2007; Waid, Orne, Cook, & Orne, 1978; Waid, Orne, & Orne, 1981). Accordingly, any technique that interferes with memory of the critical items may reduce the sensitivity of the CIT in differentiating between critical and neutral (control) items.

In the present study we examined, for the first time, the effects of a memory-interference technique, in which a culprit deliberately memorizes post-event information in order to weaken and perhaps eliminate original memory traces of a committed crime. Ample evidence has documented the detrimental effects of post-event interfering information on memory for an event or a crime scene. Classic studies investigating retroactive interference (RI) have shown that learning new material after an encoding phase hinders memory for the initially encoded items (e.g., Barnes & Underwood, 1959; Postman & Underwood, 1973; Underwood, 1948a,b). Interference is particularly robust when the experimental design involves competition among old and new items sharing the same retrieval cue (see, e.g., Anderson & Neely, 1996). Several key studies have investigated retroactive interference effects in the context of eye-witness memory and/or memory distortion induced by late exposure to misinformation. Loftus and colleagues, for instance, have repeatedly shown that exposing participants to post-event information, either through leading interrogative questions, false information, or false imagination, contaminates participants' memory for a witnessed event (e.g., Loftus, 1979, 1996; Loftus & Hoffman, 1989; Nourkova, Bernstein, & Loftus, 2004; Thomas & Loftus, 2002; see Frenda, Nichols, & Loftus, 2011, for a recent review of the misinformation effect).

While the bulk of studies examining the constructive nature of memory have stressed the unreliability of memory and its vulnerability to external interference manipulations (see, e.g., Allen & Mertens, 2009, for false memory findings in the context of the CIT), the present study aims to investigate whether post-event interference may in fact serve as a deliberate, self-initiated mental technique for distorting and/or weakening memory traces for a crime. Participants in the current research committed a mock crime (i.e., a theft) and then were instructed to learn a hypothetical alternative crime scenario. In order to optimize memory interference as a countermeasure technique, the hypothetical crime scenario was constructed of items from the same categories of the actual mock crime. Subsequently, the participants performed a CIT and were tested for knowledge of the original crime details. We hypothesized that the competing information of the alternative crime scenario (e.g., a specific amount of money or a type of jewelry stolen) would weaken memory traces for the details of the original mock crime performed by the subjects. Accordingly, detection efficiency of the crime-relevant information in the CIT would be reduced, compared to detection efficiency among members of a control group who were not exposed to the alternative crime scenario.

Furthermore, we aimed to test the interaction between the retroactive interference manipulation and the natural decay of memory traces occurring with the passage of time. We therefore added a temporal factor potentially affecting memory performance, by manipulating the time delay between the initial mock crime, the RI manipulation, and the CIT. This time-delay manipulation resulted in three different "time delay" conditions: (a) Participants committed a mock crime, immediately learned an alternative crime scenario (or performed a neutral-control task) and then immediately took the CIT; (b) participants committed a mock crime, immediately learned an alternative crime scenario (or performed a neutral-control task), but took the CIT a week later; (c) participants committed a mock crime and were invited to the laboratory a week later, during which time they learned an alternative crime scenario (or performed a neutral-control task) and then immediately took the CIT. Note that subsequent to the CIT, participants in all conditions were immediately tested for their memory of the crime details.

It was hypothesized that a delayed CIT (i.e., a CIT conducted a week after the mock crime) would be less efficient than an immediate one, due to a general decay of memory for crime details over time, in accord with findings from previous studies (Carmel et al., 2003; Gamer et al., 2010; Nahari & Ben-Shakhar, 2011, but see Hu and Rosenfeld, 2012). Namely, we hypothesized that the immediate condition would yield better CIT outcomes than the two delayed conditions. In addition, we were interested in examining whether the RI effect in these two delayed conditions is affected by the timing of the manipulation and the timing of the CIT (i.e., whether the RI variable and the timing variable interacted). Such an interaction could be driven by two factors: (1) the delay between the mock crime and the RI manipulation and (2) the delay between the RI manipulation and the CIT/memory test. The first factor potentially reflects the efficiency of the interference manipulation when conducted long after the initial encoding phase, while the second factor mainly reflects the degree to which the interference manipulation persists across long time lags and affects memory performance at test.

Notably, only one previous CIT study reported by Bergström, Anderson, Buda, Simons and Richardson-Klavehn (2013)applied a countermeasure method based on a deliberate manipulation of memory for the critical items. Specifically, these authors applied retrieval suppression instructions (e.g., Anderson & Green, 2001), in which participants were asked to voluntarily suppress memories of a simulated crime. The authors demonstrated a significant reduction in P300-based CIT detection efficiency under such instructions as compared with a standard CIT condition. Namely, when people were motivated to suppress crime details, their memory-related ERP effects were significantly decreased, allowing guilty individuals to evade detection. Here, we also used a countermeasure technique designed to impede memory performance, but rather than using retrieval suppression instructions, our research is a first attempt to examine the effect of a retroactive interference (RI) manipulation on the outcomes of the CIT. CIT outcomes were measured with the two most efficient ANS measures, SCR and RLL (see Gamer, 2011; Meijer et al., 2014).

2. Methods

2.1. Participants

A total of 142 undergraduate students from the Hebrew University of Jerusalem participated in the experiment for course credit or payment. All participants signed a consent form indicating that participation was voluntary and that they could withdraw from the experiment at any time without penalty. The participants were randomly assigned to six experimental conditions (see below). Between one third and one half of the participants in each condition received monetary reward and the rest of the participants were students who took part in the research for course credit. Twenty two participants were eliminated due to technical problems or subject-related artifacts (e.g., excessive movements and/or electrodermal non-responders).[1] These participants were excluded from the entire experiment and consequently the final sample included 120 participants (75 females and 45 males). Their age ranged between 18 and 34 years, with a mean of 23.38 (SD = 2.5) years.

2.2. Apparatus

Skin conductance was measured by a constant voltage system (0.5 V Atlas Researches, Hod Hasharon, Israel), using two Ag/AgCl electrodes (0.8-cm diameter) with a 0.05 M NaCl electrolyte conductive gel (TD-246, Discount Disposables, St. Albans, Vermont). Respiration was recorded by a pneumatic tube positioned around the thoracic area. The continuous skin conductance and respiration signals were sampled using an A/D converter (NB-MIO-16) with a sampling rate of 50 Hz. The experiment was conducted in an air-conditioned chamber, and a PC computer (Pentium 4 processor, 3.0 GHz) was used to control the stimulus presentation and to compute skin conductance and respiration volume. The stimuli were displayed on the computer monitor.

Table 1
Profiles of items used in the experiment.

Profile	Crime items				
	Sum of dollars	Article number	Jewel	Name on the door	Envelope color
Buffer	40	14	Pendant	Moshe Aloni	Blue
A	15	27	Earrings	Abraham Koren	Green
B	10	36	Ring	Sara Levin	Yellow
C	25	19	Necklace	David Yorman	Orange
D	20	22	Watch	Rivka Simchon	Red
E	30	31	Bracelet	Shulamit Chen	Purple
F	35	33	Brochette	Noa Bilu	Brown

2.3. Design

The experiment was constructed in a 2 × 3 between-subjects design with the following two factors: (1) interference condition (RI condition, in which participants were exposed to an alternative mock-crime scenario vs. a control condition where participants were engaged in a neutral task); and (2) time delay (both the alternative task and the CIT were administered immediately after mock crime; alternative task executed immediately, but CIT delayed; both alternative task and CIT delayed). As mentioned above, participants were randomly allocated to the six conditions created by this design, with approximately equal numbers in each condition (i.e., there were 20 participants in each of four conditions and 19 and 21 participants in the other two conditions).

2.4. Procedure

2.4.1. Stage 1: Mock crime

Participants arrived at the laboratory and were met by Experimenter A, who read to them aloud the instructions appropriate for their particular condition. All participants were instructed to go to an office of a staff member and retrieve a particular numbered article. They have been told that if the staff member is not in his/her office, they should open the office using a key that was handed to them in advance, enter the room and find the particular article in a pile of numbered articles, placed on the desk. In addition, they were requested to take advantage of the situation and find a colored envelope containing money and a piece of jewelry, to count the money, and steal it together with the jewelry. While leaving the emptied envelope in a drawer of the office table, the stolen items were to be hidden after the theft in a mailbox that was pointed out to them. Note that the staff member's name and the number of the article (but not other crime-relevant items) were mentioned several times while reading aloud the instructions to the participants.

Subsequently, participants were instructed to return to the laboratory and hand over the requested article and the key to the experimenter. They were told to act naturally and try not to be observed by other staff members working in nearby offices while conducting their task. Actually, the department member was never in the office, and thus all participants were able to steal the envelope. Upon arrival to the designated office, participants found on the desk a pile of numbered articles, with a newspaper next to it. They searched for the requested article and looked for the envelope in the room. The envelope was located in the second drawer of a cabinet. It was a colored envelope, which was open and contained U.S dollar bills and a jewel. After checking its contents, the participants stole the money and jewel, hid them in the mail box, and returned to the lab with the requested article.

A total of seven profiles of items were used in this experiment. Each profile was composed of five items from the five categories that were used to generate the CIT questions (color of the envelope, name on the door, type of jewel, sum of money, and the article's number). The profiles are described in Table 1. Two profiles, the buffer profile and profile F, were used only in the interrogation phase of the experiment and were never used as a relevant profile in the mock-crime phase. One of the other five profiles of items (A–E) was randomly chosen as the relevant profile for each participant, such that each profile served as the relevant profile for 20% of the participants in each condition.

2.4.2. Stage 2: RI/control task

After completion of the mock crime, participants were given further instructions according to their specific experimental condition. Participants assigned to the experimental (RI) condition were instructed to learn an alternative mock crime scenario, which consisted of a profile of five critical items, taken from the same categories as those used for the real mock crime. This alternative profile was chosen

[1] It is not possible to specify how many participants were excluded for each reason because we initially excluded questions, rather than participants. At a first stage, a question was excluded if the SCRs to the various items were less than 0.01 μS, if the response to the critical item was accompanied by excessive movements or if more than two responses to neutral items were disqualified due to excessive movement. Then, at a second stage, a participant was excluded if at least four out of the 10 questions in the CIT were disqualified.

randomly from a set of four profiles, after excluding the profile chosen earlier for the real mock crime. Each of the five profiles (A–E) served as the alternative profile for 20% of the participants. For example, 20% of the participants committed a mock crime with the five critical items of profile A (see Table 1). For these participants one of the four profiles (B–E) was randomly chosen as the alternative mock crime profile. The items of the alternative mock-crime scenario were read to the participants by Experimenter A, who explained that learning these items will help the participant to appear innocent of committing the mock crime and winning a bonus. The items of the alternative profile, embedded within a hypothetical crime scenario, were read twice to the participants who sat with eyes closed, and were asked to mentally rehearse them once more. Participants were then asked to recall the items of the alternative profile. Participants who could not recall an item in the rehearsed profile reheard it again and retook the recall test. Participants assigned to the control condition were instructed to complete a sudoku puzzle for about the same duration a standard memorizing task would take (approximately 6 min). One third of the participants in both the experimental and the control condition learned the alternative scenario, or completed the sudoku puzzle immediately and then returned to the laboratory where the CIT was administered to them ('immediate' condition). One third of the participants completed these tasks immediately but were told to return after a week to take the CIT ('delayed CIT' condition). Finally, one third of the participants were asked to return to the laboratory after a week and only then they either learned the alternative scenario or completed the sudoku puzzle. Subsequently, they took the CIT ('delayed task and CIT' condition).

2.4.3. Stage 3: CIT and memory test

The CIT was conducted by Experimenter B who had no knowledge of the experimental condition of the participant, or of the critical crime-relevant profile. The experimenter informed the participants that a theft was committed in the psychology department, and that they are suspects in committing this theft. He/she explained that the experiment was designed to test whether they are guilty or not. A bonus of 10 New Israeli Shekels (about $3) was promised for a successful performance of the task (i.e., being classified as innocents). Subsequently, two electrodes were attached to the volar side of the index and fourth fingers of the participant's left hand and a pneumatic tube was attached to the thoracic area. The CIT questions were presented after an initial rest period of 2 min, during which skin conductance baseline was recorded. All examinees were presented with five questions, each targeting a different relevant detail of the mock crime (the envelope's color, the name of the staff member written on the office door, the type of jewel, the number of the requested article, and the sum of money in the envelope). The five questions appeared twice in two separate blocks. Each question was simultaneously presented on the computer monitor for 10 s and heard through the computer speakers (e.g., "What was the color of the stolen envelope?"). The question was followed by a buffer item, designed to absorb the initial orienting response and a set of five items (the relevant item and four neutral control items). Each item was visually presented for 5 s. The inter-stimulus interval (blank screen) ranged randomly from 8 to 12 s with a mean of 10 s. Participants were asked to respond verbally, saying "no" to every item. The order of the five items within each question was determined randomly. As mentioned above, after presentation of the five questions in the first block, all questions were repeated in a second block. The order of the questions within each block was randomly determined, with the constraint that the last question in the first block was never the first question in the second block. A short, participant-terminated break was given after presentation of the first block of five questions.

Importantly, the items of the alternative scenario (learned by the participants of the RI condition) never appeared among the test items in the CIT and they were replaced by the five items of profile F. For example, participants in the RI condition who committed a mock crime involving the five items of profile A, and who later learned an alternative crime scenario involving the five items of profile B, were tested during the CIT on the items of profile A (the relevant items) and the items of profiles C, D, E, and F (as the neutral items). For these participants, the items of profile B (the alternative crime scenario) were replaced in the CIT by the items of profile F. The reason for excluding the alternative scenario items from the CIT was twofold. First, from an applied perspective, while the existence of scenario items among the neutral items could potentially protect suspects from guilt detection (as the former items possess significant value and could elicit enhanced physiological responses, thus reducing guilt detection efficiency), one cannot assure that these would indeed appear in the CIT. Thus, we have adopted a conservative approach in excluding the alternative scenario's items from the CIT, which implies that the observed effects of our manipulation on the physiological measures may serve as a lower bound for the effect that may be obtained when the learned items are included among the control items. Second, from a theoretical perspective, rather than examining the RI effect on the responses to the neutral items, we wished to examine its effect on the responses to the crime-relevant items. That is, we sought to assess the extent to which learning and retrieving an alternative scenario would weaken memory representation of the crime-relevant details and, consequently, reduce guilt detection in the CIT.

Upon completion of the CIT, the electrodes and the pneumatic tube were detached from the participants. The experimenter thanked the participants and asked them to wait until the computer program processed the data and reached a decision as to whether they were found "guilty" or "innocent". The processing took 30 s and subsequently two memory tests were administered to examine whether participants remembered the relevant items of the mock crime. Prior to the administration of the memory tests, participants were told that the detection phase of the experiment was over and that now they should do their best to correctly recall the relevant items. They were further told that if they were unsure about the correct answer they should guess it. The first test was a recall memory test consisting of the five questions used in the CIT (e.g., what was the color of the envelope?). The second test was a recognition memory test in which participants were requested to choose the correct alternative among six simultaneously presented answers on the computer screen. In addition to testing memory for the real mock-crime scenario, participants in the experimental (RI) conditions were tested about the alternative scenario they had learned, using both recall and recognition memory tests. One participant in the experimental condition was not tested about the alternative scenario due to technical difficulties. Finally, all participants were debriefed and compensated for participation in the study.

3. Scoring of the dependent measures

3.1. SCR

Responses were transmitted in real time to the computer. SCR was defined as the maximal increase in conductance obtained from the examinee, from 1 to 5 s after stimulus onset. Few responses (less than 3%) were omitted when participants made an excessive movement during stimulus presentation. To eliminate individual differences in responsivity and permit meaningful comparisons of the responses of different examinees, each participant's SCRs were transformed into within-examinee standard scores (Ben-Shakhar, 1985). To minimize habituation effects, within-block standard scores were used, after exclusion of the buffer items and outlier responses (see, Ben-Shakhar & Elaad, 2002; Elaad & Ben-Shakhar, 1997). Finally, a detection score was computed for each participant by averaging the standardized SCRs elicited by the 10 presentations of the 5 critical items, across blocks.

3.2. Respiration

The respiration responses were defined on the basis of the total respiration line length (RLL) during the 13-s interval following stimulus onset. Timm (1982) noted that the computation of the RLL from the curvilinear respiration pattern might be disproportionally affected by the starting point of measurement. For example, starting from a point in the rapidly ascending inspiration curve, and from a point at the end of the expiration curve, where changes are relatively slow, would produce different RLLs for equal time intervals. To deal with this problem, we followed the procedure used by Elaad, Ginton. and Jungman (1992) and defined each response as the mean of ten length measures (0.1 s after stimulus onset through 13.1 s after stimulus onset, 0.2 s through 13.2 s after stimulus onset, etc.). In other words, ten 13-s windows were created, each beginning 0.1 s later than the previous window, and the RLL was defined as the mean of the ten lengths computed for the ten windows. Prior to calculation of the RLL, the respiration signal was down sampled to 20 Hz. Similar standardization transformation was applied for the RLL as the one described above in relation to the electrodermal measure. However, since guilty knowledge is reflected by smaller rather than larger RLLs, the RLL Z scores were multiplied by -1 and are presented as positive values in all subsequent analyses. A RLL detection score was defined as the average RLL Z scores computed across the ten presentations of the relevant items, across blocks.

3.3. Combined detection score

Several studies demonstrated that a combination of several ANS measures outperforms the best single measure (e.g., Ben-Shakhar & Dolev, 1996; Ben-Shakhar & Elaad, 2002; Gamer, Verschuere, Crombez, & Vossel, 2008). We therefore computed a combined detection score that was defined as the sum of the SCR and RLL detection scores.

4. Data analysis

Each dependent measure (proportions of correctly recalled and recognized items, and the three detection scores constructed for SCR, RLL, and the combined measure) was subjected to a 2×3 between-subjects ANOVA, with RI condition (learning an alternative crime scenario vs. neutral sudoku task) and time-delay condition (immediate, delayed CIT, delayed task, and CIT) as the two factors. This was followed by two sets of orthogonal planned contrasts. The first, which was designed to examine more closely the main effect of time delay, included the following contrasts: (1) The immediate condition was compared with the two delayed conditions; (2) the two delayed conditions were contrasted (i.e., task performed immediately and accompanied by a delayed CIT vs. both delayed). The second set of contrasts was designed to examine more closely the interaction between the two factors. Specifically, we conducted two interaction contrasts, the first examined whether the RI effect (i.e., RI minus control sudoku) differed between the immediate and the two delayed conditions (combined), and the second contrasted the RI effect between the two delayed conditions.

One-tailed tests were used in all instances where a clear directional hypothesis was formulated. That is, when testing the main effects of the RI manipulation, we had clear directional hypotheses, according to which reduced memory rates and relative physiological responses would be observed for the critical items in the RI compared with the control condition. In all other cases, two-tailed tests were used. A rejection region of $p < 0.05$ was used for all statistical tests, and effect size estimates were computed, using Cohen's f (Cohen, 1988).

In addition to comparing the means of the detection scores between conditions, we estimated detection efficiency using a signal detection approach (e.g., Green, & Swets, 1966; Swets, Tanner, & Birdsall, 1961). Typically, detection efficiency is defined in terms of the relationship between the detection measure and the actual guilt (or knowledge of the relevant items). In SDT terms, this is measured by a receiver operating characteristics (ROC) curve reflecting the degree of separation between the detection score distributions of "guilty" and "innocent" participants. As the present experiment did not include a sample of unknowledgeable ("innocent") participants, we adopted an approach proposed by Meijer, Smulders, Johnston, & Merckelbach (2007) and simulated the expected detection score distribution among unknowledgeable participants. This simulation is based on the assumption that when the critical item is unknown, all items should produce similar responses and consequently the standardized response to the critical item in each question will have a mean of 0 and a unit standard deviation. The justification of this simulation method was recently examined by Meijer et al. (2014). These researchers applied simulations on 16 data sets from studies that used samples of unknowledgeable participants and compared the outcomes of the simulations with those reported in the original studies. The comparisons showed no systematic differences between the outcomes of the two methods, thus providing empirical validation for the use of the simulations to estimate the responses of unknowledgeable examinees.

In the present study, the detection score was the average standardized response computed across ten presentations of the critical stimuli and consequently it has a mean of 0 and a variance of 0.1. To obtain reliable estimates of the expected distributions of the detection score among unknowledgeable individuals, we adopted a bootstrapping technique (see Wasserman & Bockenholt, 1989), similar to the method that has been applied in many CIT studies based on the P300 component (e.g., Rosenfeld et al., 2008). Specifically, we sampled with replacement, for each condition, n values from a normal distribution with an expected value of 0 and a variance of 0.1, where n is the respective sample size in this condition.

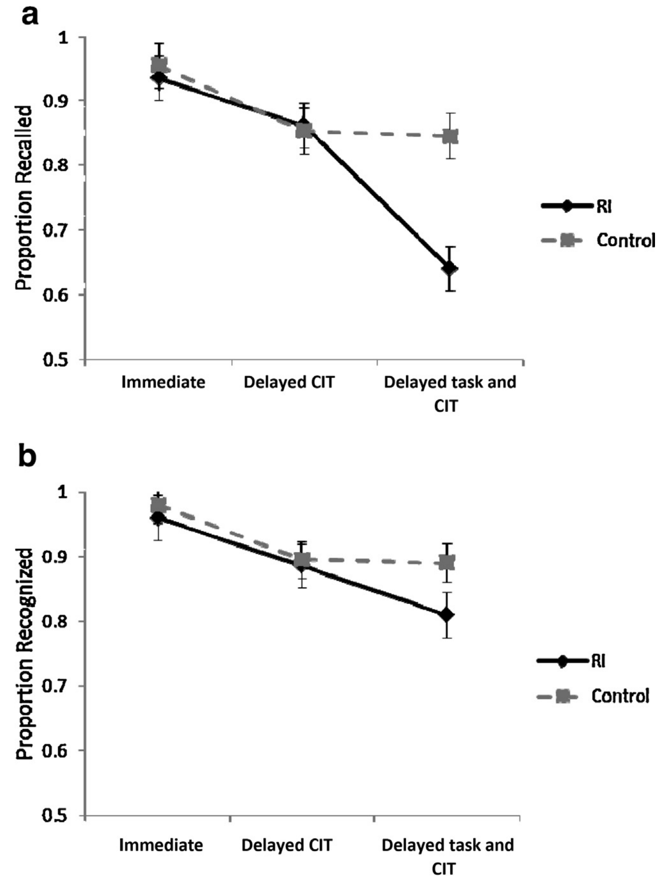

Fig. 1. Proportion of crime-related items that were correctly recalled (a) and recognized (b), as a function of experimental and time-delay conditions. Error bars represent the standard error for the different conditions.

Then, we computed the ROC curve and the area under the curve. This process was repeated 10,000 times for each physiological measure in each of the six conditions as well as for the RI and control conditions, across time delays. In addition, we computed the 95% lower bound for each area, using an estimate of the sampling error variance of the area statistic as proposed by Bamber (1975). The results of this analysis are presented in Fig. 4.

5. Results

5.1. Memory performance

The mean rates of correctly recalled and recognized items were computed and they are displayed in Fig. 1a and b, respectively, as a function of experimental condition. The ANOVA conducted on the recall rates revealed a statistically significant main effect of RI condition, $F(1,114) = 6.34$, $f = 0.21$, $p < 0.05$, reflecting better recall rate in the control condition (88.4%) than in the experimental condition (81.2%). In addition, a statistically significant main effect of time delay, $F(2,114) = 16.86$, $f = 0.51$, $p < 0.01$, along with a significant interaction between the RI and the time-delay factor, $F(2,114) = 5.51$, $f = 0.27$, $p < 0.01$, were obtained. The recognition test also revealed better recognition rates in the control groups (92.2%) than in the RI groups (88.5%). This difference, however, failed to reach statistical significance level, $F(1,114) = 2.41$, $f = 0.11$, $p > 0.1$. A statistically significant time-delay effect was obtained $F(2,114) = 9.1$, $f = 0.37$, $p < 0.001$, yet no RI by time-delay interaction effect was obtained, $F(2,114) < 1$, $p > 0.1$.

We next conducted planned comparisons designed to assess more closely the time-delay effect and its interaction with the

RI condition. With the recall measure, both contrasts designed to examine the main effect of time produced statistically significant effects. Recall rate in the immediate conditions (94.5%) was larger, on average, than in the delayed conditions (80%), $t(114)=4.85$, $f=0.43$, $p<0.01$; and recall rate when only the CIT was delayed (85.7%) was larger than when both learning the alternative scenario and the CIT were delayed (74.2%), $t(114)=3.32$, $f=0.29$, $p<0.01$. As for the recognition test measure, better recognition was observed for immediate (97%) than for delayed (87%) conditions, $t(114)=4.08$, $f=0.36$, $p<0.05$, but there was no significant difference in recognition rates of the crime-related items between the two delayed conditions, $t(114)=1.41$, $f=0.09$, $p>0.1$.

The two contrasts designed to examine the interaction between RI and time-delay conditions within the recall measure yielded mixed outcomes. No significant difference was found in the effect of learning an alternative scenario (relative to a neutral task) between the immediate and the two delayed conditions combined, $t(114)=1.29$, $f=0.07$, $p>0.1$. Importantly, however, the two delayed conditions differed significantly from each other $t(114)=3.10$, $f=0.27$, $p<0.01$. Specifically, when both the alternative scenario and the CIT were delayed (i.e., the CIT was administered a week after committing the mock crime, but immediately after learning the alternative scenario), the reduction in memory as a consequence of the alternative scenario manipulation (i.e., the RI effect) was much larger (20.5%) than when only the CIT was delayed (−1%).

It is also informative to examine the memory performance of the alternative scenario's details, in comparison with the memory performance of the actual mock crime among the participants of the experimental (RI) condition. For this purpose, we conducted a mixed 2×3 ANOVA with memory for the mock crime vs. the alternative scenario as a within-subjects factor and the three-time conditions as a between-subject factor. The mean recall and recognition rates as a function of scenario type and time are displayed in Fig. 2a and b, respectively. We describe here the results for both memory measures. When examining the effect of scenario type (i.e., mock crime vs. alternative crime scenario), there were no significant differences between the overall recall and recognition rates of the two crime scenarios, $F(1,57)=1.81$, $f=0.08$; $F(1,57)=3.16$, $f=0.13$, respectively ($p>0.5$). There were, however, statistically significant main effects of time delay, $F(2,57)=6.18$, $f=0.29$; $F(2,57)=4.45$, $f=0.24$, for the recall and recognition measures, respectively ($p<0.02$ in both measures). Importantly, statistically significant interactions were also found between the scenario type and the time-delay factors, $F(2,57)=16.14$, $f=0.5$; $F(2,57)=9.9$, $f=0.39$, for the recall and recognition measures, respectively ($p<0.01$ in both measures).

To examine this interaction more thoroughly, we computed a difference score for each participant (mock-crime memory performance minus alternative scenario memory performance) and conducted two planned interaction contrasts, identical to the ones described earlier. First, the comparison of the immediate condition with the two delayed conditions was not statistically significant in either recall or recognition measures, $t(57)=1.29$, $f=0.11$; $t(57)=0.92$, respectively. The two delayed conditions, in contrast, differed significantly, $t(57)=5.53$, $f=0.7$; $t(57)=4.36$, $f=0.55$, for the recall and recognition measures, respectively ($p<0.01$ for both measures). That is, when the alternative scenario was learned immediately after the mock crime (i.e., exposure to both crimes took place a week before the memory test), the recall and recognition rates of the scenario's details were smaller (72 and 82%, respectively) than the recall and recognition rates of the mock-crime's details (86 and 88%, respectively, see Fig. 2a and b). However, when the alternative scenario was learned just before the CIT (i.e., a week after commitment of the mock crime, and immediately prior to the memory test), it was much better recalled (92%) and recognized (97%). Critically, under these conditions, there was a dramatic decline in the memory rates of the mock-crime details (64 and 81%, respectively). Thus, among the two delayed time conditions, a stronger memory for alternative scenario was accompanied by a weaker memory for mock-crime details.

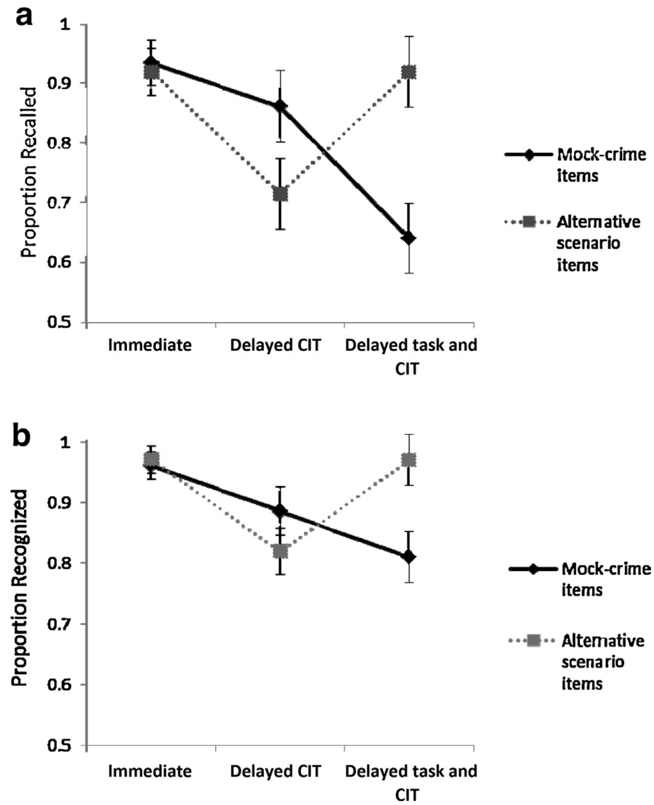

Fig. 2. Proportion of crime-related and alternative scenario items that were correctly recalled (a) and recognized (b) by participants in the memory-interference (RI) group, as a function of time-delay condition. Note that error bars represent the standard error of the difference in memory performance for the two types of items.

5.2. Physiological measures

The mean standardized SCRs to the relevant items as a function of RI condition and time delay are presented in Fig. 3a. The ANOVA conducted on this measure produced a main effect of RI condition, $F(1,114)=4.02$, $f=0.16$, $p<0.05$, reflecting smaller relative responses for participants who learned the alternative scenario (0.31) than for those who conducted a neutral task (0.44). Neither a significant main effect of time delay ($F(2,114)=0.12$), nor a time delay by RI condition interaction ($F(2,114)=0.14$) were obtained. To assess the relations between the SCR results and the explicit memory findings, we computed correlations between the two types of measures across all participants. Note that since the reliability of the SCR measure is typically low, we computed the correlations between the average SCR to the crime-related stimuli and the recall and recognition rates, after correcting for attenuation due to the low reliability of the SCR measure.[2] This computation yielded a small but statistically significant correlation with the memory-recall measure ($r=0.22$, $p<0.02$) and a nonsignificant correlation with the recognition measure ($r=0.02$, $p>0.1$). To further examine the contribution of memory factors to the SCR results, we

[2] The corrected correlation index was computed as the Pearson correlation divided by the square root of the SCR reliability coefficient, estimated by the correlation between the average SCR to the relevant items in the first block and the second block after applying the Spearman–Brown formula.

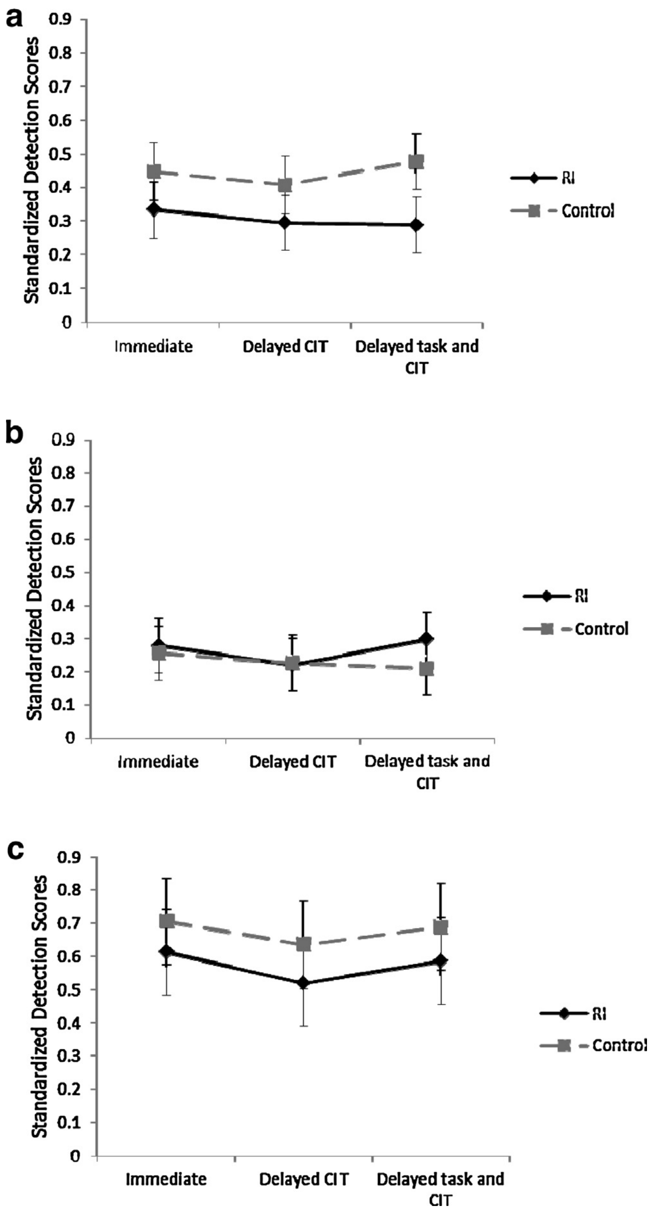

Fig. 3. Detection scores as a function of the experimental and time-delay conditions in the SCR (a), RLL (b), and the combined (c) measures. Error bars represent the standard error for the different conditions.

conducted two ANCOVAs, comparing the RI and the control conditions. The recognition and recall measures served as covariates in the first and the second ANCOVA, respectively. This analysis yielded a statistically significant RI effect when the recognition measure was the covariate, $F(1,113) = 4.09$, $f = 0.16$, $p < 0.05$. When the recall measure served as the covariate, the RI effect in the SCR was attenuated but the ANCOVA was still statistically significant, $F(1,113) = 2.95$, $f = 0.13$, $p < 0.05$ (one-tailed). Taken together, these findings imply that while memory for the original crime items has affected SCR magnitude (when measured via free recall), it did not fully account for the differences between the experimental and the control groups in the CIT.

The ANOVAs conducted on the RLL and the combined measure yielded no statistically significant results (all F values were smaller than 1, see Fig. 3b and c). The combined measure did show the expected pattern of smaller relative responses in the experimental (RI) than the control condition in all three time conditions, but the differences were small and did not reach statistical significance.

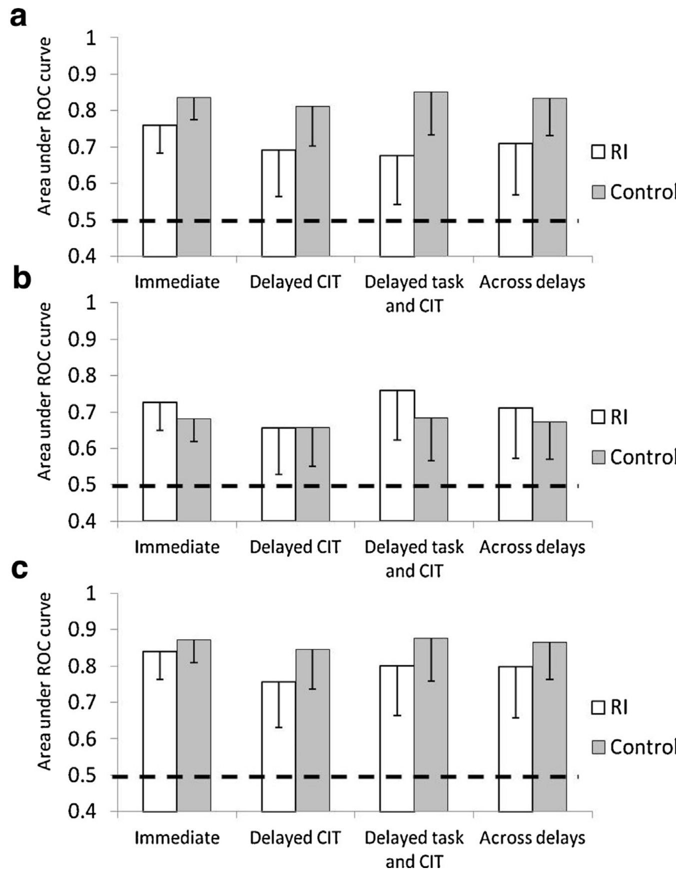

Fig. 4. Areas under the ROC curves and corresponding 95% lower bounds, as a function of the experimental and time-delay conditions in the SCR (a), RLL (b), and the combined (c) measures.

In addition, the correlations of the RLL measure with the explicit memory measures, after correcting for attenuation due to the low reliability of the RLL measure, were nonsignificant ($r = -0.1$, $p > 0.1$, for both recall and recognition measures).

5.3. Signal detection analysis

Detection efficiency of the CIT was also evaluated based on an SDT approach, by deriving receiver operating characteristics (ROC) curves, which reflect the degree of separation between the detection score distributions of "guilty" and "innocent" participants. This approach is particularly useful for analyzing psychophysiological detection data, and it has been applied extensively in this area (e.g., Ben-Shakhar and Elaad, 2003; National Research Council, 2003). As the present experiment did not include a sample of unknowledgeable ("innocent") participants, the hypothetical distributions of the detection scores were simulated (Meijer et al., 2007; Meijer et al., 2014), and reliability of the estimates was increased using a bootstrapping procedure. Fig. 4 displays the areas under the ROC curves, along with the 95% lower bounds, obtained with the three physiological measures, as a function of experimental condition.

Inspection of Fig. 4 reveals that in all experimental conditions and for all measures, the obtained areas were significantly larger than a chance-level area of 0.5. Comparisons of the areas obtained in the experimental (i.e., alternative scenario) and control (i.e., sudoku) conditions revealed that the ROC areas obtained with SCR measure reflected the expected RI effect (i.e., a smaller area under the RI than in the control condition) in all time conditions, yet this effect was statistically significant only in the last time-delay condition ($Z = 1.65$, $p < 0.05$, one-tailed) as well as across the time-delay conditions ($Z = 2.05$, $p < 0.05$). For the RLL measure, the area differences between the RI and the control condition were small and not statistically significant (their direction in most cases was opposite to that predicted). The results of the combined measure showed a similar pattern to that observed with the SCR (i.e., smaller ROC areas under the RI than the control condition), but the differences were small and did not reach statistical significance.

6. General discussion

The present study examined the efficiency of a memory-interference manipulation as a countermeasure technique in the CIT. Specifically, we used a retroactive interference (RI) paradigm to assess the effects of memorization of a hypothetical crime scenario on memory of and psychophysiological responses to critical items encountered during an execution of a mock crime. Results showed that memory for the crime was reduced overall under the RI conditions, mainly with the free recall measure. A statistically significant RI effect was also observed in the SCR measure of the CIT, yielding reduced differential SCRs to the critical items in the RI condition, compared with the control condition. These reduced responses significantly impaired guilt detection, as reflected by the ROC analysis. A similar impairment of CIT detection efficiency was demonstrated recently by Bergström et al. (2013) who used a retrieval suppression manipulation, which resulted in a reduced P300 differentiation between critical and neutral CIT items. No RI effects were seen in the present study with the respiration (RLL) measure.

While the precise mechanism underlying memory interference may be subjected to debate (see, e.g., Anderson and Neely, 1996; Bekerian & Bowers, 1983; Lindsay & Johnson, 1989; Morton, Hammersely, & Bekerian, 1985), several researchers have argued that RI effects can be accounted by a phenomenon termed *retrieval-induced forgetting* (RIF see, e.g., MacLeod & Saunders, 2005, 2008; Saunders & MacLeod, 2002). RIF has been demonstrated to affect memory presumably through inhibition associated with post-event retrieval processes (e.g., Anderson, Bjork, & Bjork, 1994, 2000; for a review, see Anderson & Levy, 2007). In the standard RIF paradigm, post-event items that are practiced and retrieved presumably inhibit competing items of the same category that had originally appeared in a memory-encoding phase but were not practiced and retrieved. The RIF effect is observed when comparing recall of these latter, non-practiced (inhibited) items with other non-practiced (uninhibited) items from a different category (for a non-inhibitory account of the RIF see, e.g., Camp, Pecher, & Schmidt, 2007; Jakab & Raaijmakers, 2009; Jonker, Seli, & MacLeod, 2013).

One may view the present paradigm as a variant of the RIF, since the memorization and retrieval of an alternative crime scenario, following the encoding of the details of an actual mock crime, elicits competition between the details of the two types of crimes, resulting in the inhibition of the latter. Note that in accord with several previous RIF studies, we manipulated the time delay between the initial encoding phase (i.e., the mock crime), the memorization/retrieval phase (i.e., learning the alternative scenario), and the final test (i.e., CIT and memory test). This time factor yielded interesting results, particularly with the explicit memory findings, as reflected by both recall and recognition measures. In agreement with our hypothesis, lower rates of explicit memory for crime details were observed when memory was tested a week after commitment of the crime, compared to an immediate testing, indicating some memory decay during this time lag (regardless of experimental group). More critically when examining the interaction between the interference/retrieval manipulation and the time-delay factor, a statistically significant interaction emerged (yet only with the free-recall measure), reflected by a robust difference in the interference effect between the two time-delayed conditions. Specifically, a strong interference effect was seen when the alternative memorization task was conducted immediately before, but not a week before, the memory test.

The pronounced effect of our memory interference manipulation observed when the test was delayed and the alternative scenario was learned just before the memory test suggests that the decline in mock-crime memory performance may result from better memory of the alternative scenario details. In other words, the memory loss for crime details in this condition may have been caused by a competition between the mock crime and the alternative scenario's details. An alternative account for these results is that the interference manipulation was most effective when memory traces of the original crime were somewhat fragile, as a consequence of the long delay from the initial encoding phase. The high memory performance of the alternative scenario simply resulted from its proximity to the test memory test. Thus, the overall pattern of results did not necessarily involve a competition between the details of the two types of crimes. Unfortunately, the present design does not allow for a firm conclusion regarding the causal relations between the two types of crime details, and these accounts will have to be examined in future research.

A somewhat surprising finding was the lack of an interference effect in the immediate condition, i.e., when both the alternative task and the CIT/memory test were conducted immediately after completion of the mock crime. While reduced memory for an encoded event is typically seen in such situations, we believe that the lack of an interference effect in our study resulted from the fact that the memory tests were of limited sensitivity under this condition, yielding very high levels of recall and recognition (94 and 96%, respectively). Several possible reasons may account for this high-level performance. First, in contrast to standard RI and RIF paradigms, and in accord with typical CIT paradigms, participants in the present study were tested on a very limited number of critical crime-related items (i.e., only five items), which likely produced a low memory load. Second, rather than learning arbitrary sets of words (as in classic memory-interference studies), passively witnessing an event (as in typical misinformation paradigms), or studying semantically related word pairings (as in the classic RIF paradigm), participants in the present study were tested on the details of a mock crime that they have actively committed. Episodic memory for the mock crime was presumably rich and vivid when tested immediately after taking an active part in the theft manipulation. In addition, the alternative crime scenario was learned through verbal rehearsal, rather than through active participation and experience with the crime features. The difference in encoding modes between the two types of crimes (i.e., original vs. alternative) may have attenuated the competition between items for memory representation and may have reduced the RI manipulation effectiveness. Finally, nearly all critical items for which memory was tested were "central" in terms of their relevance and importance to the actual crime (note that the envelope contained the money bills and the jewel, and thus it was a salient item in the mock-crime context; the name of the staff member and the article number were mentioned several times during the instructions phase and therefore their perceived saliency was high; the money and the jewelry were the actual stolen items). Previous research has shown that memory loss for stimuli or events occurs mostly with peripheral items of minor relevance (e.g., Carmel et al., 2003; Gamer et al., 2010; Kensinger et al., 2007; Loftus, 1979; Nahari & Ben-Shakhar, 2011). It appears, then, that the RI manipulation was effective only under conditions in which memory for the crime details was somewhat fragile, i.e., when tested a week from the actual crime performance. Note, however, that long time lags between crime performance and interrogation are highly common under field conditions. The interference effect observed under the long lag condition may therefore represent more faithfully the memory performance under real-world interrogative situations, given that the alternative crime scenario is learned and retrieved immediately before memory testing.

Although a general RI effect was obtained with the SCR measure, the SCR results did not strictly mirror the explicit memory performance. For instance, neither a time-delay main effect, nor a time by RI interaction effect was observed with the SCR measure. A further examination of the correlations between memory performance and SCR magnitude yielded a statistically significant correlation with the recall, but not with the recognition measure. This latter finding may seem at first sight counterintuitive since the CIT mainly relies on recognition processes (i.e., recognizing and acknowledging an item as crime relevant), rather than on free-recall processes. However, a close inspection of the results reveals that the lack of correlation between the recognition rates and the SCR magnitudes may have stemmed from the high recognition rates accompanied by low recognition variability in the different experimental conditions. Alternatively, the lack of a correlation between the two measures (recognition, SCR) may have resulted from the fact that the CIT differs from typical memory-recognition tests in several aspects, such as the serial vs. simultaneous presentation of the relevant and neutral items in the two tests, respectively. In contrast to intuition, then, memory factors potentially influencing electrodermal responses in the CIT may have been more closely related to processes associated with the free-recall test than with the recognition test. In agreement with this finding, memorization of the alternative crime scenario affected the recall measure to a much larger degree than the recognition measure. Indeed, retroactive interference manipulations are long known to be effective mainly with recall tests, but not recognition tests, presumably due to a greater competition among retrieved items in the former (e.g., Postman & Stark, 1969; see review in Anderson and Neely, 1996). The increased competition among items during free recall (but not necessarily during recognition) is also a key factor in the classic RIF paradigm (e.g., Anderson and Levy, 2007).

Yet as mentioned above, our findings clearly imply that memory recall performance cannot fully account for the SCR results in the CIT. An analysis of the contribution of explicit memory to the relative SCRs elicited by the crime-related items revealed that although the RI effect was attenuated when including the recall rate as a covariate, SCRs in the experimental group were still significantly smaller than in the control group. Namely, the RI manipulation affected SCRs above and beyond its effects on memory for the critical items. One possible mechanism through which the RI manipulation may have influenced SCR performance is by an activation of the details of the alternative scenario during the CIT. Recall that participants in the experimental group were encouraged to learn the alternative scenario and were told that its memorization would help them appear innocent in the subsequent CIT. Accordingly, participants may have explicitly activated the alternative scenario's details during the CIT interrogation, even when they remembered all the mock-crime items. Such activation may function as a mental countermeasure that typically attenuates the differential responses to the critical items. Clearly, further research is required to address the effects of interference manipulations on explicit memory as well as on the psychophysiological responses, and the relations between these two types of measures.

Our results have also revealed that in contrast to the electrodermal index, the RLL measure was not affected by the RI manipulation. Interestingly, this discrepancy is consistent with results of previous studies that applied mental countermeasures and showed significant effects with the electrodermal, but not with the respiration measure (Ben-Shakhar and Dolev, 1996; Honts et al., 1996). It should be noted that additional factors have been shown to affect the RLL and the SCR differently. For example, while the SCRs show rapid habituation when questions are repeated several times, RLL was demonstrated to be much less affected by habituation (Ben-Shakhar and Elaad, 2002; Elaad and Ben-Shakhar, 1997). These differences may reflect differences in the sensitivities of these two measures.

Furthermore, our results corroborate many previous studies in demonstrating that although the RLL measure is less efficient than the SCR in differentiating responses to critical and neutral items, a combination of the two measures yields an overall improved CIT efficiency (e.g., Gamer, 2011). Importantly, the advantage of combining SCR and RLL is particularly evident under countermeasures conditions. Our ROC analysis demonstrates that the combined measure is associated with a larger ROC area (relative to the ROC area produced by the SCR alone) in both the RI and the control conditions, but when the increase in the control condition is relatively small (from 0.83 in the SCR to 0.86 in the combined measure, across time conditions), it is much larger under the RI condition (from 0.71 to 0.80, respectively). The combined measure, thus, is more immune to countermeasure manipulations than the SCR alone.

From an applied perspective, the present results show that learning and retrieving the details of a hypothetical crime scenario may be effective in reducing memory as well as SCRs to the critical details of a real mock crime. Specifically, RI memory effects were evident when recall (but not recognition) of the crime's details was tested at a delayed time point relative to the actual commitment of the mock crime, and when the alternative, hypothetical crime scenario was memorized and retrieved immediately before the test. As mentioned earlier, long time lags between the crime and test are highly common under field conditions. Thus, our results imply that learning hypothetical crime details just before taking the CIT may serve as an effective countermeasure by guilty suspects. Furthermore, it is possible that even the complex trial protocol, proposed by Rosenfeld and his colleagues (e.g., Rosenfeld et al., 2008), which was demonstrated to be relatively resistant to physical and mental countermeasures performed during the CIT, may not be resistant to this type of self-initiated misinformation administered prior to the CIT. But of course this requires additional research.

A possible caveat of our study relates to the fact that guilty suspects may have limited knowledge regarding the critical items on which they will be interrogated in the CIT. Consequently, learning the details of an alternative crime scenario, as part of the RI manipulation, may be far less than optimal. Clearly, suspects are unaware of the precise questions that will be used in the CIT. But basic knowledge about the nature of the CIT, which can be easily acquired from the Internet, may direct suspects to the type of questions most likely to be used, such as crime scene location, type of weapon used, and the significant items that were stolen. However, as it is unlikely that suspects will be able to guess all the CIT questions, the RI effect in realistic situations may be smaller than the effect observed in this study. But at the same time, some of the learned items of the alternative scenario may be included as neutral control items in realistic CITs. As mentioned earlier, this is likely to further reduce detection accuracy because relatively large responses are likely to be elicited by these control items. Thus, from this perspective our results provide a conservative estimate to the RI effect that would be obtained in realistic settings. Future research is needed to estimate the RI effect when the learned items are used as neutral/control items in the CIT.

The present results are also relevant to an additional potential difficulty in the application of the CIT. Specifically, the accuracy of the CIT may be compromised when testing criminals who commit multiple crimes because the critical items of previous crimes may interfere with the memory of details of a currently investigated crime. Finally, future research is required to investigate the RI effect with additional physiological and behavioral measures, particularly, cardiovascular measures that are often used in CIT research and practice, ERPs, and response times.

In sum, our findings suggest that retroactive interference may serve as an effective countermeasure technique, in reducing explicit memory performance and in impairing detection efficiency of the CIT based on the electrodermal measure. Note that despite the reduced memory performance and the relative impairment of the SCR, detection efficiency in both psychophysiological measures (i.e., SCR and RLL), as well as in the combined measure, was still significantly above chance level, as measured by ROC areas. Future research may attempt to increase the effectiveness of RI as a countermeasure technique in the context of the CIT, and further explore the optimal conditions under which such a manipulation may impair guilt detection in crime-relevant situations.

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