Right Place, Wrong Time: The limitations of mental reinstatement of context on alibi-elicitation

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Abstract

Although research has focused on how inconsistencies in alibi statements are perceived by law enforcement and legal decision-makers, less attention has been paid to the cognitive factors that may mitigate such inconsistencies. Using a novel, ecologically-valid paradigm, across two experiments (507 events for $N=134$), we examined the accuracy, level of detail, and memory characteristics of participant-generated alibis for schema-consistent and -inconsistent events, and whether mental reinstatement of context (MRC) led to improved alibis compared to time-cueing. Participants reported their typical weekly activities, provided real-time reports of their activities during a two-week period, and reported participant-specific schema-consistent and -inconsistent events using time-cues and MRC. Our results suggest schema-consistent events are reported more accurately than inconsistent events, and that MRC does not improve alibi accuracy over time-cueing. When reporting atypical events, participants provided alibis by reporting what they thought they would normally be doing during that time. Together, these findings quantify how difficult remembering an alibi can be, reinforce that people often rely on schemas, and that mnemonic strategies to increase alibi accuracy are likely to fail, even if they work in other domains. We discuss how the current research adds to recent work on boundary conditions for the efficacy of MRC.

Keywords: Alibi, Memory, Mental Context Reinstatement, Schemas, Information Gathering

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Elicitation

In January 2015, the mobile application, *Alibi*, launched promising to help users prove their innocence by constantly recording the last hour of ambient audio, video, and location data from their phones (Discover Alibi, 2018). Yet typically people are not asked to produce an alibi or corroborating evidence until days, weeks, or even months after a crime has occurred. Thus, recorded ‘alibis’ would be long gone requiring innocent suspects to rely on imperfect memories that research demonstrates are riddled with inaccuracies and inconsistencies between 33% and 53% of the time (Leins & Charman, 2016; Olson & Charman, 2012; Strange, Dysart, & Loftus, 2014). To make matters worse, such errors in alibi generation are rarely interpreted as honest mistakes (Crozier, Strange, & Loftus, 2017). Indeed, the belief that alibis should remain accurate and consistent over time is pervasive among law enforcement and mock legal decision-makers (Burke, Turtle, & Olson, 2007; Culhane & Hosch, 2012; Dysart & Strange, 2012).

Although research has examined *perceptions* of inconsistent alibis (e.g., Allison & Brimacombe, 2010; Burke et al., 2007; Olson & Wells, 2012), little effort has been directed towards the discovery and mitigation of the social and cognitive factors *causing* these inconsistencies (see Charman, Matuku, & Mosser, 2019). Of the handful of studies that have asked participants to generate alibis, three did not assess accuracy (Allison, Michael, Matthews, & Overman, 2011; Culhane, Hosch, & Kenn, 2008; Olson & Wells, 2012), and two others used alibi statement *consistency* across repeated retellings as a proxy for alibi accuracy (Olson & Charman, 2012; Strange, Dysart, & Loftus, 2014). For example, Strange, Dysart, & Loftus (2014) asked student-participants to generate alibis for an afternoon 3 weeks prior and asked them to retell their alibis one week later. Results demonstrate that repeated alibis were commonly inconsistent, either partially or completely, across several details including whereabouts, timing, actions performed before and

after the time in question. More recently, alibi researchers have begun to examine alibi accuracy more directly by establishing ground truth as part of the study procedures (Leins & Charman, 2016; Portnoy et al., 2019). To demonstrate, the to-be-recalled alibi event in Portnoy et al. (2019) occurred in the laboratory: participants performed numerous tasks, including matching name tags with photos and sorting memo cards. Next, they were accused of the theft of a wallet and exposed to one of three interview styles: guilt-presumptive, innocent, or neutral. After 10 minutes, participants generated alibis of their actions. Results indicate that the guilt-presumptive interview process had no impact on the accuracy rate of innocent mock suspects' alibis. However, 96% of participants (across all conditions) accurately recalled their alibis — a proportion that reflects the short delay between the to-be-recalled event and the alibi generation phase but does not reflect the reality for alibi providers.

Here we expand on this scant research by establishing ground truth about participants' generated alibis in a naturalistic setting and over a variety of time delays. Despite criminal justice actors' assumptions that alibi providers can recall credible alibis, little evidence examines whether people are capable of doing so – and none of it very convincing due to experimental limitations. Here we ask: are alibi providers hopelessly bad at reporting their whereabouts, or are memories for some events and the questioning techniques used to elicit them more error-prone than others?

Surveying law enforcement, Dysart and Strange (2012) found that over half of respondents reported using specific time frames when questioning suspects (e.g., “Where were you on March 29th between 6:00 and 7:30 pm?”). Yet researchers have long known that such time-cueing is an ineffective strategy for retrieving detailed information about an event such as what you ate for lunch or how you spent your morning (Brewer, 1988; Wagenaar, 1988) largely because episodic memories are not automatically encoded with time details (Friedman, 1993). Thus, even when you do recall the general order and content of a day's events, you may not recall the exact start and

end time-points — at least not with the accuracy often required in criminal investigations. Time-cues require people to make an effortful, sequential search through their memories for more useful cues such as their mood that day, or a notable disruption to their routine that may trigger other memories (Conway & Pleydell-Pearce, 2000; Moscovitch, 1992). The effortful approach often ends prematurely once we access memories for events that match our schemas of the activities we routinely engage in rather than true detailed memories for the time period (Haque & Conway, 2001; Kleider, Pezdek, Goldinger, & Kirk, 2008). For example, when an officer asks a person to state their alibi for a particular Sunday morning, they might think, “Well, I *always* go to the gym on Sunday mornings, so that’s where I must have been,” rather than continue to search their memory for details of their whereabouts on that particular day and time. It’s worth noting that relying on schemas should often lead to accurate, if only superficially, alibi statements by chance alone, since schemas are based on patterns of what we normally do during particular time periods (Charman, Matuku, Mosser, 2019, p. 50). Leins and Charman (2016) found evidence of this schematic processing — a reliance on schematic expectations to fill in the gaps of our memories. In their study, students participated in an ostensibly unrelated experiment during their regularly scheduled class time (a *schema-inconsistent* event) or attended class (a *schema-consistent* event). When participants were asked to provide their alibis using time-cues one week later, those who provided inaccurate alibis for *schema-inconsistent* events provided *schema-consistent* alibis 60% of the time. Put differently, instead of correctly recalling that they had been participating in a study at the time in question, participants falsely recalled having been in class because their routine dictated that is where they should have been. Interestingly, when interviewers used retrieval cues that emphasized location, while omitting references to time (e.g., Some equipment was damaged in Graham Center room 305. Were you ever in Graham room 305?), participants demonstrated substantially better recall than when cued with time alone: 44% accuracy compared to 13%, respectively. Thus,

perhaps alternative interviewing strategies that facilitate better access to multiple retrieval cues — alerting providers to *event*-related details rather than *schematic* details — may be beneficial.

One promising mnemonic strategy is mental reinstatement of context (MRC; Memon, Meissner, & Fraser, 2010). Drawing on Tulving’s (1983) “encoding specificity” principle of memory, MRC instructions encourage witnesses to locate additional retrieval cues by recalling not only the physical environment (e.g., what they saw or heard), but also the psychological environment (e.g., how they felt) that existed during the critical time-period. This approach is widely regarded as one of the most effective components of the Cognitive Interview — the premier protocol for investigative interviewing of eyewitnesses (Memon et al., 2010) — resulting in improved recall of event details ($d = .26$; Smith & Vela’s 2001 meta-analysis). Deliberately searching for additional retrieval cues may override alibi providers’ tendencies to engage in schematic processing. MRC may even lead to greater accuracy for schema-*inconsistent* events relative to *consistent* events. We would expect increased accuracy for inconsistent events because a substantive literature demonstrates that schema-*inconsistent* events are actually recalled more accurately, vividly and often than schema-*consistent* events (Heit, 1993; Lampinen, Fariez, Neuschatz, & Toglia, 2000; Neuschatz et al., 2002; Stangor & McMillan, 1992). It is also schematic processing which is chiefly responsible for creating distorted memories of schema-consistent events (Bower, Black, & Turner, 1979; Heit, 1993; Neuschatz, Lampinen, Preston, Hawkins, & Toglia, 2002; Stangor & McMillan, 1992; Sulin & Dooling, 1974).

Despite broad consensus that the MRC is the gold standard technique of the Cognitive Interview, literature on its efficacy after a delay is mixed — particularly independent of other Cognitive Interview components (e.g., rapport building; Memon et al. 2010). A recent meta-analysis on the Cognitive Interview found that only 13 of the 65 (20%) studies employed a delay of greater than one week (Memon et al. 2010). Concentrating on those 13 studies, Memon et al. found

that although the efficacy of the Cognitive Interview decreased as retention interval increased, the advantage of the Cognitive Interview remained substantial, suggesting MRC still enhances memory ($d > 1.0$ difference between MRC and non-MRC recall of *correct details recalled* compared to $d = 1.20$ after shorter delays; see also Köhnen, Milne, Memon, & Bull, 1999, although here delays were 2 days on average). In fact, an earlier meta-analysis focusing exclusively on physical and imagined reinstatement of context found *increased* context-dependent effects on recall after longer delays (1 day to 1 week: $d = .63$) compared to shorter delays (< 1 day up to 5 minutes: $d = .22 - .28$; Smith & Vela, 2001). Yet, a handful of studies — most conducted after these meta-analyses — suggest that MRC’s potential as a mnemonic aid may disappear entirely when administered after a delay (Cutler, Penrod, & Martens, 1987; Doss, Pikart, & Gallo, 2018; Matsuo & Miura, 2016; Paterson, Eijkemans, & Kemp, 2015). Critically, there are stark differences in context-reinstatement techniques, recall stimuli (neutral objects from scenes, criminal acts), and test-types (free recall, recognition) used in these studies that make it difficult to generalize the results of these studies to an investigative setting involving recall of alibis. For example, Doss et al. (2018) tested context-reinstatement by querying recall for objects superimposed over old scenes previously shown with the object versus novel scenes; Cutler et al. (1987) used a guided interview with “mnemonic instructions” and photos of the robbery and victim; and the final two studies used the Self-Administered Interview — a self-guided paper-and-pencil instrument based on the mnemonic components of the Cognitive Interview (Gabbert, Hope, & Fisher, 2009; Matsuo & Miura, 2016; Paterson et al., 2015). Given the mixed results, as well as the fact that alibi providers may be asked to provide information about mundane events and after a longer delay than eyewitnesses, knowing whether MRC retains its efficacy after a delay is critical. That is one question we address here.

Further, prior research on alibi generation has largely focused on the consistency of initially reported alibis compared to later retellings of the same alibi (Strange et al. 2014; Olson & Wells,

2012), rather than the *accuracy* of alibis when compared to ground truth. Leins and Charman (2016) was the first paper to establish ground truth for alibis by staging schema-consistent and inconsistent events. But what constitutes a schema-consistent or inconsistent event varies considerably across participants. Would their findings extend to more naturalistic and varied events? For example, the expectation of a pre-scheduled schema-*inconsistent* event (study participation rather than attending class) may alter event memorability relative to an inconsistent event that arises unexpectedly. Moreover, Leins and Charman (2016) categorized alibis as either accurate or inaccurate, but this approach unnecessarily restricts what we can learn about how alibi accuracy may vary by domain; for example, an individual may recall being at work (location) with coworkers (person), but not what they were doing (action). Yet to generate an accurate and convincing alibi, it may be particularly helpful to recall one's whereabouts and company, even if memory for one's actions cannot be retrieved. Moreover, an assessment of discrete event details may facilitate understanding of whether an alibi is accurately *recalled*, rather than merely *reported* through schema-responding. Measuring schema consistency *and* alibi accuracy along a continuum may help us capture a greater proportion of the variability of events experienced and recalled in a naturalistic environment.

Finally, even when alibi providers overcome the challenge of generating an accurate alibi, the credibility of their alibis is substantially dependent on the quality of the available corroborating evidence (e.g., Olson & Wells, 2004; Strange, Dysart, & Loftus, 2014). For example, experimental studies demonstrate that the relationship between the individual providing corroboration — e.g., a relative versus a store clerk — and the suspect greatly influence the perceived truthfulness and credibility of the alibi statement (Lindsay et al., 1986). Accordingly, Olson and Wells (2004) outlined a taxonomy of alibi believability based on types of physical versus person-based evidence providers could ostensibly generate. In their study, participants rated vignettes with alibis containing physical evidence that was either difficult or easy to fabricate as more credible than

those containing person-based evidence from individuals with varying degrees of motivation to lie on behalf of the suspect. Believability ratings in turn predicted estimations of the likelihood of the suspect being guilty.

Thus, given the importance of generating not only alibis, but also strong corroborating evidence it may be helpful to understand whether the type of event being recalled (schema-consistent or inconsistent) and the mnemonic strategy used to elicit an alibi statement impact the amount and quality of evidence providers' can think of and eventually produce to bolster their claims of innocence.

Overview of the Current Studies

To answer our research questions across two experiments, we first instructed participants to prospectively report what activities they routinely engaged in for each hour of each day of every day of the week. Next, we asked participants to message us who, where, and what they were doing in real-time a few times every day for two weeks. Then, we rated whether each event reported in real-time conformed to (schema-consistent event) or deviated from participants' routines (schema-inconsistent event). Finally, in a within-subjects design paradigm, we asked participants to provide alibis for schema-consistent and schema-inconsistent events using time cues and MRC. Additionally, in Experiment 2 we asked participants to also provide a list of potential corroborating evidence for their alibis, and their alibis once more after a one-week delay, along with actual corroborating evidence they had located. In line with past research, we expected schema-inconsistent alibis to be more *detailed* than schema-consistent alibis (Heit, 1993; Neuschatz et al., 2002; Stangor & McMillan, 1992) and MRC-elicited alibis to be more *accurate* than time-cue-elicited alibis (Smith & Vela, 2001; but see Matsuo & Miura, 2016; Paterson, Eijkemans, & Kemp, 2015). Given mixed results in the literature over whether schema-consistent events are recalled — or at least reported — more accurately than schema-inconsistent events, we made no predictions

about the direction of a main effect of event schema consistency on alibi accuracy (e.g., Leins & Charman, 2016; Neuschatz et al., 2002). However, we expected an interaction of technique and consistency such that MRC-elicited schema-*inconsistent* alibis would be more accurate than MRC- or Time-cue-elicited schema-*consistent* alibis (Heit, 1993; Neuschatz et al., 2002; Stangor & McMillan, 1992).

Method

Experiment 1

Participants. We recruited 80 undergraduate students from an urban commuter college in the northeast to complete a three-part online study for monetary compensation (\$30). Of these, 74 (60 females; 13 Male; 1 Other) completed Phase 3 of the study (80 completed Phase 1; 75 completed Phase 2). Per our pre-registered plan for data collection (osf.io/7nqpc/), we based our sample size on an a priori G*Power (Faul, Erdfelder, Lang, & Buchner, 2007) analysis and 99% power to detect a medium effect size ($f = 0.25$).¹ This effect size is conservatively comparable to the lowest effect size of MRC ($d = .26$ in Smith & Vela, 2001). Ages ranged from 18 to 41-years ($M = 21.5$, $SD = 3.81$). Of these participants, 31 (41.9%) identified as Hispanic/Latino, 22 (28.4%) as White, 14 (18.9%) as Black/African American, 13 (17.6%) as Asian, and 3 (4.1%) as Other. Moreover, 38 (50.6%) reported being employed either full or part-time. Thus, this sample of students may not share the same characteristics as those of conventional college samples and may be more similar to community samples (see Nieuwkamp, Horselenberg, & Van Koppen, 2016 for a discussion on the importance of recruiting more diverse samples in alibi research). Participants

¹ Although we required a sample of 49 participants, we increased our collected sample until we had 49 data points per cell. Due to some technical difficulties with our surveys, we experienced partial loss of data and therefore collected data from 74 participants. Moreover, given our pre-registered plan to account for covariates, we used Linear Mixed Effects models.

gave informed consent in accordance with the guidelines of the Institutional Review Board at [Institution name redacted for anonymous peer review].

Materials and design. Our experiment was a 2 (Event schema type: Consistent vs. Inconsistent) X 2 (Questioning technique: Time-cue vs. MRC) within-subjects design. As such, participants were each asked to recall four events and two additional practice trials. As we explain below, *schema-consistent events* ($n = 139$) were those that received a high consistency score (2 or 3 out of 3) across prospectively reported (i.e., what participants reported routinely doing at a given time) person, action, and setting details as compared to details reported in real-time. *Schema-inconsistent events* ($n = 140$) were those that received a low consistency score (0 or 1 out of 3).

Because all participation took place online, we randomized event order at the schema-level, but not at the technique level: time-cue instructions always preceded MRC instructions to prevent participants from applying their knowledge of the MRC technique during the time-cue condition. We asked participants to provide alibis for two practice alibis taking place outside the texting period; the time-cue practice trial immediately preceded the presentation of the time-cue instructions and recall of two events using time-cues, whereas the MRC practice trial preceded the MRC instructions and recall of two events using MRC. We used this ordering approach to ensure that participants practiced each technique prior to actual testing but were not privy to MRC instructions when recalling events using only the time-cues. Because we did not know ground truth for practice events, we did not code or analyze them (except for a word count, as a way to assess participant fatigue, as we explain in the Experiment 1 Summary).

Interview technique.

Time-cue technique. For the two events queried using time-cues only, we asked participants to think about what they were doing, where they were, and who they were with during a 4-hour time window (hour during which crime took place ± 1.5 hours; e.g., 10:30 am – 2:30 pm), followed

by the specific 1-hour window (12:00 pm – 1:00 pm) during which the hypothetical, mock crime took place and for which we had real-time data (text messages).

Mental reinstatement of context technique. For the two events queried using MRC, we used the MRC instructions from Section A of the Self-Administered Interview. The Self-Administered Interview was designed for use with eyewitnesses and comprises several sections requesting descriptions of the perpetrator (Section B), the scene of the crime (Section C), other potential witnesses or individuals present (Section D), vehicles associated with the crime (Section E), the clarity of viewing conditions (Section F), and an open-ended section (G). Section A, the only one used in the current study, comprises written instructions for mental reinstatement of context, and is also the only section relevant to a study examining innocent alibi witnesses' memories. The instructions for Section A were adapted from the Cognitive Interview's oral administration of MRC instructions. Thus, in addition to cueing participants to the time windows of interest (4-hr and 1-hr), we also provided participants with the additional instruction to "shut your eyes" and "take a few moments to picture in your mind where you were, what you saw, what you heard, who was involved, what you were thinking and how you were feeling at the time." Importantly, we asked participants to provide alibis that were as detailed and accurate as possible, but not to guess about details they could not remember (see exact instructions here osf.io/sx54b/).

Memory Characteristics Questionnaire. To measure the phenomenological characteristics of participants' event memories, participants completed a subset of single item questions from the *Memory Characteristics Questionnaire* (Rubin, Schrauf, & Greenberg, 2003). We asked, "How confident are you that you are accurately recalling the details of that afternoon?" (1 = not at all; 7 = completely); "How vivid and clear is your memory for that afternoon (1 = not at all; 7 = completely); "As I remember the afternoon, I see it out of my own eyes rather than those of an outside observer" (1 = not at all; 7 = as much as any memory); and "As I remember the

afternoon I feel as though I am reliving it” (1 = not at all; 7 = as clearly as if it was happening right now). To assess how well participants *remembered* the event, as opposed to merely *knowing* that it had occurred, they were asked to rate the following statement: “As I think about the event, I can actually remember it rather than just knowing it happened” (1 = not at all; 7 = as much as any memory).

Phase 1: Determining schema-consistency. We recruited participants for a study on “Sleep and Emotional Well-Being.” In an online survey, participants prospectively reported their typical activities for 15 1-hour blocks for each day of the week and rated how typical these activities were for them on a 7-point Likert scale (1= Very Atypical; 7 = Very Typical; osf.io/umjqv/).

Phase 2: Establishing ground truth. Next, we texted participants 3-4 times per day over an approximately 2-week period using an automated text-messaging service (Textedly.com). Text messages contained critical questions (“Who are you with? Where? What are you doing?”) and filler items relating to emotions to bolster our cover story. We instructed participants not to look at the text messages they sent us or discuss them with anyone else and include a self-report measure of the extent to which they did as a covariate in all of our statistical models.

Coding for schema-consistency. Research assistants classified responses as either schema-consistent or schema-inconsistent based on the consistency between the participant’s prospectively reported routine activities (Phase 1 – What they thought they would be doing) and their contemporaneously-reported activities (Phase 2 – What they were actually doing). We calculated event schema consistency for all text responses at the domain-level (1-Person, 1-Setting, 1-Action); thus, each text message reported could receive a consistency score ranging from 0 (lowest) to 3 (highest – consistent across three domain types) A third rater, blind to the other raters’

decisions, resolved disagreements ($\alpha = .78 - .90$). For exact coding instructions see osf.io/abf4t/; for real examples, see the Appendix A of supplemental materials.

Phase 3: Assessing memory. In order to test participants for events that conformed to our design, we asked participants only about select events that were schema-consistent and schema-inconsistent. Thus, a unique memory test was designed for each participant, asking for alibis for specific dates and times that we already had personalized event information for (from Phase 2 text messages). Specifically, we randomly selected 2 schema-consistent (consistency score of 3 out of 3 or 2 out of 3) and 2 schema-inconsistent events (consistency score of 0 out of 3 or 1 out of 3) from each participant's Phase 2 event descriptions, with priority given to events with maximum (3) and minimum (0) schema-consistency scores. When possible, we based our selection of these events on the following criteria: 1) events occurring on non-overlapping days and 2) events *not* occurring on the first or last day of texting, and 3) events *not* occurring on days when texting malfunctioned for most participants.

One day after Phase 2, we emailed participants their personalized memory tests (for template, see osf.io/sx54b/). Participants imagined that they were suspected of being involved in a series of robberies and that they had to prove their innocence by generating alibis for four relevant time periods using the time-cue and MRC techniques. We did not impose any time limit on participants. We then instructed participants to complete the Memory Characteristics Questionnaire and a post-experiment reactions questionnaire that assessed their preference for each technique, and the frequency with which (1) they used a planner to manage their schedules and (2) rehearsed event memories (by looking at the text messages they had sent us).

Coding for accuracy and number of details. After completing Phase 3, to assess *accuracy*, two blinded, trained research assistants coded for consistency between each participant's four Phase 2 (ground truth) and Phase 3 (recalled) events using the same coding scheme described

earlier to assess consistency between Phase 1 and Phase 2 events.² To measure *number of details*, two research assistants classified each piece of information within each event as an Action (A-1), Person (P-1), Setting (S-1), or Object (O-1) detail.³ A response in the memory test, for example, would be coded as follows: I was “sitting (A-1) in class (S-1) and listening (A-1) to professor (P-1)” (Wright & Holliday, 2007). We calculated an average of the count values generated for each domain (*Person, Setting, Action, & Object Number of Details*) and for all combined (*Total Number of Details*).

To assess inter-rater reliability for *consistency/accuracy* coding, five research assistants and two authors blind to condition coded 57 activity statements. We computed Cronbach’s α for all possible two-person combinations of raters; reliability was high and ranged from $\alpha = .78 - .90$ (osf.io/9szxg/). To assess intra-class correlations for the *number of details* coding, RAs classified 20 statements into Person, Action, Setting, and Object details, producing 80 (20 x 4) number ratings. Intra-class correlation coefficients (ICC) for all possible combinations of raters was also high and ranged from .72 to .91 (osf.io/hc37k/). Based upon these analyses, we deemed inter-rater reliability and ICC acceptable for each variable.

Experiment 1 Results

Statistical analyses. We derived our inferential statistics on (generalized) linear mixed-effects (glme/lme) models with event schema consistency, technique, and the interaction term as experimental *fixed* effects; retention interval (delay between event and memory test), self-reported

² Notably, the process of coding for accuracy and consistency are identical (matching detail domains); we use different terminology (i.e., consistency versus accuracy) because the sources for the pieces of information being compared are different. Namely, matching for “consistency” does not reveal anything about the accuracy of recall — as it entails comparisons of units of information that do not contain ground truth — whereas “accuracy” suggests both consistency *and* accuracy since ground truth is one of the units of comparison.

³ We did not include ‘object’ coding for accuracy because we did not ask participants to report object details in their Phase 1 reports as this would require a level of specificity about their routine activities that would have been unrealistic and would have unduly burdened participants. We also did not discriminate between correct and incorrect details because, by design, participants provided minimally detailed text responses in Phase 2, which prevented verifying the accuracy of details reported on the memory tests.

event typicality, planner use, and event rehearsal (the degree to which participants reported looking at the texts they sent us) as *covariates* or *control fixed* effects; and by-participants *random intercepts* and by-participant *random slopes* for event schema consistency. To clarify, this analysis considers differences between participants and their individualized relationship to schematic content, while demonstrating far less loss of statistical power in unbalanced designs compared to a traditional analysis of variance (see Baayen, Davidson, & Bates, 2008).⁴ We used the lme4 and lmerTest software packages (Bates, Maechler, Bolker, & Walker, 2015) in R following Winter’s (2013) instructions and report regression coefficients (*b*; absolute effect size in consistency scoring), standard errors (*SEs*), and 95% confidence intervals for the regression coefficients (computed using parametric bootstrapping). In order to calculate the degrees of freedom and *p*-values by model parameter, we used the Satterthwaite approximation from the lmerTest::summary() function (Kuznetsova, Brockhoff, & Christensen, 2016). We present the model parameters in tables; however, we only interpret significant experimental fixed effects.

We fit outcome variables with non-Gaussian distributions with generalized linear models. Generalized linear models are appropriate for modeling the codependence among dependent variables in repeated measures designs (Brewslow & Clayton, 1993). For count data outcome variables (i.e., number of details reported in alibis), we used a Poisson distribution. For parameters from the Poisson distribution, we present the untransformed coefficients, but interpret in the geometric (mean) values (back-transformed; using inverse link function) for ease of interpretation.

Our reported analyses include important covariates we were interested in a priori; however, some of the values for these covariates are missing and thus, several cases had to be excluded from

⁴ Due to technical issues with survey construction, event data for at least one event for 26 participants was unavailable reducing the sample that could be analyzed with a repeated measures ANOVA to 49. Both repeated measures ANOVA and LMER analyses yielded virtually identical results.

the linear mixed effect model analyses reported below. In the interest of completeness, here we reported the analyses with covariates included, but fewer observations. Given that the results are virtually identical, we report the analyses for Experiments 1 and 2 with only those covariates with missing values excluded—as a way to increase the number of observations relevant to our primary independent variables—on OSF (osf.io/7fndu/).

Manipulation checks. As expected, event schema consistency (the degree to which ground truth events were consistent with routine events reported during Phase 1) significantly differed between schema-consistent ($M = 2.78$ out of 3, $SD = .41$) and schema-inconsistent events ($M = .15$, $SD = .38$), $t(122) = 58.39$, $p < .001$, Hedge’s $g = 6.53$, 95% CI [5.39, 7.81], whereas retention interval ($M = 8.28$ days; $SD = 3.41$) did not differ across all four conditions, $ps > .28$.

Accuracy of reporting. Across all participants, a total of 279 alibis ($k = 139$ schema-consistent) were generated: 30.8% of alibis were completely accurate across *person*, *setting*, and *action* domains, whereas 47.7% were partially inaccurate and 21.5% were completely inaccurate. On average, participants provided only partially correct alibis ($M = 1.7$ out of 3 detail type points, $SD = 1.12$) and were the least accurate about *action* details ($M = .45$ out of 1, $SD = .49$) compared to *person* ($M = .59$, $SD = .49$; $t(278) = 4.16$, $p < .001$, Cohen’s $d = .249$) and *setting* details ($M = .64$, $SD = .47$; $t(278) = 6.04$, $p < .001$, Cohen’s $d = .256$). Of schema-consistent alibis reported, 33.1% ($k = 46$) were fully accurate compared to 28.57% ($k = 40$) of schema-inconsistent alibis; only 12.9% ($k = 18$) of schema-consistent alibis were completely inaccurate compared to 30% ($k = 42$) of schema-inconsistent alibis. Table 1 depicts means and standard deviations across conditions.

A LMER analysis of event schema consistency (ordinal factor: 0 = most schema-inconsistent, 1, 2, 3 = most schema-consistent) by technique (binary factor: time-cue, MRC) on *total report accuracy* revealed a significant main effect of *schema*— $b = .22$, $SE = .06$, $F(1, 150.65) = 10.33$, $p = .001$, 95% CI [.08, .35]—such that events with greater schema consistency contained

0.22 (out of 3) more accurate detail type points than reports about events with lower schema consistency. There was no main effect of technique, $b = .27$, $SE = .18$, $F(1, 140.32) = 2.12$, $p = .15$, 95% CI $[-.09, .63]$.⁵ The schema by technique interaction was not significant, $b = -.10$, $SE = .09$, $F(1, 144.36) = 1.17$, $p = .28$, 95% CI $[-.28, .08]$. The pattern of results was similar when we separately examined the effect of schema ($ps < .03$), technique ($ps > .21$), and schema by technique ($ps > .74$) on *person*, *action*, and *setting recall accuracy* scores. Thus, after controlling for differences in retention interval, participant's use of planners, event rehearsal and individual qualitative differences in the schematic content of their events, participant's consistency scores revealed a significant effect on alibi reporting such that greater consistency was associated with greater accuracy, whereas retrieval method did not. Table 2 depicts the results of the LMER analysis on all inputted variables for *total report accuracy*.

[Table 1 here]

[Table 2 here]

Number of details. Alibis contained on average 6.11 ($SD = 2.59$) discrete event details (Range: 0.5 – 21). A generalized linear poisson mixed-effect model analysis of event schema consistency by technique on *total number of details* reported did not reveal a significant main effect of *schema*, $b = -.006$, $SE = .02$, $\mathcal{Z} = -.22$, $p = .823$, 95% CI $[-.06, .04]$. The interaction of schema by technique was also not significant, $b = -.016$, $SE = .03$, $\mathcal{Z} = -.42$, $p = .673$, 95% CI $[-.09, .05]$. There was, however, a significant main effect of technique, $b = .207$, $SE = .07$, $\mathcal{Z} = 2.748$, $p = .006$, 95% CI $[.05, .35]$. That is, time-cue-elicited alibis contained 1.23 more *total details* than MRC-elicited alibis (see Appendix B of online supplemental materials for full regression output). Neither

⁵ Differences in degrees of freedom are due to missing data.

schema or technique influenced the *number of person, action, object, or setting* details individually, $p_s > .06$.

Memory characteristics. LMERS of schema and technique did not reveal any significant main effects on *confidence, vividness, perspective, reliving, and remember/know judgments*, $p_s > .25$. Table 3 depicts means and standard deviations. However, as expected, greater accuracy was highly correlated with higher ratings of confidence, vividness, first-person perspective, reliving, and remember/know judgments, $r_s = .26 - .32$, $p_s < .001$.

[Table 3 here]

Experiment 1 Summary

We demonstrated that participants reported alibis more accurately for schema-consistent events than schema-inconsistent events and that this result was robust to diverse variations in content for event type. Critically, contrary to our hypothesis, MRC conferred no advantage over time-cues. It is possible participants became fatigued by the number of events we instructed them to recall ($k = 6$ including two practice trials). Also, because we queried participants about events using MRC after querying about events using time-cues, participants may have disregarded the more extensive MRC instructions entirely. If so, we would expect alibi statements produced later in the memory test to be shorter than earlier alibis.

Although we cannot definitively rule out the possibility that participants disregarded MRC instructions, we did find that MRC differentially influenced alibi production in the expected direction: MRC-elicited alibis were significantly longer than would be expected if they were the product of a fatigued alibi provider. We compared the length (i.e., *total number of words*) of participants' responses for the 4-hour time frame (e.g., What were you doing between 12:30 pm and 4:30 pm?) of time-cue- and MRC-elicited *practice* alibis (see Appendix C in supplemental

materials).⁶ We found that, although MRC-elicitation always followed time-cue-elicitation, participants provided alibi statements that were nearly twice as long for MRC-elicited *practice* alibis ($M = 61.4$ words, $SD = 46.2$) compared to time-cue-elicited *practice* alibis— $M = 36.7$, $SD = 29$, $t(73) = -3.97$, $p < .001$, $d = -0.51$ —a result consistent with participants engaging in MRC. Lengthier alibis were not due to additional time spent on the task ($M_{\text{time-cue}} = 199.3$, $SD = 149.3$; $M_{\text{MRC}} = 213.9$, $SD = 209.6$; $t(72) = -.51$, $p = .61$, $d = .05$).⁷ Notably, wordier descriptions of the 4-hour block for MRC-elicited alibis did not translate to greater *accuracy* or even greater *number of details* in subsequent 1-hour time-frame responses of MRC-elicited practice or test alibis. Thus, the result that TC-elicited alibis contained significantly more *details* than MRC-elicited alibis may be due to fatigue effects.

In Experiment 2, we tested whether our null significance findings for the effect of technique were due to methodological limitations of Experiment 1. First, we counterbalanced the order of the presentation for the elicitation technique. Second, we administered Phase 3 memory tests in person to ensure participants complied with MRC instructions. Third, to reduce fatigue, we lowered the number of events participants recalled (6 v. 4). Additionally, participants were asked to recall their alibis on two separate occasions to assess internal alibi consistency over repeated retellings. Finally, because alibis are only as strong as the corroborating evidence they lead to, participants listed and provided evidence to corroborate their alibis (Olson & Wells, 2004).

⁶ We chose to examine specifically the 4-hour time frame for *practice* alibis because these were presumably the least subject to the hypothesized fatigue effects. This is because, out of the six events participants recalled, these comprised Event 1 (time cue *practice* alibi) and Event 4 (MRC practice alibi) and the first responses following the introduction to the time-cue and MRC questioning instructions, respectively. Because for each alibi participants reported recall for the 4-hour time frame first, it is possible that they perceived the 1-hour time frame follow-up question as redundant, and therefore provided less complete descriptions there. Moreover, these events were not exclusively schema-consistent or inconsistent as we did not collect ground truth for them, and the time period queried did not vary for participants.

⁷ For exploratory analyses on *total number of words* and *time spent writing*, outliers were replaced with the highest non-extreme value as indicated by a visual inspection of a stem-and-leaf plot.

Experiment 2

Participants. Sixty (41 female) undergraduates ($N = 44$) and community members ($N = 16$) completed at least three phases and were compensated \$50.00 (72 completed Phase 1; 65 completed Phase 2). An a priori G*Power analysis indicated we required a sample of 58 participants to detect an effect size of $f = 0.225$. This effect is what we considered the smallest effect size of interest, calculated as the difference of half a detail difference based on standard deviations from Study 1 in a two-tailed t-test of two matched pair dependent means with $\alpha = .05$ and 90% power (correlation between measures = .2 from previous study, 1 group and 4 measurements). We used two-tailed tests because although our theoretical predictions suggested MRC would result in more accurate and detailed memories than time-cues, Experiment 1's results partially suggested the opposite (i.e., greater number of details for TC than MRC-elicited events). Ages ranged from 18 to 59 years ($M = 27.76$, $SD = 10.92$). Of these participants, 24 (40%) identified as White, 16 (26.7%) as Hispanic/Latino, 11 (18.3%) as Black/African American, 6 (10%) as Asian, 2 (3.3%) as Other, and 1 participant did not report their ethnicity. Moreover, 48% ($N = 29$) of participants were at least part-time employed. Recruitment of community members led to a more educationally diverse sample; 6 (10%) had a high school education, 26 (43.3%) had some college education, 16 (26.7%) were college graduates, and 11 (18.3%) had professional degrees, and 1 participant did not respond. Participants signed informed consent in accordance with the guidelines of the Institutional Review Board at [Institution name redacted for anonymous peer review].

Materials and design. Our Experiment 2 design was a 2 (Event schema type: consistent vs. inconsistent) X 2 (Questioning technique: time-cue vs. MRC) x 2 (Recall Times: First vs. Second) within-subjects design.

Procedure. The procedure for Experiment 2 was largely the same as Experiment 1 with three key differences and several additional phases. First, participants completed the Phase 3 memory test in a laboratory in which instructions were given and questions were guided by a randomly assigned interviewer (either one of four trained undergraduate research assistants, or the first or second authors), rather than an online survey. Participants were randomly assigned to interviewer based on scheduling. We made this change to have interviewers guide the recall phase to ensure participants were engaging in the MRC visualization task, and not skipping through instructions for each event. To this end, interviewers asked participants to “*shut your eyes*” and “*picture the time period . . . in your mind for a minute or so*”; interviewers also re-instructed participants who attempted to open their eyes and provide a response following 30 seconds or less of MRC visualization to “*...please just take a moment to close your eyes and imagine the details of the event. What did you see or hear, how did you feel?*” (see osf.io/6kq9n/). Second, to address the potential order confound in Experiment 1, we counterbalanced the order of elicitation technique such that half of participants used time-cues before MRC and the other half received MRC instructions before time-cues (see osf.io/ghk79/ for more details). Third, the changes described above that allowed us to carefully explain and monitor compliance with retrieval instructions, permitted us to eliminate the practice retrieval trials reducing the number of events participants recalled from 6 v. 4 alibis. Finally, during Phase 3 participants reported the evidence they believed they could produce to corroborate their alibis. Data collected for this are not reported in full here but are available at osf.io/4p7se/. The same trained research assistants as for Experiment 1 coded for schema-consistency, alibi accuracy, and alibi consistency across time for Experiment 2.

Phase 4. We gave participants one-week to email us any evidence (e.g., receipts, text messages, emails) they found to corroborate their alibis. We informed participants that they would

be entered into a raffle to win \$50 for completing this phase and that their probability of winning would be influenced by the number and quality of evidence they provided.

Phase 5. One week after Phase 3, we emailed participants personalized surveys and instructed them to (1) provide their alibis for the same Phase 3 events without the use of differing recall techniques, (2) complete the Memory Characteristics Questionnaire, (3) report the extent to which they had attempted to refresh their memories between Phase 3 and Phase 5, and (4) report typicality of the events they were asked to recall.

Coding for accuracy and consistency. Research assistants used the same coding scheme as Experiment 1; additionally, they coded for *accuracy* between Phase 2 and Phase 4 statements and *consistency*, regardless of accuracy, between Phase 3 and Phase 4 statements.

Coding for type of corroborating evidence. Research assistants collected, counted, and sorted evidence submitted by participants over email. Evidence was categorized according to Olson and Well's (2004) taxonomy: person evidence (none, motivated familiar other, non-motivated familiar other, or non-motivated stranger) and physical evidence (none, easy to fabricate, or difficult to fabricate). Evidence items were coded by two trained research assistants and disagreements were resolved by a third assistant who resolved discrepancies by assigning evidence to the more conservative (weaker evidence) category type.

Experiment 2 Results

Manipulation checks. Similar to Experiment 1, event schema consistency significantly differed between schema-consistent ($M = 2.85$ out of 3, $SD = .46$) and schema-inconsistent events ($M = .23$, $SD = .54$), $t(101) = 31.03$, $p < .001$, Hedge's $g = 4.96$, 95% CI [3.95, 6.12], whereas retention intervals for the different conditions did not (Range: 2 -16 days; $M = 7.38$, $SD = 3.52$, $p > .05$). A LMER analysis of event schema consistency by technique on *total report accuracy* (our main dependent variable) with counterbalance version added to the model as a *covariate*, or control *fixed*

effect, demonstrated that there was no effect of whether participants used MRC or TC first to elicit alibis, $b = 0.01$, $SE = .16$, $F(1, 53.34) = .004$, $p = .95$, 95% CI $[-.31, .33]$. Critically, this finding also suggests that results from Experiment 1 were not influenced by presentation order. Thus, we do not include counterbalance version in subsequent analyses.

Accuracy of alibi. Across all participants, a total of 228 alibis (114 schema-consistent) were generated. Similar to Experiment 1, 33.8% of alibis were completely accurate across *person*, *setting*, and *action* domains, whereas 22.4% were completely inaccurate. On average, participants again provided only partially correct alibis ($M = 1.7$ out of 3 detail types, $SD = 1.15$) and were the least accurate about *action* details ($M = .48$ out of 1, $SD = .50$) compared to *setting* ($M = .60$, $SD = .49$; $t(227) = 3.54$, $p < .001$, Cohen's $d = .234$) and *person* details ($M = .61$, $SD = .48$; $t(227) = 3.32$, $p < .001$, Cohen's $d = .22$). For means and standard deviations see Table 1.

At wider margins than those seen in Experiment 1, 48% ($k = 55$) of schema-consistent alibis reported were more consistently fully accurate compared to 19.3% ($k = 22$) of schema-inconsistent alibis; only 9.6% ($k = 11$) of schema-consistent alibis were completely inaccurate compared to 35% ($k = 40$) of schema-inconsistent alibis. Similar to Experiment 1, a LMER analysis of event schema consistency by technique on *total report accuracy* revealed a significant effect of schema— $b = .47$, $SE = .06$, $F(1, 130.78) = 47.94$, $p < .001$, 95% CI $[.34, .61]$ —such that events with greater schema consistency contained 0.47 (out of 3) more accurate detail type points—compare to .22 points in Experiment 1—than reports of events with lower schema consistency. Again, there was not a significant effect of technique— $b = .17$, $SE = .19$, $F(1, 153.70) = .80$, $p = .37$, 95% CI $[-.21, .55]$ —or a significant interaction of schema consistency by technique type, $b = -.15$, $SE = .09$, $F(1, 153.88) = 2.76$, $p = .098$, 95% CI $[-.34, .02]$. The pattern of results was similar when we separately examined the effect of schema ($ps < .001$), technique ($ps > .06$), and schema by technique [$ps = .04 - .61$] on *person*, *action*, and *setting* accuracy scores. Even after controlling for differences in retention

interval, participant's use of planners, event rehearsal, self-reported event typicality, and individual differences in the schematic content of their events, the event schema score revealed a significant effect on alibi event reporting, whereas retrieval method did not (see Table 2 for the full regression output). Consistent with Experiment 1, participant reporting at Phase 3 was not influenced by retention interval, $b = -.002$, $SE = .02$, $F(1, 190.85) = .01$, $p = .905$, 95% CI $[-.04, .03]$.

Alibi accuracy over time. Alibis provided a second time after a delay (between Phase 3 and 4; $N = 216$; $M_{delay} = 15.9$ days, $SD = 3.89$) were more fully accurate (across all *person*, *setting*, and *action* details) than initial alibis provided one week earlier: 39.4% of Phase 4 repeated alibis were completely accurate across all three detail types compared to only 30.8% of Phase 3 initial alibis. Notably, the proportion of alibis that were completely inaccurate also decreased from 21.5% in Phase 3 to 17% in Phase 4.

To formally examine whether alibi accuracy improved over time, we carried out a LMER analysis of time by event schema consistency by technique on *total report accuracy* (range = 0 – 3 correct detail types) using the same covariates. There was no effect of time— $b = .44$, $SE = .23$, $F(1, 368.70) = 3.69$, $p = .055$, 95% CI $[-0.01, .89]$ —or technique, $b = .17$, $SE = .18$, $F(1, 297.76) = .915$, $p = .339$, 95% CI $[-.18, .52]$. Unsurprisingly, there was a significant effect of schema— $b = .44$, $SE = .07$, $F(1, 176.12) = 39.46$, $p < .0001$, 95% CI $[.31, .59]$ —such that events with greater schema consistency contained 0.44 more accurate detail type points (out of 3) than reports of events with lower schema consistency. Overall, this pattern was similar across *person*, *setting*, and *action* details for the effect of time ($ps = .04, .24, .28$, respectively), schema consistency ($ps < .001$), and technique ($ps > .05$). There were also no significant three-way (Time X Schema Consistency X Technique) or two-way (Schema Consistency X Technique; Time X Technique; Time X Schema) interactions, $ps > .06$. Table 4 depicts the results of the LMER analysis on all inputted variables for *total accuracy as a factor of time, schema consistency, and technique*.

[Table 4 here]

Alibi consistency over time. After a delay ($M = 8.39$ days, $SD = 1.48$), participants provided alibis during Phase 4 ($N = 216$) that were entirely consistent with Phase 3 alibis (across *person*, *setting*, and *action* details) just 62% of the time and completely inconsistent alibis 10.6% of the time. On average, participants provided primarily consistent alibis ($M = 2.31$ out of 3, $SD = 1.03$), and were slightly less consistent about *action details* ($M = .73$ out of 1, $SD = .44$) compared to *setting* ($M = .80$, $SD = .40$) and *person details* ($M = .77$, $SD = .42$).

Although all participants received the survey link instructing them to report their alibis a second time exactly one-week after their initial interviews (Phase 3) and were given a 24-hour deadline, 20 participants did not comply with this request and therefore reported alibis more than 8 days after Phase 3 (total sample range: 7 – 13 day delay; $M = 8$; $SD = 1.40$). Thus, we added the following covariates to the consistency LMER models of Experiment 2: an additional measure of retention interval capturing the time elapsed between Phase 3 and Phase 4; and whether participants self-reported attempting to ‘refresh’ their memories (for each of the four individual events) using outside resources between Phase 3 and Phase 4 (Yes or No).⁸

The LMER analysis of schema event schema consistency by technique on *total consistency* revealed a significant effect of schema— $b = .24$, $SE = .07$, $F(1, 129.43) = 11.37.21$, $p < .001$, 95% CI [.10, .38]: reporting of events with greater schema consistency contained 0.24 more consistent detail type points (out of 3) than reported events with lower schema consistency. There was not a significant effect of technique— $b = .23$, $SE = .21$, $F(1, 124.74) = 1.29$, $p = .25$, 95% CI [-.17, .65] or a significant schema consistency by technique interaction, $b = -.18$, $SE = .09$, $F(1, 126.56) = 3.63$, $p = .058$, 95% CI [-.38, .01]. Notably, events for which participants reported attempting to

⁸ We added this question to the Phase 4 survey after collecting data for the first 14 participants. Out of the 46 who responded, participants reported attempting to refresh their memories for 35 (19.44%) of the remaining 180 events.

refresh their memories received .47 lower consistency scores, $b = -.478$, $SE = .21$, $F(1,107.68) = 27.87$, $p < .001$, 95% CI $[-.89, -.07]$, indicating that refreshing their memories was associated with modification of the original alibi. Overall, this pattern of results was similar across *setting*, and *action* details for the effect of schema consistency ($ps < .02$), and technique, $ps > .15$. Table 5 depicts the results of the LMER analysis on all inputted variables for *total consistency*.

[Table 5 here]

Number of details. Alibis contained on average 8.96 ($SD = 6.09$) discrete details (range: 1 - 34). In line with Experiment 1, an analysis of event schema consistency by technique on *total number of details* reported did not reveal a significant effect of *schema*— $b = -.001$, $SE = .04$, $\mathcal{Z} = -0.03$, $p = .97$, 95% CI $[-.09, .09]$. We observed similar results for *person*, *setting*, *action*, and *object* details, $ps = .67-.77$. Similar to Experiment 1, there was a significant effect of technique on *total number of details* ($b = .25$, $SE = .07$, $\mathcal{Z} = 3.32$, $p < .001$, 95% CI $[0.10, 0.39]$) and *action* details such that MRC elicited alibis contained 1.28 more *total* details and 1.57 more *action* details than time-cue elicited alibis. There was no effect of technique on *person*, *setting*, or *object details*, $ps > .06$. There were no significant interactions ($ps > .07$; see Appendix B of online supplemental materials for full regression output).

Memory characteristics. Similar to Experiment 1, separate LMER analyses of event schema consistency by technique on each of the single items of the Memory Characteristics Questionnaire did not reveal a significant effect of *schema*— $ps > .09$, nor was there a significant interaction— $ps > .30$. Contrary to Experiment 1, there was a significant effect of technique on participants' endorsement of *remember/know judgments* ($b = -.66$, $SE = .31$, $F(1, 107.07)$, $p = .04$, 95% CI $[-1.28, -0.03]$), such that participants more often reported knowing, rather than actually remembering, that time-cue-elicited alibis had occurred compared to MRC-elicited alibis. However, we note the confidence intervals near inclusion of 0. Table 3 depicts means and standard

deviations. Overall, contrary to our predictions and Experiment 1, accuracy in reporting of alibis was not associated with memory characteristics, $ps = .052 - .494$, $rs = .046 - .129$ — with the exception of the vividness of the recall, $r(228) = .147$, $p = .026$, 95% CI [.018, .272].

Corroborating evidence. During the in-lab memory test (Phase 3) participants listed at least one form of corroborating evidence they believed they could realistically provide for 220 of 228 events: 895 possible pieces of evidence ($M_{PerEvent} = 3.76$, $SD = 2.73$; Range_{PerEvent}: 0-23). On average, participants listed more person-based evidence ($k = 466$; $M_{PerEvent} = 1.94$, $SD = 2.26$.) than physical evidence ($k = 429$; $M_{PerEvent} = 1.82$, $SD = 1.5$). The most commonly listed forms of potential evidence were of the easy- to-fabricate physical kind (33.52%; 288 of 859 items) and from motivated familiar others (29.80%; 256 of 859 items). The total number of potential evidence listed was not associated with alibi accuracy, $r(228) = -.074$, $p = .266$. Total potential physical and person-based evidence were also not correlated, $r(240) = .022$, $p = .731$

In contrast, in the week following initial alibi recall only 22 of 60 participants submitted at least one piece of evidence for at least one of the four events, and generated a total of 97 pieces of evidence of the original 895 potential pieces of evidence: ($M_{PerEvent} = 2.11$, $SD = 1.26$; Range_{PerEvent}: 1-10). On average, participants generated more person-based evidence ($k = 52$; $M_{PerEvent} = 1.13$, $SD = .93$), than physical evidence ($k = 42$; $M_{PerEvent} = .98$, $SD = .81$). The most commonly provided forms of evidence included emails to the experimenter from motivated familiar others (24.64%; 35 of 142 items) and easy-to-fabricate physical evidence (28.16%; 40 of 94 items; e.g., photos, tweets). Thus, participants generated fewer pieces of evidence than they had anticipated. However, given that compensation was not contingent on providing corroborating evidence, we suspect that the low response rate of 36.66% was due to a lack of incentive to locate corroborating evidence. Indeed, participants rated the amount of effort they put into locating evidence as “a moderate amount” ($M = 4.55$ of 7, $SD = 1.57$), and this did not differ based on whether participants located

evidence or not, $p = .688$. Similar to results found for potential evidence, the total number of actual evidence generated was not associated with alibi accuracy, $r(129) = .082, p = .354$. Physical and person-based evidence were also not correlated, $r(136) = .006, p = .941$. For means and standard deviations, see Table 6.

[Table 6 here]

Reliance on schemas. Recall that only one previous study has found alibi providers rely on schemas to fill in the gaps for their memories (i.e., schematic processing), but that study used only two types of laboratory-orchestrated/scheduled events and did not manipulate schema consistency *and* mnemonic technique in the same study (Leins & Charman, 2016). Here, we were curious to what extent participants use schematic processing when they cannot remember what they were actually doing across a series of self-generated, naturalistic events. Out of 228 events participants provided alibis for (Phase 3), 111 were schema-*inconsistent*, and of these 90 (81.08%) were at least partially inaccurate (i.e., contained an incorrect person, setting, *or* action detail). To assess whether inaccurate details in participants' alibis for schema-*inconsistent* events were due to schematic processing errors, we measured consistency between incorrect alibi details in initial alibis (reported during Phase 3) and what participants expected they would be doing (as reported in Phase 1). Participants incorrectly provided a schema-*consistent* alibi for a schema-*inconsistent* event in over half of initial (54.62%; 59 of 108 events) and repeat alibis they reported (54.28%; 57 of 105).

Next, since we expected alibi accuracy to be negatively correlated with tendency for schematic processing we ran a one-tailed pearson's product-moment correlation. We found support for this prediction. When alibi providers were first asked to report their alibis for schema-*inconsistent* events, lower alibi accuracy tended to be weakly correlated with higher schematic processing, ($r(106) = -0.245, 95\% \text{ CI } [-1, -0.09], p = .005$), but that tendency became stronger over time, $r(103) = -0.383, 95\% \text{ CI } [-1, -0.24], p < .001$. Surprisingly, the tendency to engage in

schematic processing when reporting initial alibis was comparable for events recalled using MRC ($r(54) = -0.23$, 95% CI $[-1, -0.008]$, $p = .044$) compared to time-cues, ($r(53) = -0.23$, 95% CI $[-1, -0.006]$, $p = .045$). Consistent with prior research, when providing incorrect alibis for varied atypical events, participants' errors conformed to their expectations of what they thought they would have been doing at the time in question.

Experiment 2 Summary

First, we replicated our Experiment 1 findings: event schema-consistency substantially influenced alibi accuracy, whereas retrieval technique following a variable range of delays did not. Second, presentation order of mnemonic technique did not influence alibi accuracy, suggesting results from Experiment 1 were not due to lack of counterbalancing. Third, in partial support of our original hypothesis, we found that that MRC-elicited alibis contained more details than time-cue-elicited alibis.

Finally, we found that repeated alibi statements are partially inconsistent with initial alibi statements over one-third of the time, but the proportion of completely accurate alibis somewhat increases with repeated retellings. This improvement was likely due to some participants' search for and discovery of evidence contradicting initial alibis. For example, participants attempted to refresh their memories for 20% of events queried, and at minimum 36% of participants attempted to locate corroborating evidence providing ample opportunity to discover evidence contradicting initial alibis.

General Discussion

Overall, across two samples of diverse participants and variable naturalistic to-be-recalled events, our experiments demonstrate that producing an accurate alibi is a very difficult memory task without an easy solution. First, events that conform to our daily routines are likely to be reported more accurately. For example, on average, someone recalling a highly schema-consistent

event might report mostly correct information about who, where, or what they were doing, whereas someone recalling a highly schema-inconsistent event might only recall one of those details. However, greater accuracy of schema-consistent events does not rule out the possibility that accuracy was merely the product of schema-based responding. Indeed, several results suggest the likelihood of this possibility: compared to schema-inconsistent events, schema-*consistent* events were not recalled more experientially richly, nor did they contain more details; and accuracy was not associated with experiential quality for schema-consistent events (in Experiment 2, but it was in Experiment 1). It is particularly telling that participants did not report at higher rates having “remembered” — rather than merely having “known” — that schema-consistent (rather than schema-inconsistent) events had occurred. Of course, given that, by definition, schemas represent routine events, schema-based responding should typically lead to alibis that are broadly accurate (Charman, Matuku, & Mosser, 2019). However, as noted earlier, such alibis may contain fewer discrete event details which in the context of a criminal investigation could improve both the access and quality of obtained corroborating evidence. Indeed, in the current study alibi accuracy was unrelated to potential or generated corroborating evidence.

A second noteworthy result was that, despite optimal conditions and two experimental samples powered to find just a half-detail difference, MRC did not lead to more accurate alibis compared to time-cueing alone. Decades of research demonstrates that MRC, a key mnemonic strategy of the Cognitive Interview, substantially improves recall for eyewitnesses (Memon et al., 2010; Smith & Vela, 2001). Why then did MRC confer no mnemonic advantage over time-cues, a standard police questioning strategy (Dysart & Strange, 2012), especially when time-cuing increases providers’ tendencies to rely on routine events when generating alibis for non-routine events (Leins & Charman, 2016)?

Several differences between the circumstances of eyewitnesses and alibi providers may help to explain the theoretical reasons for the unexpected limited efficacy of MRC we observed. For one, the retention interval between the experienced event and the questioning phase is likely to be considerably longer for alibi providers than for eyewitnesses. Law enforcement respond to events requiring eyewitness testimonials by immediately identifying and interviewing witnesses who are often present at the scene. Thus, eyewitness studies have retention intervals that are typically between 24 and 72 hours, or mere minutes after an event (Memon et al. 2010). In contrast, most participants here provided alibis a full week or longer after the event. We know the Self-Administered Interview, when administered immediately or even after a short delay (24-hours), is effective in preserving memory for witnessed events (Gabbert et al., 2009; Hope, Gabbert, & Fisher, 2011; Matsuo & Miura, 2016); yet *delayed* administration of the Self-Administered Interview has failed to produce any substantive benefits. For example, Paterson and colleagues (2015) found that participants who completed the Self-Administered Interview one week after viewing a crime video had similar recall accuracy when tested one week later compared to participants who never completed the interview (for similar results, see Cutler et al., 1987; Doss, Picart, & Gallo, 2018; Gabbert, Hope, & Fisher, 2009; Matsuo & Miura, 2016). In contrast, earlier and more direct examinations of MRC without the added benefit of other accompanying mnemonic strategies — such as those present in the Self-Administered Interview or Cognitive Interview — have reliably demonstrated improved recall (Smith & Vela, 2001). Given the mixed results from the literature on the efficacy of MRC, it is imperative that future research examine its potential role in improving alibi generation under alternative conditions from those tested here (e.g., after shorter delays or in conjunction with other mnemonic techniques).

Alternatively, and perhaps more simply, it is possible the alibi events were not well-encoded or stored in the first place. Of course, if the event was not encoded or stored, then any subsequent

attempts to locate retrieval cues using context reinstatement are bound to fail. Supporting this failure-to-encode (or store) hypothesis, we found that when we included delay in our mixed effect models, delay was not predictive of accuracy, or experiential richness. Critically, in contrast to eyewitnessed events, schema-consistent alibi events are more likely to lack memorability due to their scripted-nature leading to decreased consolidation of event details compared to the more emotionally-arousing events that characterize witnessed events (e.g., see Kensinger, 2004; Laney, Campbell, Heuer, & Reisberg, 2004). Although lack of memorability is a limitation of memories for schema-consistent events, research suggests the same is not true of memories for schema-*inconsistent* events. According to the dynamic memory model, the content of event memories is “stored in terms of the organizational framework of the underling script and indexed in terms of how expectations generated by the script were violated” (Lampinen, Faries, Neuschatz, & Toglia, 2000, p. 552; Neuschatz et al., 2002; Schank, 1983). The increased processing required to make sense of the violations of schema-inconsistent events should make them more likely to be encoded and stored.

Still, even if participants encoded and stored a memory for either schema-consistent or inconsistent events, our results here may be attributable to a failure of the mnemonic strategy, MRC, to assist the provider in retrieving an otherwise available memory. Reinstating the context requires knowledge of the event for which contextualization should occur. But here, MRC relies on weak time-cues to inform providers for which event to reinstate the context. Reliance on time-cues is not as problematic for eyewitnesses and participants of laboratory-based experimental alibi studies who already know exactly which event they ought to retrieve — whether a staged robbery or a set of mundane tasks performed minutes earlier. In contrast, our participants (and *actual* alibi providers) may not know which memory out of the many they should attempt to recall. If a provider does not know which memory corresponds to the critical time period, instructions to consider what

they heard, felt, or saw may be incorrectly applied to an event corresponding to another time period, or not at all.⁹ Actual innocent alibi providers routinely do not know which specific memory they are asked to recall; external aids, such as calendars, other people, or other time-stamped resources may offer the cues necessary to point them in the right direction of which memory they ought to contextualize. Once the correct event is identified, MRC may demonstrate the predicted improvement on the quality of recall. Conversely, without these external aids, MRC alone may be insufficient to help providers generate detailed alibis for the correct event. To test this potential explanation, we examined in an exploratory analysis whether MRC had been more effective than time-cues in eliciting detailed recall within the subset of alibis that were reported completely accurately (Experiment 2; $k = 77$). We found that the main effect of technique on *total number of details* found in the full sample was no longer significant, nor did participants report a qualitatively different recall experience for MRC-elicited alibis (a full report of these analyses is available at osf.io/rn34w/). Thus, while we cannot definitively exclude the explanation that MRC might work on alibis for a clearly specified event, we did not observe any support for it in our study.

Finally, one other possibility for the current findings may be that they are the result of participants' decreased motivation to retrieve an alibi compared to a real suspect. For example, participants knew that an inability to generate credible alibis for the imagined crimes would have no legally relevant consequences. However, research shows that motivation to remember is more important at the time of *encoding* event information, than at the time of *retrieval* (Kassam, Gilbert, Swencionis, & Wilson, 2009). Low motivation to remember at encoding often means that an individual may not have attended to or stored event information in their long-term memory. Thus, even if an individual has high motivation to remember at retrieval (e.g., when an officer requires

⁹ We thank an anonymous reviewer for suggesting this alternative explanation for the current results.

them to produce an alibi), providers may find it difficult to accurately recall event information because the information may not have been stored in the first place.¹⁰ Significantly, research testing the efficacy of the Cognitive Interview or MRC independently has relied on participants witnessing mock-crimes or neutral non-criminal justice relevant stimuli where motivation is almost certainly comparable to the current study and yet such studies have found robust effects on recall for the Cognitive Interview and MRC independently. Still, it is possible our participants would have exerted greater effort at reinstating the context if we had deceived them into believing they would receive a penalty for failing to provide an alibi. Future research could address whether experimental realism influences reporting behaviors.

Alternative Avenues for Accurate Alibi Elicitation

Although our experiments improved on past methodologies — establishing ground truth for varied self-generated, naturalistic events — several potential avenues for improvement remain. First, given the limited efficacy of MRC for improving alibi recall, alternative mnemonic strategies should be explored such as sketching, rapport building, and the adoption of a timeline to organize recall (Hope, Mullis, & Gabbert, 2013; Matsuo & Miura, 2016). For example, Matsuo and Miura (2016) found that eyewitnesses who were asked to describe a fictional video-taped incident (of an attempted car theft) by drawing a picture (as part of Self-Administered Interview) showed improved recall over witnesses who used the Cognitive Interview or free recall instead. Additionally, rapport building, an important tool in any interviewer's toolbox, has been demonstrated to improve recall

¹⁰ Although texting event descriptions in the current study could arguably have facilitated greater initial encoding, there is no evidence suggesting that describing events also was also associated with greater consolidation of event information. When we entered event rehearsal (the extent to which participants looked at text messages they sent us) as a covariate in our models, its influence on recall accuracy yielded mixed results: Experiments 1 ($b = -.12$, $SE = .04$, $F(1, 66.40) = 6.34$, $p = .014$, 95% CI $[-.21, -.02]$), and 2, $b = .04$, $SE = -.06$, $F(1, 57.80) = .79$, $p = .37$, 95% CI $[-.06, .15]$. Moreover, we requested text messages sporadically and with no mention of an eventual memory task. Additionally, we tested participants' memories for only 7-15% (4 of 30 or 56) of events participants texted us about. Finally, some research suggests that participants may not demonstrate increased recall for events potentially made more salient as part of experimentation procedures (Leins & Charman, 2016).

among witnesses (Collins, Lincoln, & Frank, 2002; Holmberg & Madsen, 2014; Kieckhafer, Vallano, & Schreiber Compo, 2013; Vallano & Compo, 2011) and information gathering among suspects (Vallano & Compo, 2015). This interview strategy facilitates effective communication between the interviewer and the witness or suspect by discouraging the interviewer from interrupting the witness, while encouraging active listening, the use of open-ended questions about neutral topics, and, more generally, by allowing the witness to control the conversation. Similarly, rapport building within a mock investigative setting may increase participants' motivation to remember their alibis accurately, provided that the memory trace is available for retrieval.

Finally, another solution that may reduce people's tendencies to rely on schematic processing for alibi generation is the implementation of a police questioning strategy that promotes systematic source monitoring. Source monitoring errors are based on the quality of the encoded information as well as the quality of the decision process when source-monitoring judgments are made (Johnson et al., 1993). That is, if something prevents a person from fully contextualizing event information during the encoding stage it is less likely to be available for later retrieval. Also, if a person is distracted, they may be more likely to rely on effortless, non-reflective (heuristic) processing. Specifically, one way to encourage suspects to deliberate more carefully on their source-monitoring judgments is by asking them to consider a larger amount of time rather than only the time of the crime. This strategy may help the suspect generate more retrieval cues to remember what they were doing before, during, and after the time of the crime (Crozier et al., 2017). Police officers can ask suspects to construct a timeline of their day for the day in question. The adoption of a timeline technique — that is, thinking about and reporting events in a chronologically linear manner using physical timelines — is another promising strategy that reduces cognitive constraints (e.g., to working memory) associated with free recall (particularly in verbal interviews) and encourages alibi providers to consider the sequence of events temporally-adjacent to the time of

the crime (Hope, Mullis, & Gabbert, 2013). For example, rather than narrowly focusing on what an innocent suspect may have been doing during the time of the crime (e.g., from 2:00 to 4:00 PM), they may be asked to reconstruct their entire day from beginning to end, physically arranging events along a graphical timeline. Timelines may prevent the premature ending to the effortful, deliberate search through memory required to retrieve discrete event details. Indeed, graphical timelines increased correct details remembered for a witnessed multi-perpetrator event (Hope, Mullis, & Gabbert, 2013) and recall of family meetings (in a study simulating information gathering with a cooperative witness; Leins et al. 2014). The efficacy of this technique for recall of family meetings is particularly noteworthy as these autobiographical events likely represent a better analog to the types of less emotionally-arousing events — compared to witnessed crimes — that alibi providers might be asked to recall. Other studies have observed similar results in non-legal settings using event history calendars with opticians (Van der Vaart, 2004; van der Vaart & Glasner, 2007). Recent research suggests the source of cues may moderate this effect when attention is not divided, such that self-generated cues lead to improved recall over other-generated cues (see Kontogianni, Hope, Taylor, & Gabbert, 2019). Notably, standard MRC instructions, akin to those used in the current study, use other-generated cues suggesting one potential avenue for improvement.

Second, adopting a paradigm that directly manipulates motivation to remember at the time of retrieval could more definitively demonstrate whether participants motivated to avoid adverse consequences produce more accurate alibis. Although, increased stress may also lead to less accurate alibis as participants' working memories become taxed (Hope et al., 2016), particularly during verbal interviews. Third, future work should systematically manipulate retention interval to assess whether earlier administration of MRC instructions influences alibi accuracy. Assessing the impact of shorter delays between the time of encoding and the interview phase would necessarily limit the generalizability of such findings to the real-world, but it would help researchers identify

whether there truly was a failure to encode the alibi event, or whether the memory trace was merely weak. Identifying the optimal window of time during which alibi interviews should be conducted could inform law enforcement both on how soon deploy their investigative resources and what quality of alibis they should expect from innocent suspects after modest delays.

Fourth, generating an accurate alibi statement is only one part of proving one's innocence, given that a failure to supply convincing corroborating evidence may still impair the provider's credibility (Culhane & Hosch, 2004; Pozzulo, Pettalia, Dempsey, & Melo, 2012; Allison, Jung, Sweeney, & Culhane, 2014; Pozzulo, Pettalia, Dempsey, & Gooden, 2015). When asked to list the types of evidence they could reasonably generate to corroborate their alibis, participants in the current study listed primarily physical evidence that would be easy to acquire and fabricate and statements from motivated familiar others. Predicted evidence closely resembled evidence that participants eventually produced. Preliminary research suggests that the presence of physical evidence, even the easy-to-fabricate kind, lead to greater ratings of alibi believability. whereas person-based corroboration shows mixed results (Olson & Wells, 2004; Strange, Dysart, & Loftus, 2014). Corroborating evidence provided in the current study ranged from class notes, to text messages, tweets, bank statements, emails, and more. Future studies could examine whether alibi providers' perceptions of the strength of their own evidence is comparable to the perceived credibility assigned by independent judges. The varying quantities of evidence provided by participants also presents an interesting avenue for future work examining whether there are diminishing returns for evidence based on quantity and type.

Moreover, any work seeking to improve the quality of alibis and corroborating evidence should also endeavor to understand how law enforcement and legal-decision makers perceive such evidence and what the legal ramifications may be. In Experiment 1 we found that despite writing longer alibi statements using the time-cue technique and these same alibis were no more accurate

than their shorter MRC-elicited counterparts. In Experiment 2, despite being less accurate, alibis for schema-inconsistent events were more detailed than alibis for schema-consistent events. Additionally, across both studies, participants were least accurate about what they were doing than where they were or who they were with. Participants may have had difficult recalling actions because these represent smaller and more variable units of information than setting and persons, which are less likely to change from moment to moment. Examining whether legal decision-makers prioritize alibis with correct setting and person details, while putting less stock on action details may be of importance to innocent suspects. Thus, future research should examine what impact schema-consistency, and the presence of superfluous, non-probative details, or partially inaccurate alibis, might have on a juror's credibility judgments and their ultimate sentencing decisions.

Conclusion

An effective, exculpatory alibi is accurate, consistent, and corroborated by evidence—unfortunately our results—across two studies—show that generating such an alibi is exceedingly rare. Despite strong theoretical backing and optimal conditions, MRC conferred no advantage in accuracy over time-cueing, indicating that what works for eyewitnesses may not work for alibi providers. Even when people do overcome the odds, by producing a convincing alibi, a failure to supply equally convincing corroborating evidence may still impair their credibility. The practical implications of our findings for the criminal justice system are two-fold: first, our results suggest a critical need to locate and interview alibi suspects and witnesses immediately following the crime; and second, law enforcement and legal-decision makers should be careful to not equate inaccurate alibis with guilt.

Portions of both studies were presented at the 2018 and 2019 meetings of the American Psychology-Law Society in Memphis, TN, and Portland, OR, as well as at the meetings of Psychonomics in New Orleans, LA and the Society for Applied Research in Memory and Cognition in Cape Cod, MA.

Declaration of Competing Interests. The authors declare no conflict of interest.

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Table 1

Mean accuracy rates and number of details of items recalled in each condition, standard deviations in parentheses

Details		TC-Con		TC-Incon		MRC-Con		MRC-Incon		All Events	
		<i>M</i>	(SD)	<i>M</i>	(SD)	<i>M</i>	(SD)	<i>M</i>	(SD)	<i>M</i>	(SD)
Experiment 1											
Person	Accuracy	0.63	(0.48)	0.65	(0.48)	0.69	(0.47)	0.51	(0.50)	0.62	(0.49)
	Details	1.92	(1.06)	2.22	(1.08)	1.53	(0.75)	2.07	(0.95)	1.94	(0.99)
Setting	Accuracy	0.76	(0.43)	0.61	(0.49)	0.67	(0.47)	0.49	(0.50)	0.63	(0.48)
	Details	1.83	(1.24)	1.83	(1.23)	1.42	(0.73)	1.30	(0.58)	1.59	(1.01)
Action	Accuracy	0.53	(0.50)	0.39	(0.49)	(0.53)	(0.50)	0.39	(0.49)	0.46	(0.50)
	Details	1.58	(1.25)	1.80	(1.37)	1.46	(0.75)	1.32	(0.85)	1.54	(1.09)
Object	Accuracy	--	--	--	--	--	--	--	--	--	--
	Details	0.96	(0.89)	0.87	(0.91)	0.77	(0.78)	0.65	(0.83)	0.81	(0.85)
Total	Accuracy	1.92	(1.01)	1.65	(1.2)	1.89	(1.05)	1.39	(1.22)	1.71	(1.13)
	Details	6.29	(2.79)	6.71	(3.11)	5.17	(1.71)	5.34	(2.14)	5.88	(2.56)
Experiment 2											
Person	Accuracy	.71	(.45)	.56	(.50)	.75	(.43)	.40	(.49)	.61	(.48)
	Details	1.72	(2.07)	1.38	(1.36)	1.95	(2.47)	1.52	(1.68)	1.64	(1.94)
Setting	Accuracy	.77	(.42)	.38	(.49)	.86	(.35)	.40	(.49)	.60	(.49)
	Details	2.21	(1.50)	2.63	(2.12)	2.32	(1.54)	2.44	(1.71)	2.40	(1.73)
Action	Accuracy	.61	(.49)	.35	(.48)	.63	(.48)	.35	(.48)	.48	(.50)
	Details	2.95	(2.85)	2.91	(2.50)	2.74	(1.30)	3.74	(2.63)	3.08	(2.41)
Object	Accuracy	--	--	--	--	--	--	--	--	--	--
	Details	1.86	(2.47)	1.84	(2.04)	1.65	(1.47)	2.04	(1.78)	1.84	(1.97)
Total	Accuracy	2.11	(.95)	1.30	(1.12)	2.25	(1.01)	1.16	(1.15)	1.70	(1.16)
	Details	8.74	(6.84)	8.75	(6.22)	8.65	(4.96)	9.74	(6.30)	8.96	(6.09)

Note. Con = Schema-consistency Score; Incon = Schema-inconsistency Score

Table 2.

Outcome of LMER analyses of influence of fixed and random effects and covariates on total alibi accuracy

Experiment 1							
Random Effects							
Group Names	Variance	Std. Dev.	Corr				
Participant (Intercept)	.39	.62					
ConScore/Participant	.03	.18	-.88				
Residual	.94	.97					
Fixed Effects	Estimate	UL	LL	Std. Error	df	Fvalue	Pvalue
(Intercept)	1.43	.54	2.31	.45	186.38	10.09	.001
ConScore (0, 1, 2, 3)	.22	.08	.35	.06	150.65	10.33	.001
Technique (MRC vs. TC)	.27	-.09	.63	.18	140.82	2.12	.146
Retention Interval (days)	-.02	-.06	.01	.01	234.85	2.01	.157
Planner Use	.001	-.10	.10	.05	66.27	.000	.979
Event Rehearsal	-.12	-.21	-.02	.04	66.40	6.34	.014
Typicality	.048	-.07	.17	.06	180.78	.567	.452
ConScore*Technique	-.10	-.28	.08	.09	144.36	1.17	.280
Experiment 2							
Random Effects							
Group Names	Variance	Std. Dev.	Corr				
Participant (Intercept)	.27	.52					
ConScore/Participant	.01	.12	-1.00				
Residual	.83	.91					
Fixed Effects	Estimate	LL	UL	Std. Error	df	Fvalue	Pvalue
(Intercept)	5.21	1.15	8.72	1.93	56.18	7.23	0.009
ConScore (0, 1, 2, 3)	.47	.32	.59	.06	130.78	47.88	< .001
Technique (MRC vs. TC)	.17	-.13	.61	.19	153.70	.79	.372
Retention Interval	-.002	-.04	.03	.02	190.85	.01	.905
Planner Use	-.15	-.26	.01	.05	52.78	7.61	.007
Typicality	-.13	-.30	.16	.08	192.29	2.82	.093
Event Rehearsal	.04	-.06	.15	.05	57.80	.79	.37
ConScore*Technique	-.15	-.36	-.00	.09	153.88	2.75	.098

Note. *ConScore* = Consistency score; *Planner Use*: Extent to which participants self-reported using their planner on a daily basis; *Event Rehearsal*: Extent to which participants self-reported looking at the text messages they sent us in Phase 2 prior to testing.; *Typicality*: Participants' self-reported ratings (at Phase 1) for the typicality of events they reported routinely engaging in for the 1-hr window queried. Experiment 1: Number of observations: 252; participants: 71; Experiment 2: Number of observations: 198; participants: 51.

Table 3.

Mean responses for experiential characteristics of memories in each condition, standard deviations in parentheses

	TC-Con		TC-Incon		MRC-Con		MRC-Incon	
	<i>M</i>	(SD)	<i>M</i>	(SD)	<i>M</i>	(SD)	<i>M</i>	(SD)
Experiment 1								
Confidence	5.49	(1.60)	5.33	(1.77)	5	(1.99)	4.88	(2.03)
Vividness	5.16	(1.83)	5	(1.81)	5.04	(1.99)	4.86	(2.03)
Perspective	5.45	(1.46)	5.08	(1.71)	4.90	(1.92)	4.73	(2.10)
Reliving	4.84	(1.91)	4.57	(1.90)	4.47	(2.24)	4.49	(2.21)
Remember/Know	5.14	(1.74)	4.82	(1.98)	4.71	(2.20)	4.49	(2.22)
Experiment 2								
Confidence	5.00	(1.99)	5.20	(1.73)	5.33	(1.65)	5.42	(1.46)
Vividness	4.30	(2.15)	4.77	(1.98)	4.85	(1.97)	5.02	(1.72)
Perspective	4.77	(2.04)	4.72	(1.95)	5.17	(1.78)	5.39	(1.81)
Reliving	4.18	(2.05)	4.20	(2.00)	4.45	(1.84)	4.76	(1.88)
Remember/Know	4.16	(2.02)	4.33	(1.96)	4.60	(1.91)	5.10	(1.78)

Table 4.

Outcome of LMER analysis of influence of fixed and random effects and covariates on total accuracy over time in Experiment 2

Random Effects								
Group Names	Variance	Std. Dev.	Corr					
Participant (Intercept)	.50	.71						
ConScore/Participant	.07	.27	-.85					
Residual	.70	.84						
Fixed Effects	Estimate	LL	UL	Std. Error	df	Fvalue	Pvalue	
(Intercept)	3.12	-.14	6.35	1.66	51.20	3.49	.066	
ConScore (0, 1, 2, 3)	.45	.31	.59	.07	176.12	39.46	> .001	
Time (1 vs. 2)	.44	-.01	.88	.23	368.71	3.69	.055	
Technique (MRC vs. TC)	.17	-.18	.52	.17	297.76	.91	.339	
Retention Interval	-.01	-.04	.01	.02	308.82	.84	.358	
Planner Use	-.11	-.20	-.01	.05	48.81	4.81	.033	
Typicality	.01	-.06	.08	.04	373.45	.06	.792	
LookTexts	.04	-.04	.14	.05	52.14	1.06	.313	
ConScore*Time	-.15	-.31	.01	.09	291.93	3.23	.073	
ConScore*Technique	-.16	-.32	.01	.09	300.85	3.31	.069	
Time*Technique	-.05	-.55	.44	.25	292.64	.04	.832	
ConScore*Time*Technique	.15	-.08	.39	.12	291.86	1.61	.206	

Note. Number of observations 388; participants: 51.

Table 5.

Outcome of LMER analysis of influence of fixed and random effects and covariates on total consistency over time in Experiment 2

Table 1. Descriptive Statistics of the Data							
Random Effects							
Group Names	Variance	Std. Dev.	Corr				
Participant (Intercept)	.17	.41					
ConScore/Participant	.00	.00	-1.00				
Residual	.77	.88					
Fixed Effects							
Fixed Effects	Estimate	LL	UL	Std. Error	df	Fvalue	Pvalue
(Intercept)	3.69	-.83	8.21	2.31	45.97	2.566	.116
ConScore (0, 1, 2, 3)	.240	.10	.38	.07	129.44	11.37	< .001
Technique (MRC vs. TC)	.236	-.17	.65	.21	124.74	1.29	.258
Retention Interval†	.031	-.02	.08	.02	153.64	1.53	.217
Retention Interval‡	.025	-.09	.15	.06	43.70	.168	.682
Planner Use	-.07	-.20	.06	.06	43.20	1.149	.289
Typicality	-.159	-.34	.02	.08	162.36	3.175	.076
Event Rehearsal	.044	-.08	.17	.06	46	.473	.495
RefreshMem: Yes	-.478	-.89	-.07	.21	107.68	5.28	.023
ConScore*Technique	-.188	-.38	.01	.09	126.55	3.63	.058

Note. Retention Interval † refers to delay between Phase 2 and 3, whereas Retention Interval‡ refers to delay between Phase 3 and Phase 5. RefreshMem = Indicates whether participants self-reported refreshing their memories between Phase 3 and Phase 5. Number of observations 166; participants: 44

Table 6.

Mean pieces of corroborating evidence provided by participants in Experiment 2

Evidence Type		TC-Con		TC-Incon		MRC-Con		MRC-Incon		All Events	
		<i>M</i>	(SD)	<i>M</i>	(SD)	<i>M</i>	(SD)	<i>M</i>	(SD)	<i>M</i>	(SD)
Person	Motivated	1.25	(.71)	1	(0)	1.8	(1.79)	1.57	(1.33)	1.43	(1.08)
	Familiar Other										
	Non-Motivated	2	(2)	0	(0)	1	(0)	2.67	(2.89)	2.13	(2.1)
	Familiar Other										
	Non-Motivated Stranger	0	(0)	0	(0)	(0)	(0)	0	(0)	0	(0)
Total		.3	(.81)	.08	(.28)	.17	(.72)	.32	(1.35)	.22	(.87)
Physical	Easy to Fabricate	2.2	(1.30)	2	(.82)	3.25	(3.20)	1.43	(.38)	2	(1.66)
	Difficult to Fabricate	0	(0)	1	(0)	0	(0)	1	(0)	1	(0)
Total		.18	(.70)	.2	(.66)	.22	(1.09)	.15	(.44)	.19	(.76)
Total Evidence		.48	(1.20)	.28	(.72)	.38	(1.28)	.47	(1.43)	1.62	(3.69)