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Prevention Education Effects on Fundamental Memory Processes

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Abstract

This study evaluated effects of a key session from a nationally recognized drug abuse prevention program on basic memory processes in 211 high-risk youth in Southern California. In a randomized, between-subject design, the authors manipulated assignment to a Myth and Denial program session and the time of assessment (immediate vs. one-week delay). The authors examined program decay effects on memory accessibility and judgment errors. Those participants exposed to the program session generated more myths and facts from the program than those in the control group, suggesting that even a single program session influenced students' memory for program information and this was retained at least one week and detectable with indirect tests of memory accessibility. However, consistent with basic research perspectives, participants in the program delayed assessment group erroneously generated more fact-related information from the session to the prompt "It is a myth that_____" than the participants in the program immediate assessment group; that is, they retained more facts as myths. These types of program effects, anticipated by basic memory theory, were not detected with a traditional judgment task in the present sample. The results suggest that basic science approaches offer a novel way of conceptually recasting prevention effects to more completely understand how these effects may operate. Implications for program evaluation and conceptualization are discussed.

Keywords

memory processes; prevention program evaluation

Although there is evidence that some drug abuse prevention programs and components are effective in delaying the onset and trajectories of drug abuse (e.g. see discussion and publications on http://nrepp.samhsa.gov/; Skara & Sussman, 2003; Sun, Sussman, Dent, & Rohrbach, 2008), the effects are sometimes short lived and not as strong as anticipated. In

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addition, there is a lack of information about the full range of fundamental processes governing prevention effects. To date, only a subset of theoretical and assessment approaches have been applied to drug abuse prevention. In general, few approaches in use today are grounded in cognitive science and apply basic memory research to prevention, even though many of the presumed prevention mediators have a cognitive basis (e.g., MacKinnon, Taborga, & Morgan-Lopez, 2002). Yet, there are numerous studies documenting the importance of basic memory processes in health behavior (e.g., Wiers & Stacy, 2006). The present study uses an approach grounded in basic research on memory to assess prevention program effects on the relative accessibility and retention of newly learned preventive information among high-risk youth.

Generally, cognitive processes addressed in prevention-related research have focused on “rational human”, social learning, or classical decision theory approaches (for reviews of adolescent drug use research, see Donovan, 1993; Hawkins, Catalano, & Miller, 1992; Klepp, Perry, & Jacobs, 1991; Klitzner, Vegega, & Gruenewald, 1988; Petraitis, Flay, & Miller, 1995; Sussman & Ames, 2008; Turrisi & Jaccard, 1991). These approaches have not addressed memory activation or memory retrieval processes, whether considered as explicit or implicit memory processes. Without addressing these fundamental issues or including relevant parameters, evaluations of program mediators and theoretical models assume tacitly that all essential cognitions are equally and immediately accessible from memory to guide behavioral decisions. Theories of memory activation are not consistent with equal or immediate accessibility (for discussions, see Stacy, Leigh, & Weingardt, 1994; Stacy & Wiers, 2006). Although traditional public health approaches to health behavior and reasoned cognition are clearly important and should be studied, basic memory processes also deserve extensive application to this area and may have an explanatory advantage when addressing apparently non-optimal behaviors like drug abuse (Ames et al, 2007; Stacy & Wiers, 2010). The present study applies methods from basic memory research to evaluate prevention program effects with the use of tasks that tap into accessibility of learned prevention information.

Many interventions address myths about drug use and refute the positive consequences of use, in hopes of countering misleading or false expectancies or beliefs. However, if memory processes are not considered during prevention programming, it is possible that such program information could have a reverse, deleterious effect (e.g., Clayton, Cattarello, & Johnstone, 1996). Although the present study focused on myths and facts from a specific session, it is possible that any prevention message could be subject to memory errors like source confusion. For example, attributing information learned in a program to an incorrect source or even remembering the gist of a message but forgetting details about the source (see Schacter, 1999; Schacter, Chiao & Mitchell, 2003). This type of potential memory error is highly relevant to accurate content retention and accessibility of information over time. In this study, we adapted a paradigm from basic memory research that has shown that unconscious influences of past presentations of information can increase the probability of mistaking the source of messages and even completely alter the impact of a message. Jacoby, Kelley, Brown & Jasechko (1989) demonstrated such an effect which they labeled a “sleeper effect in fame judgments.”

There is a parallel to such unconscious influences in the social psychological literature, which is a specific example of the phenomenon addressed in the methodology of the present study. The term *sleeper effect*, initially coined by Hovland, Lumsdaine, and Sheffield (1949), emerged from observed phenomena to explain delayed change in attitudes associated with propaganda messages from a perceived noncredible source. Recipients of the messages subsequently forgot the source of the message and were nevertheless influenced by the content (i.e., the dissociation hypothesis). The sleeper effect has since been evaluated across a range of studies (for meta-analysis of effects, see Kumkale & Albarracin, 2004) and an alternative hypothesis regarding the effect emerged. Consistent with the cognitive approach to unconscious influences, Pratkanis, Greenwald, Leippe, and Baumgardner (1988) proposed that the sleeper effect occurs as a result of "differential decay" of the source and content of the message, with the source decaying sooner than the content. As a result, subsequent change in attitude toward messages may be observed. In an extension of this work, Jacoby et al. (1989) used a paradigm in which individuals initially read lists of non-famous names. When asked to judge the fame of those names 24-hours later on a task that included both famous and non-famous names, subjects mistakenly reported non-famous names as being famous. This "false fame" effect occurred over time and did not occur on an immediate judgment task (Jacoby, Kelley, Brown, and Jasechko, 1989). Prior presentation of non-famous names increased subjects' familiarity with the names and the likelihood that these names would be called famous in a later judgment task (Jacoby et al., 1989; Jacoby & Kelley, 1987; Jacoby, Woloshyn, & Kelley, 1989). As time lapsed, subjects had difficulty recollecting the source of familiarity of the names they were to judge, yet, the names remained familiar.

To our knowledge, only Krank, Ames, Grenard, Schoenfeld, and Stacy (2010) have applied a similar approach from basic memory research that acknowledges both conscious (or explicit) and unconscious (or implicit) effects on memory for myths and facts taught in drug prevention programs. Krank and colleagues replicated the finding that source misattributions can easily occur over time (Krank, Ames, Grenard, Schoenfeld, and Stacy, 2010; Krank & Swift; 1994). Using Jacoby's "false fame" paradigm, Krank demonstrated that myths about drug use could be retained as facts after only 24-hr had lapsed. Krank and colleagues found strong main effects for prior exposure to myth messages and fact messages on an immediate test, but the likelihood of generating myth statements as true outcomes of alcohol use increased on a delayed test. In essence, participants did not accurately attribute the source of the outcomes (myth or fact) they accessed from memory. As in the famous name studies, these results underscore the point that it is easy to forget details about information learned at an earlier time, including details about the source or validity of the information. If these types of memory processes are not considered, similar misattributions of the source of familiar messages could have a negative impact on prevention programming and accurate retention of information. That is, myths may be retained as facts (or facts retained as myths) after the program is completed.

Overview

In the present study, we applied a basic memory research approach to evaluate a cognitive misperception lesson on drug use myths and denial from an evidence based, previously

documented prevention program (Towards No Drug Abuse [TND]; for details, see Sussman, Craig, & Moss, 2001; Sussman, Dent & Stacy, 2002). Exposure to the TND prevention program has been shown to be effective in significantly reducing cigarette, alcohol, and hard drug use (for evaluation and discussion of past randomized trials, see Sussman, Sun, Rohrbach, & Spruijt-Metz, 2011). If the TND program session evaluated here corrected cognitive misperceptions (e.g., confronting myths associated with drug use and distinguishing truth from myth), then we would expect newly learned program information to be accurately retained and relatively more accessible from memory on open-ended indirect memory tests, which have been found to strongly predict health behavior (for reviews, see Ames et al., 2006; Stacy & Wiers, 2010). This study evaluated an effect of unconscious influences on a prevention program session (a more general effect than the sleeper effect) or the misattribution of incompletely remembered information with the use of methods, to our knowledge, that have not been previously used in the evaluation of prevention programs.

In a between-subject design, we manipulated: a) whether participants received a Myth and Denial curriculum session; and b) the delay before assessment. We evaluated the following: a) program effects on accessibility of target preventive information regarding myths and facts; b) differences in accessibility as a function of time delay; c) group differences in the generation of response content to indirect tests of memory, and d) group differences in judgments of studied and non-studied myths and facts from the program session. We expected, based on our previous work (Krank et al. 2010), to find strong main effects for prior exposure to prevention messages on immediate indirect tests of memory. That is, we expected these tests to detect increased accessibility of session-related cognitions. We also expected increased accuracy on an immediate judgment task questioning whether a message is a myth or fact about drug use. Importantly, we also evaluated the strong possibility that details of the myth/fact distinction in the prevention message might be inaccurately retained over time. That is, prior exposure to prevention messages may be accessed without the myth/fact source information. Previous work (cf. Jacoby et al., 1989; Krank et al., 2010) suggests that the loss of discriminating information would affect retention effects based on the myth/fact distinction while retaining the familiarity effect of the core message leading to misattribution errors overtime. By contrast, if the myth/fact information in the message is retained and accessed at the delayed test, then there should be no differences in immediate and delayed performance. Alternatively, if the effect of delay simply reduces overall accessibility of the entire message, then you would expect reduced prior exposure effects, but the pattern would be the same on the indirect memory tasks and the judgment task. The control group in this study provided the baseline for assessing the impact of any memory effects. If there were no memory effects, then the pattern of results would be the same across all groups. Finally, we examined potential gender differences on indirect memory tasks and a judgment task as a result of program exposure and time of assessment.

Methods

Participants

Participants were 211 at-risk adolescents from 8 continuation high schools in the Los Angeles area. In California, youth who are unable to remain in regular high schools for a variety of reasons, including substance use, frequently transfer to alternative or continuation high schools. These youth report more drug use than students attending regular high schools and are therefore considered relatively high risk of substance abuse (see Ames et al., 2007; Sussman et al., 1995). The students in this study were not currently enrolled in a prevention program and they had not been previously assessed using indirect tests of memory accessibility.

The students ranged in age from 13 to 18, with a mean age of 16.6 (SD=.27) years. Forty-one percent of the sample was female. Of those participants self-reporting ethnicity, 124 (59%) reported being Latino, 23 (11%) reported being White, non-Latino, 20 (9%) reported being mixed, 14(7%) reported being African-American, and 13(6%) reported being Asian, with the remaining split among Native American and other ethnicities. Most of the students reported having used alcohol (61%) in the last 30-days and having used alcohol in their lifetime (84%). Many of the students had used marijuana (46%) in the last 30-days and had used marijuana in their lifetime (66%). Additionally, 19% of participants reported having used stimulants in their lifetime with 11% reporting past 30-day use. Fifty-two percent of the population reported having smoked cigarettes in their lifetime and 33% reported past month use of cigarettes.

Procedure

The schools in this study were randomly sampled from the pool of available continuation high schools with which we have had contact in the past. At the time of recruitment, there were 22 potential continuation high schools available for the study in the Los Angeles school district. Twelve of those schools were determined to have a sufficient number of students and classes to accommodate each condition of the study. Of the schools approached, 8 agreed to participate and were able to meet the study criteria. The project manager contacted principals of potential schools and the classroom teachers identified by school principals had to agree to participate. Recruitment letters to participating schools stated that six classrooms from each school were to be recruited and randomly assigned to one of three study conditions. Classes from each school were randomly assigned to the study conditions as follows: six poker chips representing the study conditions were placed in a hat, and for each class a chip was drawn until all chips had been drawn. The procedure was repeated until all study classrooms (which included students of all high school grade levels) were assigned to a condition. A total of 33 classrooms were included in the analyses.

All students in each selected classroom were asked to volunteer for an anonymous study. A trained data collector from the University of Southern California recruited subjects. Anyone present on the day of scheduled initial contact was allowed to participate. Prior to disseminating consent forms, trained research staff visited the classrooms to explain the study. After explaining the study in detail, student assent and parental consent were

obtained. The project manager documented the acquisition of the signed consent and assent forms. Consented students who chose not to participate in the assessments at the designated time of measurement were still allowed to participate in the intervention session. On the day students received the curriculum, attendance was taken using the class roster. The class roster indicated who was fully consented and who was not; those fully consented at the time of data collection received an anonymous survey. Participants were not told that they were being assessed in any way on the materials presented or offered any incentive for accurate performance. They were informed that the self-report survey on health-related activities included questions about some behaviors that may be considered sensitive, personal and possibly unlawful (e.g., drug use, theft, etc.) At measurement, no identifying information was linked to the surveys. Participants completed assessments in a regular classroom setting but worked independently. The majority of participants in all groups were tested on the materials in the same classrooms (and were all tested at the same school location) as they received the curriculum information.

Participants were randomly assigned at the classroom level to either the 40-minute interactive TND session or to the assessment only control group. Those assigned to the TND session were assigned, at the classroom level, to an immediate or one-week delayed assessment. Participants assigned to the Myth and Denial session conditions received the assigned curriculum, consisting of a full TND lesson covering the content. The curriculum was delivered by health educators trained in the TND Abuse Prevention program from the University of Southern California. Training of health educators was done by personnel who had received training on the implementation of TND in the schools and had delivered the entire curriculum for many years. The Myth and Denial session of TND included four kinds of myths related to drug use (e.g., drug use provides emotional protection from the outside world), as well as four facts (e.g., drug use often causes bad things to happen). These statements were discussed and false information was refuted and corrected during the interactive session (for more information, Sussman et al., 2001). The session information was presented as typically presented in the standard Myth and Denial session from the TND Abuse Prevention Program. This session presents a myth and then debunks it with several explanatory statements. Participants in the Myth and Denial conditions read a list of facts and faulty beliefs associated with drug use following session implementation (e.g., Ames, Sussman & Dent, 1999; Sussman, Dent & Stacy, 1996). This list included information stating whether these items were myths or facts and included items elaborated in the session as well as items not presented in the session. Immediately following review of the lists of myths and facts, a structured questionnaire was administered to those assigned to the immediate assessment condition that consisted of indirect memory tasks and a judgment task. The same list was provided to participants in the control groups after completion of the study questionnaire.

Following completion of the indirect memory tasks, all participants completed the judgment task related to the myths and facts as well as questionnaire items assessing drug use and demographic measures. For those assigned to the delayed assessment condition, an identical questionnaire was administered one week after delivery of program content to evaluate retention of program messages.

Measures

Indirect Tests of Memory

This study used indirect tests of memory to detect program effects. These indirect tests of memory varied in the content they were trying to elicit in subject's responses and tap the relative accessibility of newly learned prevention content and memory for information in the session. Coding of responses to the memory measures followed the procedures outlined in our previous research on open-ended memory responses (e.g., Ames et al., 2007; Stacy, 1997; Stacy et al., 1994). That is, responses were entered verbatim into the computer, and then coded by two independent judges into the relevant myth and fact content categories (see below). We used a computerized coding and reliability report program to expedite and formalize the coding and data entry process for the open-ended measures (see Ames et al., 2005). Overall agreement between judges on the Myth responses coded ranged from 81% to 90%. Overall agreement between judges on the Fact responses coded ranged from 78% to 97%. Inter-judge reliability was good across the Myth and Fact responses coded with a mean κ coefficient of .64 for Myth items, and mean κ coefficient of .61 for the Fact items (see Fleiss, Levin & Paik, 2003). A final consensus coding was mediated by a third judge.

Indirect tests of memory myth/fact completion task—On these tasks, subjects were prompted as follows: "This question is about things that might occur when people use drugs or drink. Write the first thing that pops to mind to complete each sentence below." Participants responded to the prompt, "It is a myth that _____." This statement was repeated 3 times. Responses were entered verbatim in the computer, and then coded into the following categories, (a) 1 = *match to a myth that was provided in the session, 0 = not a match*, (b) *1 = mismatch to the session where a studied fact presented in the session was written down by the subject as a myth*. As an example, to the prompt, "It is a myth that _____" a student responded "drug use often causes bad things to happen." This response was presented in the session as a fact and not as a myth, therefore this response would have been coded as a mismatched myth, 0 = *not a mismatch*, and (c) 1 = *missed program-related materials altogether*, 0 = *they generated program-related materials.* Participants also responded to, "It is a fact that _____". Again, this statement was repeated 3 times. Responses to the fact statements were entered verbatim in the computer, and coded into the following categories, (a) 1 = *match to a fact that was provided in the session*, 0 = *not a match*, (b) 1 = *mismatch to the session where a myth presented in the session was written down by the subject as a fact*. As an example, to the prompt, "It is a fact that _____" a student responded "drug use provides emotional protection from the outside world." This response was presented in the session as a myth and not as a fact, therefore this response would have been coded as a mismatched fact, 0 = *not a mismatch*, and (c) 1 = *missed program-related materials altogether*, 0 = *they generated program-related materials*. Scores were summed for the two types of questions as follows: myth items (score range, 0–3) and fact items (score range, 0–3) for each category. See Table 1 for coding dimensions and example items.

Explicit Judgment Measure

Judgment task—All participants completed a judgment task consisting of the following items: 1) 8 studied myths that were content specific to the assigned program lesson (e.g., A person would know right away if they were damaged from using a drug); 2) 8 studied facts that were content specific to the assigned program lesson (e.g., People can overdose on alcohol); 3) 4 items that were non-studied myths and not related to the program (e.g., If you get drunk, coffee will sober you up); and 4) 4 items that were non-studied facts and not related to the program session (e.g., Physical damage from drugs gets worse over years). In both the Myth and Denial conditions and the control condition, participants were asked to make judgments about the various myth and fact statements about drug use. Participants were asked "How true are these statements." Response options were as follows: 1= *definitely not true*, 2 = *mostly untrue*, 3 = *mostly true*, and 4 = *definitely true*.

Data Analysis

First, we evaluated whether the random assignment yielded equivalent groups in terms of gender assignment and substance use with analysis of variance. The analytic sample consisted of 197 students who completed the indirect tests of memory and the judgment task. There were no significant differences found in terms of alcohol, marijuana, cigarette or stimulant use ($p>.05$) and no differences in gender assignment ($p>.05$) between the program immediate (PI) assessment group (N=59), program delayed (PD) assessment group (N=72), and assessment only control group (N=66).

Next, multi-level regression models were used to control for interdependencies that might occur based on the classroom level of assignment using the SAS PROC MIXED procedure (Singer 1998). All models included a random effect for classroom to partial out potential effects of clustering. Supplemental analyses were performed using multi-level models that included a random effect for school. When accounting for effects of school, the same pattern of significance, and non-significance, was found except previously significant effects became stronger in effect size and level of significance. Therefore, the more conservative findings for classroom level of analyses are presented here. In all models age was also included as a covariate. No significant effect of age was found in any of the models evaluated. Differences between groups were evaluated on the following: a) studied and non-studied myth and fact items on the judgment task and b) the open-ended memory items evaluating responses to "It is a myth that _____" and responses to "It is a fact that _____". For both the myth and fact items on the open-ended memory task, we evaluated responses based on: 1) whether or not subjects' responses came directly from or matched the curriculum, 2) whether or not the response was related to curriculum material, but in the opposite direction than the curriculum information or mismatched, and 3) whether or not the response was clearly not related to the session or the student missed program-related materials altogether.

Results

Myth Findings

Significant differences were found between those exposed to the Myth and Denial session and the assessment only control group in matching myths. Both the PI assessment group and PD assessment group matched more myths directly from the session than did the control group (see Table 2). The PI group generated significantly more myth content that matched the program session than the control group ($p<.0001$). Additionally, subjects in the PD group were significantly more likely to generate myth materials from the program session than the control group ($p<.01$).

No significant differences were revealed in matched responses between the PI and PD groups. However, females were more likely to match myths than males in both program groups (PI females mean=1.50, PI male mean=.82, t=2.66, $p<.01$; PD females mean=1.42; PD males, mean=.75, t=2.82, $p<.01$). No gender differences were found in the control group.

In terms of mismatching myths, significant differences were found between those in the PI and PD groups and between the PI and control group. A participant's response was determined to be a mismatch when an open-ended response was related to the curriculum materials but in the opposite direction than the curriculum information on drug use myths; that is, a fact was erroneously retained as a myth. Those in the PD group mismatched more myths than those in the PI group ($p<.05$), and the control group mismatched more information than the PI group ($p<.05$). The PD and control group did not significantly differ on mismatching myth statements. This pattern of findings is consistent with forgetting or lack of accessibility of the source memory. However, the PD group was expected to have greater accessibility of program information than the control group, but these groups were not significantly different. No gender differences or interactions across groups were found in mismatching myths.

Significant differences between program groups and the control group were found in producing responses not related to the session (missed), but not between the two program groups. The control group generated significantly more unrelated content than the PI group ($p<.01$) and than the PD group ($p<.001$). No gender differences or interactions across groups were revealed.

Fact Findings

Analyses for "It is a fact _____" were similar to those used to evaluate self-generated responses to the open-ended myth questions. Overall, there were no significant gender differences or gender interactions for all fact item analyses. Significant differences were revealed between those exposed to the program session and controls in matching facts (see Table 2). However, the overall means were fairly low. The PI group generated more facts with content-related to the session than the control group ($p<.01$), and the PD group generated more facts with content-related to the session than the control group ($p<.01$).

In terms of mismatching facts, no significant differences were found between groups. That is, there were no differences between groups in the generated facts that were related to

curriculum materials, but recalled incorrectly, in the opposite direction from that taught in the curriculum. But there were differences between groups in producing fact-related responses not in the session (missed). The control group produced more responses that were not facts from the session than the PI group ($p<.01$) and the PD group ($p<.025$).

Judgment Task Findings

No significant differences were revealed among those in the PI assessment group, the PD assessment group, and the control group in terms of recognition of nonstudied facts or studied facts or myths from the program session. For nonstudied myths, the PI group recognized significantly more of the nonstudied myths than the control group ($p<.05$, see Table 3). Significant gender differences in recognition of studied facts were revealed, with females recognizing more facts than males (female mean=24.11, male mean=22.63, t=2.23, p<.05). However, there were no gender differences in terms of non-studied fact recognition or studied and non-studied myth recognition[1].

Discussion

The goal of this study was to evaluate content retention effects of a key session on Myths and Facts from a nationally recognized evidence-based prevention program (see Sussman et al., 2002) and potential unintended effects on memory processes and judgments among a sample of high-risk youth in continuation high schools. To our knowledge, this was one of the first attempts to evaluate these processes in drug abuse prevention (also see Krank et al., 2010). Overall, the one-session program led to some gains in knowledge with the immediate posttest providing a fidelity check. The favorable outcomes were then sustained over a week, although there were few differences between the immediate posttest and the delayed posttest for most memory assessments. However, methodologically we showed effects with indirect tests of memory that to our knowledge have never before been used to evaluate a prevention program session. Notably, on the indirect tests of memory, those students exposed to the program session generated significantly more myths and facts from the program than those in the assessment only control group, revealing that even a single program session had effects on students' accessibility of program knowledge. More importantly, this knowledge was retained overtime for at least one week and was detectable with indirect tests of memory, designed to detect the accessibility or spontaneity of memory. Because on these tests participants are free to write down any myth or fact that comes to mind (including those acquired at any time in their life), effects on the indirect tests are important -- they show evidence of retention and accessibility of target information that can compete with previously (before program) acquired misinformation. These findings are

[1]Supplemental analyses were run with lifetime alcohol and marijuana use in our models to evaluate their relationship to the different memory conditions. With respect to *matching or missing myths or facts* across the three conditions, lifetime alcohol and marijuana use were not significant predictors of memory performance (all *ps* >.05). With respect to the *mismatch myths* models, lifetime alcohol use was a significant predictor of memory performance ($t = 2.82$, $p < .01$) and marijuana use was significant in the *mismatch facts* models ($t = 2.3$, $p = <.05$). However, adding these variables to the models did not change the pattern of the findings across the group conditions. Alternatively, marijuana use did not predict *mismatching myths* and alcohol use did not predict *mismatching facts* across the groups. With respect to the judgment task, neither lifetime alcohol or marijuana use were significant predictors of recognition of nonstudied myths, and alcohol did not affect recognition of studied myths. However, marijuana use was a significant predictor of nonstudied myths ($p = .043$), but did not change the pattern of relationships found between the program conditions.

consistent with a memory perspective that program content following program implementation became spontaneous enough to be revealed on indirect tests of memory.

Those participants in the 1-week PD assessment group generated more fact-related information from the session to the prompt “It is a myth that_____” than the participants in the PI assessment group; that is, more studied facts were retained as myths (mismatched myth). However, responses generated by the participants in the PD assessment group did not significantly differ from responses of those participants in the assessment only control group. Because the PD and control group findings were not significantly different, there is no clear evidence of unintended effects. The most parsimonious explanation is that effects detectable on the immediate assessment (PI condition) were merely not accessible or forgotten on the delayed assessment (PD condition). If the PD condition had generated more fact information on the open-ended myth task (mismatched myths) than the control group, unintended or paradoxical effects on accurate retention would have been uncovered (see Krank et al., 2010). Although it is clear that such effects can readily occur and should be investigated in evaluation of prevention programs, they did not occur in the present study. This finding may be due to the degree of processing encouraged within the TND Abuse program session. Some evidence consistent with the view that some source information was retained in the PD condition was found in the judgment task. However, overall, the exposed program groups showed very little retention of newly learned program information on the judgment task above and beyond the control group.

The findings of this study replicate some of the findings of Krank and colleagues (2010). Items studied were more likely to be reported as both myths and facts not only immediately, but also after 1-week delay. These findings demonstrate that information from the prevention materials was retained over a retention interval. Second, fact-related information presented during the session was more likely to be written down by participants in the post-session survey to the prompt “It is a myth that_____” after the delay than after immediate testing (although this was not significantly different from controls). This observation is consistent with a simple effect of delay, lack of accessibility, or forgetting of information. Although the nature of the memory effects involved is unclear, it is possible that the lack of accessible information could have resulted from interference from preexisting/competing acquired baseline information about drug use, but such a theory would need to be further tested.

The findings here differed somewhat from the delay effects found by Krank et al. (2010) where items were remembered, but misattributed, suggesting that the source information was lost over the delay. The explanation for this difference probably resides in differences in the nature of the materials studied and the assessment procedures. The prior study used ambiguous outcomes statements that were equally likely to be endorsed as true or not true in the study population. All of the items were potential outcomes of drinking. Thus, if an item easily came to mind or was accessible, it must be a likely effect of alcohol (familiarity was interpreted as a drinking outcome). The present study used less ambiguous items that were more likely to be part of materials in a prevention program. These items were not neutral to the participants. Moreover, the assessment task asked them to generate items that were true or not true. This task would be expected to interact with baseline judgments of believability.

For example, the very low level of myth items mismatched as facts in all groups including the control group suggests that at baseline the myth statements were unlikely to be believable as true statements. Thus, even unattributed familiarity would not result in a higher level of endorsement of these statements as true.

It is also notable that while there were no significant differences found between the program groups (PI and PD groups) in matching myths, females were significantly more likely to match myths than males in both groups. One could speculate that females may have more relevant baseline information about the prevention materials than males. That is, females may have stronger pre-existing associations consistent with the myth statements. Thus, the familiarity produced by study would be more evident on tasks requiring retrieval. Additionally, there were significant gender differences in the judgment of studied facts, with females more accurately judging facts than males. No other gender differences were detected on the indirect memory tasks or the judgment task.

Finally, participants did not write down studied myth statements from the program session as facts to the prompt “It is a fact that____” on the post-session survey (mismatched facts). It may be less likely for facts to be misinterpreted or judged as myths (although there appeared to be something of a trend from the immediate group to the delayed group), given the likelihood of more exposure in general (e.g., through the media) and hence more possible elaboration and familiarity with these types of factual statements about drug use consequences. This finding suggests that perhaps program information debunking myths should include significantly more elaboration of myths to help with familiarity and retention of information. In addition, exposure to some myths, such as, “drug use provides emotional protection from the outside world” may actually be a desired experience, temporarily true, and motivating to some individuals, which may actually increase the likelihood of one’s use of a substance despite attempts to debunk this myth. Negation or debunking of myths may play a part in prevention, but negation should be effectively integrated with the content to be processed (Kaup & Zwaan, 2003; Lea & Mulligan, 2002; Mayo, Schul, & Burnstein, 2004). It is therefore important to consider the content of the message and potential motivating influences of drug use among the target audience when addressing myths and developing program content. As Krank has shown, it is important that programs consider the possibility that programs presenting myths can have unintended effects such that myths are retained later as facts, producing unanticipated, counterproductive effects. Programs should routinely evaluate whether such effects, consistently demonstrated in basic research, occur in any particular prevention program. More generally, it is important for prevention research to fully consider memory assumptions and conduct evaluations of a range of likely effects on fundamental memory processes, which are rarely studied in prevention.

In sum, the findings from this study provide support for a basic memory research approach to the evaluation of prevention program effects to increase our understanding of how cognitive processing influences the impact of prevention information on memory and behavior. Program development should be based in a firm understanding of its cognitive impact (Krank & Goldstein, 2006; Stacy, Ames, Wiers, & Krank, 2009; Wiers et al., 2007). That is, it is important to understand the residual effects of prevention program materials on memory processes.

Limitations

The generalizability of the findings reported here are limited by the at-risk adolescent sample; however, it also may be argued that the youth in this study are representative of the primary target audience for evidence-based prevention programming since they are at higher risk of abuse than youth in regular schools given their known higher rates of drug use (e.g. Ames et al., 2007). Another key limitation of the study pertains to the implementation of one prevention program session, and the relatively brief time duration between exposure and delayed assessment. However, given the present findings, one would likely expect more forgetting of information over a longer duration. Future evaluation research is needed to establish the impact of program effects on memory processes following implementation of an entire evidence-based program, among varying populations and age groups, and over longer retention intervals.

In addition, it is important to point out that the indirect tests of memory used in this study may not be entirely implicit in that participants may not be completely unaware of what is being assessed. Therefore, we cannot rule out the possibility that participants might have inferred that they were expected to generate information about what they had just learned, despite being instructed to respond based on what “pops to mind.” Similarly, we cannot rule out that participants may have filtered or blocked associative responses by writing down something other than the first information that “pops to mind.” Although indirect memory measures are assumed to be less susceptible to a host of confounders such as social bias, faking, or self-justification than are traditional explicit measures, it is still possible that some participants may have strategically controlled their responses (for review, see De Houwer, 2006; Lowery, Hardin, & Sinclair, 2001; Steffens, 2004). This could potentially affect differences in findings for the immediate versus delayed performance on the indirect measures.

Implications

The paradigm used in this study represents a new way for prevention programs to conceptualize and investigate fundamental memory processes in prevention. Understanding these processes may considerably advance our understanding of how, when, and why prevention works, or does not work. Prevention programs are designed with good intentions to change risky behaviors but may in fact produce less than optimal effects, at least in some individuals, if we do not consider basic memory processes (e.g., Stacy, Ames & Knowlton, 2004; Stacy et al., 2009). Despite the potential for memory errors when addressing cognitive misperceptions, prevention programs can positively change and delay hazardous behaviors.

The most general implication for prevention resulting from this study is that previously under-emphasized cognitive processes should be acknowledged when developing prevention content and assessing program effects. Ultimately, program content should be spontaneously activated in memory without effort. Whether the activated content and the source of the content are brought to consciousness may not be the critical issue, as long as the content is activated sufficiently to trigger other prevention-related memories. For instance, repeated elaborations connecting situational features to program materials, repeated practice of alternative behaviors and skills, and practice linking new protective associations could be

added program components that could potentially improve upon program effects (for more discussion, see Stacy et al., 2004).

The findings presented here are consistent with much of the basic memory, cognitive, and social cognitive literature, suggesting that exposure to information potentially can have unintended influences on both cognitive processes and behavior. Mere exposure to information presented in a session can influence the accessibility of content, with increased accessibility consistent with priming effects on memory retrieval of cognitions related to drug use (e.g., Krank & Wall, 2006; Krank, Wall, Stewart, Wiers, & Goldman, 2005; McKee, Wall, Hinson, Goldstein, & Bissonnette, 2003; Palfai & Ostafin, 2003).

In conclusion, the present study applied a cognitive science perspective that has a well-researched theoretical basis that can help us make considerable improvements in the understanding of how prevention programs operate, and how they can be improved. The present basic memory research framework suggests that basic science approaches offer a novel way of evaluating prevention effects that could be pervasive, but are never tested and may not be detected with traditional judgment tasks.

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Table 1

Coding Dimensions With Example Responses to the Indirect Tests of Memory

Item	Student Response	Match content	MisMatch content	Miss content
It is a myth that	*Teenagers are too young to get addicted*	x		
	Drug use often causes bad things to happen		x	
	Drug use provides emotional protection from the outside world	x		x
	If you get drunk, coffee will sober you up			
It is a fact that	*Teenagers are too young to get addicted*		x	
	Drug use often causes bad things to happen	x		
	Drug use provides emotional protection from the outside world		x	
	If you get drunk, coffee will sober you up			x

Note. Match = response directly from the prevention session; MisMatch = response is related to session content, but response is in the opposite direction than the presented content; Miss = any response that is not specifically related to the session content.

Table 2

Content Retention Effects of a Prevention Session Addressing Drug Use Myths and Facts Assessed with Indirect Tests of Memory Accessibility

	Program immediate assessment (N=59)		Program-delayed assessment (N=72)		Control group (N=66)			
Associative memory measure	**M**	**SE**	**M**	**SE**	**M**	**SE**	**T**	**One-tailed sig *p***
It is a myth that…….								
Myth findings								
Matched myth[1]	1.12[a]	.18	1[b]	.16	.33[c]	.17	3.18[a,c] 3.03[b,c]	*p*<. 0001 *p*<. 01
Mismatched myths[2]	.27[a]	.11	.50[b]	.1	.52[c]	.1	1.67 [a,b] 1.69 [a,c]	*p*<.05 *p*<.05
Missed[3]	.93[a]	.16	.71[b]	.14	1.53[c]	.15	2.73 [a,c] 3.93 [b,c]	*p*<. 01 *p*<.0001
It is a fact that…….								
Fact findings								
Matched facts[1]	.86[a]	.11	.79[b]	.10	.42[c]	.11	2.62 [a,c] 2.54 [b,c]	*p*<.01 *p*<.01
Mismatched facts[2]	.08	.06	.21	.05	.1	.06		n.s.
Missed[3]	.80[a]	.14	.90[b]	.13	1.3[c]	.13	2.49 [a,c] 2.28[b,c]	*p*<.01 *p*<.025

Note: Scoring as follows:

[1] 1 = match to a myth (or fact) provided in the session, 0 = not a match; higher values indicate more accurate recall of program content;

[2] 1 = mismatch to a session where a fact from the session is provided as a myth (or a myth as a fact), 0 = not a mismatch, higher values indicate more inaccurate recall;

[3] 1 = missed program-related materials altogether, 0= generated program-related materials, higher values indicate more inaccurate recall. Age was not significant in any of the models evaluated.

Table 3

Mean Accuracy Scores of Studied and Nonstudied Drug Use Myth Statements on a Judgment Task

	Program immediate assessment (N=59)		**Program delayed assessment (N=72)**		**Control group (N=66)**			
Explicit memory measure Judgment task	M	SE	M	SE	M	SE	T	One-tailed sig *p*
Accurately judged Nonstudied myths	9.51[a]	.33	9.15[b]	.29	8.53[c]	.30	2.17[a,c]	$p < .05$
Accurately judged Studied myths	17.77[a]	.71	19.02[b]	.64	18.59[c]	.67		n.s.

Note: No significant differences were found between groups for studied and nonstudied facts.

Age was not significant in any of the models evaluated.