



Provoking lucid dreams at home with sensory cues paired with pre-sleep cognitive training

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ABSTRACT

The ability to realize that you're dreaming — lucid dreaming — has value for personal goals and for consciousness research. One route to lucid dreaming is to first undergo pre-sleep training with sensory cues and then receive those cues during REM sleep. This method, Targeted Lucidity Reactivation (TLR), does not demand extensive personal effort but generally requires concurrent polysomnography to guide cue delivery. Here we translated TLR from a laboratory procedure to a smartphone-based procedure without polysomnography. In a first experiment, participants reported increased lucid dreaming with TLR compared to during the prior week. In a second experiment, we showed increased lucidity with TLR compared to blinded control procedures on alternate nights. Cues during sleep were effective when they were the same sounds from pre-sleep training. Increased lucid dreaming can be ascribed to a strong link formed during training between the sounds and a mindset of carefully analyzing one's current experience.

1. Introduction

Despite the bizarre incongruities that can occur in dreams, people typically dream without realizing that they are dreaming. Lucid dreams, in contrast, are perceived with the knowledge that they are dreamt experiences. Individuals seek out lucid dreams for a variety of purposes including recreation, creative problem solving, and practicing skills (Schädlich & Erlacher, 2012; Stumbrys et al., 2014). Lucid dreaming is also used therapeutically as a treatment for nightmares (de Macêdo et al., 2019; Spoormaker, 2003). In the sleep laboratory, lucid dreamers can perform objectively verifiable signals (Baird et al., 2019; LaBerge et al., 1981) and respond to questions posed by experimenters (Konkoly et al., 2021), making the state highly valued scientifically as a tool to study dreams and consciousness (Appel et al., 2018). However, lucid dreaming is rare. Approximately 50% of people sampled in one study never had a lucid dream, although 20% reported having lucid dreams at least once a month (Saunders et al., 2016). Thus, developing methods to reliably provoke lucid dreaming remains a major research focus (Baird et al., 2019).

Many strategies for lucid-dream induction have been described, but considerable effort is generally required. These strategies include keeping a dream journal, waking up for periods of time during the night (Erlacher & Stumbrys, 2020; Schredl et al., 2020),

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taking pharmacological supplements (LaBerge et al., 2018a), or performing cognitive exercises before sleep such as rehearsing one's intention to become lucid in their next dream (LaBerge, 1980). Another strategy is to develop the habit of performing reality checks, in which one critically reflects upon whether they are awake or dreaming and then seeks evidence, such as by re-reading text to see if it changes or by attempting to breathe while plugging one's nose (Stumbrys et al., 2012; Tholey, 1983). There are also wearable devices that can increase lucid dreaming by delivering prearranged sensory stimulation during sleep (LaBerge & Levitan, 1995; Stumbrys et al., 2012), aiming to give dreamers a more reliable sign that they are dreaming compared to the idiosyncratic dream events that typically trigger lucidity (Adams & Bourke, 2020). Investigators have proposed that sensory stimulation is effective for inducing lucid dreams to the extent that dreamers are mentally prepared to recognize sensory cues as dream signs (LaBerge, 1988). Historically, this preparation has been achieved by asking participants to perform cognitive exercises while awake (LaBerge & Rheingold, 1990; Levitan & LaBerge, 1994; Stumbrys et al., 2012), as well as by recruiting only participants with high interest or experience in lucid dreaming (e.g., LaBerge & Levitan, 1995). Some studies also included practice trials where participants visualized becoming lucid to a cue while awake (e.g., Paul et al., 2014), though others did not (e.g., LaBerge et al., 1988; LaBerge & Levitan, 1995).

Recent progress in increasing lucid-dreaming frequency has relied on attempting to automatize the association between a stimulus and lucidity in the waking state by repeatedly and specifically pairing sensory cues with the act of entering a lucid mindset (Carr et al., 2023). The term *lucid mindset* can be defined as an orientation to pay attention to one's experiences with a critical attitude of evaluating them in fine detail. A lucid mindset can enable effective discrimination between waking and dreaming experiences, much like performing reality checks. Prior studies pairing reality checks with sensory cues produced mixed results (Erlacher, Schmid, Bischof, et al., 2020; Erlacher, Schmid, Schuler, et al., 2020; Tan & Fan, 2023), perhaps in part because cues became associated with performing particular actions repeatedly and, particularly, with the conclusion that one is awake. In contrast, Carr et al. (2023) paired cues with a lucid mindset without requiring a physical action or decision about whether one is awake or dreaming. This pairing occurred shortly before sleep onset, while participants rested in bed with eyes closed for 20 min and allowed themselves to fall asleep. Presenting the same cues again during REM sleep induced high rates of lucid dreaming—50% of participants had a lucid dream in a single nap session, and many of these participants had little prior lucid-dreaming experience (Carr et al., 2023). This procedure was termed Targeted Lucidity Reactivation (TLR) based on the hypothesis that cues increased lucid dreaming by reactivating the lucid mindset the cues were associated with during wake. The high rates of lucid dreaming produced by TLR suggest that including pre-sleep training, whereby the same sensory cues are repeatedly paired with the act of entering a lucid mindset, may provide a unique advantage over sensory stimulation during sleep alone.

In the study by Carr et al. (2023), lucid-dream induction with the TLR procedure was investigated by comparing participants who received TLR cues during REM sleep to those who received no cues. Accordingly, there are multiple possible explanations for the success: (a) cues increased lucid dreaming by reactivating a lucid mindset, (b) cues increased arousal during sleep, or (c) both. Given that sleep fragmentation has been associated with increased lucid dreaming (Gott et al., 2020; Smith & Blagrove, 2015), this factor may be particularly relevant when combined with cognitive techniques (Stumbrys et al., 2012). Reactivation could occur in various forms—cues could consciously or unconsciously remind participants of the concept of lucidity or their intention to enter a lucid mindset, or perhaps cues could directly increase critical reflection during sleep.

The TLR procedure as first described by Carr et al. (2023) and used by Konkoly et al. (2021, 2024) required at least one experimenter to monitor participants' sleep via polysomnography in order to strategically present cues during REM sleep. This methodology made it possible for the experimenter to titrate stimulus intensity to avoid awakenings while also waiting for a period of stable REM sleep before initiating stimulus presentations. Further, dreamers produced signals to objectively confirm they were lucid dreaming during REM sleep and not just reporting lucid dreams experienced during the transition to wake. Real-time sleep monitoring requires a significant investment of experimenter time and effort, slowing progress on questions about the mechanisms, benefits, and limitations of lucid dreaming and of dreaming more generally. Laboratory procedures are also not very conducive to longitudinal sleep studies.

Here we sought to investigate whether the TLR procedure could be adapted to a home setting using standard consumer devices. The first experiment constitutes a proof-of-concept demonstration that TLR can promote lucid dreams at home without requiring an investigator to conduct polysomnographic sleep-stage monitoring. This experiment did not include a sham control condition, but in Experiment 2 we included comparable cues that had not been used during wake training. The next experiment thus addresses whether there is a distinctive advantage of presenting cues during sleep that were paired with lucidity training before sleep, given the possibility that lucid dreams could conceivably arise solely because of pre-sleep expectations from training or as a nonspecific byproduct of cue-induced arousal.

2. Experiment 1: TLR at home

2.1. Methods

2.1.1. Participants

We recruited a convenience sample of 26 participants via word of mouth, posts to online dreaming groups, and online forums. Participants were invited to participate in the study if they reported owning an Android phone, remembering at least 3–4 dreams per week, sleeping at least 7–8 h per night, and expecting that they would be able to fall back asleep if woken up in the final 2 h of the night. Prior lucid-dreaming frequency was not part of the inclusion criteria. Four participants were excluded due to technical difficulties, and one participant voluntarily dropped out during the study. Two other participants were recruited and consented, but never initiated the study. Altogether a total of 19 participants completed the week-long procedure (ages: $M = 24$ years, $range = 19$ –33). Participants were compensated \$5 per waking half-hour with a bonus for each overnight session for a total of \$70. The experimental procedures were

approved by the Institutional Review Board at Northwestern University, and all experiments were performed in accordance with those guidelines. All participants gave informed consent.

2.1.2. Materials and procedure

Experiment 1 was designed to test whether the TLR procedure could be adapted to induce lucid dreams at home using an Android app. As such, all participants underwent the same 7-day protocol (Fig. 1).

Participants first read instructions that both defined lucid dreaming as a dream in which one is aware of dreaming while remaining asleep and introduced the idea that one could also be in a lucid state of mind while awake. The instructions indicated that regardless of whether they were awake or dreaming, being lucid should be thought of as the ability to think clearly and critically about the experiences of the present moment. Participants were informed that before bed, the app would provide training to become lucid in response to a sound, and that during sleep the sound would be presented again to trigger lucid dreams.

Participants then downloaded the app onto their Android device. The app was programmed using MIT App Inventor (2012, 2020). After downloading, participants answered basic questions about their sleep habits and lucid-dreaming frequency in the week leading up to the experiment (included in [Supplementary Material](#)).

Each night before bed, participants were told to turn off their phone notifications and turn their phone intensity to maximum, intensity being defined as the volume setting on the phone. Participants then placed the phone face-down on their bed beside their pillow. On the first night of the experiment before bed, the app tested participants' auditory perceptual threshold and discrimination threshold (i.e., the intensity at which they could discriminate between the two sounds used in the experiment).

Then, the app guided participants through TLR training to associate a sound (referred to as the TLR cue) with a lucid state of mind,

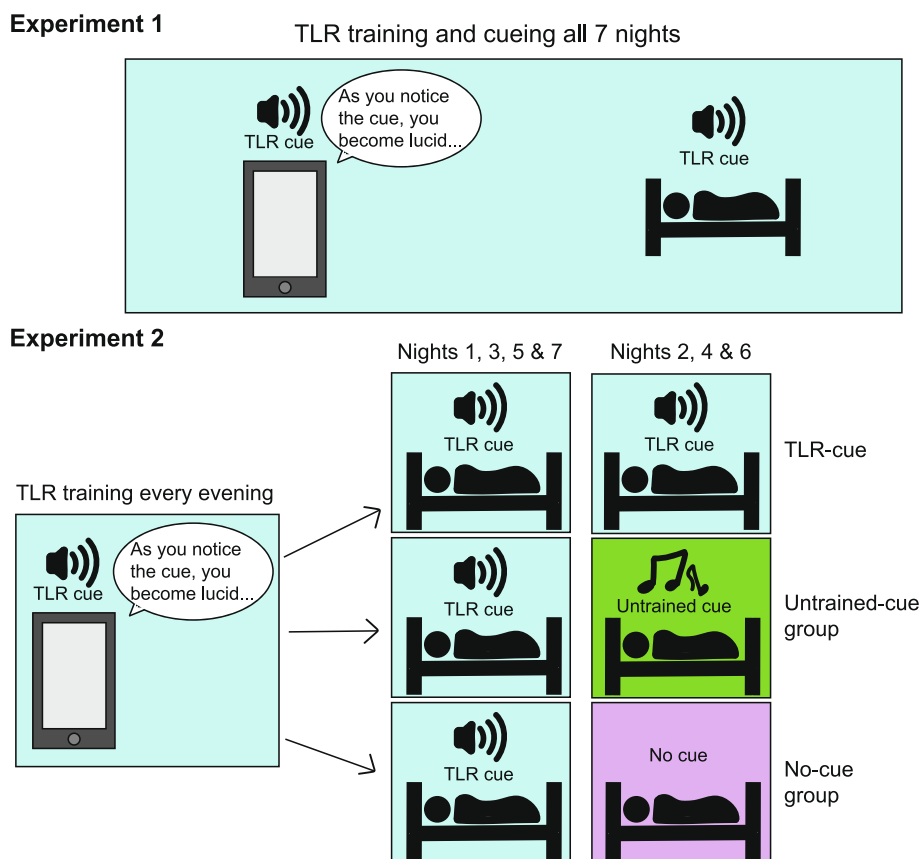


Fig. 1. Design of experiments. In both experiments, participants received training to associate the TLR cue with a lucid mindset each night before bed. In Experiment 1, this same cue was presented to participants during sleep. In Experiment 2, participants received the TLR cue during sleep on nights 1, 3, 5, and 7. On the other nights, participants were assigned to either receive TLR cues, novel cues not associated with lucidity (untrained cues), or no cues.

using the same instructions as Carr et al. (2023). Participants were randomly assigned to a TLR cue of either (a) three pure-tone beeps increasing in pitch (400, 600, and 800 Hz) lasting approximately 650 ms, or (b) a 1000-ms violin sound. Two cue types were used to collect pilot data for future research, but we presented each participant with only one cue type throughout the experiment. The first four times the cue was presented, it was followed by verbal guidance to enter a lucid state as follows.

“As you notice the signal, you become lucid. Bring your attention to your thoughts and notice where your mind has wandered [pause]. Now observe your body, sensations, and feelings [pause]. Observe your breath [pause]. Remain lucid, critically aware, and notice how aspects of this experience are in any way different from your normal waking experience [45-s pause].”

After the first four cues, participants were told that cues would now be presented without the instructions and that they could allow themselves to fall asleep while continuing to practice becoming lucid each time they heard a cue. Over the next 15 min, the TLR cue was played 12 more times at longer intervals (intervals were 45, 70, 55, 65, 70, 80, 65, 60, 75, 75, 90, and 120 s). On nights 2–7, training was abbreviated in that after the first cue, the verbal guidance to become lucid was presented once instead of four times.

Based on a prior finding that sleep interruption 6 h after sleep onset promotes lucid dreaming (Erlacher & Stumbrys, 2020), the app began presenting cues again 6 h after TLR training ended. On night 1, cues began at the intensity at each participants’ perceptual discrimination threshold (i.e., the intensity at which they could distinguish the two cues). Cues were presented every 20–40 s, and each time a cue was presented the intensity increased by 0.16%.

Once cueing started, the app showed a question that participants would see if they picked up the phone: “Did you wake up because of a sound presented by the app?” Three choices were offered: “yes,” “no,” or “I’m not sure.” After a choice was selected, the app advanced to the report form. This form asked participants to describe any dreams or experiences during sleep they could recall, whether they were lucid during their dreams, and whether they heard any cues. Participants also answered five items from the insight subscale of the Lucidity in Dreams questionnaire (Voss et al., 2013; see Supplementary Material for full questionnaire). At the end of the dream report form, participants selected whether they wished to return to sleep or to wake up for the day.

The app also made use of the device’s internal accelerometer to detect movement as a proxy for sleep disruption. As in the laboratory procedure we’ve used (Konkoly et al., 2021, 2024), we sought to avoid cue presentation during disrupted sleep and avoid causing full awakenings, thereby facilitating opportunities for cues to be presented during REM sleep. If the app detected that the participant had moved since the previous cue was presented, cue presentation was paused until 5 min after movement ceased. If the participant picked up their phone and indicated that they had been awoken by a sound from the app, cue presentation was paused until 45 min after participants indicated that they wished to return to sleep. When cueing resumed, it returned to the intensity of the perceptual-discrimination threshold.

After a week of app use, participants answered a survey about their lucid dreams during the study and more generally. The 10-item Lucid Dreaming Skills Questionnaire (LUSK; Schredl et al., 2018) was given to assess inter-individual differences in lucid-dreaming

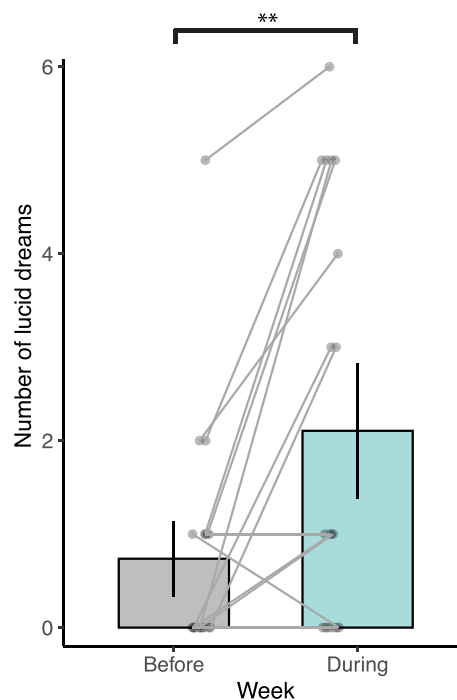


Fig. 2. Reports of lucid dreams were less frequent before the week of app use than during the week of app use in Experiment 1 ($n = 19$). The bar plot compares mean number of lucid dreams for the two periods. Grey lines connect datapoints from the same participant. Error bars represent within-subjects standard error of the mean (** indicates significance with $p < .01$).

ability. Participants rated how often they had various abilities in their lucid dreams, such as full control over their dream body (from 0 = in none, 1 = in a quarter, 2 = in half, 3 = in three quarters, to 4 = in all). Participants also responded to the 21-item Frequency and Intensity of Lucid Dreams (FILD) scale (Aviram & Soffer-Dudek, 2018), which assesses the frequency of lucid dreams and qualities therein such as duration, emotional content, and the amount of control available. The final seven questions assess how often participants engage in deliberate practices to induce lucid dreams. See [Supplementary Material](#) for full questionnaires.

2.1.3. Statistical analysis

We first assessed overall lucid-dreaming prevalence in our sample by computing descriptive statistics as well as participants' mean scores on the LUSK scale. We also calculated the mean number of dream reports collected per night while using the app. Because the primary goal of the first experiment was to determine if lucid dreaming increased during the week of app use, we used a Wilcoxon Signed-Rank test to compare the number of lucid dreams retrospectively reported from the week prior to app use to the number retrospectively reported following the week of app use. Finally, we assessed dream reports to determine how often participants reported becoming lucid because of cues.

2.2. Results and Discussion

2.2.1. Prior lucid dreaming

Our sample reported a relatively high rate of lucid dreaming in the week prior to starting the study ($M = 0.74$ lucid dreams, $SD = 1.24$, $range = 0-5$). The mean LUSK score ($M = 1.75$, $SD = 1.17$, $range = 0-3.4$) was slightly above the average reported in a large online sample ($M = 1.51$, $SD = 0.88$) reported by Schredl et al. (2018).

2.2.2. Increased lucid dreaming with app

On average, participants filled out just over one report form per night while using the app ($M=1.12$, $SD = 0.42$, $range = 0-4$). Fig. 2 shows the frequency of lucid dreaming across weeks based on a comparison of retrospective estimates from the pre-questionnaire and the post-questionnaire. Participants reported significantly more lucid dreams during the week of app use ($M = 2.11$, $SD = 2.23$, $range = 0-6$) than during the week before ($M = 0.74$, $SD = 1.24$, $range = 0-5$; $W = 2.5$, 95% CI [-3.5, -1], $p = .007$). Out of the sample of 19 participants, 14 reported at least 1 lucid dream during the week of app use, whereas only 8 reported having a lucid dream in the week prior.

2.2.3. Lucid dreams from cues

Participants reported having 38 lucid dreams and 80 non-lucid dreams during app use. Of these, 14 of the lucid dreams were reported to have come about due to cue presentation. Table 1 provides examples of these dreams. The 14 cue-provoked lucid dreams came from 7 participants. In 11 of these dreams, lucidity occurred due to cue incorporation in the dream as re-told in the dream report; the others occurred less directly, in one case after a cue awoke the participant, who then returned to sleep for a lucid dream, and in two other cases when a cue produced a slight arousal within a nonlucid dream, which then became a lucid dream.

Regarding our pilot manipulation of presenting beeps versus violin sounds as TLR cues, 6 participants were randomly assigned to receive violin sounds whereas 12 received beeps, and data from 1 participant were missing due to technical failure. There was a qualitatively larger increase in lucid dreaming for those who received violin cues ($M = 0.67$ to $M = 3.16$ lucid dreams in the week prior and during, respectively) than for those who received beeping cues ($M = 0.83$ to $M = 1.67$ lucid dreams in the week prior and during, respectively), though no strong inferences can be made on this basis due to limited sample size.

These results demonstrate that an at-home TLR app can increase lucid-dreaming frequency. However, we cannot rule out the possibility that increased lucid dreaming resulted from nonspecific factors rather than cues reactivating the TLR training mindset. Furthermore, we cannot exclude a bias to report more lucid dreams during app use or a bias to report fewer lucid dreams when estimating the prior-week lucid-dreaming rate. In a second experiment, we used a more rigorous experimental design for testing the extent to which increased lucid dreaming from these procedures can be attributed to TLR cues during sleep versus nonspecific effects such as expectations or increased arousal from sounds. Furthermore, participants were led to believe that they would receive TLR cues

Table 1
Examples of Lucid Dreams Provoked by TLR Cues.

Experiment 1
Other people in the office could hear the cues... I remembered what the sound was as the other people in my dream were trying to figure out what the beeping was.
I seemingly woke up in my normal room, but I noticed the door was open. I heard the cue sound and realized that's not possible because I closed it and locked it. I realized I was dreaming so I went out.
I was working at my job as a lifeguard, and I was getting frustrated because someone's phone kept making noise. I realized I was dreaming when I noticed that the noise going off was the cues on my phone and not the phone of a patron at work.
I went lucid in my dream then almost immediately turned to my phone which was cuing me. I looked at it and it told me it needed to download an update to the lucid app [in my dream]...
Experiment 2
I decided to get up after a long time of listening to the harp sounds and training best I could. I checked my phone and the time read 01:00. But it was morning, and the sun was shining, I thought, and I realized that I was dreaming. I was lucid!
I heard the cue as I was dreaming of walking down a street, someone told me to take my tunic off, I tried to exercise dream control...

during sleep, but they were blind to whether cues were actually presented, such that we could obtain a better estimate of change in lucidity specifically due to cues.

3. Experiment 2: Replication and inquiry into mechanisms of TLR

3.1. Methods

3.1.1. Participants

We made another version of the app available to the public so that anyone interested could participate (if over the age of 18). The app was advertised on social-media platforms, through word-of-mouth, and linked in several online articles about lucid dreaming. Data from this convenience sample were analyzed approximately 6 months after the app was first advertised, once usage had declined, and no interim analyses were performed before this time. Participants were told they could use the app as much or as little as they liked. We considered participants to have completed a night of app use if they completed at least one report form that night, regardless of whether they reported recalling a dream. Reports were reviewed by a manual rater and excluded if they were not in English, if the participant reported not having fallen asleep, or if they contained no information about the participant's dream recall. By this criterion, a total of 416 participants used the app at least once (ages: $M = 36.8$ years, $range = 18-86$). Many participants didn't recall any dream or only recalled a dream on one night (154 recalled 0 dreams, 142 recalled 1 dream). Given that high dream-recall frequency is highly correlated with lucid dreaming (Schredl & Erlacher, 2004), and is often mentioned as a precursor to lucid-dream induction techniques (Adventure-Heart, 2020), we adopted an inclusion criterion that participants were included if they recalled dreams on at least two nights during the study. The number who met this criterion and used the app for the recommended 7 nights was 50. The number who met this criterion and completed at least one night in each condition was 120 (some of whom didn't complete the first or the second night). The number who met this criterion and completed the first two nights was 112.

3.1.2. Materials and procedures

Experiment 2 was designed to further optimize the procedure and test whether any lucid-dreaming increase (of the sort observed in Experiment 1) can be specifically linked with the presentation of TLR cues during sleep. We aimed to enable all participants to have similar expectations and experiences each night by providing TLR procedures on most nights. At the same time, participants were blind to the fact that we intentionally did not present TLR cues to some participants on some nights. As shown in Fig. 1, participants were randomly assigned to one of three groups (TLR-cue group, untrained-cue group, or no-cue group) in a combined between- and within-subjects design.

All participants received TLR training before sleep and TLR cues during sleep on the first night. In the TLR-cue group, this same procedure from night 1 was also used on the next 6 nights. The TLR cue presented during sleep was the same cue used during training, as in Experiment 1. In the other two groups, TLR was delivered normally only on nights 1, 3, 5, and 7 (TLR-cued nights). In the untrained-cue group, on nights 2, 4, and 6 participants received the cue sound they heard during the perceptual discrimination test but not during training. In the no-cue group, participants received no cues on nights 2, 4, and 6. Dream reports and post-sleep questionnaires were the same as those used in Experiment 1.

The app was also updated for Experiment 2 to improve the user experience based on the first experiment. In Experiment 1, the app was designed to continue increasing cue intensity until participants woke. Several participants noted that the cue sometimes woke them up like an alarm clock. In Experiment 2, if participants indicated in a dream report that a sound was incorporated into their dream or triggered a lucid dream, we capped the cueing intensity at that level. Additionally, we replaced the sounds in favor of two distinct harp melodies, with intensity gradually fading in and out, each lasting approximately 3 s. If movement was detected, we paused cueing for 1 min rather than 5 min. These changes were intended to promote incorporation of cues into dreams and avoid awakening participants. Finally, participants were able to opt in to a mode of the app in which the cue intensity increased more slowly (0.08% instead of 0.16%) and the maximum intensity was set to the participant's discrimination threshold, with cueing halted for the night if an arousal was detected. The optional mode was not used frequently (15–17% of nights across all groups).

3.1.3. Statistical analysis

As in Experiment 1, we assessed prior lucid dreaming with the LUSK questionnaire and by calculating the average number of lucid dreams in the week prior to the study (Schredl et al., 2018). To understand the extent to which the app produced sleep disruption, we also calculated the mean number of reports collected per night while using the app. Then, we looked at how many times the app paused due to motion detection following a cue. We used a Wilcoxon signed-rank test to compare the prevalence of motion detection following TLR cues versus equivalent periods of silence in the no-cue group.

To understand the impact of cueing, we first collapsed across the two control groups to compare the proportion of nights with lucid dreams between groups (TLR-cue group, control groups) across night conditions (TLR-cued nights or cueing-condition nights) by running a 2x2 ANOVA on a linear mixed model with participant ID as a random intercept (*lmer* function in R). In follow-up t-tests using the Tukey method to adjust for multiple comparisons, we separately considered each group (TLR-cue group, untrained-cue group, and no-cue group) to assess the within-subject effect of nights (*emmeans* and *pairs* functions in R). To further assess how groups differed from one another across conditions, we repeated these analyses (2×2 ANOVAs on linear mixed models with group, night condition, and their interaction as predictors) considering only data from two of the three groups at a time (TLR-cue group versus untrained cue group, untrained-cue group versus no-cue group, and TLR-cue group versus no-cue group). In case of a significant interaction, we computed Tukey-corrected t-tests to assess group differences in lucid dreaming on odd and even nights. One additional analysis

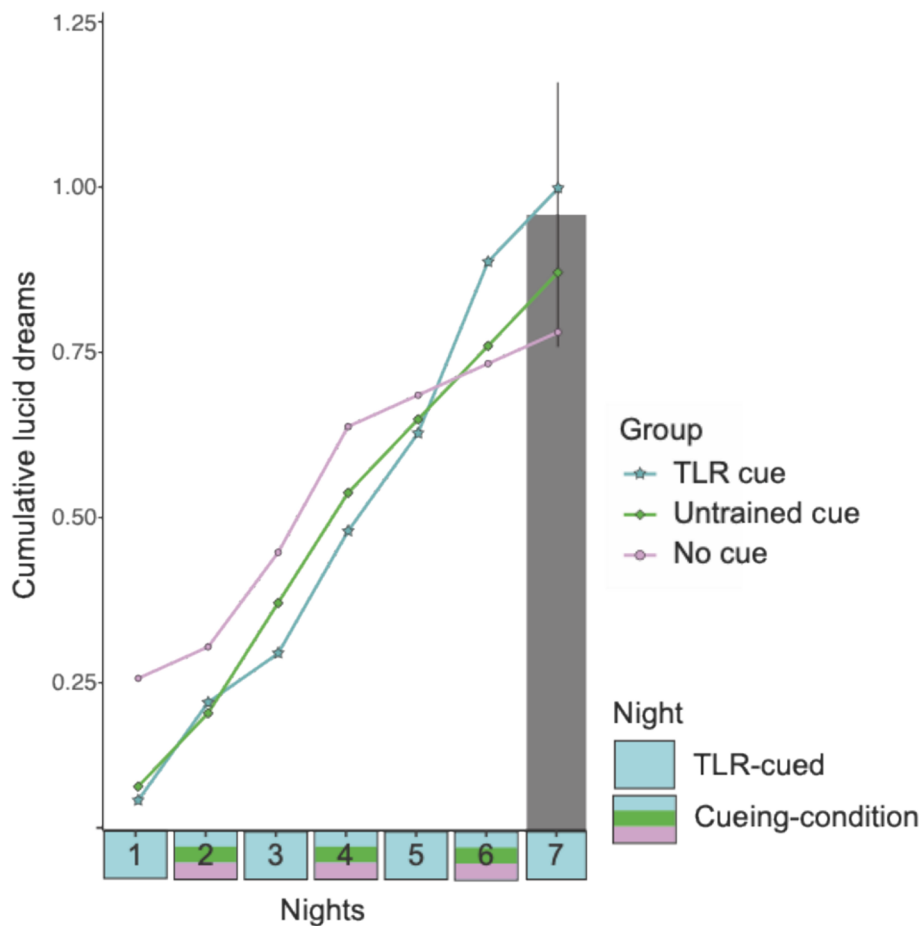


Fig. 3. Lucid-dream frequency during app use in Experiment 2 ($n = 50$). The y-axis represents the cumulative number of lucid dreams across the 7 nights of app use. The TLR-cue group received TLR cues on 7 nights, whereas the two other groups received TLR cues on 4 nights and a control condition on the other 3 nights, as indicated by the color code. The bar represents the average number of lucid dreams throughout the week, combining all groups. The error bar indicates the standard error of the mean.

included participants who completed at least one night in each condition, even if they recalled no dreams or only recalled a dream on one night ($n = 236$).

We also analyzed lucid dreaming over only the first two nights of the experiment to further understand the impact of TLR cues relative to the control conditions. This allowed us to equalize the number of observations from each participant while still maintaining a large sample size ($n = 112$). In contrast, an analysis of data from all participants across as many nights as available was subject to a potential drop-out bias (e.g., some subjects experiencing early success and then electing to use the app for longer than other subjects). We collapsed across control groups and performed a logistic regression (*glm* function in R) predicting lucidity (lucid vs. nonlucid dream) by night (1 vs. 2) and group (TLR-cue group vs. combined control groups). We then performed follow-up t-tests with the Tukey method to adjust for multiple comparisons (*emmeans* and *pairs* functions in R) to test whether lucid-dreaming frequency changed across nights within each group as well as whether lucid-dreaming frequency on night 2 varied by group. To further clarify how groups differed from one another across night conditions, we also repeated this analysis considering data from only two of the three groups at a time.

Additionally, we tested whether participants experienced fewer lucid dreams with the optional mode, when cues were presented at a lower intensity. We computed a linear mixed model testing whether the proportion of nights with lucid dreams was predicted by the proportion of nights with the optional versus standard mode, with participant ID as a random intercept.

3.2. Results and Discussion

3.2.1. Prior lucid dreaming experience

Participants who used the app at least one night in each condition ($n = 120$) retrospectively reported experiencing an average of 0.79 lucid dreams in the week prior to the study ($SD = 1.37$, $range = 0-7$). Scores from the LUSK questionnaire of lucid dreaming skills

were only available for 23 participants, as there was no incentive for completing questionnaires at the end of 7 days of app use. Scores were similar to those in Experiment 1 ($M = 1.72$, $SD = 0.64$, $range = 0-3$).

3.2.2. Number of dreams reported and sleep disruption from cues

On average, participants who completed at least one night in each condition filled out just over one report per night while using the app ($M = 1.17$, $SD = 0.48$, $range = 1-5$). Accelerometer data was available from 26 of the 40 participants in the no-cue group who completed at least one night in each condition. Per cue, these participants experienced more sleep disruption after TLR cues ($M = 0.19$, $SD = 0.15$) than after no cues ($M = 0.1$, $SD = 0.1$; $W = 59$, 95% CI [.02, 0.14], $p = .002$).

3.2.3. Lucid dreaming across a full week of app use

Fifty participants used the app for the recommended 7 nights (22 in TLR-cue group, 11 in untrained-cue group, 17 in no-cue group). The cumulative number of lucid dreams throughout the 7 days of app use is visualized in Fig. 3. Out of the sample of 50 participants, 23 reported at least 1 lucid dream during the week of app use ($M = 0.97$, $SD = 1.41$, $range = 0-6$), and 19 reported having a lucid dream in the week prior ($M = 0.74$, $SD = 1.17$, $range = 0-5$). See Table 1 for examples of lucid dreams provoked by cues.

3.2.4. Lucid dreaming across all nights between groups

Results from all nights, including participants who did not complete the full 7 nights, are shown in Fig. 4. Many more observations were included in this analysis compared to the results shown in Fig. 3. However, the number of nights completed varied across participants (e.g., $n = 113$ who used the app on night 1, $n = 50$ who used the app on night 7), which complicates comparisons across nights. Nevertheless, collapsing across control groups, we found an interaction indicating across-group differences (TLR-cue group/combined-control group $\times$ odd/even night interaction, ($\beta = 0.22$, $F(1,118) = 6.28$, $p = .01$), with no main effects of odd/even night condition ($\beta = 0.006$, $F(1,118) = 0.17$, $p = .68$) or group ($\beta = 0.006$, $F(1,118) = 0.17$, $p = .68$). Follow-up analyses showed patterns that varied within each group: in the untrained-cue group, fewer lucid dreams on untrained-cue nights than TLR-cue nights ($n = 39$, $t(117) = -2.25$, $p = .03$), in the no-cue group, no difference across no-cue and TLR-cue nights ($n = 40$, $t(117) = -0.3$, $p = .77$), and in the TLR-cue group, no difference across odd and even nights ($n = 41$, $t(117) = 1.8$, $p = .07$). Including participants who did not remember any dreams or who only remembered one while using the app would likely have diluted any differences, and indeed there were no significant effects when repeating the above analysis including those 116 additional participants ($ps > .3$).

When comparing just the TLR-cue and untrained-cue groups, there was no main effect of night condition ($\beta = 0.004$, $F(1,78) = 0.11$, $p = .73$) or group ($\beta = 0.03$, $F(1,78) = 0.78$, $p = .38$), but a significant interaction between night condition and group ($\beta = 0.29$, $F(1,78) = 7.68$, $p = .007$), reflecting that these groups differed in the proportion of lucid dreams on even nights ($t(142) = 2.3$, $p = .02$) but not odd nights ($t(142) = -0.91$, $p = .36$). There were no significant effects or interactions when comparing just the TLR-cue and no-cue groups, or when comparing the untrained-cue and no-cue groups ($ps > .05$).

3.2.5. Lucid dreaming incidence on the first two nights

Given that many participants did not continue the procedure for the full 7 nights of the protocol, we conducted an analysis focusing on the first two nights of the study, which thus included 112 participants (Fig. 5), including 40 participants in the TLR-cue group, 35 participants in the untrained-cue group, and 37 participants in the no-cue group. Again, the initial analysis was conducted with the two

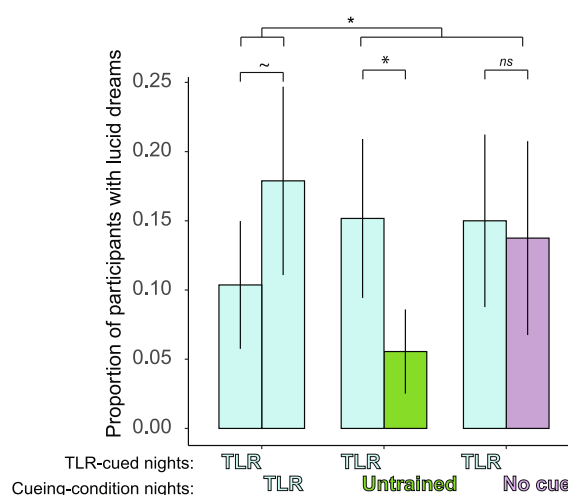


Fig. 4. Lucid dreaming across all nights between groups in Experiment 2 ($n = 120$, 4.98 nights per participant on average). The y-axis shows the average proportion of nights containing a lucid dream compared to the total number of nights completed in each night condition. Bars are colored according to whether the data were from TLR-cued nights 1, 3, 5, and 7 or cueing-condition nights 2, 4, and 6. Note that the untrained-cue and no-cue control groups were combined in the statistical analysis of across-group effects reported in section 3.2.4. Error bars indicate within-subjects standard error of the mean (* indicates p -values < .05 and ns indicates p -values > .05).

control groups combined, to yield maximal statistical power. This analysis revealed a significant interaction, indicating that the effect of night varied by group ($\beta = -1.22$, 95% CI [-2.43, -0.07], $p = .04$), with no main effect of night ($\beta < 0.001$, 95% CI [-0.82, 0.82], $p > .99$) or group ($\beta = -0.06$, 95% CI [-0.77, 0.69], $p = .87$).

Follow-up comparisons showed that the rate of lucid dreaming declined in the combined control groups ($OR = 3.4$, $p = .004$), but not the TLR group ($OR = 1$, $p = 1.0$). Further, the TLR-cue group and combined control groups differed on night 2 ($OR = 3.61$, $p = .006$) but not night 1 ($OR = 1.06$, $p = .87$). When analyzing the untrained-cue and no-cue control groups separately, lucid dreaming on night 2 declined significantly in the untrained-cue group ($OR = 5.67$, $p = .03$), and nonsignificantly in the no-cue group ($OR = 2.64$, $p = .06$). When comparing each pair of groups separately, there were no main effects of night or condition and no interactions ($ps > 0.1$).

3.2.6. Optional mode

We also checked that these results were not due to participants using the optional mode differently between groups, given that it involved a lower intensity of cues. The optional mode was used infrequently by all groups, and the proportion of nights it was used did not predict lucid dreaming occurrence ($\beta = 0.007$, $t(236) = 0.14$, $p = .89$). It was used on 36 nights (14.7% of nights) in the TLR-cue group, 31 nights (16.5%) in the untrained-cue group, and 24 nights (14.6%) in the no-cue group.

4. Discussion

Whereas TLR can induce high rates of lucid dreaming in the sleep laboratory (Carr et al., 2023; Esfahani et al., 2024; Konkoly et al., 2021, 2024), here we report initial attempts to make this method available for home use with a smartphone application. The procedure was easy for individuals to adopt on their own, which was advantageous for obtaining data from many participants. We anticipate that further improvements in the methodology will be possible in the future.

The present procedure had two parts: (1) a distinctive sound cue was paired with the act of entering a lucid mindset before sleep, and (2) cues were presented again during sleep to promote lucid dreams. Whereas prior studies attempted to produce lucid dreaming with a combination of pre-sleep training and cues during sleep (e.g., Kumar et al., 2018; reviewed in Stumbrys et al 2012) here the pre-sleep training revolved around the same cue used during sleep (Carr et al., 2023; Erlacher, Schmid, Bischof, et al., 2020; Erlacher, Schmid, Schuler, et al., 2020; Konkoly et al., 2021; Schmid & Erlacher, 2020), which differentiates TLR from the other procedures. The procedure did not include any home sleep monitoring, but instead relied on the assumption that a likely time for lucid-dream induction might begin 6 h after pre-sleep training ends. As discussed below, an obvious improvement would be to trigger cues only when an individual is in REM sleep. In Experiment 1, we demonstrated that using the app increased lucid dreaming. In Experiment 2, we tested the hypothesis that cues increased lucid dreaming by reactivating a pre-sleep association between the cue and the act of entering a lucid mindset. Whereas participants may not accurately assess lucid-dreaming frequency over the week prior to the start of the

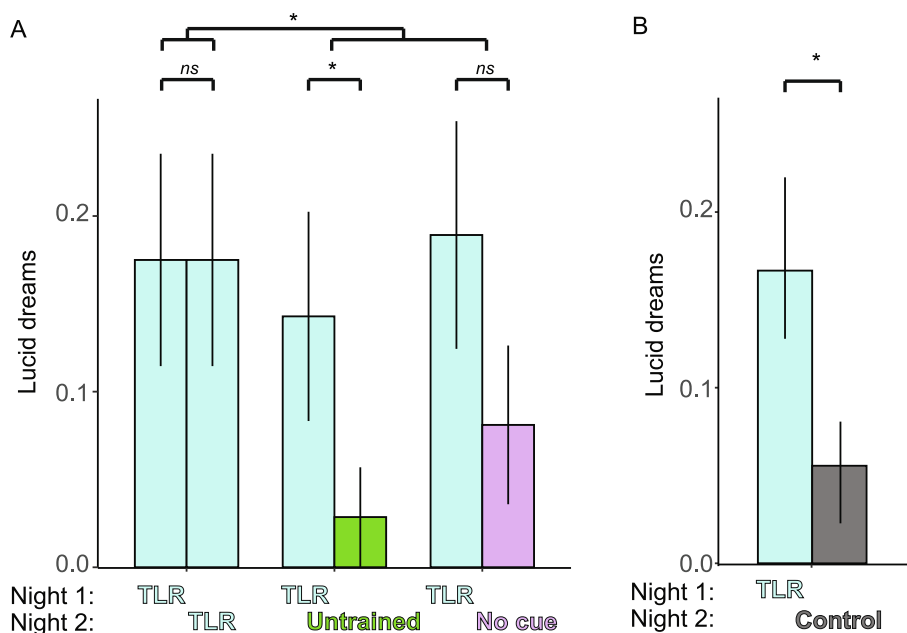


Fig. 5. Lucid dreaming on nights 1 and 2 in Experiment 2 ($n = 112$, 2 nights per participant). A) Shows the proportion of participants who had lucid dreams on each night, including night 1 on which all participants received the TLR cue and night 2 on which some participants received the TLR cue, some received the untrained cue, and some received no cue. B) Data from only the two control groups are shown again, but collapsed together for night 1, when lucid dreaming was more prevalent, and for night 2, when lucid dreaming was less prevalent. Error bars represent the within-subject standard error of the mean (* indicates p -values $< .05$ and ns indicates p -values $> .05$).

experiment, Experiment 2 included robust comparisons between TLR versus balanced control conditions — comparisons that were immune from demand characteristics because participants did not know about the control conditions.

In the study by Carr et al., 2023, the TLR procedure increased lucid dreaming based on comparisons to a control group receiving no cues during sleep. The resulting improvement in lucid dreaming was impressive, but there are two possible causes for this high level of effectiveness. The operative factor could have been either reactivating a lucid mindset during sleep or causing a nonspecific increase in arousal due to sound presentations. Here, we found that novel sounds were less effective for inducing lucid dreaming than cues paired with a lucid mindset before sleep. Importantly, these two sounds were counterbalanced across conditions (TLR-cue vs. untrained cue), such that we can exclude any stimulus-specific effects when comparing the two conditions.

The findings from these two closely matched conditions thus support the hypothesis that TLR promotes lucid dreaming at least in part by reactivating a lucid mindset. One reason to be cautious about this interpretation, however, is that in the no-cue group the number of lucid dreams on TLR-cued nights was not significantly greater than the number on no-cue nights. There was a small increase when considering only the first two nights but not when considering all nights. One possible explanation is that no-cue nights may have been especially good opportunities for natural lucid dreaming, which tends to occur late in the sleep cycle (LaBerge et al., 1986). Participants may have become more likely to turn off the app after being awoken from a cue as the experiment progressed, precluding their ability to report subsequent dreams, including lucid ones. Also, TLR cues on night 1 combined with pre-sleep training could have increased the chances of spontaneous lucid dreaming on no-cue nights. It is important to stress that participants in the two control groups (untrained-cue and no-cue) received the TLR procedure on nights 1, 3, 5, and 7. This procedure allowed for a strong comparison on night 2 particularly because expectations for lucid dreaming were matched across all participants. On the other hand, a control condition with no TLR nights at all would likely have produced much lower rates of lucid dreaming and much higher rates of attrition. Arguably the most compelling results are that the frequency of lucid dreams remained high on nights 1 and 2 in the TLR-cue group, whereas it declined considerably from nights 1 to 2 in the two control groups (Fig. 5). Additionally, in both the analyses of all nights and only the first two nights, TLR cues increased lucid dreaming compared to control groups combined (Figs. 4 and 5).

This study demonstrates the feasibility of using sound-cue reactivation to modify dream content in a home setting. Whereas implementing TLR in the lab requires advanced technology and a significant time investment from at least one experimenter as well as the participant, at-home studies can be implemented more efficiently on a wider scale. This advance is advantageous scientifically because at-home studies are well-suited for investigating longitudinal effects, as well as for evaluating how tweaks to the procedure could further increase lucid dreaming or reveal mechanisms of lucid-dream induction. Receiving the TLR cue every night appeared to promote learning over time (Fig. 3), suggesting that regular training with TLR cues during sleep beyond 7 days may further improve this method's effectiveness.

Here, we found that linking sound cues with pre-sleep cognitive training boosted the effectiveness of those same cues (compared to other cues) in inducing lucid dreams when presented during sleep. It is interesting to consider whether TLR promoted lucid dreaming in a way more akin to asking participants to consciously recognize cues as lucidity signals in a dream, or whether cues primed participants nonconsciously to become lucid, perhaps by partially restoring metacognition during sleep. In future studies, detailed phenomenological reports of experiences, both during dreams and during training, could help characterize the extent to which cues reproduced the mindset experienced during training (Demšar & Windt, 2022). Perhaps the association between the cue and lucidity can effectively provoke lucid dreaming even if it does not fully engage all the qualities of a lucid mindset. Varying the prompt used in training, for instance by focusing on engaging cognitive control, could also offer a window on whether sensory cues can be used to engage specific cognitive functions during dreams. Future studies could also collect real-time responses from dreamers to ascertain whether sounds were heard by participants while still in the midst of a dream (Konkoly et al., 2021).

The benefits of TLR were apparent in the 7-night analysis in Experiment 2 when restricted to data from participants who reported having dreamt on at least 2 nights during the study, whereas we did not find statistically significant effects when including the participants who seldom reported a dream. The results from those with low dream-recall may be telling. People seeking to experience more lucid dreams with TLR or other methods may benefit from improving their dream recall through techniques such as dream journaling (Paulsson & Parker, 2006; Tan & Fan, 2023). Frequent dream recall is often considered essential for a successful lucid-dreaming practice (Adventure-Heart et al., 2017; Tan & Fan, 2023) and is positively correlated with lucid-dreaming frequency (Schredl & Erlacher, 2004; Shafiei, 2019). It is also helpful for an individual to remember their lucid dreams in order to report them. Whereas in Experiment 1 we recruited participants who remembered at least 3–4 dreams per week, in Experiment 2 the app was available to anyone who wished to use it, and thus we focused our analyses on participants who remembered at least two dreams during the study. Various self-selection factors that influenced participation in Experiment 2, along with variations in the cueing procedure, such as surreptitiously including nights without operative cues, may explain the reduced overall effectiveness of the app in Experiment 2 compared to in Experiment 1.

The benefits of TLR were demonstrated most clearly when considering only the first two nights, in which there was one observation per night per participant (Fig. 5). In our initial analysis, in contrast, each participant completed varying numbers of nights in each condition (Fig. 4), and thus we compared the proportion of nights with a lucid dream relative to the total number of nights completed in each condition. However, this analysis had more sources of variability given the progressive participant drop-out during the study and high degree of variation in lucid-dreaming rates across subjects. Future work could provide more incentives to retain participants or employ shorter experimental protocols to continue assessing the degree to which TLR promotes lucid dreaming.

Further studies could also test whether variations of the procedure change effectiveness. Using methods to randomly assign participants to experience certain types of dream content (e.g., lucidity) could make it more feasible to scientifically assess whether specific dream content incurs benefits for waking functioning (Mallett et al., 2024). Many studies on the benefits of lucid dreaming include confounds with pre-sleep expectations (e.g., enthusiasm about lucid dreaming could increase both induction success and

benefits) or with techniques used to induce lucid dreams that are independently beneficial (Konkoly & Burke, 2019). For instance, dream journaling is a part of many lucid dreaming interventions but may have its own therapeutic benefits that confound the effects of lucid dreaming alone. Therefore, randomly assigning participants to experimental conditions that differ based on cues during sleep but not on pre-sleep instructions could enable better experimental designs by avoid such confounding factors.

A limitation of this study arose because we aimed to induce lucid dreams with minimal sleep monitoring, such that we could not verify lucid dreams or sleep stages objectively. As such, we relied on participants' own responses to the question of whether they were lucid dreaming. This was not always immediately evident in their written description of the dream, and it would have been interesting to have included additional questions in the dream report to assess the relationship between reported lucidity and dream content in finer detail (Mallett et al., 2021). Stronger evidence for lucid dreaming can be obtained with the combination of dream reports and lucidity signals during polysomnographic-verified REM sleep (Konkoly et al., 2021; LaBerge et al., 2018b; LaBerge et al., 1981). Nevertheless, our evidence for lucid dreaming in Experiment 2 is not likely to reflect differential bias across groups because participants were blind to which cueing procedure they would receive each night. They were also blind to the fact that those procedures differed systematically in different groups of participants. Therefore, differential demand characteristics cannot explain differences between groups.

The present methods did not allow polysomnographic sleep staging as in laboratory-based studies of lucid dreaming. Relatedly, a significant limitation was the difficulty in setting the ideal cue intensity, given the unknown background sound levels in participants' homes and different arousal thresholds in each stage of sleep (Ermis et al., 2010). In the first study, the app progressively increased in intensity until movement occurred, whereas in the second we capped the intensity if the participant reported dream incorporation of a cue. Our goal was to maximize the chance that some cues would be presented at the ideal intensity for cue incorporation in a REM-sleep dream. A consequence of this aspect of the methodology is that participants were often awoken by cues. Indeed, the app paused due to motion detection significantly more after cues compared to equivalent periods in the no-cue group. Whereas here we aimed to test the efficacy of using only a smartphone application, future studies could seek to reduce sleep disruption by monitoring sleep with wearable technology. For instance, sleep stages could be targeted more precisely by combining a smartphone application and commercially available wrist-worn devices for monitoring heart rate and movement (Whitmore et al., 2022). Minimizing sleep disruption due to cues will be an important step forward for improving users' experiences and reducing negative side-effects associated with disturbed sleep. Indeed, some users may decide it is not worth it to sacrifice restful sleep for the sake of lucid dreams.

Another limitation is that participants were not randomly selected from the general population; a subset were recruited through advertisements to lucid-dreaming interest groups and media articles on lucid dreaming. As such, prior lucid-dreaming experience was higher than might otherwise be expected, which may have affected participant retention or success in the experiment. Many participants in Experiment 2 did not complete the study, and those who did may have been those who were more successful when using the app early on. However, our analysis of just the first two nights confirms that differences between groups were not due to differential drop-out rates between groups. Whereas participants who were initially unsuccessful in lucid dreaming may have been more inclined to drop out over subsequent days, it is notable that lucid dreaming increased throughout the week of app use in Experiment 1, in which few participants dropped out. In sum, we took a first step towards translating the laboratory method of TLR into a smartphone application for individuals to use on their own, and we showed that cues were most effective when paired before sleep with the intention to enter a lucid mindset.

CRedit authorship contribution statement

Karen R. Konkoly: Writing – original draft, Visualization, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Nathan W. Whitmore:** Writing – review & editing, Software, Resources, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Remington Mallett:** Writing – review & editing, Visualization, Investigation, Formal analysis, Conceptualization. **Christopher Y. Mazurek:** Writing – review & editing, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Ken A. Paller:** Writing – review & editing, Supervision, Resources, Methodology, Funding acquisition, Conceptualization.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request. The app used in this research is under continual development and the latest version can be found in the Google Playstore (Whitmore, 2023).

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Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.concog.2024.103759>.

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