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Cognitive control training for urgency: A pilot randomized controlled trial in an acute clinical sample

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ABSTRACT

Urgency – rash action in the context of strong emotion – is a facet of impulsivity closely related to many psychological disorders. Deficits in working memory and response inhibition are potential mechanisms underlying urgency, and a previous study showed that cognitive training targeting these domains is efficacious in reducing urgency. However, the feasibility and efficacy of this intervention has not yet been tested in a clinical sample or naturalistic treatment setting. To fill this gap, we conducted a pilot study of cognitive training for individuals reporting high levels of urgency in a partial hospitalization program. We evaluated this intervention in an open trial ($n = 20$), followed by a randomized controlled trial ($n = 46$) comparing cognitive training plus treatment as usual to treatment as usual. Results supported the feasibility and acceptability of cognitive training. Participants in the training group showed significant improvement on cognitive tasks, but groups did not differ in urgency. In pooled analyses combining the open trial and RCT, there was a significant reduction in distress intolerance in the training group only. Results indicate the potential benefit of cognitive training for distress intolerance, but do not support the use of cognitive training for urgency in acute clinical settings.

The study conducted in the RCT phase of this manuscript is registered on ClinicalTrials.gov (NCT: NCT03527550). The full trial protocol is available on ClinicalTrials.gov.

1. Introduction

Many psychological disorders are characterized by impulsivity, but one particular facet of this trait, "emotion-related impulsivity," is of particular relevance for a diverse range of psychological disorders (Carver & Johnson, 2018). Much of the research on emotion-related aspects of impulsivity has focused on the traits known as negative and positive urgency (impulsive action during negative and positive emotions, respectively; Whiteside & Lynam, 2001; Cyders et al., 2007). Across multiple samples, settings, and methods, elevated scores on measures of urgency are robustly related to psychological symptoms and disorders, predict the onset and course of psychological disorders, and demonstrate the strongest relations to symptoms in comparison to other impulsive traits (Berg, Latzman, Bliwise, & Lilienfeld, 2015; Carver & Johnson, 2018). Moreover, studies conducted in naturalistic treatment settings show that baseline levels of urgency prospectively predict significantly worse treatment outcomes (Hershberger, Um, & Cyders, 2017; Peckham, Forgeard, Hsu, Beard, & Björgvinsson, 2019). Despite the widespread evidence for urgency as a significant factor underlying psychological disorders and treatment response, development or tailoring of specific treatment approaches for this aspect of impulsivity remain critically understudied (Um, Hershberger, Whitt, & Cyders, 2018).

Some evidence for putative mechanisms of urgency has emerged in recent years. Across a number of studies, heightened scores on the urgency scales correspond to deficits in prepotent response inhibition (the ability to withhold or cancel a behavioral response), particularly in clinical samples (Cyders & Coskunpinar, 2011; Johnson, Tharp, Peckham, Sanchez, & Carver, 2016). Importantly, these deficits are apparent regardless of affect: that is, deficits in response inhibition are present

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with or without the use of a mood induction beforehand, and are present both in tasks that use affective stimuli and those that do not (Allen & Hooley, 2019; Johnson et al., 2016). This suggests that basic deficits in inhibitory control may be a key cognitive trait underlying urgency (Johnson, Elliott, & Carver, 2020).

Additional evidence suggests that working memory deficits may also be a potential mechanism underlying urgency. Measures of inhibition are correlated with working memory performance (Miyake & Friedman, 2012), with successful response inhibition linked to the maintenance and updating processes that are key to working memory (Redick, Calvo, Gay, & Engle, 2011; Simmonds, Pekar, & Mostofsky, 2008). Not surprisingly, some evidence shows that working memory and inhibition are both relevant to urgency and related behaviors: working memory moderates the link between urgency and inhibition deficits (Gunn & Finn, 2015), and urgency is linked to lower performance on working memory tasks when distractors are present (Canale et al., 2019). Together, these studies suggest that response inhibition and working memory, either interactively or independently, are important mechanisms driving emotion-based impulsive behavior.

Based on these findings, a previous study tested a two-week cognitive training intervention targeting inhibition and working memory, which yielded significant reductions in urgency compared to a waitlist (Peckham & Johnson, 2018). However, the generalizability of this study was limited given its focus on a non-clinical sample (undergraduate students and community members with elevated urgency). Meta-analytic studies show that the correlation between inhibition deficits and urgency is significantly stronger in clinical vs. non-clinical samples (Johnson et al., 2016), yet no study has assessed whether inhibition training for urgency is feasible in clinical samples. Consistent with recent calls to test cognitive training paradigms in real-world settings (Harvey, McGurk, Mahncke, & Wykes, 2018), an important next step is to test the extent to which benefits of cognitive training are apparent in people with psychological disorders. Thus, a primary goal of the present study was to replicate cognitive training findings among a sample of adults seeking treatment for acute psychiatric symptoms. We specifically tested this intervention among people reporting high levels of urgency upon admission to treatment to maximize potential benefits of the intervention.

Beyond replicating the effects of Peckham and Johnson (2018) in a clinical sample, another goal of the present study was to test the extent to which the effects of combined working memory and response inhibition training also improves other affective vulnerability factors. Deficits in response inhibition and working memory are not specific to urgency and are present across a variety of psychological disorders (Snyder, Miyake, & Hankin, 2015). Not surprisingly, interventions that target these cognitive processes often yield improvements on emotion-relevant outcomes that rely on the same mechanisms, such as improvements in emotion regulation (Hoorelbeke & Koster, 2017; Peckham & Johnson, 2018; Siegle et al., 2014).

However, other recent studies testing the impact of cognitive training on emotion regulation have resulted in improved cognitive performance, but also showed null findings on affective outcomes such as adaptive emotion regulation strategies (Van den Bergh et al., 2020; Vanderhasselt, Demeyer, Van Imschoot, Hoorelbeke, & De Raedt, 2021), indicating the need for more specific hypotheses about which domains are most likely to be impacted by training working memory or response inhibition. One particularly relevant domain is distress intolerance (the inability to withstand negative emotions and sensations). Distress intolerance is conceptually related yet distinct from urgency (Juarascio et al., 2020), and may share some of the same underlying cognitive mechanisms, including difficulties with response inhibition (Ledgerwood, Alessi, Phoenix, & Petry, 2009; Macatee et al., 2018) and working memory (Fitzgerald, Gorlin, & Otto, 2021; Otto et al., 2016). Thus, in the present study, we tested the hypothesis that the cognitive training intervention would also result in reductions in distress intolerance, given these shared mechanisms.

2. Aims and hypotheses

The overall goal of this study was to assess the feasibility and acceptability of cognitive training as an intervention for urgency, in an acute treatment setting for adults with psychological disorders. To assess feasibility, we set an *a priori* benchmark of at least 75% of training sessions completed; regarding acceptability, we predicted that participants in the training group would report at least moderate levels of satisfaction on an exit survey. The second aim of this study was to assess the efficacy of this intervention, with the primary outcome being change in urgency. We specifically tested the hypothesis that participants receiving combined response inhibition/working memory training in addition to treatment as usual would show greater reductions in negative and positive urgency, in comparison to treatment as usual. As secondary hypotheses, we aimed to confirm the mechanisms of this intervention: we hypothesized that participants in the training condition would show greater improvement than the control condition on two response inhibition tasks and two working memory tasks. Change in distress intolerance was assessed as an additional test of the intervention's efficacy. Consistent with other cognitive training studies, we also predicted that participants assigned to the cognitive training group would show significant linear improvements in their performance on the training tasks.

3. Methods

This study was approved by the local Institutional Review Board and all participants provided written informed consent for study procedures, in addition to written informed consent to allow for clinical records (e.g., primary diagnosis) to be analyzed for research purposes. Participants were compensated for completion of the baseline and post-treatment assessments, but were not compensated for completion of training sessions. The study included an open trial phase and a randomized phase; the study conducted in the RCT phase was registered on ClinicalTrials.gov (NCT: NCT03527550).[1]

3.1. Study setting

This study was conducted at a partial hospital program (PHP) in the Northeastern United States. This PHP is a short-term day program for adults (18+ years) offering pharmacotherapy, case management, and Cognitive Behavioral Therapy (CBT) skills delivered in group and individual settings. Treatment length lasts seven to 10 days, on average. During that time, patients participate in up to five didactic groups per day, covering topics such as fundamentals of CBT, behavioral activation, and emotion regulation. Patients treated at the PHP are referred from inpatient or outpatient levels of care, and primary diagnoses often include mood, anxiety, and personality disorders (for details about treatment, see Forgeard, Beard, Kirakosian, & Björgvinsson, 2018).

3.2. Study design and procedures

Patients at the PHP were screened for participation on the basis of their scores on the short Negative Urgency and Positive Urgency scales, completed on the day of admission to the PHP (see Measures). Those patients who scored in the upper range on either or both of these measures (operationalized as greater than or equal to an average score of 3.0 on a 4-point scale) were considered for further study participation. On average, participants scored higher on Negative Urgency (open trial $M = 3.17$, SD = 0.34; RCT $M = 3.28$, SD = 0.39) than the Positive Urgency scale (open trial $M = 2.16$, SD = 0.92; RCT $M = 2.36$, SD = 0.86). In both

[1] The study protocol for the RCT phase included several other measures that were not analyzed for the present manuscript; these measures are described on ClinicalTrials.gov.

phases of the study, these inclusion criteria captured participants who met criteria based on their positive urgency scores alone (10.5% of the open trial sample, 8.9% of the RCT sample), those who met criteria based on their negative urgency score (68.4% of the open trial, 68.9% of the RCT), and those who met criteria on both subscales (21.1% open trial, 22.2% RCT). Potential participants were approached by research staff on either the second or third day of PHP treatment. Prior to approaching potential participants, members of research staff consulted with clinical staff members to ascertain eligibility criteria. Given the transdiagnostic nature of the research question, patients were recruited without regard to primary diagnosis; however, potential participants were excluded if they displayed acute symptoms of mania or acute psychosis that could potentially interfere with study participation (as reported by clinical staff), or if clinical staff had concerns about other acute symptoms that could potentially interfere with study participation. In addition, those participants who reported current ECT treatment, a history of traumatic brain injury, or other significant neurological symptoms were excluded (see Figs. 1 and 2). Participants meeting all enrollment criteria were invited to attend a baseline session later that same day. All tasks and measures conducted at the baseline session were repeated at a post-treatment session on the patient's final day of partial hospital treatment.

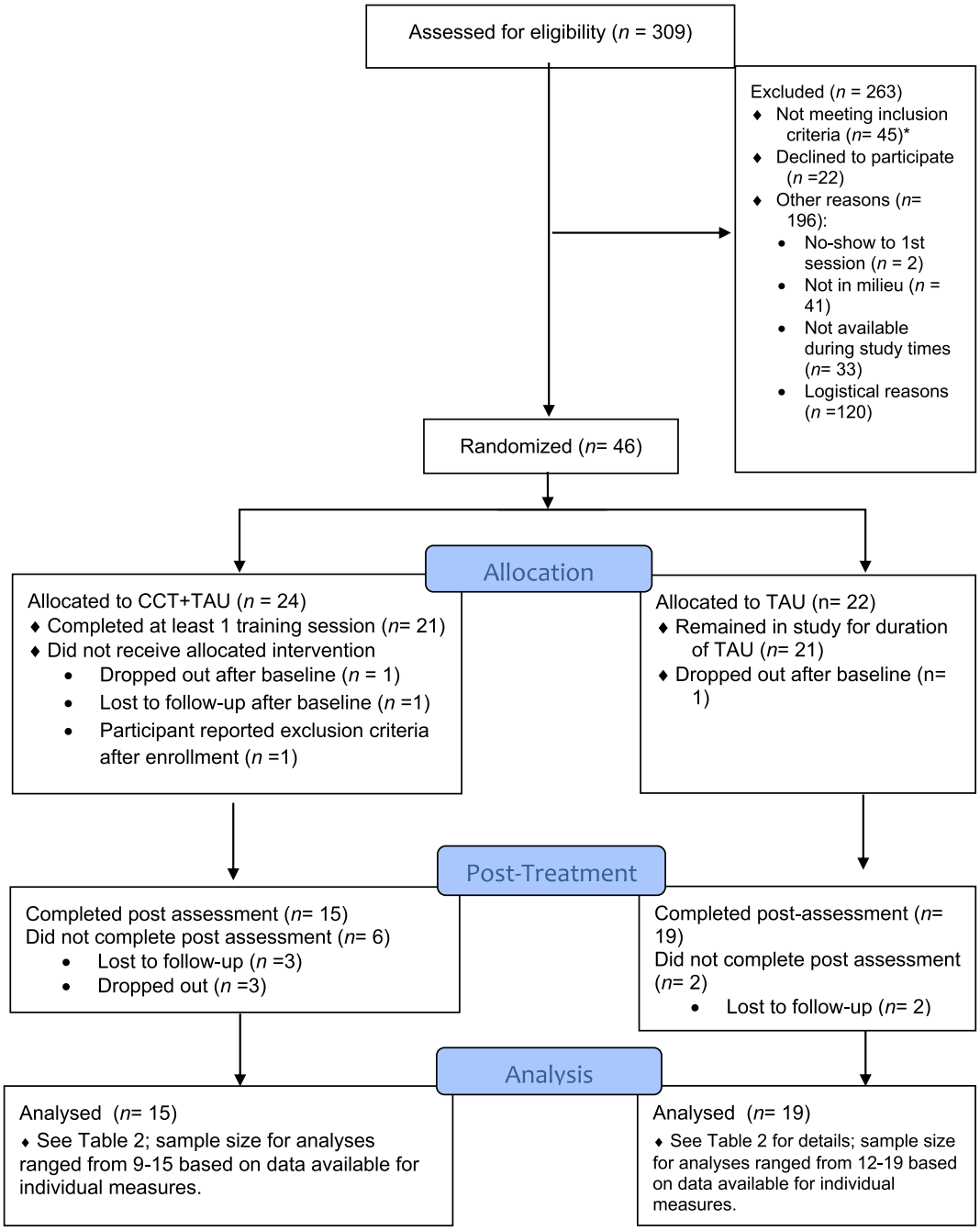

Fig. 1. CONSORT Diagram (Randomized Controlled Trial Phase).
Note. Participants excluded for failure to meet inclusion criteria included: acute mania (n = 4); acute psychosis (n = 20); other clinical acuity identified by clinical staff (n = 9); current ECT (n = 8); history of TBI or neurological disorder (n = 2); no longer enrolled in PHP on day 2 (n = 2).

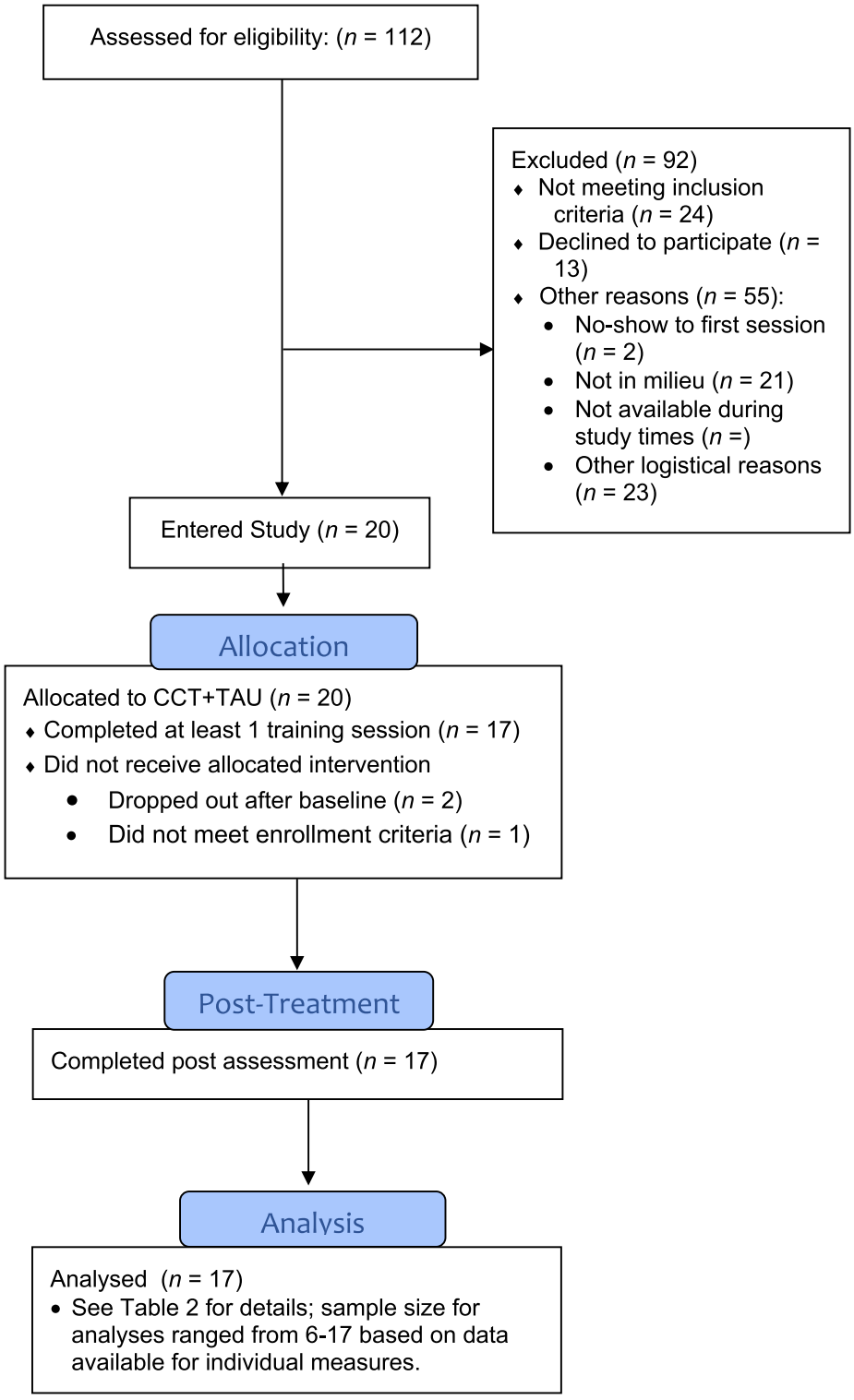

Fig. 2. CONSORT Diagram (Open Trial Phase).
Fig. 2 Note. Participants excluded for failure to meet inclusion criteria included: acute mania ($n = 5$); acute psychosis ($n = 7$); other clinical acuity identified by clinical staff ($n = 10$); history of TBI or neurological disorder ($n = 1$); no longer enrolled in PHP on day 2 ($n = 1$).

Recruitment for this study occurred in two phases. First, we conducted a non-randomized open trial to ensure minimal feasibility for study procedures in this population. During the open trial phase, study investigators met with key stakeholders in the PHP (e.g., former PHP patients serving on the clinic's Patient Advisory Board and clinical staff members) to review study procedures and develop strategies to maximize participant enrollment and retention. Second, we conducted a pilot randomized trial, in which participants were randomized to either receive treatment-as-usual (TAU) in the PHP or to TAU plus the cognitive control training. During the RCT phase, a research assistant informed participants of their study assignment at the conclusion of the baseline session, following completion of all tasks. Participants were assigned with equal probability to one of the two conditions using a random number generator. Due to the additive treatment design (i.e., no

sham or placebo control), neither participants nor the investigators were able to be blind to study condition; nor were investigators blind to treatment condition at the pre- and post-treatment sessions. Importantly, study procedures and intervention components were not altered between the open trial phase and RCT, allowing for direct comparison between these phases (as in Peckham & Johnson, 2018).

3.3. Study sessions and measures

Participants completed a battery of self-report measures described below, as well as a series of computerized tasks to assess various domains of cognitive control. Based on previous studies, we conceptualized our cognitive outcome measures as either reflecting "task-specific transfer" (non-adaptive versions of the same tasks used for training; Vanderhasselt et al., 2021) or "near transfer" (different tasks that assess the same cognitive domain but were not included in the training program; Harvey et al., 2018).

3.4. Self-report measures: baseline and post-treatment

Self-report measures described below were completed via REDCap (Harris et al., 2009). All measures were administered at the baseline and post-treatment (on the day of discharge from treatment) sessions, with the exception of the short UPPS-P scale, which was completed on the day of admission and at the post-treatment session.

SUPPS-P Negative and Positive Urgency Subscales (Cyders, Littlefield, Coffey, & Karyadi, 2014). The SUPPS-P is the short form of the Urgency, lack of Perseverance, lack of Planning, and Sensation-seeking Impulsive Behavior Scale (Lynam, Whiteside, Smith, & Cyders, 2006); in the present study, only the Negative and Positive Urgency scales were administered. The validity and reliability of this shortened version have been established by previous studies (Lozano, Diaz-Batanero, Rojas, Pilatti, & Fernandez-Calderon, 2018). The Negative and Positive urgency subscales each have four items, where items in the former measure the tendency to act impulsively during negative emotion (e.g., "*When I am upset I often act without thinking*") and items for the latter measure the same tendency during positive emotion (e.g., "*I tend to act without thinking when I am really excited*"). Items are rated on a 4-point scale from 1 ("*agree strongly*") to 4 ("*disagree strongly*") and were coded so that higher values reflect greater impulsivity. Scores for each subscale are calculated by averaging their respective items. Reliability was good for the positive urgency scale at admission (alpha = 0.89) and discharge (alpha = .88); the negative urgency scale showed low reliability at admission (alpha = 0.29) but improved reliability at post-treatment (alpha = 0.61).

Distress Intolerance Index (DII; McHugh & Otto, 2012). The DII is a self-report measure of one's inability to tolerate negative affective states. It contains 10 items, rated on a 0 ("*very little*") to 4 ("*very much*") scale, the contents of which were adapted from previous measures of distress intolerance and anxiety sensitivity. Examples of items in this scale include "*I can't handle feeling distressed or upset*" and "*I'll do anything to stop feeling stressed or upset.*" The total score is calculated by summing the response values for the 10 items, with higher scores indicating a lower ability to tolerate distress. Reliability for this measure was good at baseline (alpha = 0.83) and excellent at post-treatment (alpha = .93).

3.5. Cognitive measures: baseline and post-treatment

Paced Auditory Serial Addition Task. To assess task-specific transfer of working memory, participants completed a computerized version of the Paced Auditory Serial Addition Task (PASAT; Siegle, Ghinassi, & Thase, 2007). At both the baseline and post-treatment sessions, participants were aurally presented with numbers, one at a time, and were tasked with a) adding each number to the number they heard before it, and b) clicking the number onscreen that represented the sum of those two numbers. There were 3 s separating each trial for a total block time of 3 min, where accuracy was calculated via the proportion of correct responses out of the 59 total trials. Split-half reliability of accuracy was acceptable at baseline for both the RCT sample (Spearman-Brown coefficient $r = 0.70$) and combined sample ($r = 0.71$); reliability at post-treatment was slightly lower (RCT $r = 0.67$, combined sample $r = .64$).

Go/No-Go Task. To assess task-specific transfer of response inhibition, participants completed a computerized Go/No-Go Task at the baseline and post-treatment sessions. The task was programmed in E-Prime (version 2.0) and included one block of 100 trials, consisting of 70 "Go" trials 30 "No-go" trials. Task parameters were identical to those reported in Peckham and Johnson (2018). For each trial, participants were required to quickly press a button in response to a "Go" stimulus (particular letters), or to inhibit their response to a "No-Go" stimulus (a different letter). Stimuli were presented for 200 ms each, and the response time deadline for "Go" trials was within 1500 ms of the appearance of the stimulus. After each trial, participants were shown feedback ("Right!" for correct and "XXX" for incorrect responses) onscreen for 1000 ms. Inter-trial intervals (ITIs) varied randomly in 100 ms increments between 500 and 1500. Accuracy was based on the percentage of false alarms (incorrect responses to "No-Go" trails). Reaction time to "Go" stimuli were also captured as a secondary outcome measure. Split-half reliability of accuracy (false alarm rate) was modest at baseline in both the RCT sample and combined sample (Spearman--Brown $r = 0.66$), with lower reliability at post-treatment (RCT $r = 0.62$, combined sample $r = .52$).

Stop Signal Task. The stop signal task was administered to assess near transfer of response inhibition, using the STOP-IT2 task in MATLAB, based on the original STOP-IT task (Verbruggen, Logan, & Stevens, 2008). The task used visual stimuli for "Go" and "Stop" signals and resembles previous implementations of this code (e.g., Friehs & Frings, 2019). In this task, "Go" trials involved participants responding to a left-facing white arrow by pressing the left arrow key and to a right-facing white arrow with the right-arrow key, and "Stop" trials (blue arrows) required participants to inhibit their response and not press either key, as the stop-signal appeared after a delay (initially set to 250 ms). The subsequent delays varied with an ongoing tracking procedure to keep the probability of responding to stop-signal trials at about 50%. Participants completed a practice block of 32 trials, followed by three blocks of 96 trials each (75% "Go" trials). Each trial started with the presentation of a fixation symbol, and after an initial 250 ms delay, a right- or left-facing arrow appeared in place of that symbol and remained onscreen for a maximum of 1250 ms (or until a response was recorded). A jittered ITI ranging from 500 to 1500 ms separated trials. This task was scored using the integration method to estimate the stop-signal reaction time (SSRT; Verbruggen, Chambers, & Logan, 2013). Split-half reliability of task accuracy (commission error rate) was modest at baseline in both the RCT sample (Spearman-Brown $r = 0.64$) and combined sample ($r = 0.63$), with lower reliability at post-treatment (RCT $r = 0.52$, combined sample $r = .57$).

Dual N-Back Task. To assess working memory transfer effects, participants completed a brief version of an adaptive dual N-back task at the baseline and post-treatment sessions (Jaeggi, Buschkuehl, Jonides, & Perrig, 2008). This task was programmed in E-Prime (version 2.0) and consisted of three blocks of 20+ n trials. Stimuli consisted of simultaneously presented shapes (visual) and letters (auditory), with a 2500 ms ISI. Each trial required participants to determine whether the current visual stimulus matched the one presented n trials previous and whether the current auditory stimulus matched the one presented n trials previous. All participants began with a 1-back task. Total accuracy of over 90% increased n by one for the following block, and total accuracy below 70% decreased n by one for the following block (with a lowest level of 1-back); total accuracy between 70 and 90% carried the same n over to the following block. Participants were shown feedback about their performance at the end of each block. To assess accuracy on this

task, we evaluated both the highest level obtained over 3 blocks (1, 2, or 3-back), and the overall accuracy on the first block (subtracting false alarms from hits during this block). Split-half reliability of first-block accuracy rates were poor at baseline (RCT Spearman-Brown $r = 0.38$, combined sample $r = .46$); at post-treatment, reliability was acceptable (RCT $r = 0.80$, combined sample $r = .78$).

3.6. Cognitive training procedures

Participants in the training condition completed one of two tasks each day between the day after the baseline session through the day before the post-treatment session, alternating between tasks each consecutive day. Tasks were alternated due to the limited time available for research participation during the treatment day. The tasks included a performance-adaptive version of the PASAT to train working memory and an adaptive Go/No-Go task to train response inhibition, with each training session lasting 15 min (three 5-min blocks), in addition to self-paced breaks between blocks. During each training session, an experimenter was present in the room with participants to answer questions if needed and ensure that the task was running properly. The order of training task completion (adaptive Go/No-Go or adaptive PASAT) was assigned randomly for the first day of training and was alternated daily for all subsequent training sessions. The total number of sessions varied based on participants' length of stay in the treatment program (range: 1–7). Versions of the training tasks used in the open trial and RCT phase were identical, and both tasks had the same parameters as Peckham and Johnson (2018).

Adaptive PASAT (Siegle et al., 2007). The adaptive version of the PASAT shared the same features as the PASAT task as described above, with the only exception being that the ITI varied based on response accuracy rather than being constant. By manipulating the ITI and thus the speed at which the task progressed, the difficulty level of the task adapted to performance on an ongoing basis (Siegle et al., 2007). The ITI started at 3000 ms and increased or decreased by 100 ms after four consecutive correct or four consecutive incorrect responses, respectively. Participants competed three 5-min blocks for each PASAT training session, with a self-paced break between each block. For each PASAT session after the first day of training, the starting ITI was set to the median ITI of the third block from the previous PASAT training session. The total number of trials completed during each 5-min block varied due to the fluctuating ITIs. As in previous studies using this task, performance was scored based on the median ITI of a given training day, averaged across blocks (lower values for median ITI reflecting better performance on the task).

Adaptive Go/No-Go Task. An adaptive version of the Go/No-Go task was used for response inhibition training. The task was identical to the standard Go/No-Go task described above, with the exception that the response time "deadline" allowed for "Go" responses also varied with performance (Benikos, Johnstone, & Roodenryd, 2013a, 2013b), designed with the intent of keeping the task at a moderate difficulty level to be most effective for training (Benikos, Johnstone, & Roodenrys, 2013b; Peckham & Johnson, 2018). This deadline was initially set to be 300 ms after the appearance of the stimulus for "Go" responses, but decreased by 25 ms after each accurate "Go" response and increased by 25 ms after each missed "Go" response, with a minimum response time window of 50 ms and a maximum of 1000 ms. In each Go/No-Go training session, participants completed three 5-min blocks, and each block used a different "No-Go" letter for the stimulus. Participants were shown feedback of their false alarm rate after each block. Accuracy was evaluated based on the average false alarm rate per training day.

Treatment Acceptability Questionnaire (adapted from Beard, Rifkin, Silverman, & Björgvinsson, 2019). Participants in the training condition completed a self-report measure at the post-treatment session assessing acceptability of each task. This measure included four questions assessing the perceived helpfulness, user-friendliness, applicability to impulsivity, and perceived improvement in task performance during the training sessions; each question was asked twice to assess perceptions of the PASAT and Go/No-Go separately. Questions were rated on a 7-point Likert scale, ranging from 1 ("Completely Disagree") to 7 ("Completely Agree"), with a midpoint item of 4 ("Neutral"). Participants were also asked to provide open-ended qualitative responses about their positive and negative perceptions of the training.

3.7. Data reduction and identification of outliers

Prior to conducting analyses, data were inspected for normality and checked for outliers. On baseline and post-treatment measures of cognition, quality control checks for each measure were based on either published guidance for outlier identification (e.g., based on reasonable reaction time values), or where such guidance was not available, an *a priori* cutoff of >3SD from the mean. Stop Signal task data were examined using the "lenient" criteria identified as the best approach for outlier identification in this task in a previous investigation (Congdon et al., 2012). Using these criteria, three participants from the baseline and one participant from the post-treatment sessions were excluded based on one or more of the following: extremely fast (<50 ms) or negative SSRT values; less than 90% accuracy on "Go" trials; greater than 40% omissions on "Go" trials; or very high (>75%) or low (<25%) rates of inhibition success (Congdon et al., 2012). On the Go/No-Go task, accuracy on "Go" trials was inspected prior to analyses to ensure that participants met a minimum threshold of 75% accuracy (cf. Redick et al., 2011); all participants met this criterion at baseline (*M*: 98.26%; *SD* = 2.47) and post-treatment (*M:* 99.33%; *SD* = 1.96). Baseline and post-treatment PASAT data were excluded for one participant in the randomized phase who did not understand the instructions after repeated attempts to explain the task (this person was assigned to TAU and thus did not complete the training PASAT). Finally, N-back data for one additional participant in the RCT phase was excluded as this participant showed false alarm rates on the N-back task that exceeded 50% on all blocks at baseline and at post-treatment (>3 SDs above the sample mean).

3.8. Analysis plan

Analyses were completed in SPSS (version 24.0). To test our hypotheses regarding feasibility and acceptability, we calculated descriptive statistics. Pearson correlations, chi-square analysis, and *t*-tests were used to characterize baseline scores on study measures, and to compare baseline differences between groups. Changes in primary outcomes measures (urgency, distress intolerance) and performance on cognitive tasks were tested using a repeated measures (baseline, post-treatment) ANOVA.[2] Based on a power analysis conducted in G*Power, we aimed to enroll a total of 68 participants to detect small-to-medium effects in the repeated measures ANOVA analyses. Within the cognitive training group, change in performance on the training tasks was evaluated using Linear Mixed Models (LMM), as this approach is well-suited to the parameters of cognitive training data (i.e., correlated repeated measures within subjects and variability in the number of completed sessions

[2] Analyses presented in the main text did not include covariates. However, in separate analyses, covariates of age and gender were included, and to control for clinical heterogeneity, recent psychiatric hospitalization (a binary variable indicating inpatient psychiatric hospitalization within the past six months) was also entered as a covariate. Across the study as a whole, including covariates in repeated-measures ANOVAs did not affect the direction or significance of results, with two exceptions. In analyses of distress intolerance, adding covariates rendered the main effect of Time no longer significant, $p = .11$, partial $\eta^2 = 0.074$; however, the addition of covariates did not affect the significance of the Time × Group interaction, $p = .04$, partial $\eta^2 = 0.13$. Additionally, in the analysis of reaction time to "Go" trials, adding covariates reduced the main effect of Time to a trend, ($p = .09$, partial $\eta^2 = 0.07$), but did not affect the significance of the Group × Time interaction, $p < .01$, partial $\eta^2 = 0.15$.

between subjects). Two-tailed alpha was set to $p = .05$ for all tests. Finally, for the qualitative responses on the Treatment Acceptability Questionnaire, responses to question prompts were coded by three raters using a general inductive approach (Thomas, 2006). Discrepancies in coding were resolved via consensus. Analyses primarily focused on participants enrolled in the RCT phase; however, all analyses on the combined sample of participants from the open trial and the RCT were also conducted to maximize power to compare effects associated with the cognitive training program to the TAU group (as in Peckham & Johnson, 2018). Except where noted, RCT effects are presented first, followed by combined sample effects. Consistent with previous cognitive training studies (e.g., Siegle et al., 2014; Vanderhasselt et al., 2021) and our pre-registered analysis plan, data were analyzed on a per-protocol basis, including all participants who completed the study.[3]

4. Results

A combined total of 66 participants were enrolled in this study, including the open trial phase ($n = 20$) and randomized trial phase ($n = 46$). Two participants were excluded from further participation following the baseline session ($n = 2$) after it was determined that they did not meet enrollment criteria (one each from the open trial and RCT phase), resulting in a final sample of 64 participants (open trial $n = 19$; RCT $n = 45$). As shown in the CONSORT diagrams (Figs. 1 and 2), post-treatment data were available for 17 participants from the open trial and 34 participants from the RCT. Recruitment took place between January–June 2018 (open trial) and September 2018–March 2020 (RCT). Recruitment for the RCT was terminated prior to reaching the target sample size due to the COVID-19 pandemic. Demographic and clinical characteristics of the sample are presented in Table 1.

Participants in the open trial and RCT did not significantly differ from each other on any demographic or clinical variable. Within the RCT, participants assigned to cognitive training did not significantly differ from those in TAU on variables including age, gender, race, ethnicity, diagnosis, or recent psychiatric hospitalization ($ps > .05$). Further, groups did not statistically differ on any of the baseline self-report measures (negative and positive urgency, distress intolerance), nor any baseline measures of cognition (non-adaptive PASAT, non-adaptive Go/No-Go, Stop Signal, and N-Back). At baseline, urgency measures showed small, non-significant correlations with the working memory tasks (rs from $-.05$ to -0.30), while correlations between urgency measures and response inhibition tasks were non-significant and the direction of effects was variable (Supplementary Materials, Table S1).

Similarly, across the whole sample, participants who dropped out of the study did not significantly differ from those who completed the study on any of the above variables. As would be expected given that some participants did not complete the study due to early discharge from the PHP (documented as "lost to follow-up" in Figs. 1 and 2), participants who completed the study had a slightly longer duration of hospitalization as compared to those who did not complete, $F(1, 61) = 5.50$, $p = .02$.

4.1. Feasibility and acceptability of intervention

Initial results of the open trial showed good feasibility, with 89.5% of the sample (17 participants) completing the intervention, with an average of 5.76 training sessions (*SD*: 1.35). Completion rates were somewhat lower in the RCT phase, with 65.2% of the sample (15 participants) completing cognitive training, with an average of 4.14 sessions (SD: 2.03). Across both phases (open trial and RCT feasibility data combined), this resulted in an overall training completion rate of 76.2%. Length of stay in the PHP was directly correlated with the overall number of training sessions completed ($r = .60$, $p < .001$).

To assess the acceptability of the two training tasks, participants in the training group completed a post-training survey (added after the open trial had already started, so data were missing from seven participants). Table 3 summarizes the results of this measure: on average, participants rated both tasks as at least moderately acceptable on most dimensions, including perceived improvement on the tasks, whether the tasks were user-friendly, and applicability to impulsivity. Core themes that emerged from the qualitative analysis included helpful aspects of the training, such as awareness of improvement on the tasks over time and increased self-awareness. Comments also included unhelpful aspects of the training: frustration and stress related to the tasks and the difficulty of the tasks. Examples of quotes describing these themes are presented in Table 3.

Table 1
Demographic and clinical characteristics of sample.

Characteristics	Open Trial ($n = 20$)	RCT: CCT + TAU ($n = 24$)	TAU ($n = 22$)
Gender: *n* (%)			
Female	10 (50%)	12 (50%)	10 (45.5%)
Male	10 (47.6%)	11 (45.8%)	11 (50%)
Nonbinary	0	1 (4.2%)	1 (4.5%)
Age: M (*SD*)	28.6 (11.8)	33.75 (14.5)	29.68 (10.3)
Race: *n* (%)			
Asian	1 (5%)	2 (8.3%)	0
Black	0	1 (4.2%)	0
White	18 (90%)	18 (75%)	21 (95.5%)
More than one race	1 (5%)	2 (8.3%)	0
Unknown	0	1 (4.2%)	1 (4.5%)
Ethnicity: *n* (%)			
Hispanic/Latinx	3 (15%)	2 (8.3%)	2 (9.1%)
Not Hispanic/Latinx	17 (85%)	22 (91.7%)	20 (90.9%)
Sexual Orientation: *n* (%)			
Heterosexual/straight	16 (80%)	17 (70.8%)	14 (63.6%)
Gay, lesbian, or bisexual	2 (10%)	6 (25%)	8 (36.4%)
Not listed	2 (10%)	1 (4.2%)	0
Diagnosis: *n* (%)			
MDD	13 (65%)	12 (50%)	12 (54.6%)
Anxiety/Phobias	0	3 (12.5%)	1 (4.5%)
OCD	2 (10%)	0	0
PTSD	0	1 (4.2%)	0
Bipolar Disorder	5 (25%)	8 (33.3%)	8 (36.4%)
Psychotic Disorder NOS[a]	0	0	1 (4.5%)
Psychiatric hospitalization in last 6 months: *n* (%)	14 (70%)	7 (29.2%)	8 (36.4%)

Note. [a] Psychotic disorder not otherwise specified.

4.2. Primary outcome: change in urgency

Contrary to hypotheses, results of repeated-measures ANOVAs testing change in urgency among RCT participants revealed no significant change in negative urgency, $F(1, 30) = 0.25$, $p = .62$, partial $\eta^2 = 0.008$, with no significant Time × Group interaction, $F(1, 30) = 0.005$, $p = .95$, partial $\eta^2 < 0.001$; similarly, positive urgency showed no overall change over time, $F(1, 31) = 3.40$, $p = .08$, partial $\eta^2 = 0.10$, and no significant Time × Group interaction, $F(1, 31) = 0.22$, $p = .64$, partial $\eta^2 = 0.007$. In the full sample (RCT and open trial combined), results were parallel with regard to the urgency scales: negative urgency, Time X Group $p = .86$, partial $\eta^2 = 0.001$; positive urgency, Time X Group: $p = .77$, partial $\eta^2 = 0.77$.

[3] Intent-to-treat analyses for the RCT study are also presented in the Supplemental Materials. The direction of all effects were the same in both analyses, and the significance of findings were unchanged, with the exception of the Go/No-Go Reaction Time analyses.

4.3. Secondary outcome: change in distress intolerance

Within the RCT phase, scores on the distress intolerance measure significantly decreased over time, $F(1,27) = 7.5$, $p = .01$, partial $\eta^2 = 0.22$, with no significant Time × Group interaction, $F(1,27) = 1.42$, $p = .24$, partial $\eta^2 = 0.05$. However, combined-sample analyses of distress intolerance revealed a significant effect of training group: overall, participants showed a decrease in distress intolerance, $F(1, 36) = 13.66$, $p < .01$, partial $\eta^2 = 0.28$, qualified by a significant Time × Group interaction, $F(1, 36) = 4.22$, $p = .047$, partial $\eta^2 = 0.11$. Post-hoc paired t-tests to decompose this interaction revealed a significant decrease in distress intolerance in the training group, $t(21) = 3.82$, $p = .001$, Cohen's $d_z = 0.81$, with no significant change in the TAU group, $t(15) = 1.51$, $p = .15$, $d_z = 0.38$.

4.4. Change in cognitive measures: task-specific transfer

Table 2 summarizes the baseline and post-treatment scores for the RCT-training and TAU groups on all cognitive measures, as well as open trial participants, for study completers. In the RCT phase, a significant effect of Time was evident on the non-adaptive PASAT, $F(1, 30) = 113.62$, $p < .001$, partial $\eta^2 = 0.79$, qualified by a significant Group × Time interaction, $F(1, 30) = 28.49$, $p < .001$, partial $\eta^2 = 0.49$. Paired t-tests showed that participants in both the training and TAU group improved their performance on this task ($ps < .001$), yet the magnitude of this change was greater in the training group (Cohen's $d_z = 2.24$) than in TAU (Cohen's $d_z = 1.30$). In contrast, although accuracy on the non-adaptive Go/No-Go task improved over time, $F(1, 30) = 6.20$, $p = .02$, partial $\eta^2 = 0.17$, there was no evidence of a Group × Time interaction ($p = .96$); similarly, reaction time to "Go" trials significantly decreased, $F(1, 30) = 4.35$, $p = .046$, partial $\eta^2 = 0.13$, with no significant a Group × Time interaction ($p = .17$).

In combined analyses, results were entirely parallel for PASAT effects, with a main effect of Time ($p < .001$, partial $\eta^2 = 0.66$) and a Time × Group interaction ($p < .001$, partial $\eta^2 = 0.25$). Go/NoGo analyses were also largely parallel, with a main effect of Time on accuracy ($p < .001$, partial $\eta^2 = 0.14$), no Time × Group interaction on accuracy, ($p = .96$), and a main effect of Time on "Go" reaction times ($p = .001$, partial $\eta^2 = 0.21$). However, combined analyses indicated improved task speed ("Go" RT) in the training group only, with a significant Group × Time interaction, $F(1, 47) = 6.77$, $p = .01$, partial $\eta^2 = 0.13$. Reaction time to "Go" trials significantly increased in the training group ($p < .001$, Cohen's $d_z = 0.83$), but did not significantly change in the TAU group ($p = .54$, Cohen's $d_z = 0.14$).

4.5. Change in cognitive measures: near transfer

Contrary to hypotheses, no significant changes in Stop-Signal Reaction Time (SSRT; the hypothesized measure of near transfer for response inhibition) were observed during the RCT phase, $F(1, 26) = 0.59$, $p = .45$, partial $\eta^2 = 0.02$, nor was there evidence of a significant Group × Time interaction in predicting SSRT, $F(1, 26) = 2.49$. $p = .13$, partial $\eta^2 = 0.09$. Regarding transfer of working memory, N-back data were missing for a substantial number of participants on this task (see Table 2), largely due to time constraints during the data collection session (this task was the last scheduled task). Of the sub-sample who completed this task, no significant changes in N-back accuracy were observed, $F(1, 19) = 0.57$, $p = .46$, partial $\eta^2 = 0.03$, nor was there

Table 3
Acceptability: Quantitative and qualitative ratings.

Part 1: Quantitative Data (All items rated on 1–7 scale):

	Task			
	Adaptive Go/No-Go		Adaptive PASAT	
	Open Trial *M* (SD)	RCT *M* (SD)	Open Trial *M* (SD)	RCT *M* (SD)
1. How helpful?	4.6 (2.0)	5.1 (1.7)	4.6 (2.0)	3.9 (1.4)
2. How user-friendly?	5.7 (1.5)	5.5 (1.9)	5.7 (1.5)	3.9 (1.8)
3. How applicable to impulsivity?	6.0 (1.6)	5.8 (1.6)	6.0 (1.6)	4.8 (2.0)
4. How much improvement in performance?	5.1 (1.8)	5.8 (1.6)	5.1 (1.8)	5.4 (1.4)

Part 2: Qualitative Data

Main Theme	Subthemes	Example Quotations
Helpful Aspects	Noticing task improvement	*"I was able to get better as time went on because of the training sessions."*
		"They definitely seemed to get easier with practice."
	Increased self-awareness	*"I got a sense of when I was getting flustered and how it was not helping me complete the task with fewer errors."*
		"It showed that I am extremely impulsive."
	Globally positive	*"It put me in a mindset for creative problem-solving about how I could manage my impulses and internal distractions more effectively."*
		"I thought it was helpful to have."
Unhelpful Aspects	Frustration/Stress	*"Very difficult and stressful."*
		"Frustrating how often I was too slow when pressing the key."
	Difficulty of tasks	*"Very hard to do."*
		"Very difficult for me."
	Globally negative	*"[The tasks] were incredibly annoying."*
		"I don't quite see how this is helpful for impulse control."

Note. $n = 11$ for open trial questions; n = 10 for question 3. $n = 14$ for RCT sample; n = 13 for question 3. Part 2 includes quotes from both study phases.

Table 2
Means and standard deviations of primary outcome measures at baseline and post-treatment among study completers.

	Open Trial			RCT: Cognitive Training plus TAU			RCT: TAU		
	Baseline	Post-Treatment		Baseline	Post-Treatment		Baseline	Post-Treatment	
	M (SD)	M (SD)	*n*	M (SD)	M (SD)	*n*	M (SD)	M (SD)	*n*
Negative Urgency	3.22 (.27)	3.11 (.64)	16	3.2 (.44)	3.17 (.53)	15	3.24 (.38)	3.19 (.30)	17
Positive Urgency	2.06 (.96)	2.11 (.74)	16	2.3 (.72)	2.07 (.77)	15	2.53 (1.05)	2.39 (.81)	18
Distress Intolerance	25 (4.24)	14.89 (7.54)	9	27.0 (8.56)	21.77 (10.17)	13	24.81 (7.12)	22.75 (9.21)	16
PASAT Accuracy (% correct)	58.5 (26.4)	84.3 (17.8)	16	40.19 (22.76)	81.23 (11.94)	14	51.69 (25.77)	65.35 (24.44)	18
Go/No-Go False Alarm Rate (%)	24.7 (19.0)	18.2 (13.9)	17	21.79 (12.14)	15.9 (19.01)	13	25.79 (14.90)	19.65 (8.45)	19
Go/No-Go "Go" Reaction Time (ms)	169.2 (43.9)	122.3 (23.5)	17	181.86 (56.23)	153.29 (57.67)	13	167.19 (38.53)	161.53 (46.31)	19
Stop Signal Reaction Time (SSRT; ms)	211.2 (50.8)	220.9 (38.4)	13	234.03 (54.61)	224.87 (50.61)	11	208.52 (57.78)	236.69 (62.08)	19
N-Back (Max Level)	2 (.63)	2.2 (.75)	6	2.0 (.50)	1.89 (.60)	9	2.0 (.58)	1.85 (.56)	13
N-Back Block 1 Accuracy (%)	75 (27.1)	63.3 (18.4)	6	70.11 (23.34)	66.14 (29.6)	9	75.0 (22.10)	66.27 (30.1)	12

Note. See Figs. 1 and 2 – CONSORT diagrams for details about attrition from baseline to post-treatment.

evidence of a Group × Time interaction in predicting N-back accuracy, $F(1, 19) = 0.08$, $p = .78$, partial $\eta^2 = 0.004$. Results were entirely parallel in the combined sample, with no evidence of Group × Time interactions for SSRT ($p = .19$, partial $\eta^2 = 0.05$) or N-Back accuracy ($p = .90$, partial $\eta^2 = 0.001$.

4.6. Performance on adaptive training tasks

Among participants in the training group, Linear Mixed Models were used to test the hypothesis that participants would show significant improvements on the training tasks over time. Models were tested separately for each training task.

Adaptive PASAT. A Linear Mixed Model was used to estimate the change in performance on the adaptive PASAT over time, among participants in the training group. This model used Maximum Likelihood estimation and included a fixed effect of day of training, with random intercepts for participants; the dependent variable was accuracy (median ITI) on the PASAT, with lower scores indicating faster and more accurate performance. Results showed that accuracy on the adaptive PASAT significantly improved over the course of training sessions during the RCT phase, $b = -.42$ (95% CI: −0.57, −0.26), $p < .001$, with parallel findings in the combined sample, $b = -0.31$ (95% CI: −0.42, −0.20), $p < .001$.

Adaptive Go/No-Go. A Linear Mixed Model was used to estimate the change in performance on the adaptive Go/No-Go task over time. This model included a fixed effect of day of training, with random intercepts for participants; the dependent variable was accuracy (false alarm rate) on the Go/No-Go, with lower scores indicating fewer false alarms. Model parameters were the same as those for the PASAT. Results showed that accuracy on the adaptive Go/No-Go significantly improved over the course of training sessions during the RCT phase, $b = -4.03$ (95% CI: −6.64, −1.43), $p < .01$, and in the combined sample, $b = -5.28$ (95% CI: −7.83, −2.73), $p < .001$.

5. Discussion

Urgency is a facet of impulsivity robustly tied to poor outcomes across psychological disorders. We tested a cognitive intervention for urgency among adults in an acute clinical setting, targeting working memory and response inhibition via computerized training. The training tasks met *a priori* benchmarks for feasibility and acceptability; although feasibility (rate of completed sessions) was somewhat lower in the RCT phase. Participants in the training group showed significant improvement on the trained tasks, and in combined analyses across the open trial and RCT phases, those receiving cognitive training demonstrated a significant reduction in distress intolerance. In addition, we observed evidence of task-specific transfer of working memory performance, with participants in the training group showing significantly more improvement on a non-adaptive version of the PASAT. However, hypotheses about reductions in urgency were not confirmed, nor was there evidence of transfer to other cognitive tasks or clinical measures.

Results of this study suggest that incorporating challenging cognitive tasks into an intensive partial hospitalization setting is both feasible and acceptable to patients. Consistent with some previous research on computerized cognitive interventions in intensive outpatient or partial hospitalization programs (Beard et al., 2019; Siegle et al., 2014), these findings show that embedding cognitive training paradigms in acute treatment programs is achievable, despite the high symptomatic acuity of patients and the practical constraints on incorporating cognitive training into a treatment environment. In combined analyses, more than 75% of participants enrolled in the training condition completed all assigned training sessions, and on average, participants rated both training tasks as at least moderately helpful, user-friendly, and applicable to impulsivity. Participants also consistently reported in both qualitative and quantitative measures that they were aware of improvements in their performance on the tasks, which was consistent with significant improvements in objective measures of accuracy on the two training tasks over time. Training tasks were delivered with the identical parameters as in a previous laboratory-based study of cognitive training (Peckham & Johnson, 2018). Together, these findings bolster the argument for conducting cognitive training studies in naturalistic settings, as others have noted (e.g., Harvey et al., 2018).

Despite the acceptability of the intervention and in contrast to a previous lab-based study, there was no evidence that the training was beneficial for negative or positive urgency, and reliability of the negative urgency scale was unexpectedly low at the first assessment point. These null findings were unexpected, particularly given that putative mechanisms of urgency were engaged to some degree. That is, participants in the training group showed significantly greater improvements than the control condition on a non-adaptive version of the PASAT, a measure of near transfer of working memory. Also, although accuracy rates on the Go/No-Go inhibition tasks did not differ between groups, combined analyses revealed that those in the training condition showed a significant speeding of their reaction times to “Go” stimuli without sacrificing accuracy, which is consistent with other studies that have trained inhibitory control (Bos et al., 2019; Schroder et al., 2020). Taken together, these findings suggest that alternative interventions may be needed to address urgency in acute clinical settings, although conclusions about this are limited given the low reliability of the negative urgency measure in this setting.

Although not hypothesized, the lack of change in urgency is consistent with recent studies that report an improvement in cognitive task performance without a corresponding change in symptoms or psychological traits, including studies utilizing the PASAT (Van den Bergh et al., 2020; Vanderhasselt et al., 2021) or inhibitory control tasks (Bos et al., 2019). Given that participants receiving training were also receiving intensive psychiatric treatment, these findings are also consistent with some investigations reporting no additive effect of cognitive training interventions in treatment settings (Moshier & Otto, 2017; Van den Bergh et al., 2020). Future studies that seek to develop interventions for urgency in acute psychiatric settings may benefit from testing meta-cognitive interventions that might circumvent weaknesses in cognitive control (e.g., Javaras, Williams, & Baskin-Sommers, 2019). For example, evidence suggests that providing individuals with an intervention to practice implementation intentions, designed to circumvent the need to rely on cognitive strategies during moments of strong emotion, are useful in reducing aggression and emotion-based impulsivity (Johnson, Zisser, et al., 2020).

More broadly, findings are consistent with the observation in working memory training studies that gains in working memory may not consistently transfer to other domains (Redick, 2019). If tested in future studies, cognitive training interventions for impulsivity may benefit from adopting recent recommendations, including careful consideration of individual differences that may influence training outcomes (Redick, 2019; Smid, Karbach, & Steinbeis, 2020) and inclusion of strong control conditions (Redick, 2019). Future studies may also benefit from enhanced personalization. Although participants in the present study were recruited on the basis of high urgency scores, future studies could additionally match participants to specific interventions on the basis of baseline cognitive performance (e.g., Hsu et al., 2021). This strategy may be particularly important for future studies of urgency interventions, given the surprising absence of significant correlations between urgency and cognitive measures at the baseline session. The absence of correlations between some cognitive measures and self-reported impulsivity is consistent with other recent findings (Hedge, Powell, Bompas, & Sumner, 2020), suggesting the need for further refinement of theories about the relationship between cognition, facets of impulsivity, and psychopathology. Future studies may benefit from considering recent work that illustrates differences between disinhibition and impulsivity in predicting externalizing symptoms (Joyner, Daurio, Perkins, Patrick, & Latzman, 2021), and in experimental studies illustrating the role of arousal in predicting cognitive correlates of

urgency (Pearlstein, Modavi, Johnson, Peckham, & Carver, 2019).

In contrast to null findings on the primary urgency outcomes, participants in the cognitive training group (combined across the open trial and RCT phases) showed a significant reduction in distress intolerance, a domain that has also been linked to working memory and response inhibition deficits. Although speculative, one possible explanation for why participants showed reductions in distress intolerance, but not urgency, is that participants may have perceived the training tasks to be more relevant for tolerating distress rather than controlling impulses. Qualitative data showed that frustration and distress were very frequently experienced during both training tasks, which is consistent with other studies that have used the PASAT working memory task as a distress induction tool (e.g., Brown, Lejuez, Kahler, & Strong, 2002). Although the training tasks were performance-adaptive in order to minimize frustration and difficulty, these adaptations did not appear sufficient to minimize distress in this hospital population. Yet, participants also reported across qualitative and quantitative measures that they were aware that their performance on the tasks improved. These findings suggest that gaining mastery on distressing cognitive tasks may have helped participants perceive an improvement in their ability to tolerate distress more globally.

These findings have implications for further development of cognitive interventions for urgency and for distress intolerance. The finding that distress intolerance was reduced in the context of completing distressing cognitive tasks highlights the importance of context in developing cognitive training interventions (cf. Smid et al., 2020). Cognitive performance is closely related to state affect, time of day, and many other contextual factors (Weizenbaum, Torous, & Fulford, 2020), and cognitive training interventions for urgency or distress intolerance may have the most benefit if they are delivered at the moment in which participants are most vulnerable to expressing these traits. Although replication is needed, it may be that participants need to practice persisting in goal pursuit during moments of elevated distress, in order for a cognitive intervention to effectively reduce distress intolerance. Similarly, interventions for urgency could benefit from applied practice of inhibition or working memory skills in contexts relevant for resisting impulses. As the ability to deliver reliable and valid cognitive tasks on mobile devices rapidly advances (Germine, Reinecke, & Chaytor, 2019), it is increasingly possible to deploy cognitive interventions via smartphone at the moments in which individuals may benefit from using such interventions in their daily lives. Such interventions have yet to be tested specifically for urgency or distress intolerance.

Several limitations of this study should be emphasized. First, consistent with the pilot nature of the study, the sample size was modest, which limits the power of statistical tests to detect differences between the two treatment conditions. Related to this limitation, several participants had missing data for several outcome measures, most notably the N-back working memory task. Although the final RCT sample size was similar to previous cognitive training RCTs (e.g., Bomyea, Stein, & Lang, 2015), the premature end of RCT recruitment due to the COVID-19 pandemic resulted in a smaller sample size than initially planned. Second, the negative urgency scale showed lower reliability at the baseline session than in previous investigations; a recent investigation conducted in another acute psychiatric setting also identified lower reliability of the UPPS negative urgency measure among certain diagnostic groups (Dugré, Giguére, Du Sert, Potvin, & Dumais, 2019), which raises questions about the validity of this measure in acute settings. Thus, it is difficult to draw conclusions about the cognitive training's effect or lack thereof on negative urgency. Future studies may benefit from using more comprehensive measures of urgency as inclusion criteria; for example, the Feelings Trigger Action scale of the 3-Factor Impulsivity Index integrates items from the negative and positive urgency scales and shows good psychometric properties (Carver, Johnson, Joormann, Kim, & Nam, 2011). Use of a more comprehensive measure of urgency may help to detect change in facets of urgency over time; although some studies have reported change in urgency over brief treatment episodes using the short UPPS-P (Peckham et al., 2019), a more comprehensive measure might help identify which facets of urgency are malleable with treatment. Reliability of most cognitive measures was also low, underscoring the broader need for enhancing the reliability of cognitive measures in psychopathology research (Rodebaugh et al., 2016), Third, the combination of working memory and inhibition training tasks precludes our ability to evaluate which cognitive domain is most relevant for urgency or distress tolerance. Finally, the study lacked an active control condition, was unblinded, and did not assess treatment expectancies. Treatment as usual for many patients also includes initiation or change of psychiatric medications, which was not assessed and may have influenced the efficacy of training or other study outcomes. Although participants in the treatment as usual group were receiving intensive hospital care and were similar in many ways to those receiving cognitive training (same treatment format, length of treatment, and setting), an active control condition could reduce group differences in expectancy effects regarding the intervention.

Despite these limitations, this study extends the investigation of cognitive training for urgency into an acute clinical sample for the first time. In this setting and sample, working memory and inhibition training was not effective in reducing urgency, although unexpectedly low reliability on the measure of urgency impaired our ability to test this hypothesis. Interventions for this facet of impulsivity are needed and it may be necessary to use approaches that address underlying mechanisms with other methods beyond cognitive training. However, results of this study also support the feasibility and acceptability of using cognitive training interventions in acute clinical settings, and if replicated, results support a role for cognitive training interventions in enhancing distress tolerance.

CRediT authorship contribution statement

Andrew D. Peckham: Conceptualization, Methodology, Formal analysis, Investigation, Writing – original draft, Project administration, Funding acquisition. **Jenna P. Sandler:** Data curation, Formal analysis, Writing – review & editing. **Devin Dattolico:** Investigation, Data curation, Writing – review & editing. **R. Kathryn McHugh:** Writing – review & editing. **Daniel S. Johnson:** Investigation, Data curation, Writing – review & editing. **Thröstur Björgvinsson:** Supervision, Writing – review & editing. **Diego A. Pizzagalli:** Supervision, Writing – review & editing. **Courtney Beard:** Methodology, Supervision, Writing – review & editing.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.brat.2021.103968.

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Declaration of interests

Over the past 3 years, Dr. Pizzagalli has received consulting fees from Albright Stonebridge Group, BlackThorn Therapeutics, Boehringer Ingelheim, Compass Pathway, Concert Pharmaceuticals, Engrail Therapeutics, Nerocrine Biosciences, Otsuka Pharmaceuticals, and Takeda Pharmaceuticals; one honorarium from Alkermes, and research funding from NIMH, Dana Foundation, Brain and Behavior Research Foundation, and Millennium Pharmaceuticals. In addition, he has received stock options from BlackThorn Therapeutics. There are no conflicts of interest with the work conducted in this study. All views expressed are solely those of the authors. The other authors have no financial disclosures.

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