**Does alcohol cue Inhibitory Control Training survive a context shift?**

**Running Head: ICT and context shift**

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**Abstract**

Inhibitory control training (ICT) is a novel psychological intervention that aims to improve inhibitory control in response to alcohol-related cues through associative learning. Laboratory studies have demonstrated reductions in alcohol consumption following ICT compared to control/sham training, but it is unclear if these effects are robust to a change of context. In a pre-registered study we examined whether the effects of ICT would survive a context shift from a neutral context to a semi-naturalistic bar setting.Using a mixed design, sixty heavy drinkers (40 female) were randomly allocated to receive either ICT or control/sham training in a neutral laboratory over two sessions. We developed a novel variation of ICT that used multiple stop signals in order to establish direct stimulus-stop associations. The effects of ICT/control were measured once in the same context and once following a shift to a novel (alcohol-related) context. Our dependent variables were *ad-libitum* alcohol consumption following training, change in inhibitory control processes and change in alcohol value. ICT did not reduce alcohol consumption in either context compared to the control group. Furthermore, we demonstrated no effects of ICT on inhibitory control processes or alcohol value. Bayesian analyses demonstrated overall support for the null hypotheses. This study failed to find any effects of ICT on alcohol consumption or candidate psychological mechanisms. These findings illustrate the difficulty in training alcohol-inhibition associations, and add to a growing body of literature which suggest ICT holds little evidential value as a psychological intervention for alcohol use disorders.

***Key words: Alcohol; Inhibitory Control Training; Stimulus Value; Stop Signal task.***

**Introduction**

Alcohol use disorders are associated with impairments in the ability to suppress inappropriate behaviour(s) (Smith, Mattick, Jamadar, & Iredale, 2014; Yucel et al., 2019), commonly known as inhibitory control (Logan, Cowan, & Davis, 1984). This failure to inhibit behaviour can be measured using Stop Signal or Go/No-Go tasks (Eagle, Bari, & Robbins, 2008; Verbruggen & Logan, 2008a). In these tasks participants are required to make speeded motor responses to cues which appear on a majority of trials. On a minority of trials, the presence of a ‘Stop Signal’ or a ‘Go/No-Go’ cue requires inhibition of the motor response. Poor inhibitory control can be inferred using commission errors (failure to inhibit) and Stop-Signal Reaction time (the unobserved latency of inhibition; (Verbruggen, Aron, Band, & al., 2019; Verbruggen & Logan, 2008b).

Among people who consume alcohol impairments in reactive inhibitory control are reliably exacerbated during exposure to alcohol-related cues (A. Jones, Robinson, et al., 2018). This transient impairment is thought to arise because alcohol-related cues have appetitive motivational properties and they evoke approach behaviours that are incompatible with inhibition (Field, Kiernan, Eastwood, & Child, 2008; Field, Mogg, & Bradley, 2005). The failure of inhibitory control in response to alcohol-related cues may increase the likelihood of drinking behaviour because inhibition is required to overcome the approach behaviours triggered by these cues (De Wit, 2009; Jones, Christiansen, Nederkoorn, Houben, & Field, 2013). Consistent with this view, transient inhibitory impairments may mediate *ad libitum* alcohol consumption after exposure to alcohol cues (Field & Jones, 2017). Furthermore, in dependent patients the magnitude of inhibitory impairment in response to alcohol cues predicts likelihood of relapse following treatment (Czapla et al., 2015).

Whilst reactive inhibitory control has provided the basis for the majority of research into alcohol use flexible human control is also proactive in nature, requiring careful planning and strategic adjustments (Elchlepp, Lavric, Chambers, & Verbruggen, 2016). For example, ***when people are attempting to reduce their alcohol consumption it is implausible that would inhibit all motor movement (a global stopping response). Rather, they should adopt a proactive strategy in anticipation of being exposed to alcohol-related cues*** (Aron, 2011). Following a failure (or inefficient use) of proactive control, reactive stopping may be employed as a last resort or late-correction mechanism during self-regulation (Braver, Paxton, Locke, & Barch, 2009). ***Strategic*** proactive control adjustments can be inferred by the degree of slowing of reaction times in contexts in which an inhibitory signal is anticipated (see Verbruggen & Logan, 2009; Verbruggen, Stevens, & Chambers, 2014), ***suggesting that proactive control is top-down and influenced by the expectation of future inhibition (see Best, McLaren & Verbruggen, 2019; Baines et al, 2019)***. There have been some attempts to examine the role of proactive control in alcohol use disorders, but this warrants further investigation (see Baines, Field, Christiansen, & Jones, 2019).

The observation that alcohol-related cues impair reactive control and that these impairments increase the likelihood of alcohol consumption has led to the development of a novel behavioural intervention that is designed to improve inhibitory control in response to alcohol related cues known as Inhibitory Control Training (ICT). During ICT participants complete a modified Stop Signal or Go/No-Go task that includes alcohol-related and neutral cues. In the active training group participants are trained to respond quickly to neutral cues whereas an inhibitory signal (stop signal or no-go cue) is paired with the majority (or all) alcohol-related cues. Therefore, through associative learning participants should learn that alcohol-related cues require an inhibitory response, and become more efficient at inhibiting behaviour in the presence of these cues (see Jones & Field, 2012). Control groups are either not required to make inhibitory responses, or they are exposed to reversed response contingencies such that they are instructed to make motor responses to alcohol-related cues whilst inhibiting to neutral / control images. Following training, participants’ motivation to drink alcohol is examined using *ad libitum* consumption paradigms (see Jones, Button, et al., 2016) in which free access to alcohol is provided. Numerous studies have demonstrated that, compared to control groups, active ICT prompts reduced alcohol consumption in the laboratory (Bowley et al., 2013; Di Lemma & Field, 2017; Houben, Havermans, Nederkoorn, & Jansen, 2012; K. Houben, Nederkoorn, Wiers, & Jansen, 2011; Jones & Field, 2012), with a small-to-medium sized effect (Allom, Mullan, & Hagger, 2015; A. Jones, Di Lemma, et al., 2016).

Whilst improvements in alcohol-related inhibitory control are the proposed mechanism of action of ICT, empirical support for this claim is mixed. Jones and Field (2012) demonstrated improvements in inhibitory control to alcohol cues following ICT, but did not examine whether this mediated the effect on alcohol consumption. Furthermore, Houben et al (2012) demonstrated that ICT using a modified Go/No-Go task did not improve inhibitory control to alcohol-related cues on a Stop Signal task. A second, non-mutually exclusive proposed mechanism of ICT is the devaluation of alcohol-related cues through repeated inhibitory responses to those cues. Behaviour Stimulus Interaction theory, as proposed by Veling et al (2008), suggests that through repeatedly inhibiting to stimuli that normally evoke approach tendencies (alcohol-cues, see above) a response conflict emerges. To resolve this conflict, negative affect is attached to the stimuli, meaning that they are evaluated less positively, thereby facilitating inhibition. In support of this mechanism, Houben et al (2012) demonstrated that ICT reduced the positive evaluation of alcohol-related cues and this mediated the reduction in alcohol consumption following training. However, there have been failures to replicate this finding (Bowley et al 2013; Di Lemma & Field, 2017).

Some important issues have reduced enthusiasm for ICT as a technique for the reduction of alcohol consumption and other motivated behaviour (Jones, Hardman, Lawrence, & Field, 2017). First, there are emerging null effects in the published literature, which suggests that estimates of the average effect size in meta-analyses have been overestimated because of publication bias and small sample sizes (Adams, Lawrence, Verbruggen, & Chambers, 2017; Smith, Dash, Johnstone, Houben, & Field, 2017). Second, effect sizes could have been inflated by comparison to control conditions ***that encourage responding to alcohol cues whilst inhibiting to neutral cues. These conditions should strengthen associations between alcohol cues and approach and thereby increase the subjective value of those cues (Schonberg et al, 2014).*** Finally, any effects of ICT on drinking behaviour are seemingly short-lived and easily abolished once participants leave the laboratory (Allom et al., 2015; Bowley et al., 2013; Jones & Field, 2012).

One possible reason for these short-lived effects relates to the associative learning principles that are thought to underlie the effects of ICT on behaviour. Learned associations (e.g. alcohol → inhibition) are thought to be context dependent (Bouton, 2004; Rosas, Todd, & Bouton, 2013). This is particularly evident for extinction learning which does not erase original learned responses, but rather supresses them in the extinction context. Original responses may be renewed in a new context (known as AAB renewal, where B is the renewal of behaviour in a new context, after it is changed in the original context (A) – see Bouton et al (2014)). Any attempt to translate ICT into a viable behavioural intervention requires ICT to be administered across numerous environmental contexts. The rationale for this is that each context will contain different stimulus elements, and increasing the breadth of those elements during ICT will increase the likelihood that any novel context will contain at least some elements that are associated with extinguished responding, thereby reducing the likelihood of renewal of appetitive alcohol associations.

In a recent Randomised Controlled Trial (Jones, McGrath, et al., 2018) we examined whether multiple sessions of internet-delivered ICT combined with a brief intervention (Down Your Drink; Linke, Brown, & Wallace, 2004) led to reductions in alcohol consumption over a four week period in heavy drinkers who were motivated to reduce their drinking. In this study we demonstrated substantial non-specific reductions in alcohol consumption, but no beneficial effect of ICT compared to a control intervention. We also demonstrated little support for any proposed mechanism of ICT, including improvements in general or cue-specific inhibitory control or devaluation of alcohol-related stimuli. ***These disappointing results could have arisen because the ICT training procedure involved only a single stop signal (a red ‘=’ in this study), which may have been suboptimal for training of stimulus-stop associations (as described below). Furthermore, we did not establish if participants completed the training sessions in contexts in which they typically consumed alcohol (e.g. their living room at home, or in pubs or bars), or in contexts in which alcohol was not typically consumed (e.g. their bedroom or office).***

Therefore, if ICT is to yield beneficial effects on alcohol intake, attempts must be made to increase the robustness of training effects such that they can survive a shift in context. According to some associative learning theories of ICT, there are two potential pathways by which ICT works: a direct and an indirect pathway. The direct pathway suggests that an alcohol cue can directly signal an inhibition response (alcohol → inhibition), whilst the indirect pathway suggests an alcohol cue primes the detection of a stop signal, which increases the likelihood of successful inhibition if a stop signal is detected (alcohol → signal → inhibition, see Bowditch, Verbruggen, & McLaren, 2015; Verbruggen, Best, Bowditch, Stevens, & McLaren, 2014). This distinction is important: if ICT influences alcohol consumption via the latter indirect pathway, there are unlikely to be any beneficial effects of ICT on alcohol consumption when alcohol consumers are in contexts that are devoid of stop signals (i.e., all contexts in which alcohol is consumed outside of the laboratory). It is relevant that all of the existing alcohol (and food) ICT studies used single inhibition signals, which may favour the development of indirect (cue → signal → inhibition) associations that are less likely to persist outside of the training context. However, it is possible to train direct (cue → inhibition) associations by using multiple different stop-signals during training (Best et al 2016; Bowditch et al., 2015).

The primary aim of the present study was to apply associative learning theory to increase the likelihood that the effects of alcohol ICT would persist following a shift from a neutral training context to a novel (alcohol-related) context. We designed an ICT paradigm in which the signal or rule to inhibit changed over a series of blocks (based on Best et al., 2016). In our control group, participants were required to respond to alcohol cues on 50% of trials but inhibit responding on the remaining 50%. ***We applied these 50:50 contingencies in order to reduce the likelihood of inadvertently training alcohol-approach associations, and thereby overcome weaknesses in previous ICT studies.*** We examined whether the anticipated effects of ICT in the training context would persist following a shift to the novel alcohol-related context during a subsequent testing session. In addition, we attempted to isolate the effects of ICT on proactive control as a potential mechanism of action, which has yet to be investigated. The presence of proactive control adjustments for stop-associated alcohol cues would indicate that ICT effects are more strategic than initially thought (Best, McLaren, & Verbruggen, 2019). We hypothesised that, compared to the control group, ICT would i) reduce alcohol consumption when administered in both the same context and following a shift in context; ii) lead to an increase in reactive stopping and proactive slowing to alcohol-related cues when tested in both the same context and following a shift in context; iii) lead to devaluation of alcohol-related cues when tested in both the same context and following a shift in context.

**Method**

The study was pre-registered and data is available on the Open Science Framework (<https://osf.io/snp8d>). The experiment employed a 2 (ICT vs Control; between-subjects) x 2 (training context vs. novel context; within-subjects) design. Across two sessions participants completed a stop signal task that measured proactive control and reactive control before and after ICT or control training. In each session participants completed a measure of *ad -libitum* alcohol consumption after ICT/control training. To minimize demand characteristics, the experiment used a cover story (‘Taste perception and cognitive performance in different contexts’).

***Participants***

Sixty participants (40 Female) were recruited, with a mean age of 25.33 ± 6.82 years (range: 18 – 45 years). The study was powered was to detect a medium effect size (f = .25) for a mixed ANOVA (within-between interaction), with 90% power, α = .05 and 10% missing data. Participants were recruited from the university and local community via advertisements placed on the internet and local media. Eligibility criteria required individuals to be aged 18+, to drink in excess of UK government guidelines (14 units of alcohol per week**; *a guide to UK alcohol units was provided in online advertisements***) on a regular basis and self-report liking beer. Participants were excluded if they self-reported a history of substance use disorder and / or other psychiatric disorders. Participants had to be sober at the time of testing, confirmed in all participants by a zero BAC reading at the beginning of each session. The study was approved by the local research ethics committee.

***Materials***

***Stop Signal task***

Each trial began with the presentation of a fixation cross (‘+’) for 500 ms in the centre of the screen. This was immediately followed by an alcohol-related image, presented in portrait or landscape orientation. Images were taken from our previous studies (Di Lemma & Field, 2017) and each depicted alcoholic beverages or models drinking alcohol. Participants had to identify the orientation of the image by pressing a key (‘v’ = portrait, ‘n’ = landscape) on the keyboard as quickly as possible (‘Go trials’). On the majority of trials this was uninterrupted. However, on ‘Stop trials’ two horizontal red lines (‘=’; the Stop Signal) were superimposed over the image, and participants were instructed to inhibit their motor response when this happened. The Stop Signal Delay was set at 250 ms at the beginning of each block and followed a dynamic staircase procedure in which the delay increased by 50 ms for every successful inhibition (max: 1150 ms) and decreased by 50 ms for every failed inhibition (min: 0 ms).

The Stop Signal task consisted of three blocks; No Signal block, Low Probability block, High Probability block. In the No Signal block participants completed 40 Go Trials only (no Stop trials). In the Low Probability block participants completed 90 Go trials and 30 Stop trials (75% / 25% probability, respectively). In the High Probability block participants completed 60 Go trials and 60 Stop trials (50% / 50% probability, respectively). Blocks occurred in a random order across sessions and participants, as did trial types within blocks. All participants completed a short practice block of 10 trials. The task took approximately 15 minutes to complete.

***Inhibitory Control Training / Control*** *(based on Houben, Nederkoorn, Wiers, & Jansen, 2011)*

We used a Go / No-Go task for ICT as this yields the largest effects on alcohol consumption in laboratory studies (Jones, Di Lemma, et al., 2016). Participants were shown alcohol and neutral images in the centre of the screen (***the same images from the Stop Signal task were used***), and target stimuli were superimposed on top of the images. There were four blocks of the task in which the response rule changed on each block. In one block participants had to respond to lower case letters (‘h’ and ‘r’) and inhibit to upper case letters (‘H’ and ‘R’), in a second block participants had to respond to consonants (‘t’ and ‘n’) and inhibit to vowels (‘a’ and ‘e’), in a third block participants had to respond to two different symbols (‘£@’ and ‘@£’) and inhibit to symbols that were the same (‘££’ and ‘@@’), and in a fourth block participants had to respond to numbers higher than five (‘6’ and ‘8’) and inhibit to numbers lower than five (‘2’ and ‘4’). Blocks were counterbalanced across participants and sessions.

There were 200 trials in each block. In the ICT group participants had to inhibit on 90% (90) of trials during presentation of an alcohol cue (responding on 10% / 10 trials) and inhibit on 10% (10) of trials during presentation of a neutral cue (responding on 90% / 90 trials). In the control group participants were required to inhibit on 50% (50) of trials during the presentation of alcohol cues and (50% / 50 trials) of trials with neutral cues; for the remaining trials they had to respond. In between each block participants were asked to complete a word search for a variable amount of time (between 1 – 5 minutes), in an attempt to reduce spontaneous recovery of previous alcohol-approach/inhibition associations by spacing out training blocks (see Bouton, 2002). Across all training blocks there were 360 alcohol-inhibition pairings in the ICT group. The training task took approximately 40 minutes to complete.

**Stimulus Value task** (based on Chen, Veling, Dijksterhuis, & Holland, 2016).

Participants were shown the 20 alcohol-related images from the training task in the centre of the screen and asked to rate how attractive each image was (‘*How attractive do you do you find this image’*) using a visual analogue scale with the anchors ‘Not at all’ and ‘Extremely’ presented underneath. The midpoint of the line was at 0, with ‘Not at all’ at -100 and ‘Extremely’ at +100. Pictures were presented in a random order.

**Balloon Analogue Risk Task (BART)** (Lejuez et al., 2002)

Our rationale for including the BART was to reinforce our cover story. We informed participants that alcohol consumption would likely impair their performance on this task, in order to increase their motivation to limit alcohol consumption during the taste test (see also Christiansen, Cole, & Field, 2012). As it was not of primary interest, data from this task were not analysed.In the BART participants click a button to pump up a balloon and collect a small reward (5 pence) for each pump. Rewards accrue with each balloon and can be banked at any time. If the participant chooses to bank the reward, that is the end of the trial and a new balloon appears. However, if the participant opts to pump the balloon, the probability of it bursting increases. If the balloon bursts during a trial, all rewards accrued on that trial are lost, and the next trial (new balloon) begins. There were five trials in total. Participants were able to keep any rewards that they won.

**Procedure**

***Participants were invited to contact the researchers via phone or email to check eligibility before attending. Upon arrival*** participants attended a neutral laboratory (a conventional psychology testing laboratory, with neutral décor, containing a desk, chairs and a computer) and provided informed consent before completing: a Two Week Timeline Follow Back alcohol diary (TLFB: Sobell & Sobell, 1992) the Alcohol Use Disorders Identification Test (AUDIT: Babor, Higgins-Biddle, Saunders, & Monteiro, 2001); α = .70) to examine hazardous drinking, the Barratt Impulsivity Scale (BIS-11: Patton, Stanford, & Barratt, 1995); α = .65) to measure self-reported impulsivity, and finally the Temptation and Restraint Inventory (TRI: Collins & Lapp, 1992); α’s ranged from .82 to .85) to measure motivation to reduce alcohol consumption. These measures were only completed during the first testing session. Following this, participants completed a baseline measure of craving (Approach and Avoidance of Alcohol Questionnaire; AAAQ (McEvoy, Stritzke, French, Lang, & Ketterman, 2004) α’s ranged from .84 - .89), the Stimulus Value task and baseline Stop Signal Task in that his order. Participants were then randomised using a random number generator to ICT or control groups, before completing the relevant training task. Following completion of the task they either remained in the neutral laboratory (no context shift condition) or were relocated to a semi-naturalistic ‘bar lab’ that resembled a British pub (context shift condition) where they completed post-training assessment measures of the AAAQ, Stimulus Value task and Stop Signal task (AAAQ analyses are presented in online supplementary materials). Participants then completed an *ad libitum* taste test in which they were provided with 300 ml of Heineken (5% ABV), Budweiser (5% ABV) and Old Speckled Hen (6.5% ABV) in unmarked glasses (900 ml total) and instructed to rate each drink on a variety of gustatory dimensions whilst drinking as much or as little as they liked (see (Field & Jones, 2017). They were given 20 minutes to do this. Participants were also informed that following the *ad libitum* taste test they would be completing a task where they could win small amounts of money, and that alcohol could impair performance on this task. This was done to increase motivation to restrict alcohol consumption during the *ad libitum* taste test (Christiansen et al., 2012; Ostafin, Marlatt, & Greenwald, 2008). Following the *ad libitum* session participants completed the BART. If this was their first session their second session was then scheduled; the second session was identical to the first apart from the physical location of the post-training assessments: if these were completed in the neutral laboratory (‘no shift’ condition) in the first session, they were completed in the bar lab (‘context shift’ condition) in the second session, and vice versa. At the end of their second session participants completed a funnelled debrief to examine their knowledge of the experiment with an open-ended question (*‘What was the purpose of the experiment’*) and two multiple choice questions regarding the purpose of the training tasks and the taste test ***(see online supplementary materials)***. Finally, participants were thanked, debriefed and reimbursed £30 plus any money they won during the BART.

***Data reduction and analyses***

We pre-processed reaction time data on Go trials during the Stop Signal task by removing probable anticipatory responses (any reaction times < 200 ms), and trials with errors. We also removed any reaction times that were more than 3 standard deviations outside of the individual’s mean on each block (No Signal, Low Probability block, High Probability block), before computing the mean.  ***This led to the removal of data from 8.98% of Go trials.***

For reaction time data on the training tasks we also removed probable anticipatory responses (< 200 ms), and incorrect responses **(2.65%).** We did not use a-priori standard deviation cut offs on reaction time data from the training task because we expected increased variability in RTs given that there were unequal trial numbers across groups (e.g. 10 Alcohol Go trials in ICT compared to 50 in control). Therefore, we report median rather than means for summary data[[1]](#footnote-1). We computed block-by-block summary data for each trial type.

Shorter Stop Signal Reaction Time (SSRT) is indicative of better inhibitory control. To calculate SSRT we used the integration method with replacement of go omissions (note, in our pre-registration we stated that we would use the integration method, but we subsequently opted to follow best practice of replacing go omissions based on a recently published consensus paper for the Stop Signal Task (Verbruggen et al., 2019). This method subtracts the mean Stop Signal Delay from the *Nth* reaction time. First, we replaced Go Omissions (failure to respond on Go Trials) with the slowest reaction time in the distribution. The *Nth* reaction time wasidentified by ranking the Go trial reaction times in the distribution (including incorrect responses) from fastest to slowest, then multiplying the number of go trials by the proportion of inhibitory failures. For example, if there were 90 go trials and participants failed to inhibit on 40% of Stop trials, the *Nth* reaction time was calculated as 90 \* 0.4 = 36, and therefore SSRT = the 36th Go trial in the distribution. We did this separately for the Low Probability and High Probability Blocks. We removed any SSRTs that were negative (N = 6), or when average RTs on failed inhibition trials were slower than those on Go trials (N = 9), in line with guidance. Note, that recently published simulations (which occurred after our data collection) suggest that more Stop trials than were included in the low signal block may be required to reliably estimate the stopping process (Verbruggen et al., 2019). Therefore, the SSRTs reported below should be interpreted with caution.

We computed a measure of proactive slowing for the Low Probability and High Probability block by subtracting the mean reaction time on Go trials in the No Signal block from the mean Go reaction time on those blocks. Slower reaction times are therefore indicative of proactive slowing (Verbruggen & Logan, 2009). All relevant descriptive data from the Stop Signal tasks (Stop Signal Delays, Reaction Times for Stop Errors etc; see Verbruggen et al., 2019) are included in supplementary materials (supplementary table 2).

Where appropriate, we initially included a between-subjects factor of condition order (no context shift first vs. context shift first) for each ANOVA. If there were no main effects of order or interactions directly relevant to our hypotheses (interactions with time \* condition) we re-ran the analysis without this factor in order to aid interpretation and increase statistical power. We used JASP (JASP team, 2018) to calculate Bayes Factors for our pre-registered hypotheses based on uninformed priors. We report complete ANOVA tables for each hypothesis in online supplementary materials (supplementary tables 3 – 6). Finally, test retest reliability estimates were calculated using the Intraclass Correlation Coefficient (ICC) using single measures from a two-way random model with absolute agreement.

**Results**

**Participant Characteristics (see table 1)**

On average participants drank approximately 41.93 ± 24.64 units of alcohol in the 14-days prior to the first session of the experiment. This did not differ by experimental group (*t*(58) = 0.43, *p* = .67, *d* = 0.11), or gender (males 42.15 ± 18.50, females 41.83 ± 27.41: t(58) = 0.05, p = .96, *d* = 0.01). The average AUDIT score was 11.38 ± 4.69. AUDIT did not significantly differ across groups (*t*(58) = 1.13, *p* = .26, *d* = 0.29) or gender (males = 10.45 ± 3.91; females 11.85 ± 5.02; *t*(58) = 1.09, *p* = .28, *d* = 0.30).

[Table 1 here]

**Performance on training tasks**

***Detailed analyses of reaction times and accuracy of the training tasks are in online supplementary materials. To summarise, there was no evidence of the formation of alcohol-inhibition associations over the course of training in either group, regardless of training context. Inhibition accuracy was high (~95%) during all training blocks (in line with similar ICT studies, as reviewed by Jones et al, 2016), and there was no evidence of an improvement in inhibition across successive training blocks in the ICT group. This may be attributed to the fact that each training block contained 200 trials, which is more than in previous ICT studies conducted in the laboratory (e.g. 80 trials in Houben et al, 2011 and Jones & Field, 2012). As such, optimal performance is reached early during the ICT / control tasks and maintained throughout.***

**Hypothesis one: ICT will reduce alcohol consumption in the same context but also following context shift, compared to control (Figure 1).**

Alcohol consumption data was not normally distributed (Shift skewness statistic = 1.32 ± .31; No shift skewness statistic = 0.77 ± .31). Therefore, we square-root transformed the data which improved the distributions. However, as interpretation of the data did not change we present analyses on non-transformed data below. Similarly, there were three outliers in the Shift group who drank >800 ml in one session, removal of these data points did not significantly alter the results below. Test-retest reliability was acceptable (ICC = .76).

**Figure 1: Amount of alcohol consumed split by group and context shift.**

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Amount of alcohol consumed (ml) at the end of each session for each participant was analysed using a 2 (group: ICT vs Control) x 2 (Context: Shift vs No Shift) mixed ANOVA. There were no main effects of group (*F*(1, 58) = 0.78, *p* = .38, ηp2 = .01) or context (*F*(1,58) < 0.01, *p* = .95, ηp2 < .01). Furthermore, there was no significant group \* context interaction (*F*(1, 58) = 1.81, *p* = .18, ηp2 = .03). The Bayes Factor for group was BF10 = 0.54, and the group \* context interaction was BF10 = 0.06, indicating evidence in favour of the null hypothesis.

**Hypothesis two: ICT will lead to increased a) proactive slowing, b) and reactive stopping regardless of context compared to control (see Table 2).**

**Proactive slowing**

One participant had an outlying number of errors on Go Trials across blocks and time so was removed from analyses. Data was mostly normally distributed (Skewness statistics ranged between .117 and .687, SE = .314). Test re-test reliability was acceptable (ICC = .70). Proactive slowing was examined analysed using a 2 (group: ICT vs Control) x 2 (Block: Low Probability vs High Probability) x 2 (Context: Shift vs No Shift) x 2 (time: Baseline vs Follow-up) mixed ANOVA. The hypothesised group \* time interaction was not significant (*F*(1,56) = 0.88, *p* = .35, ηp2 = .02), nor was the group \* time \* context interaction (*F*(1, 56) = 1.40, *p* = .24, ηp2 = .02). The Bayes Factor for the group \* Time interaction was BF10 = 0.06, suggesting strong evidence for the null hypothesis. There was a main effect of block (*F*(1, 56) = 109.79, *p* < .001, ηp2 = .66), demonstrating that proactive slowing was greater in the high probability block (348.54 ms, SE = 24.38), compared to the low probability block (254.73 ms, SE = 22.74). All other main effects or interactions that are relevant to our hypotheses were not statistically significant.

**Reactive stopping**

We examined SSRT using a 2 (group: ICT vs Control) x 2 (Context: Shift vs No Shift) x 2 (Block: Low Probability vs High Probability) x 2 (time: Baseline vs Follow-up) mixed ANOVA. Test re-test reliability was poor (ICC = .20). The hypothesised group \* time interaction (*F*(1,44) = 0.05, *p* = .82, ηp2 < .01) and the group \* time \* context interaction were not significant (*F*(1,44) = 1.23, *p* = .27, ηp2 < .03).[[2]](#footnote-2) The Bayes Factor for the group \* Time interaction was BF10 = 0.03, suggesting strong evidence for the null hypothesis. There were no other significant main effects or interactions directly relevant to our hypothesis.

**Hypothesis three: ICT will increase the devaluation of alcohol-related stimuli in the same context but also following context shift, compared to control**

Data was missing on a case-wise basis from 8 participants (6 control; 2 ICT). Stimulus evaluations were not normally distributed (Skewness statistics ranged from -0.68 - -0.90, standard errors .32). Data transformations did not improve the distribution. Non-parametric tests did not alter the results, therefore we report parametric tests below. Two participants had outlying values in at least two of the four measures of value, however their removal did not significantly alter the results. Test retest reliability was poor (ICC = .41).

Stimulus devaluation was analysed using a 2 (group: ICT vs Control) x 2 (Context: Shift vs No Shift) x 2 (time: Baseline vs Follow-up) mixed ANOVA. Importantly, the hypothesised group \* time interaction was not significant (*F*(1,50) = 0.02, *p* = .88, ηp2 < .01), nor was the group \* time \* context interaction (*F*(1,48) = 0.17, *p* = .68, ηp2 < .01). The Bayes factor for the group \* time interaction was BF10 = 0.17, which was supportive of the null hypothesis. There was a main effect of time (*F*(1, 50) = 4.14, *p* < .05 , ηp2 = .08), indicating that stimulus values increased at follow-up (19.27, SE = 4.48) compared to baseline (11.40, SE = 3.48). There was no main effect of group (*F*(1,50) = 2.15, *p* = .15, ηp2 = .04). There were no other significant main effects or interactions directly relevant to our hypothesis.

**Discussion**

The aim of this study was to examine whether ICT that was intended to strengthen direct alcohol → inhibition associations in a neutral (lab-based) context would survive a context shift to a high-risk context (semi-naturalistic bar). Our findings demonstrated no support for our hypotheses that ICT would reduce alcohol consumption, strengthen alcohol cue-inhibition associations, or lead to devaluation of alcohol-related stimuli, regardless of the testing context.

We hypothesised that ICT would lead to a reduction in alcohol consumption in the same context but also after a context shift. This hypothesis was not supported, and Bayes factors suggested support for the null. Furthermore, our data does not support previous studies demonstrating reductions in alcohol consumption following ICT when training and outcomes are measured in the same context (Bowley et al., 2013; Di Lemma & Field, 2017; Jones & Field, 2012).

We also failed to find support for our second hypothesis that ICT would lead to changes in both proactive and reactive inhibitory control processes to alcohol-related cues, and these changes would survive a context shift. This is perhaps unsurprising as we have failed to demonstrate this previously (Jones, McGrath, et al., 2018), and there is limited evidence for near- or far-transfer of inhibition training elsewhere (Enge et al., 2014; Talanow & Ettinger, 2018). This suggests that it is highly unlikely that any cognitive training procedures grounded in associative learning principles would produce effects that persist across contexts under most practical circumstances (c.f. Cue Exposure Therapy, Conklin & Tiffany, 2002).

Finally, our findings did not support our final hypothesis that repeatedly inhibiting to alcohol cues would lead to stimulus devaluation. As such, there remains inconsistent evidence as to whether ICT influences stimulus evaluations (Veling et al., 2008), with the effects of food-devaluation (Chen et al., 2016; Chen, Veling, Dijksterhuis, & Holland, 2018; Lawrence et al., 2015) seemingly more robust than alcohol-devaluation (Houben et al., 2012).

We note the following limitations of our study. We were powered to detect a medium effect size for a ***context \* group*** interaction effect ***on ad-libitum alcohol consumption*** (d = .50; slightly larger than current estimates of pooled estimates from recent meta-analysis d = .43 ***for the main effect of ICT on alcohol consumption in laboratory settings [[3]](#footnote-3)*** (Jones, Di Lemma, et al., 2016)). However, we were only be able to reliably detect an effect size of d = .65 for the (between-subjects) main effect of group (at 80% power). Furthermore, the effect sizes on inhibitory control and stimulus devaluation are less clear and likely to be considerably smaller ***(e.g. d’s ranging from .16 - .37 in Chen et al, 2018; BF01 = 0.23, supporting the null hypothesis in Adams et al 2017),*** ***and our analyses required more complex 3 and 4 way interactions because they incorporated the effects of time and proactive control***. Nevertheless, our Bayes factors were broadly supportive of the null hypothesis suggesting our data was sensitive enough to support our inferences. Second, we did not administer ICT in a high-risk drinking environment (e.g. Bar or Pub). ICT may still have therapeutic benefits if administered in environments in which alcohol is present, and future studies may consider utilising Ecological Momentary Intervention techniques to administer ICT (Blackburne, Rodriguez, & Johnstone, 2016). Third***, it is possible that our measure of proactive slowing reflects increased attention to alcohol-related cues, which act as a signal for inhibition (in the ICT) group, rather than strategic slowing. We note that previous ICT studies have demonstrated decreases (rather than increases) in selective attention to trained cues (Stice et al, 2016).*** ***However, future studies should attempt to disentangle proactive slowing from increased attention directly. Finally, we did not measure ad-libitum alcohol consumption at baseline (before ICT), which complicates interpretation of the absence of the hypothesised group difference in alcohol consumption after training.***

***Given the discrepancies with previous findings, future ICT studies should also directly compare the behavioural effects of simplistic ICT training paradigms with the more sophisticated paradigm that was used in the present study. However, methodological issues aside, it is important to interpret the present findings in the context of the broader literature on ICT and related cognitive bias modification interventions, which have weak and inconsistent effects on substance use*** (Boffo et al., 2019; Cristea, Kok, & Cuijpers, 2016).

How then, should we interpret the failure to support any of our hypotheses, in order to best inform the field moving forward? If changes in inhibitory control to alcohol-related cues (alcohol → inhibition associations) are a candidate mechanism of ICT, then one potential explanation for our failure to replicate previously published effects is the absence of any measurable change in inhibitory control to alcohol-related cues during or immediately after training. ***It is clear that our ICT design did not effectively train alcohol → inhibition associations and*** there are multiple potential explanations for this. First, we used a control group of 50% alcohol inhibition contingencies, rather than reversed contingencies (10% alcohol inhibition; c.f. (Di Lemma & Field, 2017; Jones & Field, 2012). Reversed contingency designs are useful in proof-of-concept designs to identify/amplify a target mechanism. However, they are likely to be uninformative (and unethical) comparison conditions in subsequent RCTs as they may increase approach behaviours to alcohol (see, (Bakkour et al., 2016; Schonberg et al., 2014). As such, proof-of concept studies using reversed contingency designs could inadvertently generate inflated estimates of the behavioural effects of ICT. Secondly, it is possible that our training paradigm, whilst designed to amplify direct alcohol → inhibition associations, was too complex because participants had to repeatedly learn different task-rules. ***This might have a counterproductive effect particularly in heavy alcohol consumers (and individuals with alcohol use disorder), who demonstrate cognitive impairments and inability to concentrate (Bernardin et al, 2014, Tembo et al, 2017).*** Third, the reliability of the inhibition errors during the training task and SSRTs were sub-optimal (see also Wostmann et al 2013). The poor reliability of cognitive tasks inevitably reduces confidence in any inferences based on group differences in those tasks (Rodebaugh et al, 2016). ***Future research should conduct rigorous preliminary work to ensure that ICT training paradigms robustly promote learning of cue-inhibition associations, before progressing to investigate the behavioural effects of such training.***

Assuming that a change in alcohol-related inhibitory control (alcohol → inhibition associations) is the proposed mechanism through which ICT causes reductions in alcohol consumption, we cannot conclude that ICT is an ineffective tool for the reduction of alcohol consumption based on the present findings. Instead, we must conclude that our training was ineffective at changing the target construct (c.f. discussions by Boffo et al., 2019; Grafton et al., 2017; Sheeran, Klein, & Rothman, 2017). However, it is worth noting that many ICT studies have failed to test or report the changes in alcohol → inhibition associations following training (Bowley et al., 2013; Houben et al., 2011), or found no changes in alcohol → inhibition associations but have nonetheless detected reductions in alcohol consumption (Di Lemma & Field, 2017; Houben et al., 2012). This further complicates the broader interpretation of ICT effects. ***It is also possible that ICT training conducted in a single brief laboratory session is not sufficient to promote associative learning, particularly when the task complexity is increased as it was in the present study.***

An alternative viewpoint suggests that the change in candidate mechanisms of action is irrelevant when testing interventions (particularly using gold-standard intention to treat principles), and if we do not observe a robust reduction in drinking behaviour then we should interpret ICT as a failed intervention with limited clinical utility in this population (Cristea, 2018; Cristea, Kok, & Cuijpers, 2017). If we follow this line of reasoning, then repeatedly testing failed interventions for unknown mechanisms or boundary conditions serves only to increase wasteful research chasing small unstable effects.

To conclude, in this pre-registered study we add to the growing body of evidence that ICT administered in the laboratory may not yield robust reductions in alcohol consumption in heavy drinkers. Whilst ICT has proved a popular area of study, the recent emergence of negative results means that future researchers may wish to abandon ICT in favour of alternative interventions which may translate outside of the laboratory environment.

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**Table 1: Demographic characteristics of the sample, split by experimental group. Continuous variables are means and standard deviations.**

**Control (N = 30) ICT (N = 30)**

Age 25.70 (7.55) 24.97 (6.11)

Gender (F:M) 9 : 21 11 : 19

AUDIT 12.07 (5.13) 10.70 (4.19)

Units cons. 43.30 (28.18) 40.57 (20.91)

TRI CBC 17.7 (8.89) 16.47 (9.53)

TRI CEP 29.90 (12.42) 24.93 (12.12)

BIS Total 70.77 (8.80) 67.97 (9.74)

*Legend: AUDIT = Alcohol Use Disorders Identification Test; Units cons. = alcohol consumed in previous fortnight; TRI CBC = Temptation and Restraint Inventory Cognitive Behavioural Control subscale; TRI CEP = Temptation and Restraint Inventory Cognitive Emotional Preoccupation Subscale; BIS = Barratt Impulsivity Scale.*

**Table 2: Dependent variables (inhibitory control processes and stimulus value) split by group, time and context. Values are means and standard deviations.**

*Baseline No Shift*  **Control ICT**

Proactive Slowing (High) 345.88 (213.78) 389.02 (197.88)

Proactive Slowing (Low) 234.03 (193.23) 239.21 (183.24)

SSRT (High) 200.09 (74.73) 217.78 (78.62)

SSRT (Low) 226.25 (83.45) 216.07 (67.72)

Value 17.88 (30.11) 9.57 (38.27)

*Baseline Shift*

Proactive Slowing (High) 331.79 (210.64) 394.74 (185.28)

Proactive Slowing (Low) 258.11 (209.26) 323.19 (191.01)

SSRT (High) 226.90 (57.39) 231.78 (97.03)

SSRT (Low) 224.44 (70.34) 231.18 (50.31)

Value 3.70 (38.21) 6.58 (34.50)

*Follow up No Shift*

Proactive Slowing (High) 289.31 (188.42) 417.57 (218.44)

Proactive Slowing (Low) 214.53 (209.69) 321.25 (204.09)

SSRT (High) 218.59 (74.34) 220.30 (68.89)

SSRT (Low) 235.18 (80.51) 230.84 (81.50)

Value 17.38 (38.84) 19.94 (36.99)

*Follow up Shift*

Proactive Slowing (High) 316.82 (234.16) 328.87 (200.04)

Proactive Slowing (Low) 241.59 (208.09) 242.01 (218.09)

SSRT (High) 234.55 (69.41) 258.83 (67.72)

SSRT (Low) 235.54 (69.20) 272.68 (71.33)

Value 17.21 (37.51) 13.71 (36.29)

*Legend: SSRT = Stop Signal Reaction Time.*

1. Note, we did not pre-register an analysis strategy for training data. [↑](#footnote-ref-1)
2. Note the reduction in denominator degrees of freedom in this model is due to a number of negative SSRTs removed from our analyses. [↑](#footnote-ref-2)
3. At the time of pre-registration we were unaware of any studies which had examined a change in ad-lib consumption over time and thus providing an accurate between - within interaction effect size. [↑](#footnote-ref-3)