

**THE EFFECT OF A SELF-AFFIRMATION MANIPULATION
ON THREAT PROCESSING, DRINKING BEHAVIOUR, AND
IMPLICIT RESPONSES TO ALCOHOL CUES IN
HAZARDOUS DRINKERS**

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Thesis declaration form

I confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

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Overview

This thesis aims to add to the literature on self-affirmation as a means of reducing derogation of health risk messages, and engendering behaviour change. By exclusively sampling university students who drink hazardously, this study is also intended to add to the evidence base regarding interventions for alcohol misuse in this high-risk group.

Part 1 of the thesis reviews another type of intervention for student drinking, namely, parent-based interventions. Numerous experimental studies have been published in recent years which examine the efficacy of this type of intervention. Findings suggest that parental influence on young people can extend into late adolescence and the early twenties, and, consequently, delivering an intervention solely to parents can produce effects on youth alcohol misuse. Part 1 therefore aims to summarise what is known about these interventions thus far, and make recommendations for future research.

Part 2 of the thesis reports an experimental study of the efficacy of a self-affirmation manipulation on derogation of a health risk message, as well as changes in drinking behaviour one week after the intervention. Furthermore, it describes the results of a test of the effects of self-affirmation on automatic approach-avoidance biases to alcohol-related stimuli, which were assessed using a Relevant-feature Stimulus-Response Compatibility task.

The third part of the thesis critically appraises aspects of Part 2. It elaborates on the decision-making processes involved in choosing a measure of implicit cognition, and formulating a risk message. It also describes difficulties with recruitment and how these were addressed, and concludes with reflections on the work from a clinical perspective.

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PART 1: LITERATURE REVIEW

EFFICACY OF PARENT-BASED INTERVENTIONS FOR YOUTH ALCOHOL MISUSE: A SYSTEMATIC REVIEW

Abstract

Aims

To evaluate the efficacy of parent-based interventions (PBIs) in terms of youth drinking behaviour, as well as other behaviours and attitudes relevant to youth alcohol consumption.

Methods

PsycINFO, MEDLINE and Web of Science were searched. The reference lists of relevant papers were also read, but no further studies were identified.

Results

Twenty-five studies were reviewed. Ten original studies evaluated the effects of PBIs. Fifteen articles reported follow-up data from the original studies, or further analyses of the data (e.g., examined moderating and mediating variables). Of the 10 original studies, five reported significant effects of the PBI on at least one measure of drinking behaviour. Three studies reported significant effects of the PBI in combination with another intervention targeting youths directly. Three studies reported significant positive effects of the PBI on other behaviours (e.g., alcohol-specific communication) or attitudes.

Conclusions

PBI appears to be a promising intervention for youth hazardous drinking, both alone and in combination with other interventions. However, results are mixed, and further research is warranted to establish the strength of the intervention effect.

Introduction

Misuse of alcohol by young people is a major public health concern worldwide.

Research conducted in the U.S.A. suggests that as many as 19% of children may begin drinking alcohol before the age of 13 (Eaton et al., 2006). This figure rises to 45% for older secondary school adolescents, and the majority of those who report alcohol use engage in binge drinking (i.e., consuming six or more drinks on one occasion; Miller, Naimi, Brewer, & Jones, 2007). Prevalence data varies between countries, as more stringent national alcohol policies addressing underage drinking are associated with lower rates of this behaviour (Paschall, Grube, & Kypri, 2009).

However, although severity varies, youth binge drinking is undoubtedly a problem for society. Violent altercations arising from drinking are reported by 6% of European 15-16 year olds, and unprotected sex is reported by 4% (Anderson & Baumberg, 2006). Several different types of intervention have been developed to target alcohol use in younger adolescents, including family-based (Foxcroft & Tsertsvadze, 2011) and school-based (Foxcroft & Tsertsvadze, 2012) programmes.

Alcohol misuse in university students

Rates of alcohol use rise in late adolescence, and binge drinking is a particular problem on university campuses. Significant numbers of both male and female university students misuse alcohol (Craig, Bewick, Gill, O'May, & Radley, 2012; Gill, 2002). A recent large study involving over 700 UK students found that 80% of students regularly binge drink, and over half drink above the government-recommended weekly consumption guidelines (Penny & Armstrong-Hallam, 2010). Although university students in many countries are over the legal drinking age (e.g., in the U.K. and many European countries), excessive drinking in this population is problematic for many reasons. Alcohol misuse can negatively affect academic performance (Aertgeerts & Buntinx, 2002). It also increases the risk of the student becoming a perpetrator or victim of sexual assault (Blume, Standerwick, Tucker, Harris, & Sheron, 2012), becoming involved in a fight (Hughes, Anderson, Morleo, & Bellis, 2008), and having a car

accident (Fabbri et al., 2002). Over-consumption of alcohol can lead to serious acute health consequences, such as alcohol poisoning, and long-term negative health outcomes, such as cancer (Rehm, Gmel, Sempos, & Trevisan, 2003). Research has also demonstrated a link between alcohol misuse and psychological distress in university students (Markman Geisner, Larimer, & Neighbors, 2004).

Many students begin to drink alcohol for the first time when they reach university (Turrisi, Padilla, & Wiersma, 2000). Research from the U.S. has shown that, when they drink alcohol, university students drink more than age-matched peers who are not in university (Substance Abuse and Mental Health Services Administration, 2006). There is also an association between early regular drinking and later alcohol dependence (Grant et al., 2006); therefore effective early intervention with students is important.

Interventions targeting alcohol misuse on university campuses

Simply 'educating' students about the dangers of alcohol (by providing generic information about negative consequences) is not effective (Larimer & Cronce, 2002). However, there is evidence for the efficacy of several different types of intervention which are specifically targeted to university students.

Research is accumulating which supports the efficacy of personalised feedback interventions. These provide, for example, normative feedback about how much the individual drinks compared to peers (Kypri et al., 2004; Werch et al., 2000). This serves to correct misperceptions relating to the amount of alcohol others drink, and the extent to which drinking behaviour is approved of, as research shows that it is common for students to have incorrect ideas about the behaviour and attitudes of their peers regarding alcohol (Perkins, Meilman, Leichliter, Cashin, & Presley, 1999).

Another type of intervention with a growing evidence base is brief motivational intervention (BMI; Borsari & Carey, 2000; Murphy et al., 2001). These interventions borrow techniques from motivational interviewing (Miller & Rollnick, 1991), which is often used by therapists working with clients who drink hazardously but are ambivalent about change. The therapist seeks to elicit and explore discrepancies between the

client's values and goals and his/her harmful behaviour. The client might be asked to complete a 'decisional balance' sheet, to consider the costs and benefits of change.

Cognitive-behavioural therapy (CBT) techniques have also been used in alcohol interventions with students. CBT interventions involve strategies such as alcohol skills training (e.g., reflecting on personally relevant negative consequences of alcohol, and practising drink refusal skills; Donohue, Allen, Maurer, Ozols, & DeStefano, 2004), self-monitoring (Carey, Carey, Maisto, & Henson, 2006) and challenging positive alcohol-related thoughts and expectations (Weirs, van de Luitgaarden, van den Wildenberg, & Smulders, 2005).

Previous systematic reviews have examined the evidence for these different types of interventions aimed at hazardous drinking among students (Bewick, Trusler, Mulhern, Barkham, & Hill, 2008; Carey, Scott-Sheldon, Elliott, Garey, & Carey, 2012; Foxcroft, Coombes, Wood, Allen, & Almeida Santimano, 2014; Labbe & Maisto, 2011).

However, in recent years a novel intervention targeting youth drinking has emerged - parent-based interventions – which, to our knowledge, has not yet been systematically reviewed (e.g., Turrisi, Jaccard, Taki, Dunnam, & Grimes, 2001). This approach does not involve intervening with young people directly (although it is sometimes used in tandem with a separate, adolescent-targeted intervention), but rather their parents.

The rationale for this is based on new research which contradicts the previously-held notion that parental influence declines as young people progress through adolescence, and is minimal by the time they reach university age. Indeed, parents continue to exert a strong influence on their children's values and behaviour, even as they enter their twenties (Abar & Turrisi, 2008), and parental influence can moderate the effects of peer pressure (Wood, Read, Mitchell, & Brand, 2004). Parental factors such as permissibility of alcohol use, modelling of alcohol consumption (Abar, Abar, & Turrisi, 2009) and monitoring (Barnes, Hoffman, Welte, Farrell, & Dintcheff, 2006) can alter an adolescent's risk of developing high-risk drinking patterns.

Parent-based interventions therefore aim to educate parents in how to communicate with their children about the risks of alcohol and encourage them to abstain from drinking, or limit their alcohol intake (if their use is harmful). The majority of parent-based interventions for alcohol misuse have been conducted with parents of young people at university, although some studies have examined the effects on drinking behaviour of children as young as 12 years (e.g., Koning et al., 2009).

Aims of review

To summarise, evidence is now accumulating which indicates that interventions directed at parents can also have an impact on youth drinking. Therefore, this review aims to systematically examine studies which assess the impact of parent-targeted interventions on alcohol use and misuse in young people. It examines the following questions:

- 1) What effect do parent-targeted interventions have on alcohol consumption?
- 2) How are parent and child behaviours and attitudes relevant to managing harmful alcohol use affected by parent-targeted interventions (e.g., monitoring of the young person's drinking behaviour, parental attitudes and knowledge regarding youth alcohol use and misuse, and alcohol-specific communication between parents and young people)?
- 3) Which mediating and moderating factors affect the efficacy of parent-based interventions?

It should be noted that given the established role of parents influencing alcohol-related behaviour, the review relates specifically to parent-based rather than whole family interventions.

Methods

Search strategy

The PsycINFO, MEDLINE and Web of Science databases were searched for relevant papers, up to the cut-off date of 16th November 2014. Search terms were chosen by conducting initial scoping searches and reading relevant studies. Relevant 'PICO' (*participant, intervention, comparator, outcome*) terms were used to generate keywords.

However, in an attempt to prioritise sensitivity over specificity and ensure no studies were missed, words related to outcomes were not searched, as these are not always included in titles or abstracts of papers (Akers, Aguiar-Ibáñez, Baba-Akbari Sari, Beynon, & Booth, 2009).

The following text word search terms were used: *parent**, *father**, *mother**, *famil**, *alcohol**, “*binge drink**”, *child**, *adolescent**, *youth**, *teen**. The use of the truncation character * allowed for variations in terms- for example, *famil** as a search term yielded papers which included *family* or *families*. OVID subject heading searches also were used in the MEDLINE and PsycINFO databases. These were: *parents/*, *alcohol drinking/*, *child/*, *family/* and *adolescent/*. These terms were used as keywords in the Web of Science database for parity.

Furthermore, in the MEDLINE and PsycINFO databases, the search terms (parent* adj200 intervention) OR (parent* adj200 prevention) were applied to limit the results to papers in which the terms ‘intervention’ or ‘prevention’ occurred within 200 words of ‘parent’. This was decided in consultation with an information science specialist, after initial scoping searches yielded an unmanageable number of irrelevant papers. In the Web of Science database, NEAR/200 was used instead of adj200. The search terms for each concept (youth, alcohol and intervention/prevention) were then searched for separately and, subsequently, in combination (using the Boolean operator ‘AND’).

This strategy yielded 1132 papers from MEDLINE, 1321 from PsycINFO and 1236 from Web of Science. Once duplicates were removed, the total was 1320 papers.

Inclusion and exclusion criteria

English-language studies from peer-reviewed journals were included. Studies were included which evaluated interventions targeted at parents of young people, the aim of which was to reduce alcohol use or misuse, or prevent or postpone the onset of alcohol use or misuse.

Papers were excluded if parents were not the sole focus of the intervention (e.g., if the study involved schools or the wider community, or young people were addressed directly as well as their parents) or if the intervention targeted the use of other substances or other health risk behaviours or problem behaviours (e.g., delinquency) as well as alcohol use. Also excluded were studies in which a family therapy intervention was used.

Titles and abstracts of the 1320 papers were read for relevance, after which 1276 were excluded based on inclusion/exclusion criteria. Forty four papers were identified as potentially relevant and full articles obtained and read fully. A further 19 were excluded at this point, as outlined in Figure 1. The reasons were as follows:

- 1) The study did not involve an intervention (n= 3).
- 2) The intervention was not only targeted at alcohol use (e.g., other problems such as delinquency or misuse of other substances were also targeted (n=4)).
- 3) The intervention did not measure outcomes related to drinking behaviour (n= 4).
- 4) A multi-component or whole-family intervention was used, rather than parents being targeted exclusively (n=3).
- 5) The intervention also targeted youth directly in some manner (n= 5).

A hand search of references from relevant papers did not identify any further studies.

Figure 1. Study Flow Diagram

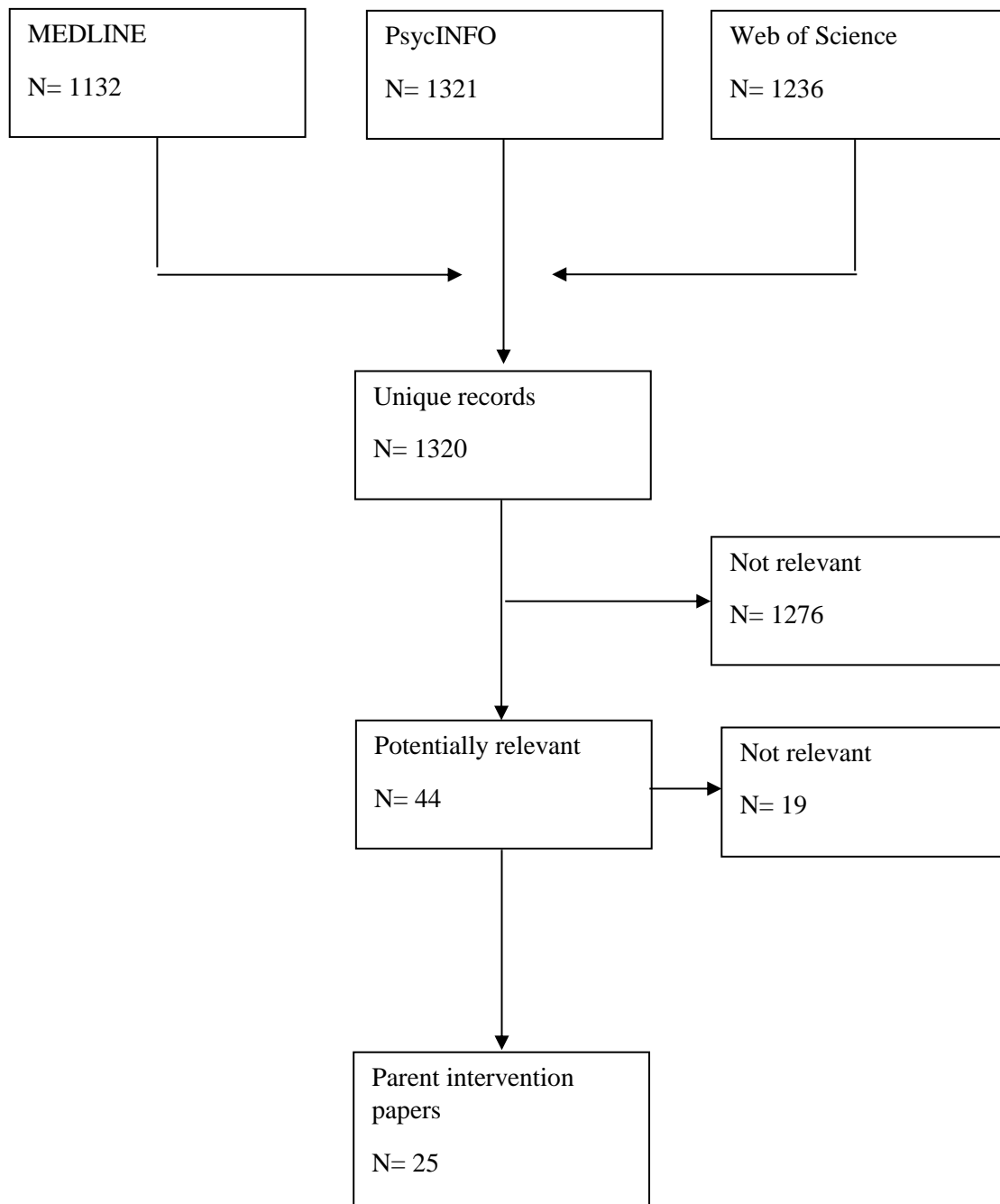


Table 1.
Characteristics of original studies

Study	Design	Sample	N	Follow-up	Measures	Study description and main findings
Bodin & Strandberg (2011)	Cluster RT	7-9 th grade students. Age range: 13-16. Mean age not reported. 51% female.	1752	12 month and 30 month	1) Drunkenness frequency 2) Life-time drunkenness 3) Alcohol consumption 4) Parent attitudes towards youth drinking 5) Parent prevention activities	Schools were randomly assigned to the Orebro Prevention Programme (OPP) or a no-treatment control group. A significant intervention effect was found for only one of three drinking outcomes (frequent drunkenness; $p < 0.02$). This was only present at 12 month follow-up and disappeared at 30 month follow-up.
Donovan, Wood, Frayjo, Black & Surette (2012)	RCT	Undergraduates. Mean age: 18. 62% female.	279	3month and 6month	1) Parent-Teen Communication Scale (Jaccard, Dittus, & Gordon, 2000) 2) Communication about Protective Behavioural Strategies Scale (Martens, Pederson, LaBrie, Ferrier, & Cimini, 2007) 3) Reading of the college alcohol and other drug policy 4) Protective Behavioural Strategies Scale (Martens et al., 2007) 5) Binge drinking	An online PBI group was compared with an e-newsletter control group. Young people in the PBI group were more likely to use protective behavioural strategies around alcohol use than controls ($p = 0.02$). The intervention had no effect on binge drinking. Parents in the PBI group were more likely to talk to their child about ways to ensure safety when using alcohol ($p = 0.04$).
Doumas, Turrisi, Ray, Esp & Curtis-Schaeffer, (2013)	RT	Undergraduates. Mean age: 17.97. 69.5% female.	443	4month	1) Daily Drinking Questionnaire (Collins, Parks, & Marlatt, 1985) 2) Frequency of drinking to intoxication 3) Peak drinking quantity 4) Binge drinking	Three conditions were contrasted: assessment-only control group, PBI, and PBI plus boosters (extra brochures). A significant effect of the PBI plus booster group was found for frequency of drinking to intoxication ($p < 0.02$) and peak drinking quantity ($p < 0.04$). There was no effect on binge drinking or weekly drinking.
Ichiyama et al. (2009)	RT	Undergraduates. Mean age not reported (it is stated that participants were 18 or 19). 63% female.	724	4month and 8 month	1) Typical number of weekly drinks-derived from Daily Drinking Questionnaire (Collins et al., 1985) 2) Heavy episodic drinking 3) Young Adult Alcohol Problems Screening Test (abbreviated; Hurlbut & Sher, 1992)	A PBI group was compared to an intervention-as-usual control group (alcohol fact sheet for parents). A significant effect of the PBI was found in terms of growth in numbers of drinks consumed per week over the first year of university ($p < 0.01$). However, this applied only to female participants. There was no intervention effect on alcohol-related problems or heavy episodic drinking.

Study	Design	Sample	N	Follow-up	Measures	Study description and main findings
Koning et al. (2009)	Cluster RCT	First year high school students. Mean age: 12.6. 49% female.	3490	10 month and 22 month	1) Heavy weekly alcohol use 2) Weekly alcohol use: quantity-frequency measure (Engels, Knibbe, & Drop, 1999) 3) Frequency of drinking	Three conditions were contrasted: PBI, a student intervention, and a combined parent and student intervention. There were no main effects of the PBI alone. The combined parent-student intervention had a significant effect on weekly drinking ($p=0.02$) and frequency of drinking ($p=0.04$) at 22 month follow-up. The combined intervention was effective at 10 month follow-up ($p=0.02$) for heavy weekly drinking, but the effect disappeared at 22 month follow-up.
Toomey et al. (1997)	Cohort	7 th grade students. Mean age not reported. 49% female.	1,028	12 month	<i>Student measures</i> 1) Parent/student communication 2) Student perception of parenting factors 3) Intentions to use alcohol in the next 7 days, 30 days, 12 months or when aged 21 or over 4) Alcohol use in the past year, in the past week and lifetime use <i>Parent measures</i> 1) Parent/student communication 2) Rules against alcohol use and monitoring	PBI materials were sent to the whole sample. Those who returned a postcard indicating they had read them were deemed 'participants' and those who did not were deemed 'controls'. There was no effect of the PBI on youth self-reported drinking. Alcohol-specific communication significantly increased between parents and youth in the parent intervention group immediately after the intervention ($p<0.05$). There was no effect on other parenting behaviours (e.g., communicating with other parents, monitoring).
Turrisi, Jaccard, Taki, Dunnam & Grimes (2001)	Cohort	Undergraduates. Mean age 18.12. 56% female.	154	3 month	1) Drinking tendencies 2) Drunkenness frequency 3) Heavy episodic drinking 4) Perceptions about drinking activities 5) Perceived peer and parental approval of alcohol consumption 6) Binge drinking consequences-3 items from the Young Adult Alcohol Problems Screening Test (Hurlbut & Sher, 1992)	A PBI group was contrasted with a no-treatment control group. Significant reductions were found in drinking tendencies and alcohol-related consequences in PBI participants compared to control participants ($p<0.05$). There was also a significant positive intervention effect on perceptions of drinking, and perceptions of parental and peer approval of drinking ($p<0.05$).

Study	Design	Sample	N	Follow-up	Measures	Study description and main findings
Turrisi et al. (2009)	RCT	Undergraduates-former high school athletes. Mean age: 17.92. 55.6% female.	1275	10month	1) Peak blood alcohol content (derived from Dimeff, Kivlahan, & Marlatt, 1999) 2) Daily Drinking Questionnaire (Collins et al., 1985) 3) Rutgers Alcohol Problem Index (RAPI; White & Labouvie, 1989) 4) Descriptive drinking norms (2 items from the Core Institute's Campus Assessment of Alcohol and Other Drug Norms; Presley, Meilman, & Cashin, 1996) 5) Injunctive norms 6) Beliefs about alcohol 7) Attitudes towards drinking	Participants were randomised to one of four groups: PBI only, Brief Alcohol Screening and Intervention for College Students (BASICS) only, combined PBI and BASICS, or assessment-only control. There were significant reductions in peak BAC, drinks per weekend, drinks per week and alcohol-related consequences ($p < 0.05$) in the combined PBI and BASICS group. There was no effect of the parent-only intervention.
Turrisi et al. (2013)	RCT	Undergraduates. Mean age: 17.94. 52% female.	1900	5month and 15 month	1) Quantity/Frequency Peak questionnaire (Dimeff et al., 1999) 2) Daily Drinking Questionnaire (Collins et al., 1985) 3) Heavy episodic drinking	Four conditions were contrasted: PBI delivered prior to university/college matriculation (PCM), PBI PCM plus booster brochures, PBI delivered after college matriculation (ACM) and control. Participants were divided into 4 categories: (a) nondrinkers, (b) weekend light drinkers, (c) weekend heavy episodic drinkers, and (d) heavy drinkers. The PBI PCM condition led to significantly higher numbers of heavy drinking participants transitioning to lower risk groups at follow-up compared to controls and participants in the ACM group ($p < 0.05$). Effects were maintained at 15 month follow-up.

Study	Design	Sample	N	Follow-up	Measures	Study description and main findings
Wood et al. (2010)	Randomised factorial. study	Undergraduates. Mean age: 18.4. 57% female.	1014	10 month and 22month	1) Heavy episodic drinking 2) Alcohol consequences: Young Adult Alcohol Problems Screening Test (Hurlbut & Sher, 1992) 3) Parent-teen communication about drinking 4) Parental disapproval and permissiveness 5) Parental monitoring :modified version of Strictness/Supervision Scale (Abar & Turrissi, 2008) 6) Assorted BMI mediator measures not listed as they are not relevant to the PBI	A PBI only condition was contrasted with: a Brief Motivational Intervention (BMI) delivered directly to students, a BMI and PBI combined condition, and an assessment-only control condition. Main effects of the PBI on alcohol use were not found. A significant interaction effect of the PBI x BMI was found for participants receiving both interventions- their likelihood of reporting negative consequences of alcohol use was significantly lower, and greater than the sum of the individual intervention effects (Cohen's <i>h</i> : 0.08 at 10 months, 0.21 at 22 months.

Table 2.

Secondary analyses and follow-up studies

Study	Data from:	Main findings
Cleveland et al. (2013)	Turrisi et al. (2013)	Injunctive norms and baseline drinking status moderated the effect of the PBI. Strongest effects were found for weekend light drinkers who endorsed 'high-risk' injunctive norms in the PBI plus booster condition ($p<0.05$).
Cleveland, Lanza, Ray, Turrisi & Mallett (2012)	Turrisi et al. (2009)	Participants in the combined PBI and BMI condition were least likely to progress to heavy drinker status. The PBI alone was most effective at preventing the progression from baseline non-drinker to heavy drinker status ($p=0.01$).
Fernandez, Wood, Laforge & Black (2011)	Wood et al. (2010)	Describes the methodology used in Wood et al. (2010); wherein non-normal data was analysed with latent growth curve modelling.
Koning, Lugtig & Vollebergh (2014)	Koning et al. (2009)	The combined parent-student intervention was effective in curbing alcohol intake in adolescents who were drinking at baseline ($p<0.01$) as well as baseline non-drinkers ($p<0.01$). Growth of drinking was also significantly slower for baseline drinkers in the separate parent intervention ($p<0.01$).
Koning, van den Eijnden, Engels, Verdurmen & Vollebergh (2011)	Koning et al. (2009)	The combined parent-student intervention was still effective at 34 month follow-up for heavy weekly drinking ($p=0.00$) and weekly drinking ($p=0.02$). There was no effect of the separate parent intervention.
Koning, van den Eijnden, Verdurmen, Engels & Vollebergh (2013)	Koning et al. (2009)	The combined parent-student intervention was still effective at 50 month follow-up for amount of alcohol use ($p=0.02$) and heavy weekend drinking ($p=0.02$). There was no effect of the separate parent intervention.
Koning, van den Eijnden, Verdurmen, Engels & Vollebergh (2011)	Koning et al. (2009)	Parental rules and attitudes about alcohol use and adolescent self-control were found to mediate the effect of the combined parent-student intervention (attitudes $p=0.03$; rules $p<0.001$; self-control $p=0.02$). These factors did not mediate the efficacy of the separate parent intervention.
Koning, Verdurmen, Engels, van den Eijnden, Vollebergh (2012)	Koning et al. (2009)	Self-control and lenient parenting were found to moderate the effect of the combined parent-student intervention (self-control $p=0.04$; parental rules about alcohol $p=0.00$). These factors did not moderate the efficacy of the separate parent intervention.

Study	Data from:	Main findings
Mallett et al. (2010)	Turrisi et al. (2009)	Age of drinking onset moderated the efficacy of a PBI. The parent intervention had a significant effect ($p < 0.01$) on peak drinking for youth who began drinking at 17, 16 or 14 or younger, and on weekly drinking in those who began drinking at 17 or 16. However, the combined PBI and BMI intervention was more consistently effective across different subsets of drinkers in terms of age of onset.
Mallett (2011)	Turrisi et al. (2009)	Participants in a combined PBI and BMI intervention with authoritarian or permissive parents had the greatest reduction in peak drinking (p values were not reported).
Strandberg & Bodin (2011)	Bodin & Strandberg (2011)	Significantly lower rates of alcohol were served to youth at home in the parent intervention group ($p < 0.01$). Stricter attitudes to alcohol and higher rates of alcohol-specific rule-setting were also reported in the parent intervention group ($p < 0.001$).
Turrisi, Abar, Mallett & Jaccard (2010)	Turrisi et al. (2001)	Efficacy of the PBI was mediated by attitudes favourable to drinking and reasonable alternatives to drinking, and beliefs about alcohol related-behaviour ($p < 0.001$).
Turrisi & Ray (2010)	Ichiyama et al. (2009)-control group examined	There was a significant association between perceived parental monitoring ($p < 0.01$), accessibility ($p < 0.05$) and approval ($p < 0.01$) and youth alcohol use in the control group.
Varvil-Weld et al. (2014)	Turrisi et al. (2013)	Student participants were divided into categories based on parent type: <i>positive pro-alcohol</i> , <i>negative pro-alcohol</i> , <i>anti-alcohol</i> , <i>negative mother and negative father</i> . Parent type was found to moderate the effects of the intervention with a marginally significant interaction effect ($p = 0.056$): participants in the PCM PBI group with <i>positive</i> , <i>anti-alcohol</i> or <i>negative father</i> parent types were less likely to be in higher-risk drinking pattern at 5 month follow-up.
Verdurmen, Koning, Vollebergh, van den Eijnden & Engels (2014)	Koning et al. (2009)	Level of education and externalising behaviour at baseline moderated the effects of a combined parent and student intervention. Specifically, the intervention was more effective in students attending lower levels of education ($p < 0.01$) and with higher levels of externalising behaviour ($p = 0.03$).

Results

Overview of studies

Of the 25 papers, 10 report original findings. The others either report follow-up data relating to previously reported studies, or different analyses of the data (e.g., examinations of moderators and mediators). All papers were published from 2009 onwards, except Toomey et al. (1997) and Turrisi et al. (2001). Fifteen papers evaluate different versions of a specific 'Parent-Based Intervention' (PBI) for university student drinkers, which was originally developed by Turrisi et al. (2001). Nine papers examine a parent intervention for younger adolescents derived from Sweden's Orebro Prevention Program. The remaining paper, Toomey et al. (1997), also describes an intervention for young students (7th graders) called the *Amazing Alternatives! Home Program*. Of the 10 studies reporting original data, eight were conducted in the U.S.A., one was conducted in the Netherlands (Koning et al., 2009) and one was conducted in Sweden (Bodin & Strandberg, 2011).

Regarding methodology, five studies (Donovan et al., 2012; Doumas et al., 2013; Ichiyama et al., 2009; Turrisi et al., 2013; Turrisi et al., 2009) were randomised trials, two (Bodin & Strandberg, 2011; Koning et al., 2009) were cluster randomised trials, one (Wood et al., 2010) was a randomised factorial study and two (Toomey et al., 1997; Turrisi et al., 2001) were cohort studies.

Nature of the parent intervention

There were two main types of parent intervention used in the included papers. The first is a specific 'Parent-Based Intervention' (PBI) for university student drinkers (Turrisi et al., 2001; n=7 studies). The second is a parent intervention for younger adolescents, derived from Sweden's Orebro Prevention Program (n=2 studies).

Koning et al. (2009) used the latter intervention, which was an adapted (abbreviated) version of the Orebro Prevention Program, consisting of a single 20 minute presentation to parents, followed by an exercise in setting rules for children around alcohol use. An information leaflet was then sent to parents summarising the

content of the meeting. The full Orebro Prevention Program (used in Bodin & Strandberg, 2011) consists of six 20 minute presentations, delivered over two years, when the adolescent is in grades seven to nine. Strict rule-setting around alcohol is promoted, and parents are advised not to permit their children to have any alcohol at home. Parents are then encouraged to make an agreement with other parents in the class to discourage underage drinking. This involves making connections with other parents and agreeing to contact them to ensure children are appropriately supervised at all times.

The remainder of the studies, except Toomey et al. (1997), used some version of Turrisi et al.'s (2001) PBI booklet. The booklet is 35 pages long and consists of four chapters. The first chapter encourages parents to talk to their children about alcohol and contains educational material about the dangers of alcohol for young people. The second chapter delineates specific strategies and techniques for promoting good communication on the subject of alcohol. The third chapter contains information about how to instruct youths in assertiveness and resisting peer pressure. The final chapter discusses alternative ways youths can celebrate special occasions. Wood et al. (2010) used a shorter, adapted version of Turrisi et al.'s (2001) handbook- focusing more on harm reduction than abstinence. Donovan et al. (2012) used an online version of the PBI. They suggest that online features such as streaming video and Flash® technology may increase persuasion and engagement.

Toomey et al.'s (1997) *Amazing Alternatives! Home Programme* comprised four educational booklets which were posted to parents. They contained information about how parents can communicate effectively with adolescents about alcohol. Parents were advised to set rules and consequences for drinking, encourage their children to socialise with their friends at home, monitor their children's whereabouts, and phone other parents to ensure children are being appropriately chaperoned by adults when with their friends.

Original studies (n=10)

Quality evaluation. The quality of the 10 original studies was assessed using the Cochrane Collaboration's tool for assessing risk of bias (Higgins et al., 2011). See Table

3. for ratings. This tool was developed for evaluating randomised trials. As two (Toomey et al., 1997; Turrisi et al., 2001) of the 10 studies in this review are not randomised trials, the tool is used as means of commenting on the methodology of the studies in a systematic fashion, rather than assigning overall quality ratings. Studies are examined for possible sources of bias using six criteria, namely: selection bias (method of random sequence generation and allocation concealment), performance bias (blinding of researchers and participants; blinding of outcome assessment), attrition bias (completeness of outcome data and treatment of non-completers in the analysis), reporting bias (selective reporting) and other sources of bias.

Of the eight randomised trials, three (Donovan et al., 2012; Doumas et al., 2013; Ichiyama et al., 2009) did not describe their randomisation procedure. This indicates a risk of bias, as it is not possible to assess whether or not the allocation sequence would have led to comparable groups.

None of the studies reported whether or not their allocation sequence was concealed. This is a significant weakness, as research has shown that failing to prevent foreknowledge of treatment allocation can lead to inflated estimates of treatment effects (Schulz, Chalmers, Hayes, & Altman, 1995).

Blinding is another area in which the majority of the randomised studies are weak. Only two studies (Bodin & Strandberg, 2011; Wood et al., 2010) reported that they used blinded assessors at baseline, and no study stated that participants were blinded. It is recognised in the research literature that blinding is not always feasible (Schulz & Grimes, 2002), and it may be particularly problematic in psychosocial research, wherein it is clear to participants and staff delivering the intervention what type of intervention is being delivered. However, Bodin and Strandberg (2011) is the only study to consider the issue of blinding as a possible limitation- they acknowledge that the fact that participants were not blinded may partially account for the strict attitudes to youth alcohol use reported by parents in their intervention group.

A category in which the included studies are stronger is attrition bias. All 10 studies reported their attrition rates. Doumas et al. (2013) lost the largest amount of participants to attrition, retaining only 60% at four month follow-up. The rest retained 70% or more. Four studies (Donovan et al., 2012; Doumas et al., 2013; Toomey et al., 1997; Turrisi et al., 2001) did not mention how missing data was handled. This is an indicator of possible bias, as the reader cannot ascertain whether or not an intent-to-treat principle was used.

Inadequate information in most studies made it difficult to assess the possibility of selective reporting. Only two (Bodin & Strandberg, 2011; Koning et al., 2009) of the eight randomised trials stated that their trial had been registered, and quoted the registration number. Emails to the lead authors of the remaining six studies yielded registration details for two further studies - Turrisi et al. (2009) and Turrisi et al., (2013)- see Table 3. for more details.

Table 3.
Quality evaluation of original studies

SELECTION BIAS			PERFORMANCE BIAS	DETECTION BIAS	ATTRITION BIAS	REPORTING BIAS	OTHER BIAS
Study	Random sequence generation	Allocation concealment	Blinding of participants and personnel	Blinding of outcome assessment	Incomplete outcome data	Selective reporting	Any other sources of bias
Bodin & Strandberg (2011)	Randomisation procedure described- school names were placed in sealed opaque envelopes which were mixed and assigned to groups.	No information re. allocation concealment. Risk of bias.	Blinded assessors were used at baseline. Participants were not blinded, which the authors acknowledge may account for the more restrictive attitudes reported by parents in the intervention group.	Follow-up questionnaires were administered by non-blinded assessors.	Attrition rates are reported. Of 1752 participants at T1, 139 (7.9%) had dropped out at T2 and 204 (11.6%) had dropped out at T3. The authors assessed differential attrition and found it was significantly higher in the control group at T2 ($p<0.05$) but there was no significant difference at T3. Analysis suggested that attrition rates were more selective regarding baseline drunkenness in the intervention group at T2 ($p=0.01$). Multiple imputation was used to account for missing data. Imputation was not used for parent reports of restrictive attitudes due to high dropout rates ($>30\%$).	Trial registered at ClinicalTrials.gov (NCT01213108). The registration number was reported in the paper. All planned outcome measures were reported on; no evidence of selective reporting.	The authors note that several national media campaigns targeting alcohol-specific parenting practices may have contaminated results to some degree. They were also unable to monitor intervention fidelity. The number of presentations varied between intervention schools (ranging from three to six), as did the number of classes in which parents made written and verbal agreements.

Donovan, Wood, Frayjo, Black & Surette (2012)	Randomisation procedure not described. Risk of bias.	No information re. allocation concealment. Risk of bias.	It is not specified whether or not participants or researchers were blinded. This study used an active control task (an e-newsletter) so participants may have been blind to condition. Risk of bias.	No information re. outcome assessment blinding. Risk of bias.	Attrition rates are reported. 89% of parents took the 3 month tests and 80% took the 6 month. There is no mention of how missing data was accounted for in the analysis, which suggests there is a risk of bias.	No information. Author was emailed but did not reply.	Youths in the study reported lower levels of binge drinking than the national average, which may have obscured the intervention effects. Also, the authors note that parents and youths who consent to participate in such a study may represent a lower risk group in which communication is already of a high quality. This particular risk of bias could apply to most studies in this review.
Doumas, Turrisi, Ray, Esp & Curtis-Schaeffer (2013)	Randomisation procedure not described- it is implied that participants were randomised electronically after filling in baseline measures online.	No details re. allocation concealment. Risk of bias.	No information re. participant or personnel blinding. Participants filled in baseline measures online after being sent a PIN number by post, which may reduce risk of bias as no researcher was present.	No information re. outcome assessment blinding. Risk of bias.	Attrition rates are reported. 60% of participants (268 of 443) were retained at 4 month follow-up. No difference was found in attrition rates between the 3 groups (p=0.18). It appears that only completers were included in the analysis. Risk of bias.	This trial was not registered (confirmed in an email from the lead author); therefore it was not possible to assess the possibility of selective reporting. Risk of bias	The researchers point out that participant characteristics limit the generalisability of the results- participants were primarily female and Caucasian. Also, intervention integrity was not monitored -parents were not given questionnaires to check they had read the brochures; therefore it was unclear whether or not they read them and implemented them.

Ichiyama et al. (2009)	Randomisation procedure not described- risk of bias.	No details re. allocation concealment. Risk of bias.	No information re participant or personnel blinding. IAU materials consisted of the alcohol policy brochure the university sends out as standard; therefore it seems likely the participants would have known whether or not they were in the intervention group.	No information re. outcome assessment blinding. Participants filled in baseline and outcome measures online, which may reduce risk of bias as there was no researcher present.	Attrition rates are reported. 521 participants of 724 (71.9%) were retained at 8 month follow-up. Completers and drop-outs were compared on baseline outcome and demographic characteristics; no significant differences were found between the groups. Full-information maximum-likelihood estimation was used to account for missing data.	This trial was not registered (this was confirmed in an email from the lead author); therefore it was not possible to assess the possibility of selective reporting. Risk of bias	The authors note that their participants were all recruited from one private Catholic university, which may limit generalisability.
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Koning et al. (2009)	The randomisation procedure is described. An independent statistician used a blocked randomisation scheme. Schools were the units of randomisation.	No details re. allocation concealment. Risk of bias.	No information re. participant or personnel blinding. This study used a no- treatment control group; therefore it seems likely the participants would have known whether or not they were in the intervention group.	No information re. outcome assessment blinding. Student data was gathered by research assistants using online questionnaires. Parent data was collected by posting questionnaires.	Attrition rates are reported. 2771 participants (94.3%) of 2937 were retained at 10 month follow-up, and 2570 (87.5%) were retained at 22 month follow-up. Drop-outs were significantly different from completers in terms of drinking more alcohol per week, being older, and being in lower levels of education. Attrition was unrelated to conditions. The intent-to-treat principle was used in this study. Missing data was accounted for using imputation.	This study was registered on the Netherlands Trial Register (NTR649). The trial registration number was reported in the paper. All measures were reported on- no evidence of selective reporting.	Only schools with less than 25% pupils from migrant populations were invited to participate (as, according to the authors, these children have lower rates of binge drinking), and no schools offering special education were included.
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Toomey et al. (1997)	NA-this study was not randomised.	N/A. All families involved in the Project Northland programme were sent intervention booklets. Those who replied were considered participants, those who did not were deemed controls.	N/A- participants were not pre-allocated to groups.	No information re. outcome assessment blinding.	Attrition rates are reported. 83.1% of 1,028 participants were retained at follow-up 2 (in 'spring 1994'- baseline assessments were in 'fall 1991'- no details re. exact amount of time between baseline and follow-up). There is no information on whether or not non-completers were included in analyses.	N/A	The authors note that both participants and non-participants had previously been exposed to the Project Northland intervention, which also aims to delay youth alcohol debut. Furthermore, the students and parents counted as non-participants were also exposed to the intervention. They were classed as non-participants as they did not return a postcard indicating that they read the materials- however this is no guarantee they were not read and acted upon. Those who were deemed participants self-selected into the programme, which is a source of bias.
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Turrisi et al. (2013)	The randomisation process is described- a computer algorithm was used.	No details re. allocation concealment. Risk of bias.	No information re. participant or personnel blinding.	No information re. outcome assessment blinding.	Attrition rates are reported. 1456 (76.6%) of 1900 participants were retained at second (15 month) follow-up. Non-completers were significantly more likely to be male and to have reported any alcohol use at baseline. The intent-to-treat principle was used; the full sample (including non-completers) was included in analyses. Missing data was handled with the full-information maximum likelihood procedure.	Trial registered at ClinicalTrials.gov (NCT01126151). The trial registration number was not reported in the paper (it was obtained in an email from the lead author). The trial protocol stated that alcohol-related consequences would be measured in addition to drinking measures; however consequences were not reported on in the published paper. Risk of bias.	The authors note that the research was carried out on one university campus with limited racial diversity; therefore results may not be generalisable.
Turrisi et al. (2009)	The randomisation process is described- a computer algorithm was used.	No details re. allocation concealment. Risk of bias.	No information re. participant or personnel blinding.	No information re. outcome assessment blinding.	Attrition rates are reported. 1090 (85.5%) of 1796 participants were retained at 10 month follow-up. There were no significant differences between completers and non-completers in terms of demographic characteristics or drinking behaviours. The intent-to-treat principle was used; a maximum likelihood approach accounted for missing data.	Trial registered at ClinicalTrials.gov (NCT01126164). The trial registration number was not reported in the paper (it was obtained in an email from the lead author).	This study sampled student athletes, so the results may not be generalisable to non-athletes.

Turrisi, Jaccard, Taki, Dunnam & Grimes (2001)	Participants were selected by randomly choosing names from high school yearbooks (randomisation procedure not described); however they were not randomly allocated to groups- a control group was recruited separately. Risk of bias.	Allocation was not concealed- the control group was recruited separately to the intervention group. Risk of bias.	No information re. participant or personnel blinding.	No information re. outcome assessment blinding.	Attrition rates are reported. 97% of 154 participants were retained at 90 day follow-up. No information is provided re. differences between completers and non-completers. No information on whether or not non-completer data was included in the analyses.	N/A	The sample was racially homogenous, which may limit generalisability.
Wood et al. (2010)	Randomisation procedure described- Urn randomisation by computer algorithm.	No details re. allocation concealment. Risk of bias.	Baseline interviewers were independent of the research team and blind to participant condition. Participants were blinded until they completed baseline measures, at which point they were informed of their allocated condition.	No information re. outcome assessment blinding.	Attrition rates are reported. 852 participants (84%) of 1014 participants at 22 month follow-up. There were no significant baseline differences between completers and non-completers on any variables. The intent-to-treat principle was used; a full-information maximum likelihood approach accounted for missing data.	No information. Author was emailed but did not reply.	The authors note that assessment reactivity in the non-PBI participants may have influenced results. They also note that participants were ethnically homogenous and recruited from one university, which may influence generalisability.

Sample characteristics. Sample sizes ranged from 154 to 3490 in the included studies. Sample size is considered in terms of the young drinkers as they are the unit of analysis; therefore they were considered the 'participants' by the researchers. However, samples were composed of parent-adolescent dyads, as the interventions were delivered through parents. Three papers (Bodin & Strandberg, 2011; Ichiyama et al., 2009; Toomey et al., 1997) did not report mean age. For the remaining studies, the mean participant age ranged from 12 to 18.

Female participants outnumbered their male peers in eight out of 10 studies (Toomey et al., 1997 and Koning et al., 2009, were the exceptions, with 51% males each). This imbalance is not representative of the wider youth drinker population, as research shows that adolescent male and female alcohol use patterns are similar, only diverging after the age of 18 when men begin to drink more than women (Schulte, Ramo, & Brown, 2009). Only half of the studies reported parent demographic factors. Mothers were in the majority in each.

Outcomes of original studies. Five of the 10 studies reported a significant main effect of the PBI alone on at least one drinking outcome at one or more follow-ups. However, Ichiyama et al. (2009) found a significant effect only in female participants. One study (Doumas et al., 2013) found a significant effect of the PBI when combined with 'booster' brochures after the original intervention. Three studies (Koning et al., 2009; Turrisi et al., 2009; Wood et al., 2010) found that the PBI was effective when combined with another intervention targeted directly at young participants. A further two studies (Donovan et al., 2012; Toomey et al., 1997) found significant effects of the PBI on non-drinking related outcomes, such as parent and child behaviour, communication or attitudes. See Table 1. for detailed descriptions of outcomes.

Follow-up. Follow-up periods ranged from four months to 30 months (see Table 1. for a full list). The two non-U.S.A. studies (Bodin & Strandberg, 2011; Koning et al., 2009) had the longest follow-up periods. Bodin and Strandberg (2011) followed up participants 30 months after baseline; Koning and colleagues published several papers of follow-up data, the latest of which (Koning et al., 2013) reports outcomes 50 months after baseline. Follow-up

periods in the other studies range from three months to 22 months. Studies which followed up participants less than one year after baseline were more likely to report a significant effect of the PBI on at least one outcome measure (all except Turrisi et al., 2009, who found no main effect of the PBI, but a significant effect when combined with a BMI). Beyond one year after the intervention, only one study (Turrisi et al., 2013) reported a main effect of the PBI alone- at 15 month follow-up. Two studies found a persistent significant effect of the PBI on drinking outcomes when combined with another intervention (Koning et al., 2013 at 50 months and Wood et al., 2010 at 22 months). However, Bodin and Strandberg (2011) found that their combined intervention was only effective on one measure at 12 month follow-up, and this effect disappeared at 30 month follow-up.

Study design. See Table 1. for descriptions of the 10 study designs. Three studies (Bodin & Strandberg, 2011; Toomey et al., 1997; Turrisi et al., 2001) reported a comparison between a PBI and a no-treatment control group, two (Donovan et al., 2012; Ichiyama et al., 2009) compared the PBI to an intervention- as-usual control group and three (Koning et al., 2009; Turrisi et al., 2009; Wood et al., 2010) compared the PBI to another intervention delivered directly to the students. Two studies (Doumas et al., 2013; Turrisi et al., 2013) looked at the effects of adding a 'booster' to the PBI.

Measures. Outcome measures are listed in Table 1. All 10 studies used one or more measures of alcohol consumption. Four studies used measures of alcohol problems or consequences (Ichiyama et al., 2009; Turrisi et al., 2001; Turrisi et al., 2009; Wood et al., 2010). One study (Donovan et al., 2012) used a measure of drinking-related protective behavioural strategies (from Martens et al., 2007). Parent or youth alcohol-related attitudes, beliefs or norms were measured in five studies (Bodin & Strandberg, 2011; Toomey et al., 1997; Turrisi et al., 2001; Turrisi et al., 2009; Wood et al., 2010). Parent behaviour (e.g., monitoring) was measured in four studies (Bodin & Strandberg, 2011; Donovan et al., 2012; Toomey et al., 1997; Wood et al., 2010) and parent-child communication was measured in three studies (Donovan et al., 2012; Toomey et al., 1997; Wood et al., 2010).

Secondary analyses and follow-up articles (n=15)

Mediators and moderators in parent interventions. Ten papers used data from the parent intervention studies to examine moderating and mediating factors. See Table 2. for a summary.

Cognitive characteristics. Cleveland et al. (2013) used data from Turrise et al. (2013) to investigate the effect of student cognitive characteristics on the PBI. They found that the PBI had the strongest effect on participants in the 'weekend light drinker' category, who were deemed high-risk due to their endorsement of injunctive peer norms regarding alcohol. Turrise et al. (2010) examined data from the Turrise et al. (2001) study, and found that favourable attitudes to drinking, beliefs about alcohol-related behaviour, and attitudes to alternative activities to drinking all mediated the association between the PBI and drinking outcomes.

Parenting factors. Mallett et al. (2011) examined parenting style as a moderator in the combined PBI and BASICS intervention used in Turrise et al. (2009). On the basis of questionnaires measuring perceived parent-child relationship quality, parent permissibility of alcohol use, communication style, monitoring, and expertise (which were all measured from the adolescent's perspective) the authors classified parents as *authoritarian*, *authoritative*, *permissive* or *indifferent*. Mallett et al. (2011) found that adolescents in their control group with *authoritarian* parents had the highest levels of alcohol consumption at follow-up, which led them to surmise that this parenting style is the most harmful regarding youth alcohol misuse. The outcomes from the combined PBI and BASICS intervention group showed that adolescents with *authoritarian* and *permissive* parents benefited most from the intervention, that is, they drank significantly less. The authors hypothesise that this is because these two types of parents both have high levels of engagement with their children, as opposed to *indifferent* parents who do not.

Varvil-Weld et al. (2014) also investigated parenting factors as a moderator in Turrise et al.'s (2013) study. They found that a PBI delivered prior to university matriculation was most effective for students with *positive*, *anti-alcohol*, or *negative father* parent types, in

terms of drinking outcomes at five month follow-up. They hypothesise that the PBI may strengthen positive parenting for the *positive* and *anti-alcohol* parents, whilst also addressing negative parenting behaviours in the negative groups, for example, *negative father*.

Koning et al. (2012) examined moderating factors in their combined parent and adolescent intervention, and found that perceived parental rule-setting and youth self-control moderated the efficacy of the intervention. This meant that the intervention was most effective for youth with low self-control and lenient parents at baseline. Koning et al. (2012) point out that this is in line with a risk moderation hypothesis, that is, high-risk groups benefit most from intervention. Parental attitudes to alcohol use did not emerge as a moderator, which the authors speculate may be because many parents disapprove of underage drinking, but effort is required to translate their attitude into behaviour and set strict rules for their children. No moderating effect of self-control or lenient parenting was found for the stand-alone parent intervention.

Turrise and Ray (2010) also investigated the moderating effect of parenting factors in their examination of data from Turrise et al. (2001) and Turrise et al. (2009). They found that perceived accessibility of the parent, parental disapproval of alcohol use and higher levels of parental monitoring were associated with less risky drinking in young participants. However, parental expertise in communicating was less important.

Pre-university drinking. Cleveland et al. (2012) examined data from Turrise et al. (2009) and found that pre-university drinking moderated the effect of the PBI. The intervention was most effective in preventing the students who did not drink at baseline from progressing to drinker status.

Age of drinking onset. Mallett et al. (2010) evaluated age of drinking onset as a moderator in the Turrise et al. (2009) study. They found that the PBI was effective in young people who had started drinking at 16 and 17 years of age, but, conversely, the intervention had an iatrogenic effect in youth who began drinking at age 14 and below. The authors propose that parents of adolescents who begin drinking at younger ages might have different parenting behaviours, and/or their relationship with the young person may have changed

after repeated rule violations on the part of the child. When combined with a BMI, the PBI was consistently effective across different categories of age of onset.

Risk moderators. Verdurmen et al. (2014) tested a risk moderation hypothesis by analysing data from Koning et al. (2009). Their theory was that participants at higher risk would respond better to the intervention, as it is most relevant to them. They found a differential intervention effect of the combined parent and student intervention based on level of education and externalising behaviour in the young participants (i.e., the intervention was more effective for youth with lower levels of education and higher levels of externalising behaviour).

Discussion

This review aimed to examine the growing body of research into parent-based interventions for youth alcohol misuse. So far, 10 unique studies have been conducted using parent interventions. For the most part, these have been high quality studies with robust methodology (randomised trials) and large samples. Furthermore, the data has been explored in depth, with many additional papers examining the effects of timing and dosage of the intervention, as well as investigating different mediating and moderating factors.

How effective are PBIs?

Half of the original studies in this review found a main effect of the parent-based intervention on one or more drinking outcomes. So far, the research seems to suggest that PBIs are more effective when used alongside a separate intervention targeted at young people. Three studies (Koning et al., 2009; Turrise et al., 2009; Wood et al., 2010) found a significant effect of the PBI combined with another intervention on drinking outcomes where the PBI alone was not significantly effective. Wood et al. (2010) found that the combination of a PBI and student BMI intervention produced effects beyond the sum of the effects of the two individual interventions. Delivering a parent intervention simultaneously with a student intervention has the potential to deliver long-lasting effects on drinking outcomes, as Koning et al. (2013) found when they followed up participants 50 months after baseline.

It is difficult to parse the potential for bias inherent in researcher allegiance (Luborsky et al., 1999), as the researcher investigated his/her own intervention in the majority (seven out of 10) of the studies. Rob Turrisi, who developed the PBI used in seven of the studies, was a named author in five of these. All five studies in which Dr. Turrisi was involved reported significant effects of the PBI on drinking outcomes (however, these were only observed in female participants in Ichiyama et al. 2009, and only when the PBI was combined with a BMI in Turrisi et al. 2009). Regarding the two studies using his PBI in which Dr. Turrisi was not a named author, Donovan et al. (2012) found no effect of the PBI on binge drinking (but found that participants exposed to it were more likely to use protective behavioural strategies around alcohol) and Wood et al. (2010) obtained a similar result to Turrisi et al. (2009) - that is, the PBI had a significant effect on drinking outcomes when combined with a BMI, but not alone. Of the remaining three studies which did not use Dr. Turrisi's PBI, only one was authored by researchers who were not involved in the development of the intervention. Bodin and Strandberg (2011) found that the Orebro Prevention Programme (OPP) had a significant effect on only one of three drinking outcomes at 12 month follow-up, and this disappeared at 30 month follow-up. The other study to evaluate the OPP (Koning et al., 2009) was co-authored by Dr. Hakan Stattin, one of the creators of this intervention. This does not appear to have biased the results, as no main effect of the OPP was found (it was effective for drinking outcomes when combined with another intervention). Equally, there does not appear to have been bias in Toomey et al.'s (1997) evaluation of the *Amazing Alternatives! Home Programme*, despite Dr. Toomey's involvement in the development of the wider Project Northland intervention of which it is a part, as the study found no effect of *Amazing Alternatives!* on drinking outcomes.

Advantages of PBIs

As Turrisi et al. (2001) point out, a major strength of parent-based interventions is that the parent can tailor their discussions about alcohol to their individual child's needs based on their close and detailed knowledge of the young person in question. This may be more

helpful than a generic, professionally-led approach. Another advantage of this approach is that parents are “willing and enthusiastic” participants (Donovan et al., 2012) and attrition may be lower when parents rather than adolescents are the participants.

Methodological issues

Most parent and student participants in the studies reviewed here were Caucasian and most parents were mothers. Therefore it is not known whether results would generalise to fathers, or mother of other ethnicities. This issue is not discussed in the majority of the studies. The fact that the young drinkers are considered the 'participants' in all 10 original studies (as they are the unit of analysis) means that demographic information about the parent samples is generally underreported.

Only five of the 10 studies reported the gender breakdown of their parent samples (85% mothers in Donovan et al., 2012; 77% mothers in Ichiyama et al., 2009; 94% mothers in Toomey et al., 1997; 100% mothers plus four additional fathers who requested to take part in Turrise et al., 2001, and 59% mothers in Wood et al., 2010). Turrise et al. (2001) stated that they specifically targeted mothers as prior research led them to believe that mothers would be more willing to participate. Wood et al. (2010) were the only researchers to make an attempt to ensure a gender-balanced parent sample. They tried to include more fathers by randomly targeting one parent of each adolescent to participate, and only recruiting the other parent if the initial target declined. They also conducted secondary analyses to investigate the effect of parent gender (these were mentioned in a footnote and not reported in full). These analyses found no evidence that parent gender moderated the efficacy of the intervention; however they did find that participants from opposite-gender parent-student dyads were significantly less likely to report experiencing alcohol-related consequences at follow-up. It would be useful for future research to consider this issue in more depth, as, although the young drinkers are the beneficiaries of the intervention, it is parents who are receiving and in turn delivering it; therefore the characteristics of the parent sample are important to take into account in determining efficacy.

It is notable that, although student participants do not have to do anything other than fill in questionnaires in these studies, the percentage of invited students who agreed to participate varies widely. These are young people who have not sought an intervention relating to their alcohol consumption, but are having one offered to them via their parents. It is possible that young people who agree to participate in a PBI study may be a lower-risk group than average young drinkers (this is noted by some researchers, for example, Donovan et al., 2012 -students in their sample reported lower rates of drinking and binge drinking than the general student population). As well as possibly drinking less than average students, they may have better relationships with their parents. This consideration applies primarily to studies involving university students, as the three studies which recruited school-aged children and therefore only asked for parental consent (Bodin & Strandberg, 2011; Koning et al., 2009; Toomey et al., 1997) were able to recruit nearly all pupils (95%, 97% and 96% respectively) in the classes they targeted, minimising selection bias.

In the university-based studies, student consent to participate was required, as students had to fill in the measures in their own time (the secondary school pupils in the studies listed above were instructed to fill them in during school hours). The percentage of invited students who agreed to participate ranged from 37% (in Doulas et al., 2013) to 97% (in Turrise et al., 2001). Studies which recruit less than 60% of invited participants may be considered vulnerable to selection bias, according to the Effective Public Health Practice Project Quality Assessment Tool (National Collaborating Centre for Methods and Tools, 2008). This is a weakness in three of the seven studies which recruited university students (Doulas et al., 2013; Ichiyama et al., 2009; Turrise et al., 2009). Of the remaining four studies, Turrise et al. (2013) did not perform significantly better, with only 65% of invited students agreeing to take part, and Donovan et al. (2012) did not report how many students they invited, leaving only two studies (Turrise et al., 2001; Wood et al., 2010) performing well in this area.

Turrise et al. (2001) managed to recruit 97% of invited participants, and was the sole university-based study to recruit parents before students. This may have made it more

difficult for students to decline participation, compared to other studies in which students were able to refuse before their parents became aware of the opportunity to participate. Both Turrise et al. (2001) and Wood et al. (2010) also contacted their potential student participants by telephone, as opposed to post or email in the other studies. All of the university-based studies except for Doumas et al. (2013) paid their student participants a small fee. Doumas et al. (2013) entered them into a prize draw instead, which may explain why they had the lowest reported participation rate at 37%.

Participant ethnicity is another demographic factor which is liable to be implicated in selection bias. Koning et al. (2009) stated that they specifically excluded schools with more than 25% minority ethnic students- their explanation was that these students are less likely to binge drink. Eight of the 10 studies reported the ethnic breakdown of their samples (Koning et al., 2009 and Bodin & Strandberg, 2011 did not report this information, but the latter did state that 13% of the mothers in their sample were not Scandinavian). Caucasian participants were in the large majority in all studies, ranging from 79% in Ichiyama et al. (2009) to 95% in Toomey et al. (1997). Wood et al. (2010) was the only study to explicitly compare their (89% Caucasian) sample to the overall student population to demonstrate representativeness, finding that it was representative of the student body at their North American university.

A minority of studies considered potential confounding factors. Koning et al. (2009) noted that their randomisation process resulted in uneven distribution of gender, level of education and age across the groups; therefore they attempted to control for these factors by including them as covariates in the analysis. Wood et al. (2010) reported that they used a computer algorithm to ensure that groups were balanced on 'prognostic indicators' of college drinking, such as frequent binge drinking and gender. Turrise et al. (2009) stated that gender was imbalanced between the control and intervention groups and controlled for it in analysis. Toomey et al. (1997) attempted to control for gender and demographic factors by including them in the ANCOVA model, as participants not receiving the intervention were more likely to be boys and have more demographic risk factors for drinking. However, they

could not control for the fact that most participants had previously been exposed to the community-wide Project Northland intervention, which may have influenced results.

All studies used one or more psychometrically sound outcome measures. Self-report measures were used in all studies. This is understandable, as although it would be preferable to use objective measures to avoid self-report bias, this is not usually feasible in large-scale studies. Koning et al. (2009) cited research demonstrating that self-report measures are reliable and valid regarding adolescent alcohol use. Bodin and Strandberg (2011) dichotomized their measures of youth alcohol consumption into frequent versus infrequent drunkenness (to enable calculation of number needed-to-treat), which sparked criticism from Ozdemir & Stattin (2012). These researchers (of whom Stattin was involved in developing the Orebro Prevention Programme which Bodin & Strandberg, 2011 were evaluating) argue that the result of the dichotomization may have been a reduction in sensitivity to detect between-group differences.

Regarding drop-out rates in the studies, all but three studies (Doumas et al., 2013; Ichiyama et al., 2009; Turrisi et al., 2013) performed well, retaining over 80% of participants at follow-up. Doumas et al. (2013) retained the least participants, with 60% of participants at four month follow-up. Koning et al. (2009) had the longest follow-up period: four years, with yearly data published in separate papers. The most recent (Koning et al., 2013) reports results 50 months after baseline, and 1064 out of 3490 participants were retained.

Regarding intervention integrity checks, the majority (n= 6) of studies asked parents to evaluate the intervention materials (or, in the case of Toomey et al., 1997, to send a postcard to the researchers) to ensure they had read them thoroughly. However, Turrisi et al. (2013) note that having read the materials does not give any clues as to how the parent actually went about delivering the intervention to their son or daughter. Doumas et al. (2013) stated that they deliberately omitted an integrity check in an attempt to maximise ecological validity, although other aspects of the study resembled an efficacy trial. Similarly, Koning et al. (2009) did not report whether or not they measured integrity-they stated that their trial was intended to be pragmatic and mimic real-world conditions. Bodin and Strandberg (2011)

reported that they were unsuccessful in their attempts to monitor fidelity; they acknowledged that only a small proportion of parents received the full intervention dose of six sessions.

Limitations of this review

Due to the necessity of ensuring methodological rigour, only peer-reviewed journal articles were included in this review. This may mean that unpublished studies have been missed. Furthermore, the decision to only include studies which focused exclusively on the effect of PBIs on youth alcohol use means that PBI studies which examine multiple outcomes (e.g., delinquency, drug-taking etc. as well as alcohol consumption) were not included, which may limit our ability to draw conclusions about the overall efficacy of PBIs.

Future research

The majority of studies in this review were conducted in the U.S.A. There is a need for more research in this area in other countries, for example, the U.K. This is particularly important as it is difficult to generalise the results of these studies to youth in other countries- Koning et al. (2014) note that there are large differences in drinking cultures between different countries (for example, the legal drinking age is 16 in the Netherlands, compared to 21 in the United States). This is an important research area, as PBIs have the potential to be an effective treatment for a widespread problem, that is, alcohol misuse in a population of young drinkers who are unlikely to seek help directly. The clinical implication of this is that parents can be utilised as a resource to implement an intervention promoting physical and psychological health in young people.

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PART 2: EMPIRICAL PAPER

THE EFFECT OF A SELF-AFFIRMATION MANIPULATION

ON THREAT PROCESSING, DRINKING BEHAVIOUR, AND

IMPLICIT RESPONSES TO ALCOHOL CUES IN HAZARDOUS

DRINKERS

Abstract

Aims

Self-affirmation has been shown to enhance receptivity to health risk messages. However, effects on behaviour change have been less well researched. The aim of this study was to investigate the effect of a self-affirmation manipulation on message acceptance in a sample of students who drink hazardously, and determine whether or not self-affirmation would lead to behaviour change at one week follow-up. Furthermore, this study aimed to examine the effects of self-affirmation on implicit alcohol-related cognition with a Relevant-feature Stimulus-Response Compatibility Task (R-SRC).

Methods

Seventy-eight participants (all university students who consume alcohol at hazardous levels) were shown a personalised health risk message relating to their drinking. Prior to this, half of them (n=39) completed a self-affirmation exercise and half completed a control task. All participants then completed the R-SRC task. The amount of alcohol consumed in the week following the experiment was compared to reported consumption in the week before participation.

Results

The self-affirmation intervention had no effect on risk message receptivity, alcohol consumption, or implicit approach-avoidance biases to alcohol-related stimuli.

Conclusions

These findings suggest that self-affirmation may not be an effective intervention for heavy social drinkers. More research with bigger samples of hazardous drinkers should be carried out to establish whether or not self-affirmation can affect attitudes and behaviour in this group.

Introduction

Preventable diseases of lifestyle have become one of the principal causes of ill-health in modern Western societies (World Health Organisation, 2009). Psychological processes play a key role in health-related decision making (French, Vedhara, Kaptein, & Weinman, 2010). Therefore, there is a strong need for psychologists to investigate how health risk messages can best be formulated to maximise their impact and likelihood of being acted upon.

Alcohol-related illness is a particularly pressing public health concern in the United Kingdom. Hospital admissions due to alcohol misuse now exceed one million per year after doubling between 2002 and 2009, and deaths from liver disease have nearly doubled since 2002 (NHS Information Centre, 2013). Aside from liver disease, other well-documented, preventable alcohol-related illnesses include heart disease, stroke, diabetes mellitus and various types of cancer (e.g., breast, upper respiratory and digestive tract), as well as increased risk of accidental injuries (Lopez, Mathers, Ezzati, Jamison, & Murray, 2006). Even relatively low levels of alcohol intake (25g of pure alcohol -the equivalent of three units or one large glass of wine per day) are associated with significant risk for many of these conditions (Corrao, Bagnardi, Zambon, & Arico, 1999). Men and women who drink up to 20g of alcohol per day are nearly twice as likely to develop cancer of the oesophagus -the relative risk is 1.8 compared to non-drinkers- and women in this category have a relative risk of breast cancer of 1.14 compared to abstainers (Rehm, Gmel, Sempos, & Trevisan, 2003).

Over-consumption of alcohol is normative in British society. The Health Survey for England (2013) found that 37% of men and 27% of women drink more than the recommended safe daily unit limit on a weekly basis (Health and Social

Care Information Centre, 2013). Therefore, it is crucial that the health risks outlined above are communicated in an effective manner. The rationale behind many public health campaigns is that fear-inducing health messages will motivate behaviour change. However, the use of graphic representations of illness and disease in such campaigns is often ineffective in changing attitudes or behaviour, particularly in the case of addictive behaviours which are difficult to change (Peters, Ruiter, & Kok, 2013). A number of theoretical perspectives help shed light on this.

Psychological theories of threat processing

Leventhal's (1970) parallel process model distinguishes between cognitive and emotional reactions to a threat. Cognitive processing of threat engages 'danger control processes' resulting in adaptive behaviour change (i.e. those behaviours that reduce the likelihood of avoidable health problems). Conversely, if the threat is predominantly processed emotionally, the individual's focus may be on the experience of fear rather than the threat itself. This engages 'fear control processes' leading to, for example, denial, dismissiveness, minimizing and message denigration (Jacks & Cameron, 2003).

Rogers' (1983) protection motivation theory identifies four factors affecting response to threat: perceived susceptibility, perceived severity, perceived response efficacy and perceived self-efficacy. A combination of high levels of perceived susceptibility, severe threat, and high self-efficacy- coupled with a belief in the efficacy of the suggested response to the threat- is proposed to improve message acceptance. The self-efficacy element may be particularly important in the case of substance dependence (such as alcoholism). As mentioned above, fear messages relating to addictive behaviours are more likely to provoke anger and denial

(Wolburg, 2006), as these behaviours are highly reinforcing and require a large amount of effort to change.

Self-affirmation

Self-affirmation theory (Steele, 1988) proposes a way of overcoming defensiveness towards unwelcome health fear messages in at-risk individuals who might otherwise dismiss them. The theory posits that individuals are often motivated to react to threats to 'self-integrity' by attempting to restore their sense of self-worth. Sherman and Cohen (2006) define self-integrity as the idea that one is a "good and appropriate" (p.7) person, who conforms to cultural norms. Health fear messages regarding diseases of lifestyle can constitute a threat to self-integrity or self-worth, as individuals can feel threatened by information that is not in line with their beliefs and behaviours (Sherman & Cohen, 2006). Threat-compensation effects can be seen across an array of social psychology experiments involving different types of threat (Proulx, 2012). Proulx suggests that any event that violates an individual's self-understanding, or induces anxiety or discomfort, will prompt compensation behaviour to restore 'ego-strength' (Proulx, 2012).

Self-affirmation theory thus suggests that defensive biases can be overcome using an indirect psychological mechanism; namely, affirming other aspects of the self that are not relevant to the threat. This could take the form of reflecting on valued or important aspects of life, allowing the individual to boost his or her self-worth.

Sherman and Cohen (2006) propose that, by bolstering an individual's sense of self, affirmation can promote open-mindedness when considering an anxiety-provoking message. This is because people are motivated to maintain a strong global sense of self-integrity, which, if achieved, makes the individual less susceptible to

distressing effects of individual threats (Steele, 1988). Vohs, Park, and Schmeichel (2013) succinctly summarise: "What self-affirmation does is to get people to accept information about their personal flaws as credible and view those flaws as plausible causes of future problems" (p. 14).

Several previous studies have investigated the effects of self-affirmation on alcohol risk message acceptance in harmful/hazardous drinkers. Harris and Napper (2005) found that women who self-affirmed felt themselves to be at higher risk of breast cancer from alcohol consumption than a control group. However, this did not translate into greater motivation to drink less (Harris & Napper, 2005). Klein, Hamilton, Harris, and Han (2015) also found a significant effect of self-affirmation on receptivity to a message about the link between alcohol and breast cancer.

Klein and Harris (2009) went on to shed further light on how self-affirmation might work to increase message acceptance. They found that women drinkers in their self-affirmation condition paid more attention to threatening words linking alcohol to breast cancer, whereas participants in the control group paid less attention to those words (seeming to avoid them). Napper, Harris, and Epton (2009) found that women who self-affirmed reported giving more thought to the link between alcohol and breast cancer than the non-affirmed group. This fits with the idea that self-affirmation helps reduce defensive avoidance and increases willingness to take threatening information on board. Ferrer, Shmueli, Bergman, Harris, and Klein (2012) found that self-affirmed female student drinkers were more likely to form implementation intentions about changing their drinking behaviour, but this was only the case for participants who were experiencing positive affect. The researchers speculate that self-affirmation is most relevant when affect is positive, as feeling happy engenders heuristic processing, and thus, perhaps, a bias to see alcohol as less

dangerous. Ferrer et al. (2012) propose that self-affirmation facilitates 'mood maintenance', therefore enabling participants to process the message in a more systematic fashion.

However, not all self-affirmation studies have yielded significant results. Good and Abraham (2011) found that, while self-affirmation magnified the impact of efficacy information relating to photoageing and sunscreen use, it had the opposite effect with a skin cancer message. There is also evidence that self-affirmation can make occasional smokers *less* receptive to warning labels (Zhao, Peterson, Kim, & Rolfe-Redding, 2012). Furthermore, Jessop, Sparks, Buckland, Harris, and Churchill (2014, Study 2) found that a self-affirmation manipulation delivered prior to a message about exercise did not make a significant difference to attitudes, response efficacy, intentions or perceived behavioural control.

Harris and Epton (2009) reviewed the literature on self-affirmation and health message acceptance. They concluded that, although many papers report evidence of greater intention to change after self-affirmation, there is less evidence of actual behaviour change. Meta-analytic findings (Webb & Sheeran, 2006) suggest that interventions which have a medium to large effect on intention to change only lead to a small to medium effect on behaviour, on average. A small number of studies have investigated participants' self-reported health behaviour change after self-affirmation. Three studies in the health domain (Harris, Mayle, Mabbott, & Napper, 2007; Harris & Napper, 2005; Reed & Aspinwall, 1998) found that self-affirmation had no effect on self-reported health behaviour. However, more recently Epton and Harris (2008) found that self-affirmation led to significant dietary improvements, and two alcohol studies (Armitage, Harris, & Arden, 2011; Scott, Brown, Phair,

Westland, & Schuz, 2013) found that self-affirmation led to a statistically significant decrease in alcohol consumption.

Aims of the present study

Given the promising findings with mixed samples of drinkers (with varying levels of alcohol consumption -e.g. Armitage et al., 2011; Harris et al., 2009; Klein et al., 2015; Scott et al., 2013), this study aims to extend the investigation of the effects of self-affirmation to hazardous and harmful drinkers. This is one of the first studies which examines behaviour change to exclusively sample problem drinkers. Klein and Harris (2009) sampled 'moderately heavy' drinkers in their study, and Napper et al. (2009) sampled students in the 'top quartile' for alcohol consumption. However, neither of these studies looked at behaviour change, and Napper et al.'s (2009) sample was small (n=35). Therefore, there is a need for more research with samples of hazardous drinkers. The present study recruited only students who exceed the recommended guidelines for weekly alcohol consumption or regularly binge drink.

Self-affirmation and implicit cognition

In their review, Harris and Epton (2009) noted that there is a lack of self-affirmation studies using implicit measures, which limits our understanding of the level of cognition and behaviour at which self-affirmation operates. The present study uses an implicit measure, the Relevant-feature Stimulus-Response Compatibility Task (R-SRC; De Houwer, Crombez, Baeyens, & Hermans, 2001) to ascertain whether implicit feelings about alcohol are affected by a threat message coupled with self-affirmation. A meta-analysis of 89 studies has found that implicit cognition reliably predicts substance use (Rooke, Hine, & Thorsteinsson, 2008). The idea underlying the R-SRC task is that congruent or compatible trials (in this instance, trials in which participants approach alcohol-related stimuli and avoid non-alcohol stimuli) facilitate

quick responding, and incompatible trials (in which alcohol-related stimuli are avoided) have the opposite effect (Krieglmeyer, De Houwer, & Deutsch, 2013).

Research has shown that heavy drinkers approach alcohol-related pictures more quickly than they avoid them on R-SRC tasks (Field, Caren, Fernie, & De Houwer, 2011). There is also evidence that formerly alcohol-dependent patients who are abstaining from alcohol show an avoidance response on the R-SRC task (Spruyt et al., 2013). These findings suggest that automatic cognitive processes are activated when substance-dependent individuals are confronted with stimuli related to addictive substances. There is evidence that substance-related stimuli can provoke automatic approach/avoidance tendencies (Barkby, Dickson, Roper, & Field, 2012; Field et al., 2011; Field, Kiernan, Eastwood, & Child, 2008; Spruyt et al., 2013).

Incentive-sensitization theory (Robinson & Berridge, 1993) suggests that a dopaminergic response is produced each time a substance of abuse is used, and becomes more sensitive with each subsequent use. This leads to motivation to acquire the substance, and experiences of craving. In their review, Field and Cox (2008) explain that substance-related stimuli seize the attention of the substance user and automatically activate valence associations. As a result of classical conditioning, the sight of a substance-related stimulus creates an expectation of availability. This expectation then leads to increased attention to substance-related cues, which consequently leads to increased craving. Thus, attentional bias and craving have a "mutual excitatory relationship" (Field & Cox, 2008, p.3). Some evidence suggests that approach biases can be modified through training, and this can lead to lower rates of relapse in abstaining alcohol -dependent patients (Eberl et al., 2013; Wiers, Eberl, Rinck, Becker, & Lindenmeyer, 2011). However, other studies have found no

relationship between attentional bias and later use of substances (Waters, Shiffman, Bradley, & Mogg, 2003).

Spruyt et al. (2013) argue that these mixed findings indicate that the relationship between substance abuse behaviours and automatic cognitive processes is complex and needs to be studied in different groups and for different substances. This study aims add to the literature by examining implicit responses to alcohol cues in a sample of university students who drink heavily, but are not necessarily alcohol-dependent. It is of interest to ascertain whether this response can be affected by increased processing of a threatening message related to alcohol.

Hypotheses

1. Pairing a self-affirmation manipulation with an alcohol risk message will lead to a statistically significant increase in sense of threat produced by the threat message and intention to change in a group of hazardous drinkers, compared to a control group who view the risk message without prior self-affirmation.
2. Participants exposed to a self-affirmation task will exhibit different approach/avoidance responses to alcohol cues on a Relevant-feature Stimulus-Response Compatibility Task compared to a control group who view the alcohol risk message without prior self-affirmation.
3. An alcohol health risk message coupled with a self-affirmation manipulation will lead to a statistically significant decrease in participants' alcohol intake compared to a control group who are shown the risk message without prior self-affirmation at one-week follow-up.

Methods

Participants

Seventy-eight participants were recruited from the undergraduate and postgraduate student population in universities around London. Ethical approval was gained from the UCL Research Ethics Committee, by submitting an amendment to an existing application for a programme of research relating to substance use in the Clinical Psychology department (see Appendix 1. for documentation). The study was advertised on posters around campus, as well as on a university-wide email system and other internet-based recruitment sites. Mean participant age was 21.8 years. The majority (61.5%) of participants were male (n=48); 38.5% were female (n=30). Participant ethnicity was requested, and 43.6% of participants (n=34) identified themselves as White British, 20.5% (n=16) as Other White, 19.2% (n=15) as Asian

or Asian British, 7.7% (n=6) as Black Caribbean, 6.4% (n=5) as Black African, and 2.6% (n=2) as White Irish. A minority (16.7%) of participants (n=13) were postgraduate students; the rest were undergraduates. See Table 1. for a comparison of demographic factors between the active task group and control group.

Advertisements for the study notified potential participants that they could be eligible to participate if their alcohol consumption regularly exceeded the government-recommended safe upper limit of 21 units per week (for men) or 14 units per week (for women). Guidance was provided as to what constitutes one unit of alcohol, for example, half a pint of regular beer, lager or cider, a small glass of wine, or a single pub measure of spirits. Potential participants either contacted the researcher by email after seeing an advertisement, or signed up to a timeslot if they were recruited via the Sona online participant pool.

At this point, participants were sent the study information sheet by email (see Appendix 1.). Their telephone number was requested and they were screened over the phone with the AUDIT-C (Bush, Kivlahan, McDonell, Fihn, & Bradley, 1998). The AUDIT-C is a short (3 item) version of the Alcohol Use Disorders Identification Test (AUDIT; Saunders, Aasland, Babor, & Grant, 1993). Bradley et al. (1998) report that the psychometric properties of the AUDIT are sound. It has moderate to good test-retest reliability (Kendall's tau coefficient= 0.65-0.85) and moderate criterion validity (based on correlations between the AUDIT and clinical interview- Kendall's tau-b coefficient=0.47-0.66). Regarding discriminative validity, the AUDIT questions are moderately sensitive (54-79%) but highly specific (90-93%). The AUDIT was also found to be highly responsive to change, with a Guyatt responsiveness statistic of 1.04 in terms of discerning a change of seven drinks per week (Bradley et al., 1998). The AUDIT-C has a similar area under receiving

characteristic curve (AUROC) to the full AUDIT for detecting risky drinking (0.891 vs 0.881; Bush et al., 1998)

Scores of five or above indicate higher risk drinking, therefore only students who scored five or above were permitted to participate in the study. The average AUDIT-C score was 9.6, with scores ranging from six to 12. The median score was 10.

Participants were excluded if they reported symptoms of health anxiety, lest the health risk message be too disturbing. This information was obtained in the screening telephone call. Participants were asked whether or not they tend to worry about their health more than their peers. If a participant answered in the affirmative, this was explored in more depth to ascertain whether they truly experienced health anxiety or were merely health conscious. The investigator explained that the experiment would involve viewing health risk messages about alcohol, and warned that this could potentially be distressing.

Sample size

A power calculation performed in G*Power (Buchner, Erdfelder, Faul, & Lang, 2009) indicated that a minimum sample of 74 participants was required to find a medium effect in an independent samples t-test. This was based on $\alpha = 0.05$ and $\beta = 0.8$. The medium effect size was estimated from previous studies (e.g., study 3 in Napper et al., 2009, in which participants were moderate-heavy drinkers and an effect size of $d = 0.59$ was found for a message acceptance measure).

Effect sizes relating to the extent to which participants took on the threat were examined rather than behaviour change, as the former has been more thoroughly researched; therefore it was assumed that the likelihood of finding an effect would be higher for this outcome.

Measures

Alcohol consumption. Self-reported alcohol consumption was measured using the Sobell and Sobell's (1992) timeline followback technique, which is designed to minimise memory errors. Test-retest reliability for this measure is good ($r=0.83-0.95$; Sobell, Brown, Leo & Sobell, 1996).

Self-affirmation task. The Values in Action Strengths Scale was used to construct a writing task (Peters & Seligman, 2004; adapted from previous affirmation studies e.g. Napper et al., 2009) for the self-affirmation manipulation. Respondents are asked to write about their most important value (or least important, in the case of control group participants).

Threat message. Personalised risk messages were constructed regarding the link between alcohol consumption and adverse health outcomes based on participant gender and amount of alcohol consumed, as indicated on the timeline followback measure. Relative risk probabilities were taken from epidemiological data and converted into percentages. For example, a female participant who had consumed between 25 and 50g of alcohol per day on average (approximately 21-42 units in a week) would see the following message: "Your personal risk of developing oral or pharyngeal cancer is 40-231% higher than a non-drinker. Your personal risk of developing liver cancer is 97- 257% higher than a non-drinker. Your personal risk of developing oesophageal cancer is 52-124% higher than a non-drinker". This information was based on research into the relative risks of developing different types of disease based on gender and quantity of alcohol drunk (Bagnardi, Blangiardo, La Vecchia, and Corrao, 2001; Turati et al, 2013).

Message derogation A two -item message derogation measure (adapted from Jessop, Simmonds, & Sparks, 2009) was used to assess what participants thought of

the threat message. Respondents are asked to rate the extent to which they think the message was "overblown or exaggerated" and "tried to manipulate [their] feelings" on a 7-point Likert scale ranging from "strongly disagree" to "strongly agree". The internal reliability of this measure is high ($\alpha = 0.88$; Jessop et al., 2009)

Perceived threat A one-item perceived threat measure (taken from Witte, 2010; adapted by Armitage et al., 2011) was used to ascertain how frightening the participants found the message. Respondents are asked to rate how much the message made them feel frightened tense, nervous, anxious or uncomfortable. This is also assessed on a 7-point Likert scale, with responses ranging from "not at all" to "very much".

Intention to consume less alcohol. A one-item measure of participants' intentions to cut down on alcohol (modified from Ferrer et al., 2012) was used. Participants are asked to respond to the statement "I intend to cut down on my alcohol use in the next seven days". Responses range from "strongly disagree" to "strongly agree" on a five-item Likert scale.

Implicit response to alcohol (R-SRC) measure. In the R-SRC computer-based task, participants move a manikin away from or towards alcohol-related pictures, while their reaction times are assessed. Stimuli consist of 32 pictures. Half (16) are alcohol-related pictures (e.g., pubs, people drinking different types of alcoholic beverages, glasses and bottles of alcohol in different settings). The other half are alcohol-unrelated pictures, which are matched for content to serve as controls (e.g., a glass of water as a control for a glass of beer). Pictures are 246 pixels in height and between 182 and 343 pixels in width. The width and height of the manikin is 52 pixels and 79 pixels, respectively. In the 'approach alcohol' task, the participant moves the manikin towards alcohol-related pictures and away from neutral pictures.

In the ‘avoid alcohol’ task they do the opposite. It is hypothesised that if the participant’s reaction times are faster in the “approach alcohol” block, this reflects an implicit motivation to drink alcohol.

Procedure

Testing took place in a quiet room in an academic clinical psychology department. Participants were presented with a copy of the information sheet to re-read and keep if they wished. Each participant gave written informed consent (See Appendix 1. for consent form). They were notified that their data would be held securely, in accordance with the Data Protection Act 1998. It was explained that this meant that information would be held electronically in strictest confidence, and names would be divorced from data by assigning a numbered code to each participant. Furthermore, participants were told that their signed consent forms would be kept in a locked cabinet, and shredded when no longer needed.

All self-report questionnaire and SRC stimuli were presented and recorded on a laptop computer, with a screen width of 15.6 inches. The experimenter remained in the room during data collection, but sat facing away from the laptop to put participants at ease. The Qualtrics (Provo, UT) online platform was used to collect questionnaire data. Measures were completed in the order referred to above, beginning with the timeline followback questionnaire (Sobell & Sobell, 1992). Participants were asked to recall the number and type of alcoholic beverages they drank each day in the previous week, both before the manipulation and again at one-week follow-up. Units of alcohol were calculated with the help of the experimenter.

Participants were then randomised to either the self-affirmation condition or the control condition, using a randomisation algorithm provided by the Qualtrics system. Participants in the self-affirmation group were asked to reflect on their

values, which were explained as "the moral principles and standards by which people try to live their lives". An example was given describing how one might display the value 'honesty' in one's life (by trying to be honest in all one's dealings with others). A list of 11 further values was then presented (conscientiousness, trustworthiness, spirituality/religiousness, creativity, compassion, spontaneity, intelligence, friendliness, generosity, kindness and hedonism) for participants to choose from. The message specified that participants should choose their most important value, which did not necessarily need to be one from the list. They were asked to write two to three paragraphs about how their most cherished value influences their behaviour and attitudes, and how they display it in their everyday life.

Participants were urged to think about specific occasions in which they displayed the value in question. The control group was presented with the same task, but asked to write about why their least important value might be important to someone else (as in Fein & Spencer, 1997). Participants were asked to alert the experimenter upon completion of the writing task, to ensure they had indeed written at least two paragraphs. Adherence was checked by very briefly scanning how much had been written, rather than actually reading the statements, as this might have made participants feel uncomfortable.

Participants were then shown one of seven risk messages. These were automatically displayed by Qualtrics based on the participant's previously-recorded gender and alcohol consumption data. They then completed the message derogation, perceived threat, and intention to reduce alcohol consumption measures. Following this, the R-SRC task was introduced and explained by the experimenter. Participants moved the manikin by pressing the arrow keys on the laptop keyboard. They could either press the key continuously to move the manikin, or use a succession of shorter

keystrokes. The task consisted of two blocks, comprising 64 'trials' in total. In the 'compatible' block of the task, participants were directed to make the manikin approach alcohol-related pictures and avoid alcohol-unrelated pictures. In the 'incompatible' block, the instructions switched to the opposite- i.e. approach alcohol-unrelated pictures and avoid alcohol-related pictures. Participants were given eight practice trials in each block, to help them get accustomed to the task. Each picture in each block was shown twice- once with the manikin above the picture, and once with it below, so that participants had to approach or avoid each picture from both directions. The blocks were counterbalanced in order across participants. Accuracy of responses and reaction times were recorded for each trial.

One week after taking part in the study, participants were contacted by telephone to ascertain how much alcohol they had drunk each day since the experiment, again using the timeline followback measure (Sobell & Sobell, 1992). Units of alcohol were calculated by the experimenter. Participants were paid £7 for participation or (in the case of UCL participants) granted course credits, depending on their preference.

Results

As outlined in Table 1., the self-affirmation group did not differ from the control group in terms of gender, age, ethnicity, level of education, AUDIT score or BDI score. Adherence was ensured using forced responses on questionnaire measures. The majority of participants wrote at least one paragraph in the self-affirmation/control task. The average number of words written was 153 words in the control group, and 186 words in the self-affirmation group.

Table 1.
Demographic data by group and between-group difference statistics

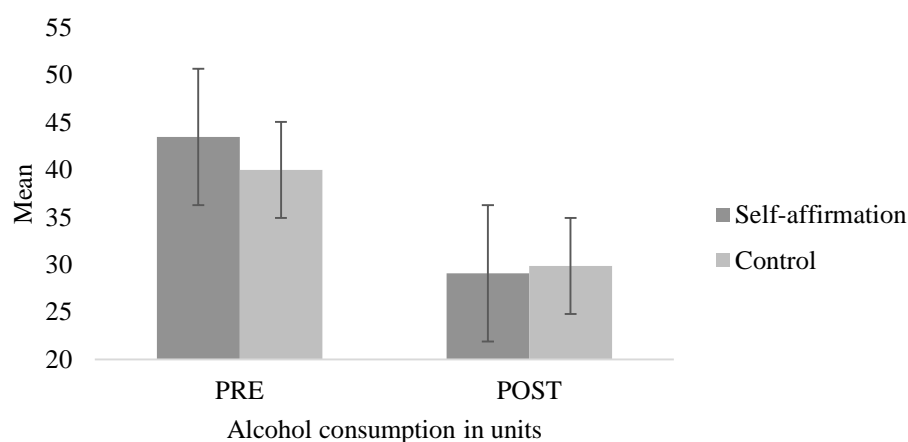
	Self-affirmation (n=39)		Control (n=39)		t	χ^2	p-value
	<u>Mean</u>	<u>SD</u>	<u>Mean</u>	<u>SD</u>			
Demographic							
Age	21.72	3.61	22.00	5.19	-0.28		0.78
AUDIT score	9.59	1.19	9.59	0.94	0.00		1.00
BDI score	11.69	6.08	10.21	4.70	1.21		0.23
	<u>Frequency</u>	<u>%</u>	<u>Frequency</u>	<u>%</u>			
Gender						0.22	0.64
Female	14	35.9	16	41			
Male	25	64.1	23	59			
Ethnicity						3.52	0.62
White British	19	48.7	15	38.5			
Other White	5	12.8	11	28.2			
Asian/Asian British	9	23.1	6	15.4			
Black African	2	5.1	3	7.7			
White Irish	1	2.6	1	2.6			
Other	3	7.7	3	7.7			
Level of education						0.22	0.9
Undergraduate	32	82.1	33	84.6			
Postgraduate	7	17.9	6	15.4			

Notes: Bootstrap p-value based on 5000 bootstrap replications. Equal variance not assumed.
BDI: Beck Depression Inventory. AUDIT: Alcohol Use Disorders Identification Test

Alcohol consumption

In the week preceding the experiment, participants reported consuming a mean of 41.7 units (SD= 17.7, range= 15-88). Mean reported consumption fell to 29.4 units (SD= 17.9, range= 0-84) in the week between baseline and follow-up. This drop was reflected in the results of the repeated measures ANOVA, which revealed a significant main effect of time ($F(1, 76) = 30.6, p < 0.01$) on consumption. However, there was no group x time interaction ($F(1, 76) = 0.92, p = 0.34$), suggesting that self-affirmation had no differential effect on alcohol intake.

Figure 1:
Alcohol consumption means



Effects of self-affirmation on measures

Consistent with the finding that the self-affirmation intervention made no difference to behaviour, it also made no difference to reported responses to the threat message. Independent samples t-tests were carried out to investigate the effect of the intervention on four post-manipulation measures, namely message derogation, sense of threat, intent to reduce alcohol consumption, and likelihood of developing cancer (self and other). As scores were not normally distributed in over half of the items, 95% confidence intervals were estimated using accelerated, bias-corrected bootstraps with 5000 replicates. These tests revealed no effect of the self-affirmation intervention. See Table 2. for statistics. Each item of each measure was investigated separately (as opposed to using summary scores). This increased the risk of Type I error and would have necessitated a correction (such as the Bonferroni correction) if there had been any significant findings. However, this was not necessary as there were no significant p-values.

The median score for both groups on the likelihood of developing cancer measure (on both the 'self' and 'other' questions) was 6 for both groups, indicating

that the most popular response was *undecided*. For all items of the threat measure, the median score was 3, which falls between *not at all* [threatening] and *moderately* [threatening]. These scores indicate that, on average, participants were somewhat dismissive of the message, either because it was genuinely ineffective or they were defensively motivated to dismiss it. The median scores on the message derogation items were also consistent with the other findings. For the *overblown or exaggerated* item, the median score was 4, indicating a response of *neither agree nor disagree*. This rose to 5 on the tried to *manipulate your feelings* item, which corresponds with a response of *somewhat agree*. For the intentions item (*I intend to cut down on the amount of alcohol I drink in the next seven days*) the median score was 3, indicating a response of *neither agree nor disagree*.

Table 2.
T-test results

	Self-affirmation (n=39)		Control (n=39)		T	Df	p-value
	<u>Mean</u>	<u>SD</u>	<u>Mean</u>	<u>SD</u>			
<i>Cancer risk</i>							
Self cancer risk	5.85	2.39	5.51	2.3	0.63	76	0.53
Other cancer risk	5.44	2.37	5.26	1.85	0.37	72	0.71
<i>Message derogation</i>							
Information overblown	4.1	1.37	3.62	1.6	1.44	74	0.15
Information manipulated feelings	4.85	1.48	4.28	1.76	1.53	74	0.13
<i>Sense of threat</i>							
Frightened	3.41	1.71	3.13	1.53	0.77	75	0.45
Tense	2.97	1.65	2.69	1.56	0.78	76	0.44
Nervous	2.9	1.52	2.95	1.54	-0.15	76	0.88
Anxious	3.15	1.69	2.9	1.62	0.68	76	0.5
Uncomfortable	3.74	1.9	3.23	1.74	1.24	75	0.22
<i>Intent to reduce</i>							
Intent to reduce	3.1	1.1	3.18	1.1	-0.32	76	0.75

Notes: Bootstrap p-value based on 5000 bootstrap replications. Equal variance not assumed.

Relevant-feature Stimulus-Response Compatibility Task

A repeated measures ANOVA was carried out to gauge the effect of the self-affirmation manipulation on implicit responses to alcohol. All variables were significantly positively skewed, therefore a square root transformation was applied to every variable. Results showed a main effect of task ($F(1,76)=89.41, p<0.001$) and of stimulus ($F(1,76)=91.91, p<0.001$), but no effect of task x group ($F(1,76)=0.93, p=0.34$). From this we can infer that the intervention had no effect on participants' implicit responses to alcohol. The main effect of task showed that participants were faster to approach than avoid stimuli overall. The main effect of stimulus demonstrated that participants were quicker to avoid *and* approach alcohol stimuli compared with non-alcohol stimuli.

Discussion

This is one of the first self-affirmation studies to exclusively sample hazardous drinkers, and to investigate the effects of a self-affirmation manipulation on implicit attitudes to alcohol. There was no difference between self-affirmed and control participants on measures of defensive message processing, sense of threat, message derogation or intention to reduce alcohol consumption. There was also no difference in alcohol consumption between the two groups at one-week follow-up.

Furthermore, there were no between-group differences on implicit responses to alcohol-related stimuli following the manipulation. The results of this study replicate recent null findings from a study which sampled heavy-drinking undergraduates (Meier et al., in press). However, they contradict previous studies suggesting that self-affirmation reduces defensive processing of alcohol risk messages (Harris & Napper, 2005) and can prompt a greater reduction in alcohol consumption than a control task (Armitage et al., 2011; Scott et al., 2013).

Self-affirmation manipulations are low-cost, quick, and easy to implement. The findings of this study are of considerable relevance to clinicians working with alcohol-dependent clients. For example, Motivational Interviewing (which therapists often use with alcoholic clients to build motivation to change) frequently involves giving information about alcohol and the health risks of drinking to excess (Miller & Rollnick, 1991). Self-affirmation has the potential to be a useful tool for clinicians using this model (Ehret, LaBrie, Santerre, & Sherman, 2013). Moreover, many brief interventions for alcohol involve providing self-threatening personalised feedback related to the health risks of continued drinking (White, 2006). This study investigated ways to optimise information-giving to reduce the likelihood of defensive reactivity. However, the null findings suggest that self-affirmation may not be effective in hazardous drinkers.

Both the self-affirmation group and the control group drank significantly less at follow-up, suggesting the personalised risk message was effective in itself. There are a number of potential explanations for this, including the Hawthorne effect (McCambridge, Witton, & Elbourne, 2014), which is difficult to rule out in the absence of a control group that received no risk message. Nonetheless it is worth considering that effective risk messages can prompt behaviour change, particularly when risk is high (Waldron, van der Weijden, Ludt, Gallacher, & Elwyn, 2011). Furthermore, there is evidence that personalised risk feedback alone, without self-affirmation, can have an effect on alcohol consumption (Bewick, Trusler, Mulhern, Barkham, & Hill, 2008), which may account for this result.

In fact, it is possible that the personalised risk message was *so* threatening that (although it later led to behaviour change) immediately after viewing it provoked defensive reactivity and, consequently, message rejection, even in the self-

affirmation group. For example, a female participant who had drunk more than 42 units in the previous week would have seen the following message: "Your personal risk of developing oral or pharyngeal cancer is over 231% higher than a non-drinker. Your personal risk of developing liver cancer is 257-815% higher than a non-drinker. Your personal risk of developing oesophageal cancer is 124- 345% higher than a non-drinker". Many participants told the researcher after the experiment that their knowledge of probability and risk meant they were aware that, while their relative risk may appear high, their absolute risk is low.

If the message was rejected as it did not inspire fear (as opposed to being too frightening), it is confusing that participants drank significantly less at follow-up. One possible explanation lies in Sweeney and Moyer's (2015) meta-regression finding that intentions did not predict behaviour. Therefore, participants may not have recorded an intention to change their behaviour if the message struck them as overblown and irrelevant, but found themselves drinking a smaller amount in the ensuing week nonetheless. They note that this does not mean that the two variables are not correlated, and it is not possible to shed further light on this as very few self-affirmation studies have reported correlation data. It could also be the case that, as young students, the prospect of developing cancer seemed too remote. Epton et al. (2015) found in their meta-analysis of self-affirmation studies that effects were stronger in studies in which the hazard was proximal.

Another possibility (which has also been considered by previous researchers, for example Harris & Napper, 2005) is that, as high-risk drinkers, participants may have discerned an implicit instruction from the researcher to drink less at follow-up. However, there was no direct suggestion to participants to drink less, as no support or action plan was provided.

It is somewhat surprising that this study did not find an effect of self-affirmation on message acceptance, as this has been found in many previous studies focusing on alcohol risks (Armitage et al., 2011; Klein & Harris, 2009; Napper et al., 2009; Scott et al., 2013). Looking beyond alcohol to self-affirmation research in the health domain more broadly, these effects have also been found for messages related to many different health behaviours aside from alcohol, including smoking (Armitage, Harris, Hepton, & Napper, 2008), condom use (Sherman, Nelson, & Steele, 2000), physical exercise (Cooke, Trebaczyk, Harris, & Wright, 2014), caffeine intake (Klein, Harris, Ferrer, & Zajac, 2011) and sunscreen use (Good & Abraham, 2011).

However, this result was not unprecedented, as several studies did not find increased message acceptance in self-affirmed participants (Dillard, McCaul, & Magnan, 2005; Harris & Napper, 2005; Meier et al., in press). Recent meta-analytic findings (Epton et al., 2015) suggest that the aggregate effect size for message acceptance ($d=0.17$) is smaller than anticipated based on the research available at the time this study began in 2012. The evidence base in this area is growing very quickly- Epton et al. (2015) note that more than 75% of studies have been published since 2008. Self-affirmation may have a smaller effect on intentions to change behaviour than it does on message acceptance, as several studies failed to find a significant group difference in intentions (Epton & Harris, 2008; Good & Abraham, 2011; Jessop et al., 2009). Reed and Aspinwall (1998) even found that control participants had stronger intentions to reduce their caffeine intake than affirmed participants.

Regarding behaviour change, several health studies have found no effect of self-affirmation (Harris et al., 2007; Harris & Napper, 2005; Reed & Aspinwall,

1998). Two recent meta-analyses (Epton et al., 2015; Sweeney & Moyer, 2015) on self-affirmation and health messages found small to medium effect sizes for both intentions and behaviour. Sweeney and Moyer (2015) also noted that there was significant variability in effect sizes for intentions. The aggregate effect size was $d=0.26$ for intentions and $d=0.27$ for behaviour in Sweeney and Moyer (2015) and $d=0.14$ for intentions and $d=0.32$ for behaviour in Epton et al. (2015). It is therefore possible that this study was underpowered to find an effect. However, Sweeney and Moyer (2015) found that effect sizes were larger for studies which focused on health-damaging as opposed to health-promoting behaviours. They cite an effect size of $d=0.33$ for the effect of self-affirmation on behaviour change specifically for studies focusing on health-damaging behaviours, for which this study was sufficiently powered.

Moreover, effect sizes were higher for studies which used distal rather than proximal measures of behaviour (an example of a proximal measure would be taking a leaflet directly after the experiment). The aggregate effect size for distal measures - such as the one-week follow-up used in this study - was $d=0.33$. Again, the present study was powered to find an effect of this size, but did not. Furthermore, as noted above, this is one of the first studies to include only high-risk drinkers, which should have increased the likelihood of finding an effect. This is because self-affirmation functions as a self-integrity booster (Steele, 1988); therefore it is only relevant to individuals whose behaviour poses a health risk to themselves and might consequently feel threatened by a health risk message. However, it is important to note that Armitage et al. (2011) found that risk status did not moderate the effect of the intervention in a mixed sample of heavy and light drinkers.

Aggregate data was unavailable during this study's inception; therefore power analysis was based on the medium effect sizes for message receptivity reported by studies such as (Napper et al., 2009; study 3). However it must be noted that the present study did not use exactly the same measures as Napper et al. (2009), which may have affected results. It must also be stated that, although sufficiently powered to find an effect for behaviour, this study was underpowered to find the aggregate effect size Epton et al. (2015) calculated for intentions, $d=0.14$. Sweeney and Moyer's (2015) meta-analysis calculated a larger aggregate effect size ($d=0.39$) for intentions. This is similar to the effect size for behaviour, for which this study was sufficiently powered. However, unfortunately, the statistical methods used meant that an effect of this size for intentions could not have been found. This is because the independent samples t-test used to examine the between-subjects effect would have required a larger sample than the repeated-measures ANOVA (which is more sensitive to variation) used to examine behavioural effects.

However, almost half of the studies reviewed in Sweeney and Moyer's (2015) meta-analysis sampled fewer participants than this study, yet found effects of self-affirmation on health intentions and/or behaviour. Additional evidence for the theory that the study was sufficiently powered and there simply was no effect comes from a similar, as-yet-unpublished internet study on self-affirmation and alcohol use (Kamboj et al., 2015) which had over 500 participants yet also found no effect.

It is unlikely that the type of self-affirmation manipulation used (a values exercise, as opposed to a kindness questionnaire) had any effect on the outcome of the study, as Sweeney and Moyer's (2015) meta-analysis found that the type of experimental method was not significant as a moderating variable. Furthermore, Epton et al.'s (2015) meta-analysis found that the values essay had the largest effect

on behaviour. However, combining self-affirmation with another task (which was not included in Sweeney and Moyer's (2015) moderator analyses) may have boosted efficacy. Specifically, Armitage et al. (2011) used an implementation intentions exercise. Participants were asked to complete 'if-then' statements to trigger self-affirmation when confronted with a threat message -for example: "if I feel threatened then I will think about the things I value in myself" (Armitage et al., 2011, p.636). They found that this was more effective than a kindness questionnaire in a direct comparison. It is therefore important to consider the possibility that it may also have been more effective than the values exercise in the present study, as (by directly linking self-affirmation to the risk message) this exercise goes a step further than merely presenting a risk message following a self-affirmation manipulation.

Furthermore, it is possible that participants in the current study's control group may have indirectly experienced self-affirmation by writing about their least important value and the reasons it might be important to someone else.

Spirituality/religiousness was chosen as the least important value by 27 of 39 control participants. Of the remainder, 6 chose hedonism. Many participants did not entirely adhere to the instructions to write only about why their least important value might be important to someone else- they also wrote about why they did not endorse said value. Sample statements include "I personally don't agree with the ideas and restrictions of religion" (which may affirm the writer's progressive, liberal or scientific/intellectual values) and "[hedonism] may lead to a lack of personal responsibility in everyday life" (possibly affirming the value the writer places on responsibility). By contrast, the control task for the self-affirmation kindness questionnaire used in Reed & Aspinwall (1998) and Armitage et al. (2008) contains

no self-relevant questions, precluding the possibility of indirect self-affirmation in the control group.

One way in which the present study differs from previous research is that the majority (61%) of participants were male. Epton et al. (2014) found that gender did not moderate the effects of self-affirmation. However, Sweeney and Moyer's (2015) review reported that all 16 included studies had between 50 and 100% female participants, and in five studies only women were included. The non-significant findings in this study and Meier et al.'s (in press) study (in which male participants were also in the majority) raise the question that women may be more susceptible to the effects of self-affirmation than men. Further research is needed with more evenly gender-balanced samples to confirm or disconfirm Epton et al.'s (2015) result regarding gender as a moderating variable. Aside from gender, the participants in this study were similar to those sampled in the majority of previous studies, i.e. university students who were mostly in their late teens and early twenties. This could be considered a weakness, due to the difficulty of generalising results to the overall population. However, it could also be considered a strength, as the sample was homogenous, and students can be considered a specific high-risk group in terms of alcohol consumption (Substance Abuse and Mental Health Services Administration, 2006).

R-SRC task results

In light of the result that the self-affirmation manipulation made no significant difference to participants' explicit attitudes and behaviour, it is perhaps unsurprising that no between-group differences in response latencies were found on the R-SRC task, which aimed to measure implicit responses to alcohol cues. However, the repeated measures ANOVA revealed a highly significant ($p < 0.001$) main effect of

stimulus. This indicates that participants were faster both to approach *and* avoid alcohol stimuli than non-alcohol stimuli.

This finding contradicts the hypothesis that participants would be quicker to approach alcohol-related stimuli only. Although the quantities of alcohol drunk by students may place them in the clinically harmful range, it could be argued that their heavy drinking is (for the majority) context-specific. Student alcohol consumption seems to be influenced by a university drinking culture, as university students drink significantly more than young adults who are not in university (Kypri, Cronin, & Wright, 2005) and 80% of students binge-drink regularly (Penny & Armstrong-Hallam, 2010).

Alcohol stimuli may have both positive and negative valence for university students; this ambivalence may have been reflected in their more rapid approach *and* avoidance responses to alcohol cues. This is in line with studies such as Eberl et al. (2013) and Barkby et al. (2012), who did not find a clear overall approach bias in heavy drinkers. Eberl et al. (2013) argue that drinkers hold both avoidant and approach associations with alcohol. For university students, alcohol may be associated with socialising and fun, but equally with negative consequences in terms of academic performance.

The fact that the participants in this study are young and thus have shorter drinking histories cannot explain the lack of clear and unambiguous approach bias, as this bias has in fact been found in much younger drinkers (Peeters et al., 2012; van Hemel-Ruiter, de Jong, & Wiers 2011), suggesting that approach biases are formed shortly after a young person begins drinking. These studies also found that the strongest bias was evident in the young adolescents with the lowest levels of inhibition capacity. Self-control seems to be linked to approach bias for alcohol-

related stimuli in drinkers (Teunissen, Spijkerman, Schoenmakers, Vohs, & Engels, 2012) therefore a sample of university students -who presumably have stronger inhibition capacity than the youths sampled in the aforementioned adolescent studies, by dint of the fact that they are older, and have reached tertiary education level- may not show a clear approach bias.

Strengths and limitations

Several strengths and limitations of the current study have been discussed above. To summarise, strengths included a homogenous sample (all university students, and all heavy drinkers), a greater proportion of male participants than many previous studies, and the fact that behaviour was studied as well as intentions. Potential limitations of the study include the possibility that it was underpowered to find a small effect of self-affirmation on message receptivity and intention to change (although not behaviour) and that some participants in the control task may have been indirectly self-affirmed.

A further drawback of the study is the short (one-week) follow-up. It may take up to six months for health behaviour change to be firmly established (DiClemente et al., 1991); therefore a short follow-up may not give a true reflection of behaviour change. However, an advantage is that, unlike studies with longer follow-ups, there was no attrition in the current study.

This study used self-report measures to record participant alcohol consumption, which are arguably open to social desirability effects. This was justifiable on the basis that self-report measures can be as accurate as biological measures in alcohol studies (Babor, Steinberg, Anton, & Del Boca, 2000). Nevertheless, it would have been ideal to have taken measures to preclude the possibility that participants perceived an implicit invocation to report lower

consumption at follow-up. Participants in this study were aware that they were recruited because they are hazardous drinkers. Previous studies have managed to conceal this- for example Napper et al. (2009; study 3) recruited participants on the basis of their high scores on a drinking questionnaire at the start of the academic year, but did not tell them that this was why they had been recruited.

One final limitation of the present study is that not all aspects of message processing were assessed. For example, Jessop et al. (2009) used measures of defensive avoidance (e.g., "my first reaction [to the message] was I didn't want to think about it" p. 535), response efficacy (belief in the efficacy of the proposed behaviour change) and self-efficacy. Harris and Napper (2005) asked participants if they had heard of the link between alcohol and cancer before, and asked how easily they could imagine themselves getting the disease. Additional measures such as these were not included in this study to minimise the risk of Type I error. Furthermore, Harris and Napper (2005) also used a manipulation check from an independent rater to ensure participants were sufficiently self-affirmed based on their responses in the task. However this was not possible in the present study as the experimenter was a lone researcher.

Conclusions

This study contradicts previous research in finding no effect of self-affirmation on measures of alcohol risk message acceptance, behaviour, or implicit responses to alcohol cues. Future studies should be adequately powered to detect small effects, and should recruit samples of exclusively heavy drinkers (rather than mixed samples of heavy and light drinkers, as the risk message is not as relevant to light drinkers) to discover whether this result was a product of type II error, or reflects a true lack of efficacy of self-affirmation in this group.

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PART 3: CRITICAL APPRAISAL

Introduction

The following critical appraisal comments on several issues I grappled with during the research process. Firstly, I explore the process of choosing a measure of implicit cognition to investigate automatic approach and avoidance biases in relation to alcohol-related stimuli. Secondly, I comment on the construction of the personalised risk message participants were shown to engender a sense of threat prior to the self-affirmation manipulation. Thirdly, I discuss the problems I encountered in recruitment, and how I attempted to overcome them. Finally, I reflect on my ideas about myself as a clinical researcher, and consider how the process of conducting this research will influence my future clinical practice.

Choice of implicit cognition measure

There were two options available to me when choosing a task to measure implicit responses to alcohol-related stimuli: Stimulus-Response Compatibility Tasks (SRC; De Houwer, Crombez, Baeyens, & Hermans, 2001) or Approach-Avoidance Tasks (AAT; Rinck & Becker, 2007). Both tasks can be used in either a relevant-feature or irrelevant-feature form; however prior to a recent study by Kersbergen, Woud, and Field (2014), only the irrelevant-feature form of the Approach-Avoidance task had been used in research assessing cognitive biases in alcohol users, whereas the SRC has primarily been used in relevant-feature form. In irrelevant-feature tasks, participants do not have to judge the valence of the stimuli as they are presented. Pictures are categorised on the basis of other features, such as their orientation. By contrast, in relevant feature tasks the participant must approach or avoid stimuli based on their alcohol-relatedness.

It seems to make intuitive sense that a task in which alcohol-relatedness is the relevant feature and participants directly approach or avoid alcohol would be more

sensitive to cognitive biases than one in which they do not. Studies using the irrelevant-feature AAT have yielded mixed results. Wiers, Rinck, Dictus, and Van den Wildenburg (2009) found evidence of an approach bias in heavy drinkers, however this effect was not found in van Hemel-Ruiter, de Jong, and Wiers (2011). Peeters et al. (2012) found that adolescent heavy drinkers had stronger alcohol-approach tendencies than lighter drinkers, especially those with lower inhibition skills. The same researchers subsequently found that stronger approach tendencies predicted greater alcohol use at six-month follow-up, but only for youth with less-developed inhibition skills (Peeters et al., 2013).

By contrast, studies using the relevant-feature SRC have consistently found positive correlations between approach biases and alcohol use. This effect has been found most frequently in cross sectional studies (Christiansen, Cole, Goudie, & Field, 2012; Field, Caren, Fernie, & De Houwer, 2011; Field, Kiernan, Eastwood, & Child; 2008). However, a prospective study of young adolescents (Pieters, Burk, Van der Vorst, Wiers, & Engels, 2012) also found an association (albeit only in males).

Furthermore, it seemed to me that SRC tasks have a slightly sounder theoretical basis than AATs. In AATs, a joystick is manoeuvred towards or away from the user, enabling him or her to directly 'approach' or avoid' pictorial stimuli. By contrast, in SRC tasks the participant presses buttons on a keyboard towards or away from stimuli, thereby symbolically approaching and avoiding. A theory of specific muscle activation (Chen & Bargh, 1999; Rotteveel & Phaf, 2004) suggests that employing an arm extending movement when moving the joystick on the AAT is congruent with positive stimuli (i.e. participants can perform faster when they are extending their arm towards positive stimuli and slower when doing the same for negative stimuli), and, conversely, a flexion movement is congruent with negative

stimuli. This has been hypothesised to occur automatically and unconsciously (Chen & Bargh, 1999) or as a result of conditioning (Rotteveel & Phaf, 2004). However, doubt has been cast on this idea by studies such as Eder and Rothermund (2008). This study assigned different labels to identical lever movements in their task instructions- *towards* and *away* or *upwards* and *downwards*. An initial independent study (in which participants were asked to rate words on positive and negative valence) provided evidence that *away* and *downwards* have negative connotations, while *towards* and *upwards* are coded positively. By subsequently using the same stimuli but labelling the lever movements as *upwards* and *downwards*, the researchers found that the standard affective-congruency effect produced by asking participants to go towards or away from the stimuli was reversed- e.g. participants were significantly slower to manoeuvre a lever *downwards* for positive stimuli than they had been to perform the same motion for the same stimuli when it was labelled *towards*. These findings can be explained with reference to event coding theory (Hommel, 2009). This theory posits that actions are represented in the mind by codes of their perceived consequences, and there is no difference between representations of stimuli and the actions produced. Therefore, approach and avoidance responses are not caused by the intrinsic motivational properties of the stimulus, but rather by the evaluative codes we attach to behaviours, which overlap with stimulus valence. Thus, Eder and Rothermund (2008) proved that the same action can be negatively coded in one context but positively coded in another.

A different theory of approach and avoidance reactions is the distance - regulation account (Solarz, 1960). This hypothesis suggests that reaction times are influenced by the compatibility between the stimulus valence and the motion to increase or decrease space between the individual and the stimulus. Evidence from

the R-SRC studies cited above (involving keyboard presses and a manikin which approaches or avoids a stimulus) suggests that arm extension/flexion is not required to activate valence associations- symbolically regulating the distance between the task user and the stimulus is sufficient (De Houwer et al., 2001).

For these theoretical reasons, as well as the larger amount of evidence for the R-SRC in capturing alcohol approach biases in drinkers, it seemed that the R-SRC was a better choice than the AAT at the time of this study's inception. Furthermore, Field et al. (2011) note in their comparative study that the AAT had much lower reliability than the R-SRC. They suggest that this might account for the less consistent findings (in terms of alcohol approach biases in drinkers) in studies using the AAT compared with studies using the R-SRC.

However, a recent study (Kersbergen et al., 2014) used a relevant-feature version of the AAT (R-AAT) for the first time in an alcohol study, and found it superior to the R-SRC in that it predicted alcohol consumption in general, and hazardous drinking specifically, at follow-up. The R-SRC also performed well- it also predicted hazardous drinking (but not alcohol consumption in general) and in the R-SRC this finding was resilient to different methods of data aggregation, whereas the R-AAT only predicted alcohol consumption and hazardous drinking when raw reaction time scores were used to create a bias score. The authors suggest that this indicates the R-AAT is sensitive to errors and outliers, whereas the R-SRC is less so. In light of these new findings, if I was to repeat this study I might have chosen the R-AAT as its greater power to predict drinking outcomes suggests it may be the more powerful task. However, it is likely I would still choose the R-SRC considering it is less sensitive to error and outliers, considering the small size ($n=78$) of my sample.

Choice of risk message

When formulating my risk message to present to participants, I was keen to ensure that it would engender a strong enough sense of threat to render the self-affirmation manipulation relevant. I was aware that if participants did not feel threatened by the message, no defensive response would be aroused for the self-affirmation task to guard against. Messages used by other researchers investigating self-affirmation effects in drinkers vary. For example, Armitage, Harris and Arden (2011) used a diagram from a guide to the AUDIT (Babor, Higgins-Biddle, Saunders, & Monteiro, 2001, as cited in Armitage et al., 2011). The diagram drew attention to various different parts of the body which can be damaged by alcohol use, and was accompanied by a list of 39 alcohol-related health problems. Harris and Napper (2005) constructed a leaflet about the link between alcohol misuse and cancer using material from a newspaper article and a Cancer Research UK press release.

To some degree, perception of risk depends on the manner in which it is presented (for a review, see Ahmed, Naik, Willoughby, & Edwards, 2012). I decided to present the information numerically (specifically, as a percentage increase in the likelihood of developing cancer compared to a non-drinker), as Lipkus (2007) points out that there are a number of benefits inherent in this approach (compared to verbal or pictorial messages), including precision, overtones of scientific authority, and verifiable accuracy.

People weigh up their risk of negative health outcomes based on the severity of their individual risk, and how it compares to the risk level of others (Waldron, van der Weijden, Ludt, Gallacher, & Elwyn, 2011). There is evidence from a Cochrane review that personalised or individualised risk information can increase the likelihood that an individual will take a health screening test (Edwards, Evans, Hood,

& Elwyn, 2006); which indicated to me that this might be a particularly effective means of presenting my message. I therefore decided to construct messages displaying an individual's risk of three types of cancer (oral or pharyngeal, liver, and oesophageal) depending on their gender and the number of units of alcohol drunk per week, based on epidemiological data (Bagnardi, Blangiardo, La Vecchia, & Corrao, 2001; Turati et al, 2013). I hoped to also include age; however research in the area has not yet yielded sufficiently specific data to allow me to factor this in, considering my sample were all young university students. Another pitfall of using epidemiological data is that I necessarily had to give very wide ranges in my probability estimates (e.g. "Your personal risk of developing oesophageal cancer is 124-345% higher than a non-drinker"). The broadness of these ranges may have undermined confidence in the message.

It was important for me to formulate the message in such a way as to grab the attention of participants, as risk messages are more effective when they are fully attended to (Lipkus, 2007). Therefore, although alcohol misuse is implicated in many different diseases, I decided to focus on only one - cancer- to keep the message short, clear and focused. The resulting message was three sentences long. I thought that keeping it short would facilitate comprehension and minimise the likelihood of not reading the whole message. If I had decided to make it longer, I could have included social norm comparative feedback, which is often combined with personalised health risk messages and has been proven to be effective in promoting behaviour change in students who drink hazardously (White, 2006).

The idea behind this type of intervention is that many students overestimate how much their peers drink, and the extent to which alcohol misuse is approved of in their peer group. Many participants in my study expressed shock and dismay at how

many units they had drunk upon seeing their total displayed. The addition of comparative information specifying that other students drink less might have made this message even more powerful. However, many other participants in my study (notably, only males) expressed pride in the amount they had drunk. Therefore, social norms feedback may have had the opposite effect than intended on these individuals, i.e. confirmed their ego-syntonic image of themselves as especially heavy drinkers.

When formulating the fear messages, I was aware that people often find it hard to understand risk. Merely giving numerical risk information is not enough; understanding is moderated by numeracy skill (Peters, Hibbard, Slovic, & Dieckmann, 2007). Research informed me that levels of innumeracy are high, even in highly educated samples (Lipkus, Samsa, & Rimer, 2001). This was important information for me as my sample consisted of university students, and I worried that their knowledge of probabilities might make the message less threatening. As mentioned in the Discussion, several participants did in fact tell me after the experiment that they understood that the relative risk data I presented gave a misleading picture, explaining they assumed their absolute risk would be much lower than their relative risk compared to a non-drinker. A potential weakness in my study is that message understanding was not evaluated; therefore it is not possible to find out whether level of understanding of the message moderated the response to it in terms of message derogation, sense of threat etc.

As mentioned above, risk information was presented in comparative terms (i.e., compared to a non-drinker). Risk messages may be more difficult to comprehend when they are presented as single event probabilities in terms of percentages rather than natural frequencies (Gigerenzer & Galesic, 2012). An

example of a single event probability would be informing someone that "you have a 10% risk of developing cancer" as opposed to a natural frequency message, which would be "10 in 100 people who drink as much alcohol as you will develop cancer". I chose not to provide base rate information, as there is evidence that this leads to lower levels of perceived risk (Natter & Berry, 2005). Conversely, perceived risk is often overestimated when it is communicated in purely relative terms (Edwards, Elwyn, Covey, Matthews, & Pill, 2001).

Furthermore, medical research has shown that treatments are more positively evaluated when risk is presented in relative terms (Covey, 2007) which suggested to me that this format of message presentation has the biggest impact. I was aware of the ethical issues inherent in how I chose to present my message. There is debate on whether or not it is appropriate to present people with single probability risk data (Gigerenzer & Edwards, 2003). Lipkus (2007) points out that it is problematic in that the characteristics of the individual in question may not be represented in the researched population; furthermore it is not possible to conclusively specify what any one individual's risk is.

Moreover, if participants did overestimate their risk as a result of seeing it presented in comparative terms, this may have caused some undue distress. I sought to avoid provoking excessive worry by screening for health anxiety at the recruitment stage. However, a few participants did tell me that they found the message very shocking. I made sure to debrief these participants after recording their one-week follow-up data by reminding them that the message was designed to be frightening, and that just because their risk of cancer is higher than a non-drinker does not mean it is very high overall.

Recruitment and power

As mentioned in the Discussion, it is possible that the study was underpowered to find a small effect with a sample of 78 participants. There were several reasons for this. The first reason was that 'UCL Announce', the university-wide email system which researchers use to recruit participants at UCL, was abolished this year; therefore I did not have any way to advertise the study to a large number of participants at once. I was permitted to place a small ad in a general 'Student News' email; however it is unlikely this was seen by many students as it generated a very small amount of responses, in contrast to the hundreds of replies which UCL Announce emails usually garnered as standard.

An additional problem was that there were two other researchers recruiting heavy social drinkers at the same time as me. I was permitted to share participants with one other researcher by employing a "wash-out period" of a week between studies. However, many participants did not wish to take part in more than one study. Furthermore, a significant number were no longer drinking heavily enough to qualify for a second study after undergoing an intervention targeting their drinking in the first.

The loss of UCL Announce was a significant setback. Other researchers and I spent a large amount of time and energy attempting to appeal the decision, to no avail. As mentioned in the empirical paper, I advertised the study on posters around campus and on various recruitment websites. However, these methods did not yield a high enough volume of participants; therefore I eventually decided to broaden my recruitment to encompass another university (the University of Hertfordshire).

Another difficulty was that I was limited to recruiting university students only. During the study's inception, consideration was given to whether or not a clinical

sample of alcohol-dependent drinkers should be recruited from NHS services, to maximise ecological validity. However, a researcher who had completed a similar project the year before I started my research had attempted this and had not managed to recruit any participants from this hard-to-reach population. Therefore, it was decided that, as a lone investigator with limited time to devote to research, I should use heavy social drinkers as a proxy. Once I began to recruit, my supervisor and I realised that most of my participants would be drawn from the student population. We therefore decided to stipulate that only students could participate, to maximise homogeneity in the sample. Another reason was that there is a large body of literature about alcohol misuse interventions for university students as a specific group; therefore it was decided that my study could add to it. This meant that data from eight non-student participants had to be scrapped, which was a minor setback.

One final hindrance to recruiting a larger number of participants was funding. The experiment involved meeting the researcher and spending an hour filling in questionnaires and engaging in the R-SRC task. Participants also had to make themselves available for two phone calls, at the pre-screening stage and again one week after participation to provide follow-up data. Therefore, it was necessary to pay participants (except the UCL students who were eligible for course credit in return for participation) in cash. The level of effort required meant that very few students would have been willing to take part for a more minor reward (e.g. entry into a prize draw). I paid participants £7 each, and a funding limit of £500 meant I ran out of money after 71 participants. I did not want to spend a large amount of my own money, as I was aware this would have been problematic from an ethical standpoint.

Reflections on the research process from a clinical perspective

I first became interested in the idea of the self-concept and how it relates to harmful human behaviour when working in a drug and alcohol service in my first year of clinical training. I was running CBT groups for men and women struggling to stop abusing substances, and using Motivational Interviewing (MI) strategies in my one-to-one sessions. The nature of the work meant I often found myself having to confront or challenge clients about their behaviour and "roll with resistance" (to use MI parlance). This felt difficult and uncomfortable at times, as I had previously been used to 'getting alongside' the client and working from a shared agenda.

I found I could easily relate to the emotions my clients expressed when questioned about their substance use. Although I have never battled an addiction, like most people I'm accustomed to experiencing a sudden surge of hostility and defensive reactance when faced with information that indicates that I'm not making healthy choices (for example, regarding diet or exercise). For my clients to hear that their behaviour is not only seriously damaging their own health but also negatively impacting the lives of their loved ones is highly threatening, and directly contradicts any notion that one is a rational person who makes good decisions. I also noticed that the societal stigma attached to substance abuse meant that my clients often had the experience that their whole person was being shamed, that is, they are nothing but a burden on services and society in general. Therefore, the idea that affirming valued aspects of the self (which have nothing to do with the problem behaviour) can make one more amenable to working on that behaviour sounded highly therapeutic, and useful for my clinical practice.

It occurred to me that clinicians had been using this idea long before it began to be experimentally tested in the field of social psychology. The psychoanalytic

concept of 'ego-strength' suggests that the client's *global* sense of self must be strong enough to withstand challenging interpretations if they are to make good use of therapy. In Dialectical Behavioural Therapy (Linehan et al., 1999), validation - a similar concept to affirmation- is a central tenet. Therapists repeatedly validate the client and encourage them to self-validate, and this paves the way to collaboratively tackling unhelpful behaviours.

Therefore, when my results showed that self-affirmation had no effect on receptivity to a risk message in my sample of student drinkers, it prompted me wonder whether what was missing was a relationship between the person delivering the intervention and the person receiving it. As discussed in my empirical paper, multiple studies have shown that a self-affirmation manipulation alone can enhance message receptivity in samples of less risky student drinkers. However, as my sample were hazardous drinkers and therefore likely to feel more defensive when viewing the risk message, it made sense to me that an intervention presented by a computer would not have the same impact as if it had been delivered in conversation with a trusted other. Despite my null findings, doing this research has influenced my clinical practice by making me more aware of the importance of explicitly focusing on all the ways in which clients are already living life in line with their values, rather than taking an exclusively problem-focused approach.

Thinking about my research from a clinical perspective also prompted me to reflect on how my clinical skills may or may not have influenced me as a researcher. It occurred to me that, as a novice researcher, my anxiety about 'getting it right' led me to construct an artificial separation between myself as a clinician and myself as a researcher. This meant that I may have relied too heavily on prior research in the field as a model, rather than thinking for myself. For example, most previous studies

have used the threat of disease as their threat message. I therefore thought carefully about which disease to choose (as explained in my section on choosing a risk message above) but it did not occur to me to think about what I myself consider to be the most worrying possible consequence of drinking to excess. I realised that, for my friends and I, the possibility of becoming a victim of assault was the biggest threat, as it is proximal -as opposed to a distant and hard-to-imagine future risk (i.e. getting cancer). If I had been thinking with my 'clinical brain' switched on, I might have conducted a small focus group when designing the study, to find out what students said they worried about the most regarding alcohol. Therefore, an important lesson for me to learn was that when doing research I should think of myself as a clinical researcher, as opposed to a trainee who does clinical work and research work as entirely separate activities.

Summary

In this critical appraisal I reflected on my decision-making process in constructing a personalised risk message and choosing an implicit cognition task. I also explained how I came to sample university students who drink hazardously, as well as the difficulties I encountered during recruitment and the measures I took to circumvent them. Finally, I reflected on the impact the process of doing this research had on my clinical practice, and how it will inform my future research activities.

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Appendix 1: Ethical approval and related paperwork

1. Information sheet for participants
2. Consent form for participants
3. Ethical approval letter for the original programme of substance use research
4. Approval letter for the ethics amendment relating to this specific study

Information Sheet for Heavy Social Drinkers Involved in Verbal and Visuospatial Stimulus-Processing Research Studies

You will be given a copy of this information sheet.

Title of Project: **How do verbal and visuospatial strategies modify alcohol intake in heavy drinkers**

This study has been approved by the UCL Research Ethics Committee (Project ID Number): 0760/002

Name

Work Address

Contact Details

Details of Study: This study examines the effects of psychological task performance on drinking behaviour in heavy social drinkers (i.e. those who regularly drink more than the government recommended levels). We are interested in whether drinking behaviour changes when people engage in either visuospatial tasks (those involving images, shapes and object locations) or verbal tasks (those involving memory or instructions to use attention in a particular way). By learning more about the mental activities that affect drinking behaviour we may be able to develop more effective interventions to reduce alcohol intake in problem drinkers.

Who can take part? If you are generally healthy and drink more than the daily government-recommended amount of alcohol (recommended amounts are 3-4 units for men and 2-3 units for women) or binge drink (consume over twice the recommended daily amount of units) at least once a week and are between 18-50 years old, fluent in English, have normal or corrected to normal vision, have no current serious psychological or physical illness, no history of alcohol or drug dependence and have not taken part in a similar study, you may be eligible to take part.

If you agree to participate in this study you must complete a series of questions about your level of drinking, physical and mental health history. This should take around 2 minutes. Please note that, based on your answers to these questions; you may not be eligible to take part in the study. If you are eligible to take part you will arrange a convenient time with an experimenter to come to the Clinical Psychopharmacology Unit at UCL.

What will happen to me if I take part?

We will arrange for you to attend an appointment at UCL at a time convenient for you. You will then be given some questionnaires to measure your cravings, mood, attitudes

about alcohol and drinking history.

Next you will take part in computerized and pen and paper tasks. All of these tasks are very safe.

The experiment will take up to one hour. After this you will be paid for your time.

We would like to contact you again a week later to ask you some very brief (up to 5 minutes) additional questions about your experience since the appointment. You may contact the researcher at any time after the study if you experience any difficulties.

Are there any risks in taking part?

There are no known risks in completing the questionnaires or tasks but looking at negative pictures and thinking about negative consequences of heavy drinking can be temporarily, mildly distressing.

Are there any benefits to taking part?

You will not benefit directly from taking part in this research but your participation will help us gain a better understanding of the experience of craving which may lead to better strategies for managing these challenging experiences. In addition, some of the tasks involved in the experiment can be interesting and enjoyable.

Please discuss the information above with others if you wish or ask us if there is anything that is not clear or if you would like more information.

It is up to you to decide whether to take part or not; choosing not to take part will not disadvantage you in any way. If you do decide to take part you are still free to withdraw at any time and without giving a reason.

All data will be collected and stored in accordance with the Data Protection Act 1998.

All information which is collected about you during the course of the research will be kept strictly confidential and will be securely stored electronically, using a numbered code so that you cannot be identified. Only researchers directly involved in the study will have access to the data. All data will be stored in accordance with the Data Protection Act 1998. The data will be used only for informing the research question in this study and the results of the research will be disseminated in peer-reviewed scientific journals, but you will in no way be identifiable from such publications.

Informed Consent Form for Heavy Social Drinkers Involved in Verbal and Visuospatial Stimulus-Processing Research Studies

Please complete this form after you have read the Information Sheet and/or listened to an explanation about the research.

Title of Project: **How do verbal and visuospatial strategies modify craving experiences in heavy smokers and drinkers**

This study has been approved by the UCL Research Ethics Committee (Project ID Number): 0760/002

Thank you for your interest in taking part in this research. Before you agree to take part, the person organising the research must explain the project to you.

If you have any questions arising from the Information Sheet or explanation already given to you, please ask the researcher before you to decide whether to join in. You will be given a copy of this Consent Form to keep and refer to at any time.

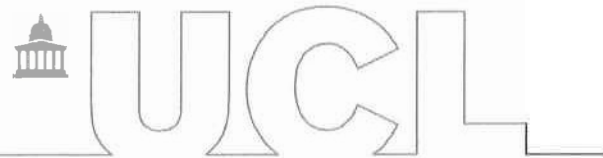
Participant's Statement

I

- have read the notes written above and the Information Sheet, and understand what the study involves.
- understand that if I decide at any time that I no longer wish to take part in this project, I can notify the researchers involved and withdraw immediately.
- consent to the processing of my personal information for the purposes of this research study.
- understand that such information will be treated as strictly confidential and handled in accordance with the provisions of the Data Protection Act 1998.
- agree that the research project named above has been explained to me to my satisfaction and I agree to take part in this study.

Signed:

Date:



Dr Sunjeev Kamboj
Research Department of Clinical, Educational and
Health Psychology
UCL

19 March 2013

Dear Dr Kamboj

Notification of Ethical Approval

Project ID: 0760/002: Craving changes? How do verbal and visuo-spatial strategies modify craving experiences in heavy smokers and drinkers

I am pleased to confirm that in my capacity as Chair of the UCL Research Ethics Committee I have approved your study for the duration of the project i.e. until March 2014 on condition that the Information Sheet is amended to include reference to the fact that a breathalyser test will be administered following the consumption of the alcoholic drink.

Approval is also subject to the following conditions:

1. You must seek Chair's approval for proposed amendments to the research for which this approval has been given. Ethical approval is specific to this project and must not be treated as applicable to research of a similar nature. Each research project is reviewed separately and if there are significant changes to the research protocol you should seek confirmation of continued ethical approval by completing the 'Amendment Approval Request Form'.

The form identified above can be accessed by logging on to the ethics website homepage: <http://www.grad.ucl.ac.uk/ethics/> and clicking on the button marked 'Key Responsibilities of the Researcher Following Approval'.

2. It is your responsibility to report to the Committee any unanticipated problems or adverse events involving risks to participants or others. Both non-serious and serious adverse events must be reported.

Reporting Non-Serious Adverse Events

For non-serious adverse events you will need to inform Helen Dougal, Ethics Committee Administrator (ethics@ucl.ac.uk), within ten days of an adverse incident occurring and provide a full written report that should include any amendments to the participant information sheet and study protocol. The Chair or Vice-Chair of the Ethics Committee will confirm that the incident is non-serious and report to the Committee at the next meeting. The final view of the Committee will be communicated to you.

Reporting Serious Adverse Events

The Ethics Committee should be notified of all serious adverse events via the Ethics Committee Administrator immediately the incident occurs. Where the adverse incident is unexpected and serious, the Chair or Vice-Chair will decide whether the study should be terminated pending the opinion of an independent expert. The adverse event will be considered at the next Committee meeting and a decision will be made on the need to change the information leaflet and/or study protocol.

On completion of the research you must submit a brief report (a maximum of two sides of A4) of your findings/concluding comments to the Committee, which includes in particular issues relating to the ethical implications of the research.

With best wishes for the research.

Yours sincerely /1



Professor John Foreman
Chair of the UCL Research Ethics Committee

Cc: Professor Peter Fonagy, Head of Department



Amendment Approval Request Form

1	ID Number: Re: 0760/002	Name and Address of Principal Investigator: Dr Sunjeev Kamboj, Research Dept Clinical, Educational and Health Psychology
2	Project Title: Craving changes? How do verbal and visuospatial strategies modify craving experiences in heavy smokers and drinkers	
3	Information about the amendment: (a) Is the amendment purely administrative? <input type="checkbox"/> Yes <input checked="" type="checkbox"/> No <input type="checkbox"/> N/A (b) Has the Participant Information Sheet/Consent Form been changed as a result of the amendment? If yes, please enclose a copy. <input checked="" type="checkbox"/> Yes <input type="checkbox"/> No <input type="checkbox"/> N/A (changes to info sheet are in bold)	
4	Summarise the issues contained in the amendment: <p>The studies in this programme of research examine the effects of various psychological coping strategies on craving and other addiction-related behaviours. Specifically we examine the effects of threat-related images and mental imagery on craving and smoking and drinking behaviour. We seek to extend the current studies in the following ways:</p> <ol style="list-style-type: none"> 1) To extend the projects to July 2015 to accommodate data collection for three current 2nd year DClinPsy and one MSc students' theses. 2) One of the 'verbal' (cognitive) conditions we wish to test entails 'self-affirmation' which involves participants writing about cherished value. The control condition involves completing a neutral task (writing about how a least-important value may be important to someone else). Participants are given up to 10 minutes. We will determine whether self-affirmation reduces the processing of threatening health information. It is predicted that the boost to global 'self-image' (Steele, 1988) produced by self-affirmation will reduce defensive processing of threatening health information. This will be assessed, as in previous studies (Sheeran, Harris & Epton, 2013) using personal and comparative risk estimates (e.g. "How much do you believe you will be affected by an alcohol (or smoking)-related cancer at some point in the future" and "Compared to the average person who drinks as much as you, are you more or less at risk of an alcohol(or smoking)-related cancer," rated on a continuous Likert scale). These are aimed at assessing optimism bias. Behaviour will also be a focus of these studies: number of drinks or cigarettes consumed will be monitored at 24 hr, 7 days and 28 Days by remote questionnaire completion (either telephone follow up or via Qualtrics, a web-based questionnaire tool) along with intention to reduce drinking/smoking and confidence in reducing consumption. Because of the additional time required for completion of the follow-up questions we will pay participants £10 instead of the currently approved £7.50. <p>The threatening health information will be presented in the form of likelihood estimates of contracting a smoking/alcohol-related illness in the future according to demographic characteristics of the participant (Parkin, 2011). In experiments involving heavy drinkers, an additional video will be displayed (see http://reducemyrisk.tv/).</p> <ol style="list-style-type: none"> 3) For experiments involving heavy drinkers, the inclusion criteria will be based on objective assessment of harmful/hazardous drinking (AUDIT scores or >8) and/or binge drinking 	

(drinking more than twice recommended daily amount of alcohol more than once a week).

- 4) In addition to the lab-based experiments, we wish to perform these same experiments online, using the same stimuli and questionnaire format as used in the face-to-face experiments but delivered by Qualtrics, a survey instrument which is currently used to administer questionnaires in face-to-face experiments. The same verbally and written instructions, will simply be translated to online instructions/questionnaires. In these experiments, the only measure of drinking and smoking behaviour will be self reported (rather than objective, as in the lab-based studies which use breathalysers and CO monitor to assess alcohol and cigarette consumption). To increase the likelihood of retention and for ease, these participants will be compensated using Amazon vouchers of up to £7.50 rather than cash.

5 Please give any other information you feel may be necessary:

Participants in the online study will be asked to provide consent by endorsing a statement indicating that they have read the study information and that they consent to taking part.

Signature of Principal Investigator:

Date of Submission:

13/12/2013



FOR OFFICE USE ONLY:

Amendments to the proposed protocol have been approved by the Research Ethics Committee.

Chair's Signature:

Date: 16/12/2013.

JOHN WICKMAN (PROF.)

Please return completed form to:

Secretary of the UCL Research Ethics Committee
Graduate School, North Cloisters, Wilkins Building
Gower Street, London WC1E 6BT