

The impact of a 'looming vulnerability' manipulation on
perceptions of risk, intentions and behaviour among
harmful drinkers

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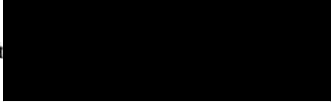
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Signature



Name: ROBYN CARRINGTON

Date: 10 / 06 / 2015

Overview

This thesis explores the effectiveness of attempts to increase risk perceptions among at-risk groups.

Part one provides a meta-analytic review examining the efficacy of manipulations aimed at increasing risk perceptions among regular alcohol drinkers and smokers. Outcomes from 23 randomly controlled designs were categorised according to manipulation type, and were assessed for their overall effects and methodological quality, separately for the drinking and smoking groups. Results revealed that deliberative manipulations were the most effective at enhancing smokers risk perceptions. Lack of appropriate data precluded any firm conclusions being drawn for alcohol users. Issues with methodology and heterogeneity, as well as directions for future research are discussed.

Part two presents an empirical study involving a randomised controlled design investigating the impact of inducing a sense of 'looming vulnerability' towards the threat of liver disease among harmful drinkers; using a novel guided imagery approach previously piloted on smokers (McDonald, O'Brien, Farr & Haaga, 2010). The results tentatively suggest that this approach can significantly increase anxiety and, in turn, lead to significantly healthier intentions among harmful drinkers. Larger scale studies are required to add strength to these findings. Methodological limitations and implications for research and practice are discussed.

Part three provides a critical appraisal of the empirical study. It includes a discussion of my background interest in this area and critically reflects on three key aspects of the research process: Designing the intervention, measuring outcomes and recruiting participants. A reflection on the main challenges that arose is provided alongside suggestions for future research.

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Part 1: Literature Review

A meta-analysis of the effectiveness of manipulations designed to increase risk perceptions among regular drinkers and smokers

ABSTRACT

Background: Perceived risk is proposed to play a vital role in the adoption of health protective and preventive actions, yet individuals who are at most risk (e.g. drinkers, smokers) typically do not believe they are likely to suffer the severe health consequences.

Aims: To examine the effectiveness of manipulations designed to increase risk perceptions among regular drinkers and smokers, using meta-analytic procedures.

Method: Systematic electronic and hand searches were conducted on 22nd November 2014 to identify all studies prior to that date that involved a randomised controlled design (involving active, matched or no treatment controls) aimed at increasing personal or comparative risk perceptions among regular adult drinkers or smokers. Twenty-three studies met full inclusion criteria (twenty smoking, three drinking). The methodological quality of studies was assessed against Cochrane criteria. Data was analysed in Review manager (version 5.3).

Results: Thirteen of the smoking studies provided sufficient data for a meta-analysis. Only the deliberative category produced significant small-medium positive effects on smokers' perceived personal risks. None of the manipulation types significantly increased smokers' perceptions of comparative risk. In contrast, the combined and affective types were found to make smokers significantly more comparatively optimistic. A meta-analysis of the overall effects for drinkers could not be performed due to a lack of eligible studies.

Conclusions: Taking methodological and heterogeneity issues into account, it is tentatively concluded that the provision of information clearly linking smoking to its severe health consequences is an effective way in which to help smokers personalise these risks. However, further research involving all manipulation types for both groups is needed before firmer conclusions can be made.

1. INTRODUCTION

1.1 Theoretical background to risk perception

Perceived risk has been highlighted as a key motivating factor within most major models of health behaviour, including Protection Motivation Theory (Rogers, 1975); the Health Belief Model (Janz & Becker, 1984); the Extended Parallel Process Model (Witte, 1992) and the Precaution Adoption Model (Weinstein, 1988). Each of these theories argue that when an individual is faced with a potential health threat (e.g. hearing about new evidence linking smoking to cancer) they need to not only believe the threat has severe consequences, but that they are personally susceptible to those consequences, before they will engage in protective actions (e.g. take steps to quit smoking if they are a smoker). Indeed there is converging evidence from over three decades of research spanning a wide range of health domains and research designs to confirm that a relationship between perceived risk and protective behaviour does exist (for reviews see Brewer et al; 2007; Janz & Becker, 1984; Harrison, Mullen & Green, 1992; Milne, Sheeran & Orbell, 2000).

1.2 Definitions and measurement issues

Within the literature a distinction has been made between perceptions of absolute and comparative risk (e.g. Weinstein & Klein, 1996). Perceived absolute risk reflects an individual's estimated personal likelihood of experiencing a threat. This is often referred to interchangeably as perceived susceptibility (Rogers, 1975), vulnerability (e.g. Milne et al; 2000), or probability (e.g. Weinstein, 1993). Perceived absolute risks are usually

assessed with questions such as: “How likely is it you have a smoking related illness?” with respondents given Likert response options to base their estimates on (from 1 = “not at all” to 7 = “very much”; Weinstein, 1998, p.136). In contrast, comparative risk reflects an individual’s perception of their own risk compared to that of relevant peers (Weinstein & Klein, 1996). This is typically measured by asking questions like: “Compared to others your same age and sex, how would you rate your risk of cancer?” (“much below average” (- 3) to “much above average” (+ 3); Weinstein, 1998, p.136). It has been argued that these risk estimates represent qualitatively distinct aspects of risk perception (van der Pligt, 1996; 1998; Shepperd, Klein, Waters & Weinstein, 2013) and should be measured as separate outcomes in research (Shepperd et al; 2013). As such, for the remainder of this review, the terms personal and comparative risk will be used to discuss absolute and comparative risk estimates, respectively.

Despite general agreement on how risk perceptions should be defined, the accurate assessment of these constructs remains complicated by their overall complexity (Weinstein, 1998). As noted by Weinstein (1998), the information people use to develop perceptions of risk can vary widely between individuals. Some people may rely on pre-existing knowledge about other smaller or larger risks when asked to estimate their personal risk of experiencing a given threat. Others might focus predominantly on the absolute risks based on factual evidence. Whether people attend more to verbal or numerical risk information when creating risk estimates also remains unclear (Weinstein, 1998). For these reasons, researchers have emphasised the need to incorporate a range of different questions when attempting to measure risk perceptions (e.g. measures of absolute verbal

and absolute numeric risk, comparative risks in relation to others and compared to different risks, overall agreement with statements about the presented risk; Weinstein, 1998). Others have further stressed the importance of including a variety of different scales of measurement (e.g. using a numerical 0 – 100 % scale and a verbal Likert scale) to achieve more reliable and valid assessments (Shepperd et al; 2013). The need to use conditional risk estimates (e.g. asking individuals to base their risk estimates on whether they take preventive action or take no action) versus unconditional risk estimates (e.g. simply asking people to estimate their perceived level of risk without taking their current or future behaviour into account) has also been highlighted by many authors to be necessary in order to clarify what factors are being used by individuals to base these risk estimates on (Brewer et al; 2007; van der Pligt; 1996, 1998; Ronis, 1992).

For comparative risks, it has been recommended that researchers measure these estimates indirectly (e.g. asking participants to provide two risk ratings separately, one for themselves and one for the comparison other, with comparative risk rated as the difference between these scores) as opposed to directly (e.g. using a single question to determine whether individuals believe their risk to be above or below the risk of comparable peers) since the former approach is more likely to focus respondents on the relevant peer group and therefore provides a more accurate measure of comparative risk perceptions (Covey & Davies, 2004). It has been argued that researchers should also specify the age, gender and location of the comparison group to achieve greater certainty about who individuals have compared themselves against (Shepperd et al; 2013).

1.3 The relationship between perceived risks, intentions and behaviour

Although both types of risk perception have been proposed to play an important role in the adoption of health protective or preventive behaviours (e.g. Rogers, 1975 for personal risks; van der Pligt, 1998 for comparative risks) the majority of evidence to support this assumption has been found with regards to personal risks (e.g. Brewer et al; 2007). Indeed there is extensive research linking high levels of perceived personal risk to healthy behavioural intentions and actions for a variety of health conditions, across a range of demographic groups (for a meta-analytic review of this experimental and correlational research see Milne et al; 2000).

Considerably less attention has been paid to the role of comparative risk in determining these outcomes. In a review of the few studies that have examined this link, it was concluded that "Comparative risk appraisal does not seem to add to the prediction of behaviour over and above perceived (own) risk (van der Pligt, 1996, p. 40)".

Despite the limited evidence to support the usefulness of communicating high levels of comparative risk to promote healthy behavioural choices, it has been argued that comparative optimism (i.e. believing one's risk to be lower than the risk of comparable peers), may in any case prevent individuals from engaging in healthy behavioural practices (van der Pligt; 1996, 1998).

Strong support for this assumption has come from a study by Dillard et al. (2007). They used a prospective design conducted over a one and a half year period to explore the relationship between comparative optimism and negative consequences among a sample of alcohol drinking college students. The study found that students categorised as being comparatively

optimistic about their risks of experiencing alcoholism or alcohol poisoning reported significantly more negative consequences (e.g. experiencing a hangover, causing damage to property, having to receive medical treatment) as a result of their drinking across the full study period. Furthermore, the analysis revealed that the negative effects of comparative optimism remained significant after controlling for the influence of previous negative events. Together this study provides strong evidence to support these author's conclusions that comparative optimism can not only have a significant negative effect on behaviour, but that its negative effects can build up over time.

1.4 Prevalence of inaccurate risk perceptions

Overall, the theoretical and empirical research together suggests that people need to hold accurate risk perceptions to avoid the possibility of experiencing negatives outcomes. The problem however is that people tend to be unrealistically optimistic about their risks (Weinstein & Klein, 1996). This phenomenon was first described in the seminal paper by Weinstein (1980) who showed systematic levels of unrealistic optimism for a range of negative and positive events (e.g. developing lung cancer, owning a home) among a group of students. Unrealistic optimism appears to be a robust finding as it has since been replicated across numerous studies involving a range of different groups and outcomes (for reviews see van der Pligt, 1996,1998; Shepperd et al; 2013; Weinstein & Klein, 1996).

Unrealistic optimism also does not appear to be limited to one type of risk perception or method of assessment. Studies have shown high levels of unrealistic optimism when individuals have been asked to compare their

risks to an objective or personal outcome (termed 'unrealistic absolute optimism') and when asked to rate their risk compared to that of other people ('unrealistic comparative optimism'); when this has been assessed at both an individual level (whereby the individuals absolute or comparative risk rating is compared to an objective standard, such as a risk calculator) or group level (whereby the average risk estimate from a group is compared against the base rate risk for that group in the population (for unrealistic absolute risks) or to the mean risk rating obtained from a comparison group (for unrealistic comparative risks); Shepperd et al; 2013). Of most concern, is the fact that unrealistic optimism has been found among individuals who would be considered objectively at risk due to their behaviour, such as people engaging in unsafe sexual practices (van der Velde, Pligt & Hooykaas, 1994) current smokers (Weinstein, Marcus & Moser, 2005) and harmful drinkers (Wild, Hinson, Cunningham & Bacchioni, 2001).

1.5 Attempts to support the development of accurate risk perceptions

Given the theoretical and empirical findings emphasising the importance of perceived risks in promoting adaptive behaviour or avoiding negative outcomes, alongside the consistent findings that people tend to underestimate their risks, considerable effort has been made to encourage individuals to develop more accurate risk perceptions. These attempts can be broadly divided into two categories: Those aimed at increasing perceived personal risks and those focused on enhancing perceived comparative risks.

Experimental manipulations and health care interventions aimed at changing personal risk perceptions have typically been based on the assumptions put forward by protection motivation theory. This model

proposes that beliefs about risk can be effectively altered when an individual is exposed to information highlighting the likelihood of experiencing a particular threat. For example, in these studies, at risk groups (e.g. smokers) are presented with a general message stating that their behaviour is either likely or unlikely to lead to a health threat (e.g. lung cancer). In general, high risk messages have proved effective at increasing beliefs about personal risk (e.g. Maddux & Rogers, 1983).

In contrast, manipulations targeting comparative risks have focused on increasing individuals awareness of their personal risks, changing their impressions about the risks of comparable peers, or have tried to alter both perceptions simultaneously (e.g. Weinstein, 1983; Weinstein & Klein, 1995). A range of methods have been used to achieve this, each based on different theories about what might be responsible for causing these biased risk estimates in the first place. For example, it has been proposed that unrealistic optimism may come about simply because individuals lack awareness about the risks associated with a particular threat, and as such, informing them about the relevant risk factors and asking them to rate where they stand on those factors may help them develop more realistic comparative risk estimates (Weinstein & Klein, 1995). Another possibility is that the tendency to underestimate one's own risks and exaggerate others' risks is caused by a defensive reaction motivated by the desire to reduce anxiety and maintain self- esteem (Weinstein & Klein, 1995). Interventions based on this assumption have involved manipulating the comparison target, for example, by asking individuals to compare their own risks to someone considered to be at a low risk of experiencing the same threat (e.g. Weinstein & Klein, 1995). It has also been argued that biased comparative

risk estimates may come about from individuals lacking information about the risk status of the comparison group and failing to focus on their own risky behaviours, whilst selectively recalling the healthy behaviours they perform (Weinstein and Klein, 1995). Therefore, making individual's focus only on their risky behaviours (e.g. Weinstein & Klein, 1995), or asking them to select risk factors that apply to them whilst being given information regarding the actual risk status of their peers (e.g. Weinstein, 1983) may also reduce these biases.

Although the effectiveness of these interventions at debiasing comparative risks is somewhat mixed, with research showing that comparative optimism can be highly resistant to change at times (e.g. Weinstein & Klein, 1995), others have found manipulations such as these can help individuals develop more realistic comparative risk judgements (e.g. Weinstein, 1983).

1.6 Recent approaches targeting risk perceptions

Since efforts to alter risk perceptions first began, there has been an expanse of research into the development of alternative approaches to help at-risk individuals develop more accurate risk perceptions. A recent review of this research distinguished between four distinct categories of manipulations; namely: (1) Deliberative manipulations (e.g. presenting individuals with factual information about their personal risks, by providing them with research evidence linking their unhealthy habits to specific illnesses); (2) Affective manipulations (e.g. those aimed at inducing fear or regret from not performing the recommended health action or from continuing to engage in unhealthy behaviours, by showing graphic images depicting the severe

health consequences associated with their risky behaviour); (3) Decision science based manipulations (e.g. manipulations informed by theory and research from the decision sciences, which typically involve providing risk information in a new format, such as presenting the risk in both a numeric and graphical form) and (4) Social psychology based manipulations (e.g. manipulations based on the social sciences and theories about self-concept, such as using a self-affirmation manipulation whereby individuals would be asked to list their valued attributes before being exposed to information about the negative consequences of their risky behaviour in an attempt to elicit less defensive reactions to this information) (Portnoy, Ferrer, Bergman & Klein, 2014).

1.7 The present review

The present review attempts to assess the effectiveness of each of the aforementioned types of manipulations at increasing personal and comparative risk perceptions among two at risk groups; namely, regular drinkers and smokers.

To the best of our knowledge, only one other review involving the same categories of manipulations has been conducted, which differed from the aims of the current review in a number of important ways. For example, Portnoy and colleagues (2014) sought to establish the impact of these same manipulations on perceived risks and worry across a range of different health domains. In terms of the results relevant to this review, they found only the deliberative category to have a significant positive effect on perceived risks.

The current review aims to expand on this area of research by focusing solely on manipulations targeting personal or comparative risks

within a drinking and smoking population. These two groups were chosen as the main focus for this review since they have been consistently shown to hold highly inaccurate beliefs about their risks (for unrealistic optimism among smokers see Weinstein, 1998; Weinstein et al; 2005; for unrealistic optimism among drinkers see Hansen, Raynor & Wolkenstein, 1991; Wild et al; 2001) despite widespread evidence of the severe health problems associated with these behaviours (e.g. World Health Organisation, 2007). Thus exploring which interventions are successful at creating more accurate risk perceptions among this group remains an important empirical and clinical goal.

This review further differs from that of Portnoy et al. (2014) by only including studies that attempted to increase perceived susceptibility and/or comparative vulnerability, rather than simply change these constructs. Furthermore, by exploring the effects of these manipulations on personal and comparative risk perceptions separately, this review will extend our understanding about whether manipulations are equally effective at increasing both types of risk perception, or whether these perceptions respond differently to different approaches.

2. METHOD

2.1 Aims

This review aims to assess the overall effectiveness of experimental manipulations and health care interventions designed to increase personal and/or comparative risk perceptions among regular alcohol users and smokers.

2.2 Research questions

The following questions will be addressed in this review:

1. What types of interventions or manipulations have been used to increase perceived risks (personal and comparative) among regular drinkers and smokers?
2. What types of risk perceptions have been targeted in this research and is there any consistency in their measurement across different studies?
3. What is the overall combined and individual effectiveness of the different types of manipulations at increasing risk perceptions among regular drinkers and smokers, immediately post treatment and at longer term follow up(s).

2.3 Electronic search strategy

To identify relevant publications for this review, a systematic search was conducted on the 22nd November 2014 using the following databases:

- Web of Science (1900 to present)
- Cochrane Central Register of Controlled Trials (CENTRAL, *The Cochrane Library*; all years)

- Psych Info (OVID SP; 1806 to present)
- MEDLINE (OVID SP; 1806 to present).

Databases were limited to search for (a) journals; (b) written in English; (c) involving studies with English speaking participants; (d) including humans only; (e) aged between 18-65; who were not (f) pregnant, (g) cannabis smokers or (h) patients.

A keyword search was initially run separately for each database. To maximise the number of potentially relevant studies that could be identified, only terms related to the population (e.g. drinkers and smokers) and outcome (e.g. perceived susceptibility) of interest were included. The specific population and outcome terms were chosen by consulting previous reviews (e.g. van der Pligt, 1996, 1998; Portnoy et al; 2014) and studies (e.g. Hansen et al; 1991; McDonald, O'Brien, Farr & Haaga, 2010; Myers, 2014; Weinstein & Klein, 1995) in the area (see Table 1 for the exact search terms used). Intervention terms (e.g. fear appeals) were not included in this search to avoid the possibility of missing out any interventions or manipulations explicitly targeting or likely to influence risk perceptions that might have been labelled with unusual terms.

Identical search terms were used for each database and were entered individually before being combined using the AND/OR functions available. These terms were then exploded in Psych Info, Cochrane and Medline thesauruses to identify further studies related to drinking, smoking and risk perception indexed with synonyms not already identified (see Appendix 1 for an example of the strategy used). Together, this resulted in 6,542 hits, which reduced to 5,850 once all duplicates were removed. The

titles and abstracts of all 5,850 studies were then screened for inclusion and exclusion criteria.

Table 1
Electronic search terms

| Search term | Keyword terms |
|--------------------|--|
| Participant group | Smoke*or smoking/cigarette smoke* or smoking/ tobacco smoking or smoker*or using or user*/ binge drink*/ alcohol user* or using or consumer* or consuming or consumption or drinker* or drinking/harmful or hazardous drinking |
| Outcome | Perceived susceptibility/perceived or looming vulnerability/ comparative or unrealistic optimism/ optimis* bias/ perceived risk*/ denial/ personal immunity/ protection motivation/ negative consequence* |

2.4 Inclusion and exclusion criteria

To be included in this review studies had to meet the following *a priori* inclusion criteria:

- Scope of studies
 1. Published in a peer reviewed journal article
 2. Written in English

- Study design
 1. Randomly controlled design
 2. Cluster randomised controlled design

➤ Types of participants

To be included studies had to consist of samples of either smokers or drinkers who were:

1. Not currently engaged in or seeking treatment for their drinking/smoking
2. Not currently making changes to their drinking/smoking
3. At or above the legal age for drinking/smoking according to their country of residence (e.g. for alcohol use, the sample had to include only participants over 18 years of age if it was based in the UK, or over 21 years if the study took place in the USA)

➤ Types of manipulation

Any manipulation or intervention that was stated to be aimed at increasing personal or comparative risk perceptions or was considered likely to increase these outcomes

1. Presented in any modality (e.g. written, visual, verbal)
2. Delivered in any setting (e.g. laboratory, community)

➤ Types of primary outcome measures

Studies had to include at least one quantitative measure of personal or comparative risk perceptions assessed in one of the following ways (as defined in Portnoy et al; 2014):

- Established risk perception scale
- Absolute verbal estimate of risk (e.g. "What is the likelihood you will get cancer?", from "very unlikely" to "very likely")
- Absolute numerical estimate of risk (e.g. "What is the likelihood you will get cancer?", from 0 % to 100 %)

- Comparative risk (e.g. “Compared to someone your age and gender, are you more, less, or as likely to get cancer?”)
- Indirect comparative risk (absolute risk compared to base rate)
- Unrealistic optimism (risk perception compared to objective risk estimates)
- Feelings of risk (e.g. “I feel vulnerable to cancer” or “I feel I will get cancer” from “strongly disagree” to “strongly agree”)

➤ Excluded studies

1. Narrative reviews

2. Only correlational data provided

3. Less than 20 participants per condition since these studies would likely be underpowered to detect significant effects

4. Articles that explicitly stated (within the results section of the paper) perceived risk to be a secondary outcome of interest

5. No separate data provided for drinkers/smokers in studies that investigated a range of different health behaviours

6. Studies including participants with established physical or mental health problems based on either self- report or objective reports (e.g. medical history, validated scales)

7. Studies including participants who were dependent on alcohol based on their medical history.

2.5 Screening and selection

All 5,850 titles and/or abstracts were initially screened for relevance and included for further assessment if they referred to any measure of

personal or comparative risk. A total of 5,656 articles were subsequently excluded. Thereafter, the full texts of the remaining 195 articles were retrieved and reviewed against the main inclusion criteria. One hundred and twenty nine studies were immediately excluded for failing to include risk perception as an outcome. The remaining 64 articles were then assessed against strict exclusion criteria. This resulted in the exclusion of a further 44 studies, with the most common reason being a lack of any control/comparison group (e.g. Bansal-Travers, Hammond, Smith & Cummings, 2011; Ben-Ahron, White & Philips, 1995; Bisset, Wood, Cox, Scott & Cassell, 2013; Borland, 1997; Leffingwell, Neumann, Leedy & Babitzke, 2007; Moscato et al; 2001; Myers & Frost, 2002; Van- Wel & Knobbout, 1998).

2.6 Searching other resources

The reference lists of the remaining 20 eligible studies were screened for further studies that could be included, which were assessed against the same criteria. From this a further four studies were identified, none of which fulfilled strict eligibility criteria (Ito et al; 2006; McBride et al; 2002; Jansen, Van der Berg, Buurman & Smits, 2006; Simmons & Brandon, 2007).

Finally, the reference lists of previous reviews and meta-analysis in the area (e.g. De Hoog, Stroebe & De Wit, 2007; Floyd, Prentice-Dunn & Rogers, 2000; Harrison et al; 1992; Janz & Becker, 1984; Marteau et al; 2010; Milne et al; 2000; Rothman & Kiviniemi, 1999), identified from an electronic search of Psych Info, Cochrane and Google Scholar, were checked for any additional studies that could be included. Three studies were identified (Leventhal et al; 1967; Rogers & Deckner, 1975; Rosen,

Terry & Leventhal, 1982) for screening which resulted in one additional eligible study (Leventhal et al; 1967) (see Figure 1 for a flow chart of the screening and selection process).

2.7 Data extraction and coding

For each article that met full inclusion criteria, data were extracted on the following areas: (a) publication year and country; (b) mean age and range of participants; (c) their gender distribution; (d) the behavioural domain targeted; (e) how drinking and smoking status was assessed; (f) current and history of use; (g) type of experiment and recruitment setting; (h) health threat targeted; (i) type of risk perception measure used; (j) number, length and dose of manipulation; (k) number and length of follow up(s); (l) main findings; and (m) their quality rating as established from the Cochrane risk of bias tool.

2.8 Data synthesis

Studies were categorised according to (1) the type of problem targeted (e.g. drinking or smoking), (2) the timing of risk perception measurement (e.g. pre -post or follow up) and the type of intervention/manipulation employed, using the coding scheme developed by Portnoy and colleagues (2014). This scheme distinguishes between 22 strategies designed to alter risk perceptions (e.g. presentation of threat information) each belonging to one of four categories of manipulation: Deliberative, Affective, Social Science, Decision science (see Appendix 2 for a list of all strategies used within each manipulation category). Studies that

did not include manipulations that fitted into one of the above categories were coded as 'other' and their details were specified.

2.9 Measures of treatment effect

Effect sizes for each type of manipulation were calculated using Review manager 5.3 software. The standardised mean difference (SMD) for continuous data was chosen as the effect size measure since risk perception outcomes across studies were measured on different scales. To estimate effect sizes with 95 % confidence intervals, the treatment group involving the most intensive level of the manipulation was compared to the treatment group given the lowest level of the manipulation, or to a control condition that was not exposed to any experimental manipulation. Thus, a positive effect size indicated an increase in risk perception for participants in the treatment versus the control/comparison group¹. Effect sizes were interpreted based on Cohen's (1988) guidelines whereby .2 = small, .5 = medium and .8 = large effects by convention. Studies which failed to report means and standard deviations for either the treatment or control comparison were excluded from the analysis, but were described in the results.

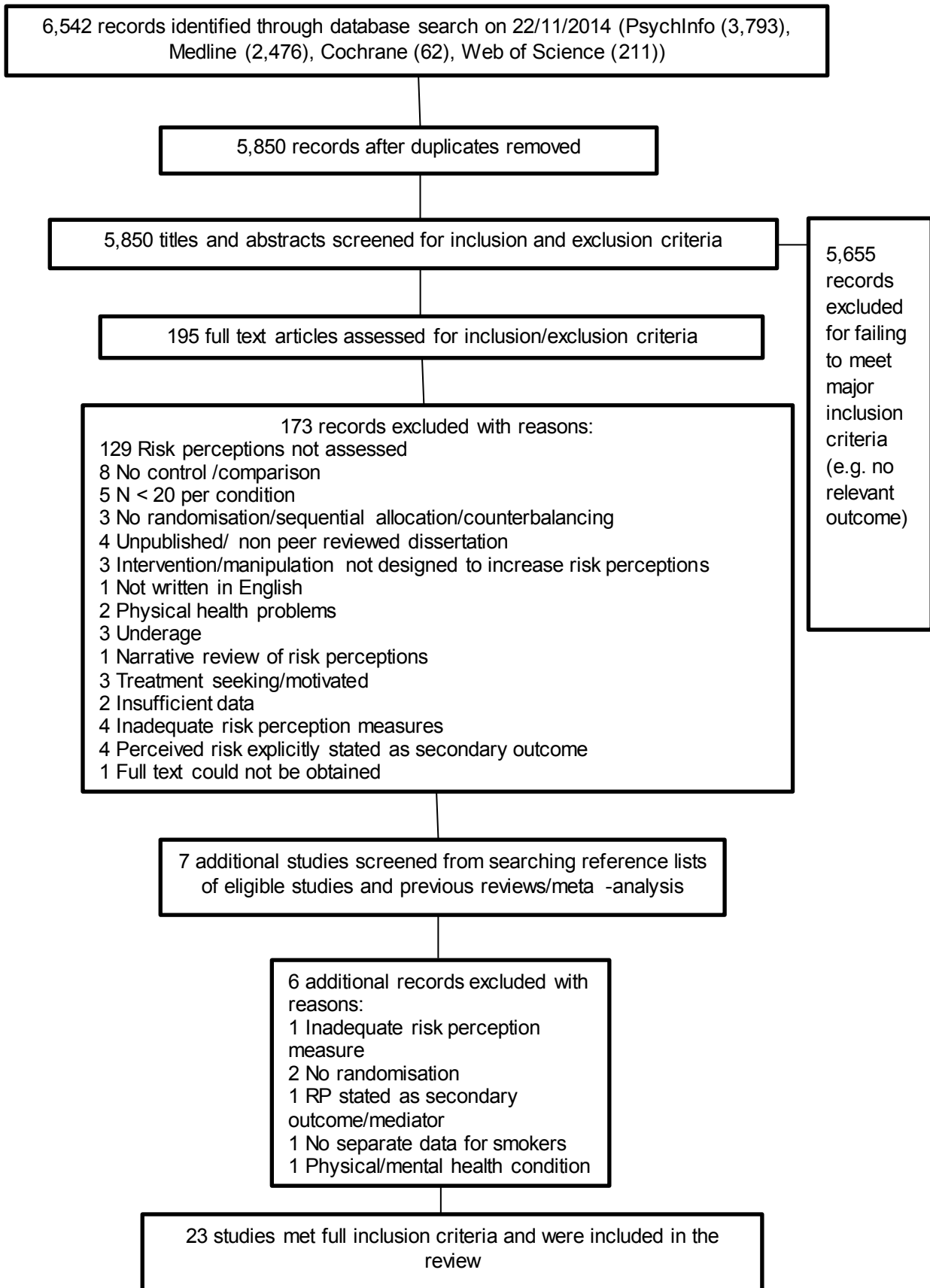


Figure 1. Flow chart: Identification and selection of studies

2.10 Data analysis

Meta-analytic procedures were used to establish the overall effectiveness of each type of manipulation at increasing risk perceptions among drinkers and smokers, both at post treatment and longer term follow up. To achieve this, separate post treatment and follow up comparisons (if available) were planned for the four different types of manipulations for drinkers and smokers using Review Manager 5.3 software. An assessment of effect sizes of other important outcomes (e.g. motivation, behaviour change) was beyond the scope of the current review, and these effects were not included in the analysis. In line with recommendations by Field and Gillet (2011), since we expected the effect sizes across individual studies to be heterogeneous, a random effects model was applied to all the data to estimate the weighted average effect of each manipulation category on risk perception outcomes.

2.11 Unit of Analysis issues

To prevent overweighting of effect sizes only one effect size per comparison for each study was included. When studies measured personal and comparative risk separately, the effect sizes for both outcomes were included separately in the final analysis. Where more than one risk perception measure for each type was included, only the main measure, as reported by the authors, was included. When the main measure had not been explicitly stated, the most widely used risk perception scale was chosen. When studies reported including two main measures for the same type of risk perception, each using the same scale, the combined average score was included in the analysis². For studies that assessed perceived risk

using a series of individual risk perception questions measured on the same scale, as opposed to a single measure, scores for each question were combined and an average mean and standard deviation was derived.

In three armed trials where more than one control or comparison condition was used, the condition that represented the minimum level of contact (e.g. no treatment) or lowest level of treatment (e.g. high threat versus low threat) was used in the analysis.

In studies using factorial designs with more than one type of manipulation, the condition most likely to influence risk perceptions based on theory or existing evidence was chosen. If both manipulations were equally likely to affect risk perceptions, and each had their own respective control group, both conditions were included in the analysis³.

Similarly, when studies had separated participants into different groups, either the average score across groups was combined in studies where the separate group factor was not relevant to the current review (e.g. numeracy condition in Wright, Whitwell, Takeichi, Hankins & Marteau, 2009) or only results relating to the highest risk groups were included (e.g. only results for the daily smokers were included in Vidrine, Simmons & Brandon, 2007).

2.12 Assessment of risk of bias

All eligible articles were critically appraised based on the guidelines outlined by the Cochrane Handbook for Systematic Reviews of Interventions version 5.0.1 (Higgins, Altman & Sterne, 2011). This approach argues against focusing purely on the methodological quality of studies and instead emphasises the importance of assessing the risk of bias within each study,

in order to determine the internal validity of the results. Using this procedure, the included studies were assessed against six key areas of bias (a) selection bias (e.g. random sequence allocation, allocation concealment); performance bias⁴ (e.g. blinding of personnel); detection bias⁵ (e.g. blinding of outcome assessors); attrition bias (e.g. incomplete outcomes); reporting bias (e.g. selective reporting) and other bias (e.g. any other sources of bias that can be identified).

2.13 Assessment of heterogeneity

Three types of heterogeneity have been highlighted to be necessary to assess when conducting a meta-analysis. Statistical heterogeneity refers to inconsistency in the results of studies included in a meta-analysis and arises when the results of the individual studies differ significantly more than would be expected due to chance (Deeks, Higgins & Altman, 2011). Statistical heterogeneity is an issue since it undermines the ability to draw firm conclusions about the average manipulation effect (Deeks, Higgins & Altman, 2011). In the present review, the significance of between study heterogeneity was measured using the chi-squared test ($p < .10$) and the overall percentage of the variability in effect estimates due to heterogeneity was measured using the I^2 test. An I^2 of 25% or less was considered to indicate low heterogeneity; I^2 of 50% was taken to indicate moderate heterogeneity and an I^2 of 75% or above was considered to indicate high levels of heterogeneity (Higgins, Thompson, Deeks & Altman, 2003). When significant statistical heterogeneity was found, sensitivity analysis (for manipulation types with enough studies included) was performed to

determine which studies may have been responsible for producing the observed variance.

Other aspects of heterogeneity that are also important are clinical heterogeneity (variability in the participants, interventions or outcomes studied) and methodological heterogeneity (variation in the study design and risk of bias) (Deeks, Higgins & Altman, 2011). Both of these types of heterogeneity can be responsible for producing significant statistical heterogeneity and can create complications in generalising the results and developing clear recommendations (Deeks, Higgins & Altman, 2011). These types of heterogeneity were therefore also considered when interpreting the results.

3. RESULTS

3.1 Search results

Out of the 5,857 studies identified from the electronic and reference list searches, 23 met full inclusion criteria and were included in this review.

3.2 Description of included studies

See Table 2 for a detailed description of the included studies.

3.2.1 Study design

Due to the inclusion criteria all of the included studies involved randomised controlled designs. However, one study (Hall, French & Marteau, 2009) used a cluster randomised design with clinic weeks as the unit of randomisation. The majority of studies used between group designs comparing experimental groups with either a control group or active

comparison ($n = 22$); with risk perception assessments conducted immediately after the manipulation. Only two studies incorporated purely repeated measures designs to assess within subject changes in risk perceptions (Walters & Woodall, 2003; Westmaas & Woicik, 2005).

3.2.2 Scope of studies

From the 23 studies included, a total of 1,320 participants made up the experimental groups and 1,233 participants comprised the control/comparison conditions. Four studies failed to provide data on the numbers of participants allocated to each condition⁶. Across all the studies combined the sample sizes ranged from 48 (Walters & Woodall, 2003) to 568 participants (Kozłowski et al; 1999) with a mean sample size of 159.

Most of the studies were conducted in the USA ($n = 14$), and nine took place in the UK. The majority ($n = 14$) recruited students from university settings. Eight studies recruited from community samples. One study recruited from both student and community settings (Magnan, Koblit, Zielke & McCaul, 2009). Twenty of the included studies consisted of samples of smokers, and three involved samples of drinkers.

3.2.3 Participant characteristics

Drinkers

There were a total of 257 adult drinkers included across the three alcohol studies (N in experimental conditions = 115; N in control/comparison = 142). Only one drinking study reported the age of their participants (Ayers & Myers, 2011), which ranged from 18 – 30 years. Across samples, the percentage of females varied from 56 % (Walters & Woodall, 2003) to 100%

(Klein, Harris, Ferrer& Zajac, 2011). Most of the drinking samples were comprised of students recruited from university settings ($n = 2$), with only one study (Walters & Woodall, 2003) recruiting drinkers from the community.

All three drinking studies reported exclusion criteria, with the main criteria being that participants were alcohol users prior to the study. One study made more specific requirements about the level at which participants had to be drinking to be eligible (e.g. drinking at least seven or more alcoholic beverages per week, Klein et al; 2011). Another study required participants to be aged 18 - 30 to take part (Ayers & Myers, 2011). None of these studies provided data on the number of participants that were excluded.

Table 2
Description of included studies

| Author (year) | Sample | Health domain | Manipulation category | Control/comparison | Risk outcome(s) | Post manipulation outcome(s) | Follow up(s) |
|----------------------------|---|--|---|---|---|---|--------------|
| Ayers et al. (2011) | Students Total $N = 124$ Age: $M = 21.9$ % Female = 60.4 | Alcohol Ax tool: self-report <u>Current use</u> M drinking episodes p/w = 4.7 M units p/e = 5.6 <u>History of use</u> Not ax | Affective: Presentation of 1 minute clip of health threat made personally relevant $n = 30$ | No treatment control $n = 59$ | Comparative risk for accident, unprotected sex, RTA, cirrhosis (vs. average student of same age and gender with similar drinking behaviours/ not conditional on current drinking) | <u>Compared to controls</u> 'Imagine' condition showed significantly higher CR for 3/4 events (except cirrhosis) | None |
| Brown et al. (2007) | Students Total $N = 102$ Age: $M = 22.5$ % Female = 58.8 | Smoking Ax tool: self-report <u>Current use</u> M p/d = 12.5 <u>History of use</u> Not ax | Deliberative & Affective: Presentation of 4 distressing photographs depicting negative health risks of smoking labelled to be caused by smoking & pamphlet with statements about the risks $n = 50$ | Active comparison: Deliberative & Affective: P's shown 4 minimally distressing photos linking smoking to negative health consequences & pamphlet (same as distressing image condition) $n = 52$ | Absolute verbal personal & average risk for lung cancer, stroke, emphysema, bronchitis, throat/mouth cancer, heart attack (conditional on continued current smoking) | <u>Compared to active comparison</u> 'Distressing image' condition showed significantly lower PR and significantly higher CO | None |

| | | | | | | | |
|---------------------------|--|---|---|--|--|--|------|
| Hall et al. (2006) | Community sample (commercial survey) Total <i>N</i> = 178 Age: <i>M</i> = 37.5 % female = 100 | Smoking Ax tool: self-report/ FTND <u>Current use</u> <i>M</i> FTND score = 4.9 <u>History of use</u> Not ax | Deliberative: Presentation of written threat information about smoking & efficacy information about quitting <i>n</i> = 60 | No treatment control <i>n</i> = 59 | Absolute verbal personal & comparative risk of cervical cancer (vs. non-smokers/not conditional on smoking) | <u>Compared to controls</u> 'Threat before efficacy' condition showed ns difference in PR and significantly higher CO | None |
| Hall et al. (2009) | Community (GP clinics) Total <i>N</i> = 242 Age: <i>M</i> = NS % Female = 100 | Smoking Ax tool: not stated <u>Current use</u> NS <u>History of use</u> NS | Deliberative: 5 minute presentation of written threat information linking cervical cancer to smoking <i>n</i> = 121 | No treatment control <i>n</i> = 121 | Absolute verbal personal & comparative risk for cervical cancer (vs. non-smokers/not conditional on smoking) | <u>Compared to controls</u> At 2 week follow up manipulation group showed significantly higher PR and CR | None |
| Hall et al. (2004) | Community (GP clinics) Total <i>N</i> = 172 Age: <i>M</i> = 42.7 % Female = 100 | Smoking Ax tool: self-report/FTND <u>Current use</u> <i>M</i> FTND score not provided <u>History of use</u> Not ax | Deliberative: Presentation of written threat information & how smoking adversely affects the cervix. Total both groups ('detailed' and 'minimal' explanation conditions) <i>n</i> = 103 | No treatment control <i>n</i> = 69 | Absolute verbal personal risk of cervical cancer (conditional on continued current smoking) | <u>Compared to control</u> Both leaflet conditions showed significantly higher PR at post ax 1 week later | None |

| | | | | | | | |
|-------------------------|--|---|---|--|---|---|------|
| Harris et al. (2007) | Students Total $N = 87$ Age : $M = 21.0$ % Female = 55.0 | Smoking Ax tool: self-report <u>Current use</u> $M p/d = 8.1$ <u>History of use</u> Not ax | Social science: P's wrote their desirable characteristics for 3 minutes & then presented with 4 unpleasant photographs showing negative health consequences of smoking with caption "smokers die younger" $n = 44$ | Matched control: P's recalled and listed everything they had eaten in past 24h for 3 minutes & then presented with 4 unpleasant photographs (same as experimental condition) $n = 43$ | Absolute verbal personal risk for high blood pressure, chest problems, lung cancer, bronchitis, stroke and heart disease (conditional on smoking) | <u>Compared to controls</u> 'Self-affirmed' condition showed ns difference in PR estimates | None |
| Klein et al. (2011) | Students Total $N = 120$ Age: $M = NS$ % Female = 100 | Alcohol Ax tool: self-report <u>Current use</u> M alcoholic beverages consumed in typical week = 11.5 <u>History of use</u> NS | Social science: P's wrote most important values and why they were important with examples of when they had used them & then given written article linking drinking and breast cancer $n = 60$ | Matched control: P's wrote why their least important value might be important to someone else & then presented with written article (same as experimental group) $n = 60$ | Absolute verbal personal risk of breast cancer (conditional on drinking) & Comparative risk of breast cancer (due to current drinking vs. average student of same age and gender) | <u>Compared to controls</u> 'Self-affirmed' participants showed ns differences in risk perception estimates (combined PR & CR) | None |

| | | | | | | | |
|-------------------------|---|---|--|---|---|---|------|
| Kozlowski et al. (2007) | Community (telephone survey) Total <i>N</i> = 568 Age: <i>M</i> = 39.0 % Female = 59 | Smoking Ax tool: self-report <u>Current use</u> <i>M</i> p/d = 20.0 <u>History of use</u> Not ax | Affective: Presentation of 1 minute verbal testimonial explaining the risks about smoking light cigarettes & played twice <i>n</i> = 293 | No treatment control (between groups) <i>n</i> = 275 Delayed message control (repeated measures): Heard message after answering interview q's <i>n</i> = 108 | Other: Perceived risk of health problems from smoking light cigarettes compared to regular cigarettes | <u>Compared to controls</u> P's in 'message' and 'delayed message' conditions were significantly less likely to state light cigarettes decreased their risks | None |
| Leventhal et al. (1967) | Students Total <i>N</i> = 129 Age: <i>M</i> = NS % Female = NS | Smoking Ax tool: self-report <u>Current use</u> NS <u>History of use</u> NS | Affective: P's presented with 8 minute video visually illustrating the negative effects of smoking on the body and highlighting the links between smoking and lung cancer & 6 minute film of a lung cancer operation <i>n</i> = 61 | Active comparison: P's shown same 8 minute video only <i>n</i> = 45 | Absolute verbal personal risk for lung cancer (not conditional on smoking) | <u>Compared to active comparison</u> ns difference in PR between 'high fear' and 'moderate fear' film conditions | None |

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|-------------------------|---|---|--|---|---|--|------|
| Lipkus et al. (2007) | Students Total <i>N</i> = 124 Age: <i>M</i> = 20.5 % Female = 39.0 | Smoking Ax tool: NS Current use <i>M</i> p/d = 15.3 <u>History of use</u> <i>M</i> smoking years = 5.6 | Deliberative: P's presented with factual written threat information (7 page brochure) about smoking (e.g. smoking related diseases) and description of lung age and its measurement (spirometry) & medical feedback : lung age and respiratory symptoms <i>n</i> = 65 | Active comparison: P's presented with factual 7 page brochure (same as experimental group) <i>n</i> = 59 | Absolute verbal personal risk of: serious health problems, lung cancer & emphysema (conditional on continued smoking) & Comparative risk for same conditions and health outcomes (vs. average college smoker) | <u>Compared to matched controls</u> Post ax 3 weeks later experimental group showed ns difference in PR or CR | None |
| Maddux et al. (1983) | Students Total <i>N</i> = 153 Age: <i>M</i> = NS % Female = NS | Smoking Ax tool: self-report <u>Current use</u> NS <u>History of use</u> NS | Affective: Fear appeal: P's provided with 'educational essays' stating smoking was likely to lead to lung cancer and heart disease <i>n</i> = 20 | Active comparison: P's provided false 'educational essay' stating smoking is unlikely to lead to lung cancer or heart disease) <i>n</i> = 20 | Absolute verbal personal risk in the years ahead for lung cancer or heart disease (conditional on continued smoking) | <u>Compared to active comparison</u> 'High probability' group showed significantly greater PR compared to 'low probability' group | None |

| | | | | | | | |
|-----------------------|--|--|---|--|---|--|------|
| Magnan et al. (2009) | Students and community (advert) Total N = 119 Age: M = 26.3 % Female = 57.1 | Smoking Ax tool: self-report FTND-R <u>Current use</u> <i>Experimental:</i> M p/d = 13.7, M FTND- R score = 3.5 <i>Control:</i> M p/d = 13.5, M FTND- R score = 3.1 <u>Hx of use</u> <i>Expt:</i> M age started = 15.1 M years smoked = 10.7 <i>Control:</i> M age started = 16.0 M years smoked = 10.6 | Deliberative: P's given written statements about the negative consequences of smoking & reminded to read them 6 x p/d in week 1 & 8 x p/d in week 2 n = 81 | Matched control: P's reminded to read statements about daily hassles 6x p/d week 1 & 8x p/d week 2 n = 38 | Absolute numeric personal risk of some type of cancer within lifetime (conditional on continued smoking) | <u>Compared to controls</u> Experimental group showed significantly higher PR at 2 week post -test ax | None |
| McBride et al. (2000) | Community (adverts) Total N = 144 Age: M = 39.0 % Female = 54.2 | Smoking Ax tool: self-report <u>Current use</u> M p/d = 19.0 <u>History of use</u> Not ax | Deliberative: presentation of genetic feedback in person (GSTM1 missing or present) & CO feedback n = 36 | Active control: Presentation of genetic feedback in person (GSTM1 missing or present) but no CO feedback n = 35 | Absolute verbal personal risk for lung cancer (conditional on continued smoking) & Comparative risk for lung cancer (vs. smokers and non-smokers) | <u>Compared to controls</u> Experimental group showed no difference in PR or CR at 2 month post -test ax. | None |

| | | | | | | | |
|-------------------------------|--|---|---|--|--|--|--|
| McCaul et al. (2007) | Students (university) Total <i>N</i> = 138 Age: <i>M</i> = NS % Female = 52.1 | Smoking Ax tool: self-report <u>Current use</u> <i>M</i> p/d= 10.0 <u>History of use</u> <i>M</i> age started = 16.0 <i>M</i> years smoked = 4.0 | Deliberative & Affective: After hearing an alarm set to go off 4x day for 1 week & then p's chose 1 of 8 cards depicting distressing images (e.g. blackened lung of smoker) & read statement about negative effects of smoking <i>n</i> = 33 | Matched control: After hearing an alarm set to go off 4x day for 1 week p's chose 1 of 8 cards to read statements about the effects of studying <i>n</i> = 52 | Absolute verbal and numeric personal risk for: emphysema, heart attack, lung cancer (sometime in lifetime/ not conditional on smoking) & Comparative risk for same health conditions (vs. other smokers) | <u>Compared to controls</u> 'Image group' showed ns difference in 'Average risk' (PR & CR combined) at 1 week post- test ax | None |
| McDonald et al. (2010) | Community (adverts) Total <i>N</i> = 72 Age: <i>M</i> = 46.6 % Female = 44.4 | Smoking Ax tool: self-report/FTND <u>Current use</u> <i>M</i> p/d = 12.8 <i>M</i> FTND score = 4.6 <u>History of use</u> <i>M</i> years smoked daily = 27.1 | Affective: presentation of 4x 3 minute guided imagery exercises relating smoking to negative health consequences <i>n</i> = 36 | Matched control: Presentation 4 x 3 minute guided imagery exercises matched for movement and sensory qualities with no reference to smoking or its consequences <i>n</i> = 36 | Established scale: Health risk subscale of smoking consequences questionnaire – adult (SCQ-A) | <u>Compared to controls</u> Experimental group showed ns difference in PR immediately post -test | 1 month ns difference in PR between groups |

| | | | | | | | |
|-------------------------------|--|---|--|--|--|--|------|
| Myers (2014) | Students (university) Total N= 120 Age: M= 24.4 % Female = 47.5 | Smoking Ax tool: self-report <u>Current use</u> M p/d = 15.6 <u>History of use</u> M years smoked= 4.7 | Affective: P's shown 1 minute clip of teenager discussing her father contracting lung cancer & asked to imagine it being personally relevant beforehand n = 40 | No treatment control n = 40 | Comparative risk for: lung cancer, stroke, bronchitis and heart disease (vs. same aged and gendered smokers/not conditional on smoking). | <u>Compared to controls</u> 'Imagine group' showed significantly higher levels of CO for all 4 health conditions | None |
| Shepperd et al. (2013) | Students (university) Total N = 128 Age: M = 19.9 % Female = 49.2 | Smoking Ax tool: self-report <u>Current use</u> Not ax <u>History of use</u> M years smoked= 2.6 | Decision science: New format of risk Information: P's presented with absolute risk of lung cancer associated with GSTM1 null type & risk presented visually to supplement written info n = 21 | Active comparison: P's presented with incremental risk associated with GSTM1 null type & no visual information n = 21 | Absolute verbal personal risk of lung cancer (conditional on having the GSTM1 null type gene variant) | <u>Compared to active comparison</u> 1. 'Absolute' vs 'incremental risk' group: ns difference in PR 2. 'Foreground' vs 'no display of risk': ns difference in PR 3. ns interaction between GSTM1 null type & foreground vs no display | None |

| | | | | | | | |
|------------------------------|--|--|---|--|---|--|------|
| Simmons et al. (2004) | Students (university) Total <i>N</i> = 144 Age: <i>M</i> = 22.1 % Female = 72.2 | Smoking Ax tool: self-report/FTND <u>Current use</u> <i>M</i> p/d = 16.8 <i>M</i> FTND score = 3.4 <u>History of use</u> <i>M</i> years smoked = 6.3 | Social science: P's read 16 point information sheet about health risks of smoking & then incorporated at least 8 points into persuasive message to encourage adolescents not to smoke & read speech in front of camera) <i>n</i> = 36 | No treatment control P's just provided with historical information about tobacco <i>n</i> = 36 | Absolute verbal personal risk of smoking related conditions (e.g. lung cancer and emphysema) (not conditional on smoking) | <u>Compared to controls</u> 'Smoke risk' group showed significantly higher PR immediately following manipulation | None |
| Vidrine et al. (2007) | Students (university) Total <i>N</i> = 227 Age: <i>M</i> = 21.3 % Female = 82.0 | Smoking Ax tool: self-report <u>Current use</u> <i>M</i> p/d = 10.4 <u>History of use</u> NS | Deliberative: P's presented with factual pamphlet describing 16 health risks associated with smoking using logical verifiable evidence supporting these links <i>n</i> = 35 | Matched control: P's presented with pamphlet including 16 messages about causes and symptoms of food borne illness equated for length <i>n</i> = 20 | Absolute verbal personal risk of GI problems, circulatory diseases, cancer, fertility/sexual problems & general risk of any smoking related condition (conditional on smoking/ no time period specified) & Absolute numeric general risk & Comparative specific risk (vs. other smokers, women and men) | <u>Compared to controls</u> 'Factual' condition showed ns differences in any PR outcomes immediately following the manipulation | None |

| | | | | | | | |
|------------------------|---|--|--|--|---|---|---|
| Walters et al. (2003) | Community (employees newsletter offering free check-up) Total <i>N</i> = 48 Age: <i>M</i> = NS % Female = 56.0 | Alcohol Ax tool: self-report Q/F measure <u>Current use</u> <i>M</i> drinks p/w = 5.8 (immediate) 7.5 (delayed) <u>Hx of use</u> Not ax | Deliberative: presentation written threat & personal feedback. P's provided with personal feedback about their drinking, including level of risk and negative consequences of drinking after completing baseline ax <i>n</i> = 25 | Delayed control: P's provided with personal feedback (same as experimental group) 8 weeks after completing baseline ax <i>n</i> = 23 | Absolute personal riskiness of alcohol consumption (no disease specified/ not conditional on drinking) | <u>Compared to baseline RP</u> Repeated measures: 'immediate' and 'delayed' feedback (DF) groups showed significant increases in RP following feedback at 8 weeks (IF) and 16 weeks (DF) | 16 weeks IF group's PR declined to baseline levels at 16 week follow up |
| Westmaas et al. (2005) | Students (university) Total <i>N</i> = 186 Age: <i>M</i> = 18.9 % Female = 48.9 | Smoking Ax tool: self-report/FTND <u>Current use</u> <i>M</i> p/d = 10.0 <i>M</i> FTND score = 2.3 <u>History of use</u> <i>M</i> years smoked = 2.2(men), 2.4 (women) | Deliberative: False feedback of risk: P's presented with hypothetical scenario in which genetic test indicated they were at high risk for lung cancer compared to peers <i>n</i> = 186 | Active comparison (repeated measures): P's also presented with hypothetical scenario in which genetic test indicated they were at low risk for lung cancer (counterbalanced order with high risk message) compared to peers <i>n</i> = 186 | Comparative risk for lung cancer and/or oral cancer (vs. other smokers their age/other men or women their age/ and non-smokers/ not conditional on smoking) | <u>Compared to active comparison</u> Repeated measures: P's reported significantly higher CR (combined scale) in 'high risk' compared to 'low risk' condition | None |

| | | | | | | | |
|----------------------|--|--|---|--|---|---|------|
| Wright et al. (2006) | Students (university) Total <i>N</i> = 198 Age: <i>M</i> = 20.4 % Female = 51.0 | Smoking Ax tool: self-report/HSI <u>Current use</u> <i>M</i> HSI = 1.3 <u>History of use</u> <i>M</i> years since 1st smoked = 6.47 | Deliberative: False feedback & genetic results: hypothetical scenario asking them to imagine attending their GP, having a genetic test to identify increased risk of heart disease due to genes and smoking and told they had the positive gene so were high risk <i>n</i> = 66 | Active comparison: P's presented with hypothetical scenario asking them to imagine attending Gp and being told of links between smoking and heart disease and of personal increased risk due to smoking but with no genetic test <i>n</i> = 66 | Absolute verbal personal risk of heart disease (conditional on continued smoking) & Comparative risk for heart disease (vs. same aged and gendered smokers and non-smokers) | <u>Compared to controls</u> 'Gene positive' group showed significantly higher CR for heart disease compared to other smokers ns difference between groups on CR compared to non-smokers ns difference in PR conditional on continued smoking | None |
| Wright et al. (2009) | Community (internet) Total <i>N</i> = 140 Age: <i>M</i> = 44.3 % Female = 56.4 | Smoking Ax tool: self-report/HSI <u>Current use</u> <i>M</i> p/d = NS <i>M</i> HSI score = 2.6 <u>History of use</u> Not ax | Decision Science-False feedback of risk & new format of risk presentation: Hypothetical scenario imaging undergoing risk assessment for Crohn's disease, presented with 50% risk & risk presented in dispersed dot display <i>n</i> = 46 | Active comparison: P's presented with same hypothetical scenario indicating they were at low risk for Crohn's disease & risk presented in grouped display <i>n</i> = 46 | Absolute verbal personal risk of Crohn's disease (conditional on continued smoking) & Comparative optimism for Crohn's disease (compared to same aged smokers) | <u>Compared to active comparison</u> 1. 'High risk' group reported ns differences on all RP outcomes 2. 'Dispersed display' p's reported ns differences on all RP | None |

outcomes

3. Significant
Risk magnitude
x numeracy
interaction:
High numeracy
p's in higher
risk (50%)
condition
reported higher
PR conditional
on smoking
compared to
low risk (3%)
p's.

Notes. M = Mean. Health domain assessments: FTND = Fagerstrom test of nicotine dependence (Heatherton, Kozlowski, Frecker & Fagerstrom, 1991). HIS = Heaviness of smoking index (Heatherton, Kozlowski, Frecker, Rickert & Robinson, 1989). Current use: p/e = per episode; p/d = per day; p/w = per week; NS = not stated; ax = assessed. Outcomes: PR = Personal risks; CR = Comparative risks; CO = Comparative optimism; ns = not significant. Studies in bold were included in the meta-analysis.

Current drinking levels across studies varied widely from an average of 5.87 ($SD = 4.90$; Walters & Woodall, 2003) to 11.52 ($SD = 4.51$; Klein et al; 2011) mean alcoholic beverages a week; and from 1 to 7 drinking episodes per week ($M = 4.76$, $SD = 2.66$; Ayers & Myers, 2011) involving between 1 and 20 UK units per episode ($M = 5.65$, $SD = 3.85$; Ayers & Myers, 2011).

All of the drinking studies used self-report measures to establish participants' current drinking status. However, the specific measures used differed within each study. For example, one study assessed average drinking episodes and average UK units consumed per episode in a typical week (Ayers & Myers, 2011). Another (Klein et al; 2011) assessed the number of alcoholic beverages consumed each day over the past seven days as well as during a typical week, with responses combined to create an average typical drinking score. The final study (Walters & Woodall, 2003) used the well-established and reliable quantity frequency measure, which involved asking participants about the number of standard drinks (defined as 0.5 ounce of pure ethyl alcohol) they consumed over a typical week over the past 30 days, in addition to assessing peak blood alcohol concentration levels during the past month by asking about their heaviest drinking episode in the past 30 days and the number of hours during that episode that alcohol was consumed. None of the drinking studies asked questions about the history of participants' alcohol consumption, such as the age at which they started drinking or the number of years they had been drinking alcohol.

Smokers

There was a total of 2,305 adult smokers included in the 20 smoking studies (N in experimental conditions = 1,352; N in control/comparisons =

953) with a mean age ranging from 18.9 (Westmaas et al; 2005) to 46.6 years (McDonald, O'Brien, Farr & Haaga, 2010). Four studies failed to provide data regarding the ages of participants. The percentage of females across these studies ranged from 39 % (McDonald et al; 2010) to 100 % (Hall, Bishop & Marteau, 2006; Hall, French & Marteau, 2009; Hall, Weinman & Marteau, 2004). Two studies did not provide data regarding the gender distribution of their samples. The majority of participants were students recruited from university settings ($n = 12$). The remaining studies recruited community samples using a range of sources (e.g. Gp clinics, internet).

Across studies, current smoking levels ranged from 8.1 mean cigarettes per day (Harris, Mayle & Napper, 2007) to 20.0 mean cigarettes per day (Kozlowski et al; 1999) from the 16 studies that reported this data.

Smoking status was typically assessed using self-report questions (e.g. average daily number of cigarettes smoked as in McCaul, Mullens, Romanek, Erickson & Gatheridge, 2007). Six studies also included validated measures such as the Fagerstrom Test of Nicotine Dependence (FTND; Heatherton, Kozlowski, Frecker & Fagerstrom, 1991; e.g. McDonald et al; 2010) or the Heaviness of Smoking Index (HIS; Heatherton, Kozlowski, Frecker, Rickert & Robinson, 1989; e.g. Wright, French, Weinman & Marteau, 2006). None of these studies used more objective procedures (e.g. measuring smoker's carbon monoxide levels), to assess current use (however Simmons, Webb & Brandon, 2004 assessed carbon monoxide levels as part of their eligibility assessment).

Eight of the twenty included studies provided data regarding the self-reported history of participants' smoking. Among these studies, the mean

length of years participants had smoked varied widely from 2.2 years (Westmaas et al; 2005) to 27.1 years (McDonald et al; 2010).

A large proportion of the smoking studies reported exclusion criteria ($n = 18$), with general consistency in the criteria participants had to meet to take part across these trials (e.g. Over 18, English speaking daily smokers, not motivated or already enrolled in programmes to reduce/quit smoking). However, the number of cigarettes participants were required to smoke varied considerably between studies (e.g. one cigarette per week in Shepperd et al, 2013 to at least ten cigarettes per day in Simmons et al; 2004).

Out of the eighteen studies reporting eligibility criteria, only two reported the number of participants subsequently excluded from taking part, with valid reasons provided (e.g. due to taking part in other smoking related studies run by the authors or smoking less than five cigarettes per day in Mc Bride et al; 2000, or for not receiving the leaflet or having already given up smoking prior to the study in Hall et al; 2004).

3.2.4 Characteristics of the manipulations

Drinkers

All of the drinking studies used different manipulations. One study used a deliberative manipulation (e.g. Walters et al; 2003). One study used an affective manipulation (Ayers & Myers, 2011). Another study included a social science 'self-affirmation' manipulation (Klein et al; 2011). Each of these consisted of one off, single sessions made up of at least two distinct elements (e.g. presentation of written information about risk plus personal feedback, as in Walters et al; 2003). Only Ayers and Myers (2011) reported

how long participants were exposed to their presented threat (approximately 1 minute). In addition, only one of these studies reported adequate checks for the credibility and persuasiveness of their threatening message, which was conducted prior to the study (Klein et al; 2011).

Smokers

The deliberative category was the most common manipulation type used for smokers ($n = 9$), followed by the affective manipulations ($n = 5$), social science-based interventions ($n = 2$) decision science-based manipulations ($n = 2$), and combined deliberative and affective manipulations ($n = 2$). Across smoking studies, manipulations typically included between one ($n = 8$) and two distinct elements ($n = 10$) delivered in single one off sessions ($n = 18$). The length of exposure to the presented threats in these studies varied from 1 (Myers, 2014) to 14 minutes (Leventhal, Watts & Pagano, 1967). Only two studies implemented more intensive daily manipulations lasting from 1 (McCaul, Mullens, Romanek, Erickson & Gatheridge, 2007) to 2 weeks (Magnan, Koblitz, Zielke & McCaul, 2009). The majority of studies failed to specify the exact duration of their manipulations ($n = 12$).

Within each category of manipulation used among smokers , there was large variability with regards to the specific strategies employed (e.g. presenting written threats only in studies using deliberative manipulations or combining this with reminders to read the threats or with medical/genetic feedback; see Table 2 for details of the specific strategies used within each manipulation category).

A large majority of the smoking studies assessed the credibility of their manipulations ($n = 12$), either by ensuring the manipulation had

successfully worked as intended ($n = 6$) or assessing this in other ways; for example, by exploring the persuasiveness of the threat message (Hall et al; 2006); the level of coherence in the link reported between smoking and a particular health threat (Hall et al; 2004); the level of accuracy in participants overall recall and interpretation of the threat (e.g. McBride et al; 2000; Wright et al; 2006); or by evaluating participants' level of compliance with the study procedures (e.g. Magnan et al; 2009; McCaul et al; 2007).

3.2.5 Characteristics of comparison/control conditions

Drinkers

Each of the manipulations used in the drinking studies were compared against different control conditions. For example, the affective manipulation used a no treatment control comparison (Ayers & Myers, 2011). The social science study used an adequately matched control condition to their self-affirmation experiment, and the deliberative manipulation employed a delayed control group who received the same personal feedback information eight weeks after the experimental condition (Walters & Woodall, 2003).

Smokers

A range of controls were also used across the smoking trials. The majority of these studies compared their experimental manipulations or interventions against an active comparison or matched control conditions (both n 's = 7). Five studies used no treatment controls and one added a delayed control group (Kozlowski et al; 2007) who received the manipulation immediately after the initial interview. There was also large variability in the

comparison groups used between studies using the same manipulation category (except for the decision science based manipulations).

3.2.6 Outcome measurement characteristics

All of the included studies measured risk perceptions via self-report questions. Among these, only one study used a validated measure (McDonald et al; 2011).

Drinkers

Each of the three drinking studies assessed risk perceptions in slightly different ways. For example, one study assessed participants' perceived personal 'riskiness' of drinking using a single question (Walters & Woodall, 2003). One study assessed drinkers' perceived absolute risks of developing breast cancer due to their current level of drinking, as well as their comparative optimism for this threat (versus the average student their age and gender) and combined these scores into a composite risk perception scale (Klein et al; 2011). The final alcohol study used four separate questions to assess participants' comparative risks for a range of negative outcomes, compared to same aged and gender peers with similar drinking behaviours (e.g. cirrhosis, unprotected sex; Ayers & Myers, 2011). All but two of these studies assessed risk perceptions immediately after the experiment. One study measured participants' risk perceptions over two separate time points, ranging from 8 to 16 weeks (Walters & Woodall, 2003). None of these studies gave participants a specific time frame to base their risk estimates on, or used questions that conditioned participants risk estimates on their continued current drinking levels.

Smokers

There were 27 risk perception outcome assessments included across the smoking trials. The majority of these assessments involved multiple questions (15 out of the 18 personal risk assessments; 5 out of the 9 comparative risk assessments) ranging from 2 (Magnan et al; 2009) to 9 items (Vidrine et al; 2007); with responses typically combined to create an average composite risk perception score (except in Wright et al; 2006). Four studies assessed risk perceptions with only single item questions (Hall et al; 2004; 2009; Kozlowski et al; 2007; Leventhal et al; 1967). All except for one of the smoking studies measured risk perceptions using absolute verbal risk questions in which participants were given Likert response options whereby higher scores represented greater perceived personal or comparative risk. One study measured perceived risk by asking participants to rate their risk of lung cancer on a numerical scale from 0 (“no chance”) to 100 (“guaranteed to happen”; Magnan et al; 2009). Only one study used both absolute verbal Likert as well as numerical percentage risk estimate measures (McCaul et al; 2007).

Half of the smoking studies measured personal risks only. A minority of smoking studies focused solely on comparative risks ($n = 2$, Myers, 2014 Westmaas et al; 2005). Eight studies assessed both personal and comparative risks. Among these studies, the majority kept the two risk scores separate in keeping with arguments that they represent distinct aspects of risk perception (e.g. Shepperd et al; 2013). However, McCaul and colleagues (2007) combined these estimates to create an average risk composite score.

Most of the smoking studies assessed risk perceptions immediately post intervention. However, six studies assessed risk perceptions following delays ranging from 1 week (e.g. McCaul et al; 2007) to 2 months (e.g. McBride et al; 2000). Only one study (McDonald et al; 2010) measured changes in participants risk perceptions after a 1 month follow up.

The number of threats targeted in the risk perception assessments among smokers ranged from 1 ($n = 8$), to 6 ($n = 2$) with lung cancer being the most commonly used threat across the smoking trials. Only Vidrine and colleagues (2007) assessed participants perceived risk of contracting a series of specific health problems as well as their general risk of developing any smoking related health condition.

A large proportion of smoking studies failed to condition their risk questions on participants current or continued smoking ($n = 7$). One study made participants base their estimates on the absence of a gene type they were told could increase their risks of experiencing health problems as a result of their smoking⁸ (Shepperd et al; 2013). Four studies enquired about participants' perceived risks of a particular health threat due to their smoking. None of these studies gave smokers a specific time frame in which to base their risk estimates on.

In terms the of comparative risk assessments conducted across the smoking trials ($n = 11$), all except for one of these studies measured these estimates directly (e.g. by asking "Compared to other people who smoke cigarettes, how would you rate your chances of developing a smoking-related medical condition?" from 1 ("much less") to 5 ("much greater") as in McCaul et al; 2007). Only Brown and Smith (2007) assessed comparative risks using the recommended indirect method (e.g. Covey & Davies, 2004)

by asking participants to rate their own probability of experiencing a range of smoking related illnesses (e.g. lung cancer, bronchitis, a heart attack etc.) as well as asking them, in a separate question, the probability of the average student smoker of the same age and gender experiencing these outcomes; with comparative risks rated as the difference between these scores.

Across studies that measured comparative risks, there was a lack of consistency in the reference group used for participants to compare their risks against (e.g. compared to smokers and/ or non-smokers and/or same aged peers and/or same gendered peers, etc.)

3.3 Risk of bias

Tables 3 and 4 present details regarding the risk of bias assessed across the drinking and smoking studies, respectively.

Overall risk of bias among drinking studies: Across each of the areas of bias assessed in the alcohol studies the overall risk of bias remains unclear; with bias in key areas (e.g. selection, performance) possible.

Overall risk of bias among smoking studies: Altogether the smoking studies were also rated as unclear risk of bias given that the majority of studies received this rating for most of the areas of bias assessed. Thus bias in important domains (e.g. selection and performance bias) is likely in these studies.

Table 3
Risk of bias among drinking studies

| Study | Random Sequence generation | Allocation concealment | Blinding of personnel | Incomplete outcome data | Selective outcome reporting | Overall risk of bias |
|-----------------------|---|--|--|---|--|-----------------------------|
| Ayers et al. (2011) | No method of randomisation reported/ Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not reported/ Unclear risk | No incomplete or missing outcome data/ Low risk | All pre-specified outcomes reported including non-significant findings/ Low risk | Unclear risk |
| Klein et al. (2011) | No method of randomisation reported/Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was ensured and unlikely to have been broken/ Low risk | Numbers randomised to conditions not stated/Unclear risk | All pre-specified outcomes reported including non-significant findings/ Low risk | Unclear risk |
| Walters et al. (2003) | No method of randomisation reported/Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not reported/ Unclear risk | 26 participants excluded from analysis due to being non-drinkers at pre-test. Attrition reported across follow ups (4.2 % at 8 week follow up | All pre-specified outcomes reported including non-significant findings/ Low risk | Unclear risk |

and 4.2% at 16
week follow up). Not
stated if equal drop -
out rates across
immediate and
delayed feedback
groups,
reasons for drop out
not provided and no
intention to treat
analysis but such
low attrition rates
are unlikely to have
biased overall
findings /Low risk

Table 4
Risk of bias among smoking studies

| Study | Random Sequence generation | Allocation concealment | Blinding of personnel | Incomplete outcome data | Selective outcome reporting | Overall risk of bias |
|----------------------------|---|--|---|---|--|-----------------------------|
| Brown et al. (2007) | Method of randomisation reported (coin tossing)/ Low risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not reported/ Unclear risk | No incomplete or missing outcome data/ Low risk | All pre-specified outcomes reported including non-significant findings/ Low risk | Unclear risk |
| Hall et al. (2006) | No method of randomisation reported/Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not reported/ Unclear risk | No incomplete or missing outcome data/ Low risk | All pre-specified outcomes reported including non-significant findings/ Low risk | Unclear risk |
| Hall et al. (2009) | Method of randomisation reported (computer generated random numbers)/Low risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not possible since they were directly involved in administering key parts of the intervention which may have biased | Attrition from baseline to study reported (31% of eligible sample) with reasons (difficulties with recruitment). Loss to follow up reported (29%, not | All pre-specified outcomes reported including non-significant findings/ Low risk | Unclear risk |

| | | | | | | |
|-----------------------------|--|--|---|--|--|--------------|
| | | | outcomes/ High risk | stated if equal across groups), no reasons provided and no intention to treat analysis performed, but responders and non-responders not found to differ on relevant demographic health or motivational factors that could affect risk outcomes /Low risk | | |
| Hall et al. (2004) | No method of randomisation reported/Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not reported/ Unclear risk | 73 participants excluded prior to study with valid reasons (e.g. given up smoking). Missing data reported only for demographic or smoking variables not for risk perception outcomes/Low risk | All pre-specified outcomes reported including non-significant findings/ Low risk | Unclear risk |
| Harris et al. (2007) | Method of randomisation reported (random number tables)/Low risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was ensured and was unlikely to have been broken/Low risk | No incomplete or missing outcome data for risk perception outcomes/Low risk | All pre-specified outcomes reported including non- | Low risk |

| | | | | | | |
|-------------------------|---|--|--|--|---|-----------|
| | | | | | significant findings/ Low risk | |
| Kozlowski et al. (1999) | Random digit dialling used to select eligible participants. No method of randomisation into conditions/Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not possible since they were directly involved in administering key parts of the intervention/ High risk | Some missing data reported (< 6%) on 'key variables'. Not stated if equal across groups but missing patterns describe as 'random'. No reasons provided. Addressed using AMOS software/Low risk | Risk perception outcome not stated in method but included in the results with no means of standard deviations / High risk | High risk |
| Leventhal et al. (1967) | No method of randomisation reported/Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not reported/ Unclear risk | No missing or incomplete outcome data reported. Numbers randomised to groups not stated/ Unclear risk | No means and standard deviation provided for risk perception outcome and 1 week follow up data of risk perceptions not reported/High risk | High risk |
| Lipkus et al. (2007) | No method of randomisation reported/Unclear risk | No method of allocation concealment described / Unclear | Blinding of experimenters was not possible since they were directly involved | Some data excluded from experimental group (n = 8) with valid | No standard deviation provide for risk outcomes /High | High risk |

| | | | | | | |
|-----------------------------|--|--|--|---|--|--------------|
| | | risk | in in administering key parts of the manipulation/ High risk | reasons provided (lung age could not be obtained), no intention to treat analysis /Low risk | risk | |
| Maddux et al. (1983) | No method of randomisation reported/Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not reported/ Unclear risk | Some data excluded from analysis for valid reasons (e.g. suspicion about study aims/uncooperative, $n = 2$).Numbers randomised to groups not stated/Unclear risk | All pre-specified outcomes reported including non-significant findings/ Low risk | Unclear risk |
| Magnan et al. (2009) | No method of randomisation reported/Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not reported/ Unclear risk | Some participants were excluded for failing to complete the second week of the protocol ($n = 4$, not stated if equal across groups). No missing outcome data for risk perception outcomes/Low risk | All pre-specified outcomes reported including non-significant findings/ Low risk | Unclear risk |
| McBride et al. (2000) | No method of randomisation reported/Unclear risk | No method of allocation concealment described / Unclear | Blinding of experimenters was not possible since they were directly involved | Attrition from baseline to study reported prior to randomisation (43 | All pre-specified outcomes reported | Unclear risk |

| | | | | | | |
|-------------------------------|--|--|---|---|--|--------------|
| | | risk | in in administering key parts of the manipulation/ High risk | % of original sample), minimal attrition reported at follow up (1 %), no reasons provided and intention to treat analysis not conducted but such low attrition rates unlikely to have biased findings/ Low risk | including non-significant findings/ Low risk | |
| McCaul et al. (2007) | No method of randomisation reported/Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not reported/ Unclear risk | No incomplete or missing outcome data/Low risk | All pre-specified outcomes reported including non-significant findings/ Low risk | Unclear risk |
| McDonald et al. (2010) | Method of randomisation reported (computer random number generator)/Low risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not possible since they were directly involved randomising participants to experimental conditions/ High risk | No incomplete or missing outcome data for risk perception outcomes/Low risk | All pre-specified outcomes reported including non-significant findings/ Low risk | Unclear risk |
| Myers (2014) | No method of randomisation reported/Unclear risk | No method of allocation concealment | Blinding of experimenters was not possible since they | No incomplete or missing outcome data /Low risk | All pre-specified outcomes | Unclear risk |

| | | | | | | |
|-------------------------------|--|--|--|--|--|--------------|
| | | described / Unclear risk | were directly involved in administering key parts of the manipulation/ High risk | | reported including non-significant findings/ Low risk | |
| Shepperd et al. (2013) | No method of randomisation reported/Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not reported/ Unclear risk | No incomplete or missing outcome data /Low risk | No means or standard deviations for risk perception outcomes from absolute vs incremental risk condition/High risk | Unclear risk |
| Simmons et al. (2004) | No method of randomisation reported/Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not reported/ Unclear risk | Numbers randomised to groups not stated)/ Unclear risk | All pre-specified outcomes reported including non-significant findings/ Low risk | Unclear risk |
| Vidrine et al. (2007) | No method of randomisation reported/Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not reported/ Unclear risk | No incomplete or missing outcome data /Low risk | No means or standard deviations for control group on one risk outcome reported (risk specific to men) | Unclear risk |

| | | | | | | |
|-----------------------------|--|--|--|---|--|--------------|
| | | | | | /High risk | |
| Westmaas et al. (2005) | No method of randomised counterbalancing reported/Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not reported/ Unclear risk | No incomplete or missing outcome data /Low risk | All pre-specified outcomes reported including non-significant findings/ Low risk | Unclear risk |
| Wright et al. (2006) | No method of randomisation reported/Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not reported/ Unclear risk | No incomplete or missing outcome data /Low risk | All pre-specified outcomes reported including non-significant findings/ Low risk | Unclear risk |
| Wright et al. (2009) | No method of randomisation reported/Unclear risk | No method of allocation concealment described / Unclear risk | Blinding of experimenters was not reported/ Unclear risk | No incomplete or missing outcome data /Low risk | All pre-specified outcomes reported including non-significant findings/ Low risk | Unclear risk |

Note. Studies in bold were included in the meta-analysis.

3.4 Treatment effects

Across all 23 studies, consisting of 48 comparisons of immediate post treatment risk perception outcomes, there were a total of 16 significant increases in risk perceptions reported among the drinking and smoking groups combined. However, over half of all comparisons were non-significant ($n = 25$). In addition, seven of the comparisons reported significant negative effects (e.g. significantly *lower* personal risk estimates or significantly *higher* comparative optimism after the experiment).

Only two studies, involving the deliberative (McDonald et al; 2010) and affective (Walters & Woodall, 2003) manipulation types assessed longer term changes in risk perceptions. Both of which revealed no lasting effects of these manipulations on risk perceptions.

3.5 Meta-analytic results

To establish the overall and individual efficacy of the different manipulation categories on personal and comparative risk perceptions a meta-analysis was performed. However this analysis was limited to the immediate effects of these manipulations given the lack of follow up data collected across studies.

3.5.1 Overall effects of manipulations on drinkers risk perceptions

Unfortunately, it was not possible to determine the overall effect of each of the manipulation categories on drinkers risk perceptions (measured immediately post treatment or at follow up), given the small number of diverse manipulations employed. However, in terms of their individual results, the affective manipulation used by Ayers and Myers (2011) had a

moderate and significant positive effect across the four comparative risk outcomes they assessed ($k = 1$, $n = 89$; $SMD = 0.66$, 95 % CI [0.21, 0.11], $z = 2.86$, $p = .004$); with experimental participants reporting significantly higher levels of comparative risk immediately following the experiment, compared to controls.

The remaining two drinking studies failed to provide means and standard deviations for their risk perception outcomes and so their effects sizes could not be computed. Out of these results, the social science manipulation study found no significant differences in drinkers' risk perceptions between the experimental and control groups (Klein et al; 2011). However the deliberative manipulation was found to significantly increase participants risk perceptions in both the immediate and delayed feedback groups (Walters & Woodall, 2003). Despite this, the observed changes did not remain over time, with the authors reporting the immediate feedback groups' levels of perceived risk declined to baseline levels after 16 weeks (Walters & Woodall, 2003).

3.5.2 Overall effects of all manipulations (combined) on smokers risk perceptions

Thirteen out of the twenty smoking studies provided sufficient data to analyse the immediate effects of all the manipulation types on personal and comparative risk perceptions, separately. However, insufficient data was provided to analyse the longer term effects on these outcomes.

Personal risks: The overall effect of all four manipulation types across the twelve smoking studies that included this outcome was small and failed to reach significance ($k = 12$, $n = 1189$; $SMD = 0.19$, 95 % CI [- 0.05, 0.44], $z = 1.55$, $p = .12$). However there was high levels of significant statistical

heterogeneity between the effects from the different manipulation types ($\chi^2 = 47.44$; $df = 11$; $p < .001$; $I^2 = 77\%$), indicating that the effect sizes for smoker's personal risks varied significantly between the manipulation categories (see Figure 2).

Comparative risks: Seven smoking studies provided sufficient data to meta-analyse the overall effects of all manipulation types on smokers comparative risks (except for the social science based studies). The combined effect of these manipulations was small and non-significant ($k = 7$, $n = 705$; $SMD = -0.1$, 95% CI [-0.60, 0.41], $z = 0.38$, $p = .70$). The results also revealed significantly high levels of statistical heterogeneity between these studies ($\chi^2 = 63.94$; $df = 6$; $p < .001$; $I^2 = 91\%$) (see Figure 3), suggesting there were significant differences in the size of effects produced by the different manipulation types on smokers comparative risks.

In the following section, the overall effects of each type of manipulation are presented separately for the personal and comparative risk outcomes.

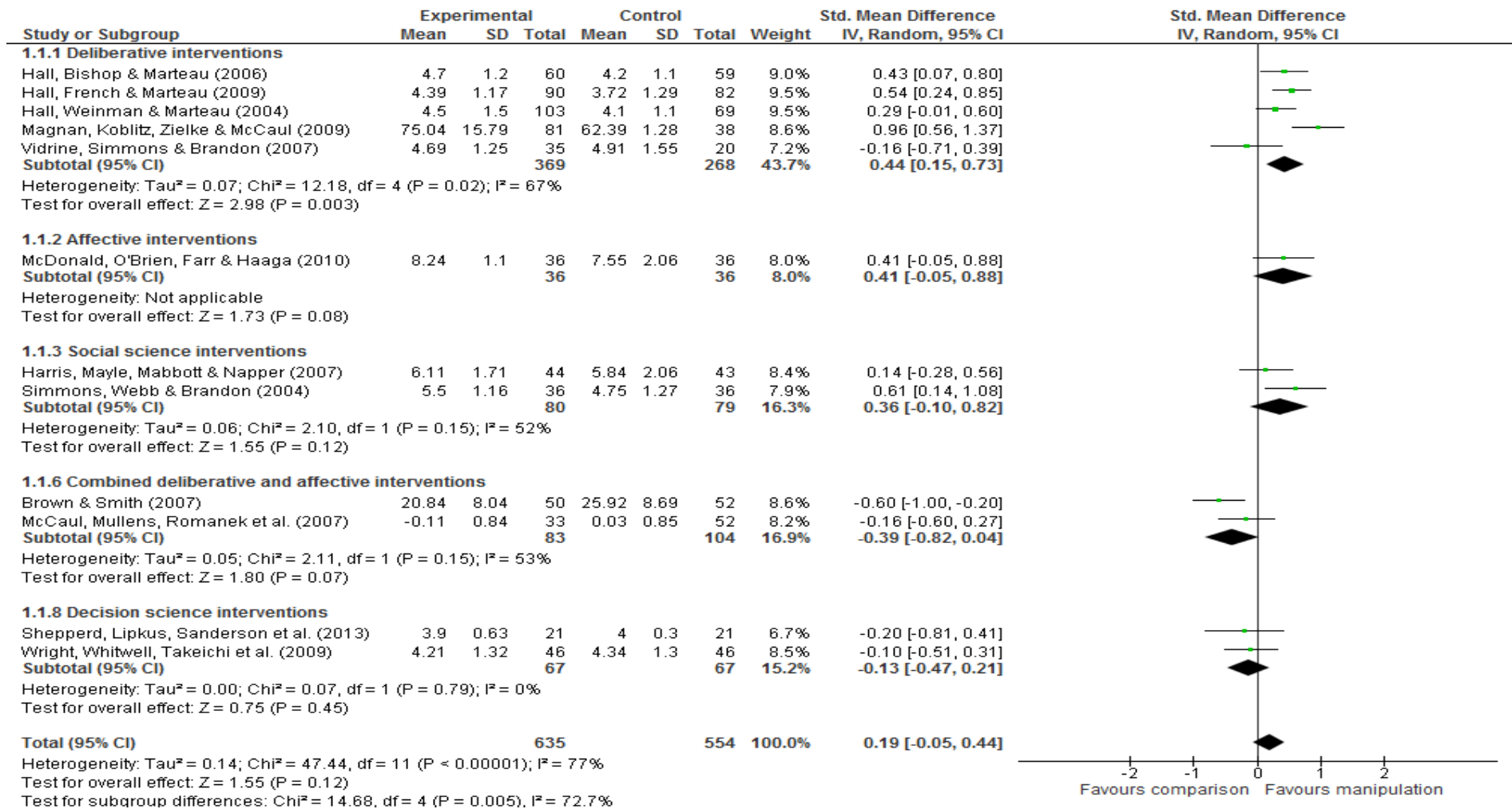


Figure 2. Overall efficacy of all manipulations at increasing smokers' personal risk estimates.

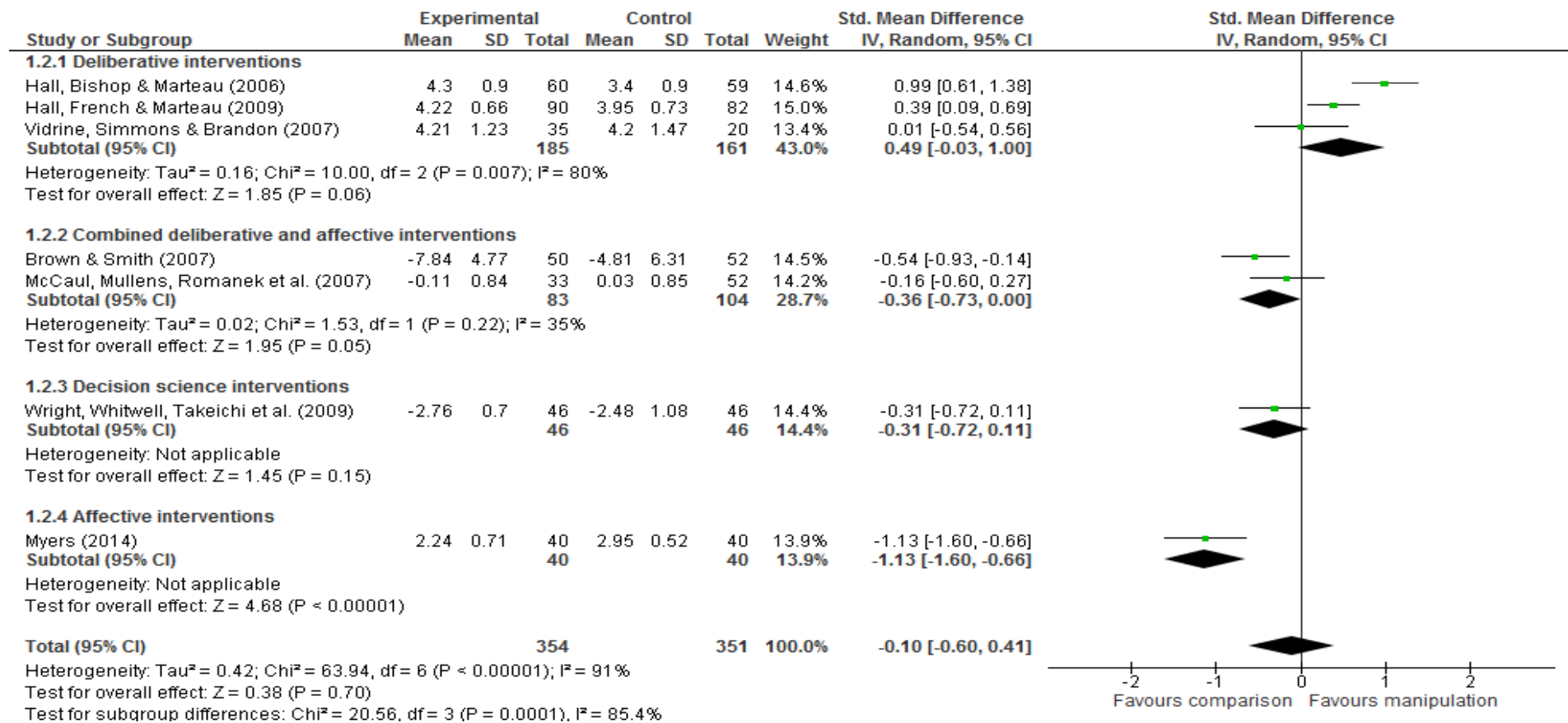


Figure 3. Overall efficacy of all manipulations at increasing smokers' comparative risk estimates.

3.5.3 Overall effects of deliberative manipulations on smokers risk perceptions

Comparison one: Immediate effects of deliberative manipulations on smokers' personal risk perceptions

Six out of the nine smoking studies that used deliberative manipulations and measured personal risks provided the necessary data to be included in this analysis. Across these studies there were seven separate comparisons of personal risk⁹. The overall effect of the deliberative manipulations on smokers perceived personal risk was significant, and produced a small- medium effect size ($k = 5$, $n = 637$; $SMD = 0.44$, 95 % CI [0.15 - 0.73], $z = 2.98$, $p = .003$); with participants receiving the deliberative manipulations reporting significantly higher personal risk estimates compared to the controls. However, there was significant moderate levels of heterogeneity between the results of these studies ($\chi^2 = 12.18$; $df = 4$; $p = 0.02$; $I^2 = 67$ %). Following a sensitivity analysis whereby the effects of the two outliers were removed (Magnan et al; 2009; Vidrine et al; 2007), heterogeneity was reduced to 0 % and was not significant ($\chi^2 = 0.00$; $df = 2$; $p = .53$), yet the overall effect of the model remained medium and significant ($k = 3$, $n = 463$; $SMD = 0.42$, 95 % CI [0.24, 0.61], $z = 4.46$, $p = < .001$).

The three remaining deliberative manipulations that could not be included in the meta-analysis (due to missing data) all reported no significant differences in personal risk estimates between experimental and control participants.

Comparison two: Immediate effects of deliberative manipulations on smokers comparative risk perceptions

Three deliberative studies reported full details of their results of comparative risk outcomes to be included in this meta-analysis. Among these studies, there were three separate analyses of comparative risks. Four studies failed to report adequate data to be included. Across included studies, the deliberative manipulations overall had a small-to medium effect on smokers comparative risk perceptions, with a trend towards higher comparative risk estimates among the deliberative group compared to controls which just failed to reach significance ($k = 3$, $n = 346$; SMD = 0.49, 95 % CI [- 0.03, 1.00], $z = 1.85$, $p = .06$). Again, there was high levels of significant statistical heterogeneity between the results of these trials ($\chi^2 = 10$; $df = 2$; $p = .007$; $I^2 = 80$ %). After the results of the one outlying non-significant effect (Vidrine et al; 2007) was removed from the model the overall effect became significant and large ($k = 2$, $n = 291$; SMD = 0.68, 95 % CI [- 0.08, 1.27, $Z = 2.24$], $p = .03$). However the overall heterogeneity remained high and significant ($\chi^2 = 5.66$; $df = 1$; $p = .01$; $I^2 = 83$ %). Only when the results of the outlier producing the largest significant overall effect (Hall et al; 2006) was removed from the model, heterogeneity reduced to small and non-significant levels ($\chi^2 = 1.41$; $df = 1$; $p = 0.24$; $I^2 = 29$ %), but this caused the effect of the overall model to become small and non-significant ($k = 2$, $n = 227$; SMD = 0.27, 95 % CI [- 0.07, 0.61, $z = 1.53$], $p = .12$).

From the four deliberative studies that could not be included in this meta-analysis there were a further three non-significant effects reported on this outcome immediately post intervention. One study reported significantly

higher comparative risks from their combined comparative risk scale (Westmaas et al; 2005). Another study resulted in significantly higher comparative risk amongst experimental participants compared to controls following the delivery of their deliberative manipulation on one measure (comparative risk of heart disease compared to other smokers) (Wright et al; 2006).

3.5.4 Overall effects of affective manipulations on smokers risk perceptions

Comparison one: Immediate effects of affective manipulations on smokers personal risk perceptions

It was not possible to conduct a meta-analysis for the overall effect of the affective manipulations on smokers perceptions of personal risk since only one study provided sufficient data for an effect size to be computed (Mc Donald et al; 2010). Mc Donald et al's. (2010) affective manipulation produced a small to medium, but not significant effect on personal risk estimates; with a trend towards higher perceived risk among the experimental group ($k = 1, n = 72$; $SMD = 0.41, 95\% CI [- 0.05, 0.88], z = 1.73, p = .08$).

The remaining three studies using affective manipulations that could not be included in this analysis reported three significant increases in personal risk estimates and one non- significant effect on this outcome post treatment.

Comparison two: Immediate effects of affective manipulations on smokers comparative risk perceptions

Only one study using an affective manipulation assessed comparative risks and therefore a meta-analysis of the overall effect of this manipulation type on this outcome could not be conducted. Myers (2014) found a large and significant effect of their affective manipulation on comparative risk perceptions ($k = 1$, $n = 80$; $SMD = -1.13$, 95% CI [-1.60, -0.66], $z = 4.68$, $p = < .001$). However, the results were not in the expected direction, with participants receiving the affective manipulation reporting significantly higher comparative optimism than controls.

3.5.5 Overall effects of social science based manipulations on smokers risk perceptions

Comparison one: Immediate effects of social science-based manipulations on smokers personal risk perceptions

Only one comparison was performed for the social science manipulations since neither study using these interventions measured comparative risks. Both of the social science based manipulations reported adequate data to be included in a meta-analysis, involving two separate comparisons of perceived risk. Tests of heterogeneity between these studies were not significant ($\chi^2 = 2.10$; $df = 1$; $p = .15$; $I^2 = 52\%$), Overall, the social science based manipulations produced a small non-significant effect on smoker's personal risks ($k = 2$, $n = 159$; $SMD = 0.36$, 95% CI [-0.10, 0.82], $z = 1.55$, $p = .12$); with no significant post manipulation differences found

between the personal risk estimates among the experimental and control groups.

3.5.6 Overall effects of decision science manipulations on smokers risk perceptions

Comparison one: Immediate effects of decision science based manipulations on smokers personal risk perceptions

All of the decision science based studies reported enough data to be combined in a meta-analysis, involving six comparisons of personal risks¹⁰. There was no significant statistical heterogeneity found between these studies ($\chi^2 = 0.07$; $df = 1$; $p = .79$; $I^2 = 0\%$). The effects of the decision science manipulations on personal risk outcomes immediately post treatment was small and not significant ($k = 2$, $n = 134$; $SMD = -0.13$, 95% CI [- 0.47, 0.21], $z = 0.75$, $p = .45$); overall there were no significant differences in the comparative risks estimates reported between experimental and controls participants directly after the manipulation.

Comparison two: Immediate effects of decision science based manipulations on smokers comparative risk perceptions

There was insufficient data from the decision science studies for a meta-analysis to be performed on comparative risk outcomes for smokers. The only decision science that reported enough data for an effect size to be computed (Wright et al; 2009) found a small and non-significant effect on this outcome ($k = 1$, $n = 92$; $SMD = -0.31$, 95% CI = [- 0.72, 0.11], $z = 1.45$, $p = .15$), indicating that estimates of comparative risk among experimental and control participants were similar after the experiment.

3.5.7 Overall effects of combined deliberative and affective manipulations on smokers risk perceptions

Comparison one: Immediate effects of combined manipulations on smokers personal risk perceptions

The two combined deliberative and affective interventions each reported all necessary data to be included in the meta-analysis. These studies consisted of two separate comparisons of personal risks. Tests of heterogeneity were not significant ($\chi^2 = 2.11$, $df = 1$; $p = .15$; $I^2 = 53\%$). The results revealed a small and non-significant effect of the combined interventions on smokers personal risk perceptions ($k = 2$, $n = 187$; $SMD = -0.39$; 95 % CI [- 0.82, 0.04], $z = 1.80$, $p = .07$), with a slight trend towards *lower* rather than higher personal risk estimates after the experiment in the experimental group compared to the controls.

Comparison two: Immediate effects of combined manipulations on smokers comparative risk perceptions

Both studies included measures of comparative risk and could therefore be subjected to meta-analytic procedures. From these studies two comparisons were included¹¹. Tests of heterogeneity were not significant ($\chi^2 = 1.53$; $df = 1$; $p = .22$; $I^2 = 35\%$). The results showed there was a small and significant effect of the combined affective and deliberative interventions on comparative risk outcomes ($k = 2$, $n = 187$; $SMD = -0.36$, 95 % CI [- 0.73, 0.00], $z = 1.95$, $p = .05$). However, in contrast to the studies intentions, the combined interventions produced significantly *lower* comparative vulnerability estimates.

4. DISCUSSION

This review attempted to synthesise all existing research on experimental manipulations and health care interventions designed to increase risk perceptions among drinkers and smokers; groups who remain arguably among the most important targets for these interventions given the high levels of risk these behaviours impose, yet who have typically been found to be highly defensive to risk information (e.g. Weinstein & Klein, 1995).

A total of 23 RCTS's aimed at increasing risk perceptions were eligible, consisting of 3,663 participants. There were twenty studies involving smokers ($N = 2,305$) and three involving alcohol users ($N = 257$). Across studies, the most frequently used manipulation categories were deliberative ($n = 10$), followed by affective ($n = 6$), social science-based types ($n = 3$), decision science-based types ($n = 2$) and combined deliberative and affective manipulations ($n = 2$).

The following discussion presents a summary of the main findings, separately for the drinking and smoking groups, together with methodological considerations and implications for research and practice.

4.1 Main findings for drinkers

This section has been kept deliberately brief relative to the discussion for smokers due to the small number of eligible drinking studies that were found.

Overall, the results showed deliberative and affective manipulations significantly increased drinkers' personal and comparative risks, respectively. The one study using a social science manipulation found no

significant effect on personal risk estimates. In addition, the only study using a deliberative manipulation that included longer term follow ups revealed no lasting effect on drinkers' perceived personal risks.

4.1.1 Overall completeness and generalizability of the evidence for drinkers

The current review was not able to determine the overall or individual efficacy of manipulations at increasing drinkers risk perceptions through a meta-analysis, due to the small number of clinically and methodically diverse trials that were eligible. Although some promising results were revealed for the deliberative and affective manipulation categories, the small number of studies these results were based on limits the robustness of these findings. In addition, important methodological issues among these trials, such as a failure of most of the studies to adequately describe their method of randomisation or to check for group equivalence, further limits the internal validity of their findings.

4.1.2 Implications for practice and research for drinkers

Due to the limited number of clinically and methodologically diverse alcohol studies included in this review, it is not possible to make clear recommendations about the specific manipulations to use to increase drinkers' risk perceptions. Although the deliberative and affective categories could be potentially useful strategies given the positive effects that were found, the small number of studies these findings were based on limits the ability to generalise these findings beyond these samples.

In general, this review has highlighted the need for more research from methodologically robust trials into the effects of all types of

manipulations on drinkers personal and comparative risk perceptions. A primary focus could be to examine the effect of the decision science or combined manipulations since none of the included studies used these manipulations. Future research would also benefit from including longer term follow ups to determine whether these manipulations are able to produce sustained changes in drinkers' perceptions of risk.

4.1.3 Overall conclusions for drinkers

In conclusion, more research is needed for all types of manipulations before strong claims about their ability to increase the risk perceptions of at-risk drinkers can be made.

4.2 Main findings for smokers

Thirteen out of the twenty smoking studies provided adequate data to perform a meta-analysis on post manipulation personal and comparative risk outcomes. One primary finding was an overall positive (yet small and non-significant) effect of all manipulation types combined on smokers' personal risks. In contrast, for the comparative outcomes, the manipulations overall appeared to have no effect.

The results of the meta-analysis also revealed the effects varied across the different categories of manipulations. With regards to personal risk estimates, only the deliberative category produced a significant positive, small to medium, effect on this outcome. Nevertheless, small-medium non-significant trends towards higher personal risk estimates among the experimental participants were also observed for the affective manipulation.

In contrast, the combined deliberative and affective manipulations led to non-significant trends towards *lower* personal risk estimates among the experimental groups. Neither the social science nor the decision science based manipulations were found to significantly affect smokers' perceptions of personal risks.

In terms of enhancing smokers comparative risks, overall, all of the manipulation types were ineffective. Although the affective and combined manipulations produced significant effects on this outcome, both of these manipulations resulted in significantly *higher* rather than lower comparative optimism among experimental participants. Neither the decision science nor deliberative manipulations produced any significant effects on comparative risk estimates; however the deliberative category was found to produce non-significant trends towards higher comparative vulnerability among experimental participants.

4.2.1 Overall completeness and generalizability of the evidence for smokers: Personal risk perceptions

It was possible to uncover the overall and individual efficacy of manipulations aimed at increasing smokers personal risk perceptions. However, missing data across certain types of manipulations (especially for the deliberative and affective interventions) may have impacted the observed effects. Lack of follow up data also meant we were unable to determine whether the manipulations were capable of producing longer term changes in smokers' risk perceptions. Despite this, overall, the results suggest that current efforts to enhance personal risk estimates among

smokers are largely ineffective, typically producing minimal and often mixed results ranging from small negative to small positive effects.

4.2.2 Completeness and generalizability of the results for each type of manipulation on smokers' personal risk perceptions

Deliberative manipulations and personal risk perceptions

Promising evidence was found to suggest that the deliberative manipulations are effective at increasing smokers' perceptions of personal risk. These findings are consistent with previous theoretical and empirical research which emphasises the importance of giving people information explicitly linking their unhealthy behaviour to specific health problems for them to believe they are susceptible to those consequences (e.g. Maddux & Rogers, 1983). However, the overall effect of the deliberative category on personal risks in the present review was small to medium. This coupled with the fact that three of the deliberative studies that were eligible but could not be included in the meta-analysis, all of which yielded non –significant results, means that the observed effect may have been artificially inflated. Furthermore, only two studies using this type of manipulation assessed and controlled for baseline levels of personal risks. This raises doubts about whether the effects were caused by the manipulation itself or were the result of pre-existing between group differences along this variable.

The significantly high levels of statistical heterogeneity between the deliberative studies further complicates interpretations about the overall effectiveness of this approach, as this highlights that the effects varied significantly between the different deliberative trials involving smokers. It is

noteworthy that from the two outliers that were found, the one non-significant effect came from the only study to include a purely student sample of smokers (Vidrine et al; 2007). In contrast, Magnan and colleagues (2009) study, which produced the largest positive effect, differed from the rest of the deliberative studies by exposing participants to written threats for a longer period (two weeks). Magnan and colleagues (2009) also assessed smokers' perceived risks of contracting any type of cancer, whereas the other deliberative studies, which all produced notably smaller individual effects, asked smokers to imagine their chance of developing specific smoking related illnesses (Hall et al; 2004; 2006; 2009).

Social science based manipulations and personal risk perceptions

The results of the current review also suggest that interventions based on theories from the social sciences are unlikely to be effective at increasing smokers personal risk perceptions. However given the lack of studies in this review that used this approach ($n = 2$), such a conclusion may be premature. Furthermore, the moderate statistical heterogeneity between these studies shows there was variability in their individual effects. In fact only the dissonance enhancing manipulation was found to produce a significant medium effect on its own (Simmons et al; 2004). This could be explained by the fact that the dissonance manipulation, involved a more 'intensive' application of their procedure by exposing participants to their written threats on three consecutive occasions (Simmons et al; 2004); whereas the self- affirmation study (which produced a small non-significant effect) presented smokers with their visual threats only once (Harris et al; 2007).

However, it should be noted that the self- affirmation study by Harris and colleagues (2007) did find a significant positive effect on other arguably related constructs, such as the personal relevance of the threatening images and negative thoughts and feelings about smoking. Therefore, given the small sample in their study ($n = 87$) and the large number of outcomes assessed ($n = 9$) they may have simply lacked power to detect additional significant effects on our outcome of interest.

Decision science based manipulations and personal risk perceptions

The present findings also indicate that the decision science based interventions are unlikely to have any effect on smokers' perceptions of personal risk. This apparent lack of effect could simply be due to confounding factors since neither of these studies checked for group equivalence on baseline characteristics. It could also be that experimental participants found the threatening scenarios they were given either unbelievable or difficult to vividly imagine since these studies also did not check the success of their manipulations. Alternatively, this approach may have been less successful at enhancing personal risk estimates given that the only other components they employed (e.g. supplementing the written threat information with graphical displays illustrating the numeric risks) were directed towards helping participants develop more accurate risk estimates, rather than encouraging them to imagine how these risks might personally apply to them. Regardless of these possibilities, the small number of studies this finding is based on means that these conclusions should be interpreted cautiously.

Affective manipulations and personal risk perceptions

The current review also found tentative evidence to suggest that affective manipulations may potentially be a useful strategy to enhance smokers personal risk perceptions, as the one study using this manipulation found it to have a small – medium positive (but non-significant) effect on this outcome. Although this is consistent with the majority of other affective studies in this review that could not be included in the meta-analysis due to missing data, it remains for future research to conclusively determine the overall effectiveness of this approach at enhancing personal risk perceptions among this group through a meta-analysis.

Combined deliberative and affective manipulations and personal risk perceptions

Finally, the present review found evidence to suggest the affective and deliberative manipulations combined are not effective at enhancing smokers' perceived personal risks, and might instead produce more harmful effects on this outcome. Such findings are consistent with a large body of evidence showing fear appeals in general, which attempt to change unhealthy beliefs and behaviours by using distressing graphic images depicting the severe negative consequences associated with risky behaviour, have traditionally not proven to be that effective at increasing perceptions of risk among at-risk individuals; and instead can have the unintended effect of making them more defensive against these messages (e.g. by denying the personal relevance of the presented threat; for a review see Ruiter, Abraham & Kok, 2001).

One explanation for these counterintuitive results might be due to fact that the combined manipulations included in the present review, which aimed to induce high levels of fear by including distressing graphic images alongside factual written information about the negative effects of smoking, did not provide specific recommendations about how participants could effectively deal with these threats. According to the extended parallel process model by Witte (1992), this would have left participants in a high state of distress which, in the absence of any adaptive solution to overcome the threat, may have caused them to resort to defensive coping strategies to reduce these unpleasant feelings (e.g. denying that the threat applied to them or minimising its personal significance).

However, it is worth noting that between the two studies that used combined manipulations, there was also moderate statistical heterogeneity in their results, with only Brown and Smith's (2007) study producing significant negative, and large effects, on smokers' personal risk estimates. In their study, experimental participants were exposed to distressing images and statements about the negative consequences of smoking in a single session. In contrast, Mc Caul and colleagues (2007) produced only a small negative, and non-significant, effect on smokers' personal risks after they exposed their experimental group to distressing images and negative statements four times a day over a week long period. Thus it could be that due to the high levels of fear the combined manipulations are likely to elicit, when no clear solution to reduce the threat is given, individuals need time to habituate to the distressing feelings before they can begin to consider how the threat might apply to them.

4.2.3 Overall completeness and generalizability of the evidence for smokers: comparative risk perceptions

With regards to comparative risks, in general these manipulations appeared to be less effective, with none of the manipulation categories being able to produce significantly higher comparative risks estimates among experimental smoking groups. However only eight out of the twenty-three included studies assessed this outcome, and from those that did there was substantial missing data, making it difficult to draw any firm conclusions about the average effect of these manipulations along this outcome. Furthermore, since none of these studies followed up changes in smokers comparative risk perceptions over time it was not possible to ascertain whether these manipulations were able to produce sustained changes on this outcome.

4.2.4 Completeness and generalizability of the results for each type of manipulation on smokers' comparative risk perceptions

Deliberative manipulations and comparative risk perceptions

Considering each category in turn, the most promising results were again found from the deliberative manipulations, which showed positive trends in the intended direction. However, a large proportion of deliberative studies that could not be included in the meta-analysis due to missing data produced non-significant effects on this outcome ($n = 4$). In addition, the significant statistical heterogeneity found across the deliberative trials on comparative risk outcomes makes it hard to ascertain the average effect of this approach on smokers' comparative risk perceptions.

Decision-science based manipulations and comparative risk perceptions

The finding that the decision science category had no significant positive effects on smokers' comparative risks estimates may reflect problems in the effectiveness of the manipulation itself or could be due to confounding factors. More problematically is the fact that this finding came from only one study which severely limits any conclusions that can be drawn.

Combined and Affective manipulations and comparative risk perceptions

In contrast, both the combined affective and deliberative, and the affective manipulation alone, appeared to make smokers more comparatively optimistic. Such reactions might again be explained by the fact that none of these studies gave participants detailed information about how they could reduce their risks of experiencing the severe negative consequences they were shown would happen to them if they continued smoking. However, these findings should also be treated with caution given the small number of purely student samples they are based on. This is particularly the case for the affective category since the negative result came from only one study which did not provide details regarding group equivalence on smoking related variables (Myers, 2014). They also showed that certain factors, such as smoking history, had significant negative effects on two out of the four comparative risk outcomes they assessed (perceived comparative risk of developing bronchitis or stroke in the future). As a result, it cannot be certain that the affective intervention itself, rather than other

factors, was responsible for causing higher comparative optimism in this study.

Furthermore, for the combined intervention types, the results of trials included in the analysis of comparative risks varied to a moderate degree. As was the case regarding the effects on personal risks, the only study to find a significant medium negative effect on comparative risks came from Brown and Smith (2007). In this instance, it is important to note that the higher comparative optimism they found among experimental participants was primarily the result of the lower levels of personal risks produced by their manipulation. Together this raises doubts about whether the combined types do in fact lead to greater comparative optimism.

Social science based manipulations and comparative risk perceptions

Finally, for social science based manipulations, the overall usefulness of this type of manipulation at increasing smoker's comparative risks remains uncertain as none of the studies using this approach assessed this outcome.

4.2.5 Quality of the evidence for smokers

Heterogeneity issues

Moderate to high levels of statistical heterogeneity were revealed in the analysis of all but one of the manipulation types involving more than one trial (all except for the decision science category). This was dealt with in the present review by using a random effects model which assumes that different studies measured different, yet related, effects, and incorporates

the within and between study variance to produce wider confidence intervals.

Only the deliberative category had enough studies to be able to explore the possible causes of this variance, by performing a sensitivity analysis on the outlying results. More sophisticated methods for addressing issues of heterogeneity include subgroup analysis or meta-regression; however neither of these procedures were possible for any of the manipulation types included in this review since there were not enough studies within each category to compare on different clinical characteristics.

Important clinical and methodological differences were also apparent across all of the manipulation categories among the smoking group. For example, although all of the deliberative trials involved exposing smokers to written threats, notable differences were found between their manipulation lengths, samples, control groups and the content and timing of their outcome assessments.

The combined interventions, despite using similar manipulations and samples, also differed in the length they exposed participants to their manipulations, as well as in the timing of outcome assessments, use of control groups and the way in which risk perceptions were assessed (e.g. conditional vs not conditional estimates).

With regards to the social science based manipulations, although both trials included student samples and assessed risk perceptions immediately after their manipulation, they each used different control groups and outcome measurements (conditional vs unconditional). Most problematically, the distinct theories they were based on produced widely different manipulations in terms of their overall content and length.

Finally, even though the decision science based manipulation trials were the most homogenous among the included studies in terms of their results and overall approach, they both used different samples and assessed smokers' perceived risks for arguably different outcomes (e.g. risk of lung disease conditional on having the null type GSTM1 gene vs risk of Crohn's disease conditional on continued smoking).

Together, this diversity between the included trials could be taken to suggest that the smoking studies may not have been comparable enough to combine using a meta-analysis. In particular, the utility of aggregating the results of the different types of manipulations together seems questionable due to the variety of strategies used between trials and the considerable lack of consistency in the magnitude and direction of their individual effects ($I^2 = 77%$ for personal risks; $I^2 = 90%$ for comparative risks).

However, since studies using the same type of manipulation at least showed consistency in terms of the direction of their effects, and were similar in either their overall approach or theoretical background, it seemed plausible that their individual results could be meaningfully summarised using a meta-analysis.

Methodological limitations of the smoking studies

Important methodological weaknesses were also found among the smoking studies included in this review. For example, across studies there was a consistent lack of reporting of key methodological procedures, including randomisation and allocation concealment. As such, bias in the selection of participants could have been possible. Almost half of all the trials included in this review ($n = 9$) also did not statistically check for group

equivalence on key demographic variables (e.g. age, smoking history). Together, these methodological issues raise concerns about the internal validity of some of the findings. Selective reporting was also an issue in many trials. However this risk of bias was controlled for in the current review since none of these studies were included in the meta-analysis.

Problems with the measurement of risk perceptions were also noted. For example, despite recommendations laid out by many researchers (e.g. Shepperd et al; 2013; Weinstein, 1998) most of the trials did not include a range of strategies to assess smokers risk perceptions, with over a third of the included studies focusing solely on personal risks and a small minority incorporating both verbal and numeric scales into these assessments. This raises concerns about the overall reliability and validity of these measurements. Of most concern, was the fact that a large proportion of these studies did not condition their risk perception questions on participants current or continued smoking ($n = 7$). This may have undermined the effectiveness of the manipulations since participants would have been able to base their estimates of risk on any intentions to engage in healthier behaviours in the future (e.g. plans to give up smoking), or they may have taken other protective factors into account when deciding upon their personal level of risk (e.g. healthy eating practices) (e.g. Weinstein, Rothman & Nicolich, 1998).

For the comparative outcomes, the majority of studies did not assess this via the recommended indirect approach (e.g. Covey & Davies, 2004) and as such may have failed to adequately tap into smokers beliefs about the risks of comparable peers, meaning that any scores from these measures may have simply reflected beliefs about their personal risks

(Covey & Davies, 2004). Furthermore, none of these studies specified the precise comparison group along all of the domains that have been suggested to be important (e.g. gender, age and location; Shepperd et al. 2013). Together this makes it hard to determine the type of people participants were comparing their perceived risks against. None of these studies gave participants a specific time frame to base their perception of risk on, which creates further difficulty in making sense of these responses. Furthermore, the lack of follow up data among these studies severely limits the ability to determine the long term effectiveness of any of the manipulations on these outcomes.

Despite these methodological weaknesses, all of the smoking studies reported significant and non- significant findings, and as such reporting bias in this respect was considered to be low. Importantly, a majority of these studies assessed the credibility of their manipulations ($n = 12$) which adds further strength to their findings.

Another strength was the fact that most of the smoking studies included multiple questions in their risk assessments, thereby overcoming some of the aforementioned reliability and validity issues. The large majority of smoking trials also relied on absolute verbal Likert style questions rather than numerical risk estimates which have been criticised on the basis that they require a higher level of numeracy in order to be interpreted and answered accurately (e.g. Shepperd et al; 2013; Weinstein, 1998), and even then responses to these questions remain difficult to interpret (selecting 50 % risk of lung cancer could mean they believe the chance is exactly 50 % or that they believe this may or may not happen; Weinstein, 1998). In addition, all except one study kept their personal and comparative risk outcomes

separate in their analysis, which meant it was possible to assess the unique effect of manipulations on these qualitatively distinct outcomes.

With regards to the overall strength of the findings for smokers, the most robust findings came from the deliberative category of interventions since they were based on the largest number of studies and total participants ($n = 637$ for personal risks, $n = 346$ for comparative risks). However the large amount of missing data on both outcomes alongside the aforementioned methodical issues discussed limits the overall confidence in these findings. Furthermore, the statistical heterogeneity that was found for both outcomes among the deliberative category raises doubts about the accuracy of the average effect sizes that were found. This is particularly the case for the comparative outcomes since the overall medium positive trend that was found was caused largely by the results of one study that produced a large positive effect. When this outlier was removed, the overall effect of the deliberative category on comparative risks became smaller with a greater likelihood of being due to chance (SMD = 0.27, $p = .12$). Encouragingly, for personal risk perceptions, taking the outliers out of the deliberative model completely removed the statistical heterogeneity that was present without impacting the overall effect on personal risks, which remained significant and positive, albeit small (SMD = 0.42). This suggests that the significant effect found for the deliberative category on smokers' personal risks was not artificially exaggerated by the results of one influential case and provides greater confidence in the reliability of the positive, yet small to medium, effect that was found on this outcome. Despite these findings, it was not considered appropriate to permanently remove any of the

outlying studies on the basis of their differing results as this would have introduced bias (Deeks, Higgins & Altman, 2011).

In general, the observed effects of manipulations on comparative risks would be considered the least robust since very few of the included studies measured this outcome, and from those that did, the sample sizes were small, ranging from a total of 80 to 346 participants. This raises the issue that the results may have been due to chance.

4.2.6 Implications for practice for smokers

Overall, the results of the present meta-analysis provide tentative support for the use of deliberative manipulations, which involve providing clear information highlighting the health risks associated with smoking, to enhance smokers' perceptions that they are personally susceptible to those risks. Such efforts could easily be implemented into routine clinical practice by taking a few minutes at the end of a scheduled health visit to discuss with smokers their increased risks of contracting serious health problems if they continue (whilst briefly outlining the key mechanisms involved); and emphasising how stopping smoking can effectively reduce their risks (as in Hall et al; 2009). Such an approach would also be consistent with Nice guidance (2006) that explicitly recommends all smokers who attend a GP consultation should be routinely provided with discussions about quitting. Alternatively the present findings also suggest that simply sending smokers leaflets containing evidence highlighting the health problems associated with smoking (as in Hall et al; 2006), and checking during their next health appointment that they received and understood this information, may also be

an effective and potentially more feasible way in which to encourage smokers' to accept their increased health risks.

The results of this meta-analysis also provide preliminary evidence to suggest that increasing smokers' fears about their health risks, either by showing them the severe consequences of long term use through visual images, or asking them to imagine health problems actually happening to them as a result of their smoking, is not more effective than simply providing them with factual information about the risks. This is consistent with previous research that has found graphic and written threats to be equally persuasive (De Hoog, Stroebe & De Wit, 2007). In addition, our findings tentatively indicate that combining written warnings with graphic images illustrating the severe health consequences should be avoided in routine practice or public health campaigns until their (potential negative) effects are further investigated.

4.2.7 Implications for research for smokers

The current review highlights the need for more research in a number of important areas.

In general more research is needed to conclusively determine the overall effectiveness of the social, decision, affective and combined types on personal and comparative risk estimates. This research should overcome the methodological issues raised in the present review (e.g. ensure adequate randomisation and allocation concealment) and undertake long term follow up assessments to identify whether the effects are long lasting.

Issues with heterogeneity also need to be addressed. In particular it would be interesting for future research to directly compare the effects of

manipulations using different methodological procedures (e.g. intensive vs brief interventions) demographic groups (e.g. student vs community) and outcomes (e.g. specific vs general risks estimates, personal vs comparative risk perceptions, conditional vs un-conditional risk questions) to highlight possible moderators of the effects.

Perhaps most importantly, it remains necessary for future studies to determine whether or not the effects of these categories of manipulations on risks perceptions lead to meaningful changes in smokers actual behaviour given that very few studies identified in this review assessed these outcomes.

Another important avenue for future research should be into the development of reliable and valid scales to measure personal and comparative risk perceptions, to overcome the inconsistencies in measurement approaches found in the current research and achieve greater confidence in the reported findings.

4.2.8 Conclusions for smokers

Overall the present review found evidence to suggest that deliberative interventions are currently among the most effective strategies available in terms of helping smokers accept the personal risks this behaviour is likely to pose towards their health. In particular, our results support the use of providing information about the health risks associated with smoking in order to enhance smokers perceived personal risks; an approach that can be implemented relatively easily in routine practice. However the robustness of this conclusion is limited by the methodological and heterogeneity issues found across these trials, together with the fact

that the overall effect was small to medium at best, and only reached significance for the personal risk estimates assessed.

It was not possible to clearly determine the overall efficacy of the other types of manipulations included in this review due to the small number of clinically diverse studies that were eligible. However, preliminary evidence was found to suggest that for certain types of interventions (e.g. combined) the effects on these outcomes may be more harmful than beneficial among smokers. It remains for further research to conclusively determine the reliability of these findings.

Overall the review highlighted a general need for more research across all types of manipulations, starting with the affective types since these were the least researched among the included trials for both outcomes. Other recommended avenues for future research include an examination of the long term effects of these interventions and their impact on actual behaviour from methodologically robust trials. Potential moderators of the effects should be explored in studies directly comparing different groups, risk outcomes and intervention methods.

4.3 Agreement with other reviews

Together the findings of the present review for both groups are largely consistent with the recent meta-analysis conducted by Portnoy et al. (2014), who showed an overall positive (and significant) effect on risk perceptions involving the same categories of manipulations. In addition, among the different manipulation types they also found the deliberative category to be the most effective at enhancing risk perceptions. Furthermore, they reported no significant effect of the social science or

decision science based manipulations overall on risk perceptions. Together this adds strength to the reliability of the present results.

4.4 Limitations of the review

Certain issues with how this review was conducted may have biased the results that were found. For example, due to the *apriori* exclusion criteria the results were biased towards English written trials. In addition, non- peer reviewed unpublished studies were also not included based on the rationale that only studies with high methodological quality would be eligible for this review. This, together with the fact that there was substantial missing data (especially for the deliberative and affective categories) further reduces the overall strength of the observed effects that were found.

Further bias in the selection of studies for this review may have also been introduced due to the fact that this process was carried out by only one reviewer, and as a result, potentially eligible studies may have been missed out.

Despite these limitations, the current review was guided by recommendations set out in the Cochrane guidelines and followed clearly specified inclusion and exclusion criteria, which would have reduced bias in the selection of studies. Ideally the review would have involved more than one coder to ensure agreement on how study categories and methodological quality was determined.

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Footnotes

¹One of the included smoking studies measured risk perceptions using a scale in which higher scores reflected lower comparative risk (e.g. Wright et al; 2009). Another smoking study measured comparative optimism rather than comparative risks (Brown & Smith 2007). Therefore for these outcomes, mean scores were converted to negative numbers to ensure that lower, rather than higher numbers, would reflect a positive effect of the manipulation.

²In Vidrine and colleagues (2007) study it was not possible to combine all three comparative risk measures that were used since data on one measure (risk specific to men) was not provided for the control group. Instead, for this meta-analysis we chose to include the means from the most common of the three comparative measures they used: specific relative risk compared to other smokers.

³This applied only to Wright and colleagues (2009) study who investigated the effects of both high vs low risk messages and dispersed vs grouped risk displays on smokers risk perceptions; among high and low numeracy groups. As we assumed that group sizes were equal since the authors did not report otherwise, for the purposes of this meta-analysis we combined the average effect of the two highest conditions (50 % risk & dispersed display) and compared this to the average effect of the two lowest

conditions (3% risk & grouped display) collapsed across the low and high numeracy participants.

⁴The Cochrane risk of bias tool also considers blinding of participants' as part of the overall assessment of performance bias. However given that it is widely acknowledged that blinding participants to certain types of manipulations, such as psychological interventions, is almost impossible, the risk of bias in this area was not assessed. Instead risk of performance bias was based solely on whether studies had taken appropriate steps to ensure experimenters remained blind to participants' condition.

⁵It was not possible to assess detection bias from the included studies since they all relied on self-reported outcome assessments and therefore blinding in this area would not have been achievable.

⁶For the purposes of the meta-analysis when group size had not been reported these numbers were estimated by dividing the total number of participants by the number of conditions.

⁷Three of the smoking studies also assessed perceptions of risk conditional on quitting smoking (McBride et al; 2000; Wright et al; 2006; Wright et al; 2009). However since these questions reflect beliefs about the effectiveness of performing a healthy behaviour and should therefore yield

lower risk estimates these outcomes were not considered relevant to the aims of the current review.

⁸ In Shepperd et al's. (2013) study they also measured participants' risk perceptions associated with the presence of the GSTM1 wild type gene variant which they had previously told participants' decreases the risk of lung cancer. Again this outcome was not included in the meta-analysis since this review is exclusively concerned with the effects of manipulations aimed at increasing participants risk perceptions.

⁹The comparison of absolute % personal risk estimates between the experimental and control groups from Vidrine et al's. (2007) study could not be included in the meta-analysis since it was measured on a different scale from the other two personal risk measures they used (absolute general and absolute specific risk).

¹⁰Only one of the comparisons of personal risks from Shepperd et al's. (2013) study could be included in the meta-analysis, namely that of imagined gene status and display type (foreground vs no display for imagined GSTM1 null type). This was because they did not report the means and standard deviations for the other experimental factor relevant to this review (absolute vs incremental risk for imagined GSMT1 null type).

¹¹In McCaul et al's. (2007) combined deliberative and affective manipulation study the authors did not provide separate data for their personal and comparative risk questions and instead combined scores on these measures to create an average risk composite (consisting of personal risks (absolute % and verbal risk estimates) and comparative risks (1 verbal risk question)) . As a result, caution is required in interpreting this overall result since it conflates personal and comparative risk estimates and because using the same data twice (to estimate the average effect sizes for the combined manipulation types on personal and comparative risk perceptions) may have produced an overweighting of the overall effect of this manipulation type.

Part 2: Empirical Paper

The impact of a 'looming vulnerability' manipulation on perceptions of risk,
intentions and behaviour among harmful drinkers

ABSTRACT

Background: There is widespread evidence highlighting the harmful effects of drinking over the government recommended limits, yet many drinkers refuse to accept they are personally at risk of experiencing negative consequences from drinking, and continue to drink at harmful levels.

Aims: To explore whether inducing a sense of 'looming vulnerability' towards the threat of liver disease among harmful drinkers increases perceptions of risk for this disease, manifesting in greater motivation and intentions to reduce as well as lasting reductions in drinking over a one week period.

Method: Thirty-eight harmful drinkers were randomised to receive a 'looming' imagery manipulation or a matched control imagery condition. Immediate post- test measures were taken of risk perceptions, motivation, intentions, drinking behaviour and self- efficacy. At the one week follow up, risk perception measures were repeated and drinking consumption was assessed.

Results: Participants who received the looming manipulation reported significantly greater levels of anxiety and intentions immediately after the experiment. No between group differences in risk perceptions or in short term drinking rates were observed.

Conclusions: It is tentatively concluded that a looming vulnerability manipulation can be a powerful method to enhance intentions to drink within safer limits. It is recommended that larger scale studies, addressing some of the studies limitations, are conducted to determine whether these effects can be replicated. Other avenues for further research are discussed.

1. INTRODUCTION

1.1 Background

There is extensive research highlighting the negative impact of alcohol use on a range of physical, financial, social and legal outcomes (World Health Organisation, 2007). Despite this, the overall prevalence of harmful drinking remains high. Recent estimates have reported that over 10 million adults in England regularly drink more than the recommended daily alcohol limit (National Audit Office, 2008).

Given the widespread problems associated with excessive alcohol consumption considerable effort has been made to reduce the levels of harmful drinking (e.g. Alcohol Health Alliance, 2013). Much of this work has come from government initiatives designed to increase awareness of the problems associated with alcohol through the use of mass media campaigns and school education programmes (e.g. Alcohol Health Alliance, 2013; Department of Health, 2015). However, despite increasing awareness of the risks and promoting healthier attitudes towards drinking, the overall success of these approaches at producing actual reductions in harmful drinking has tended to be only minimal (for reviews see Anderson, Chisholm & Fuhr, 2009; Babor et al; 2010; Wakefield, Loken & Hornik, 2010).

1.2 The role of perceived susceptibility to harms

Numerous theories have been put forward to explain why people continue to engage in harmful drinking despite knowledge of the risks (for a review see Armitage & Conner, 2000). According to social- and health psychology theory, preventive or protective action requires not only an

awareness that severe risks exist, but also the belief that the individual is personally susceptible to those risks (e.g. Janz & Becker, 1984; Maddux & Rogers, 1983; Rogers, 1975; Rosenstock, 1966; Schwarzer & Luszczynska, 2008). More specifically, threat appraisal is proposed to be influenced by a combination of perceived susceptibility, perceived severity and fear (Rogers, 1975).

1.3 The negative impact of optimistic biases among drinkers

There is substantial evidence to support modification of perceived susceptibility in promoting healthy behavioural intentions and actions, across a range of health domains (e.g. Floyd, Prentice-Dunn & Rogers, 2000; Janz & Becker, 1984). However, a main challenge faced by these interventions is that they must overcome the 'optimism bias' found among harmful drinkers (e.g. Weinstein, 1984; Wild, Hinson, Cunningham & Bacchioni, 2001). Specifically, at-risk drinkers have consistently been found to minimize their personal risk of experiencing negative outcomes as a result of their drinking (e.g. Hansen, Raynor & Wolkenstein, 1991; Weinstein, 1984) and to view themselves as having a lower risk of suffering the harmful short (e.g. falling down) and long term (e.g. developing liver disease) effects of drinking than comparable peers (Hansen et al; 1991; Weinstein, 1984; Wild et al; 2001). Together, these biased risk appraisals tend to counteract perceptions of personal susceptibility to harmful outcomes and may render interventions less effective (e.g. Weinstein & Klein, 1995).

1.4 Interventions to develop accurate risk perceptions among at risk groups

To overcome drinkers' inaccurate beliefs about their personal susceptibility to the risks it has been suggested that interventions need to strongly emphasise the link between behaviour and vulnerability, rather than simply pointing out that severe risks exist (e.g. Weinstein, 1984). Recent studies that have successfully achieved this have involved asking drinkers to vividly imagine negative events happening to them as a result of their drinking (e.g. Ayers & Myers, 2011). For example, in Ayers and Myers (2011) study, regular student drinkers were asked to watch a short anti-drinking scenario depicting a serious accident caused by alcohol use (e.g. a man climbing and subsequently falling off some scaffolding whilst under the influence of alcohol and landing on the floor lying in a pool of his own blood). Before viewing the clip, one group was asked to imagine the event being personally relevant, another was asked only to watch the clip and the control group just completed the same measures without having watched the film. All participants were asked to rate their level of risk for a range of alcohol related negative events compared to that of the average student of the same age and gender with similar drinking habits to them. The results showed that the 'imagine' group rated themselves as being at significantly greater risk of having an accident than their peers, compared to participants in both the watch condition and the controls. However neither the watch nor imagine conditions were able to produce any significant effects on their perceived risk of experiencing more severe long term health conditions (e.g. liver disease).

A key issue therefore remains regarding how to support at risk drinkers to accept the serious longer term health consequences of drinking.

One potentially promising approach could be to encourage individuals to imagine their personal risk to be increasing in a very particular way: Namely for risk to 'loom,' approaching the individual rapidly in time or space (McDonald, O'Brien, Farr & Haaga, 2010). For example, McDonald and colleagues (2010) used imagery to induce looming vulnerability in current smokers to the dangers of smoking. This was based on the looming vulnerability model of anxiety developed by Riskind and colleagues (Riskind, 1997; Riskind, 1999; Riskind, Williams & Joiner, 2006) that argues threatening events are not experienced by the individual as stationary events thought to occur at an unspecified time in the future, but are instead perceived as "...rapidly rising in risk as they approach through time or space..." (Riskind et al; 2006; p.78). The looming vulnerability model further predicts that threats that are perceived to be growing or moving closer will lead to higher and more sustained levels of fear and anxiety that is harder to minimise or ignore (Riskind, 1997, 1999), as well as an increased sense of personal vulnerability and greater urgency to protect oneself against the impending threat (Riskind et al; 2006); compared to threats presented in a purely static form.

By conceptualising and manipulating susceptibility in this way, McDonald and colleagues (2010) showed small to medium (although not significant) effects on increasing smokers perceptions of the health risks associated with smoking, as well as on other important variables including contemplating quitting and intrinsic motivation to quit smoking; with many of these positive effects lasting over a one month period (McDonald et al; 2010). Most encouragingly, this intervention significantly reduced the amount of self -reported smoking at the one month follow up (McDonald et al; 2010).

To date, however, no other studies have been conducted to examine the effect of this intervention on other health behaviours, including heavy drinking.

1.5 The present study

The present study attempted to develop this area of research by examining the immediate and short term (one week) impact of inducing a sense of looming vulnerability, using a pilot guided imagery manipulation, in individuals currently drinking at hazardous and harmful levels.

We were primarily interested in assessing the effect of a looming threat manipulation on drinkers' perceived risk of developing one specific serious health condition widely known to be caused by long term harmful alcohol consumption, namely liver disease. The range of risk perceptions measured was also extended to include estimates of personal and comparative risk. Previous research has typically examined the effect of imagery manipulations on either personal (e.g. McDonald et al; 2010) or comparative (Ayers & Myers, 2011) risks. However, given that both types of risk perceptions have been shown to be biased among drinkers (e.g. Hansen et al; 1991), and since each of these biased risk appraisals have been linked to health related actions (e.g. for personal risks see Brewer et al; 2007; for comparative risks see Dillard, Midboe & Klein, 2009) it is important to establish whether the same manipulation can positively influence both of these distinct, yet related, constructs simultaneously.

In addition to these effects, we were also interested in exploring how the manipulation might impact other important variables, such as threat related anxiety, drinking intentions; motivation; self –efficacy and behaviour.

To further add to this research, in addition to assessing the immediate effect of this manipulation on drinking rates measured objectively via a 'taste-test' ostensibly unrelated to the experiment's aims; the effect on short term drinking (at one week) was also assessed.

1.5.1 Hypotheses

Based on the preliminary findings of McDonald and colleagues (2010) it was hypothesised that following the experiment, compared to controls, individuals who received the looming threat manipulation would report:

1. *Perceived risks*: Greater perceived personal and comparative risks towards developing liver disease in the future immediately after the experiment and at a one week follow up.
2. *Anxiety, motivation, intentions and self-efficacy*: Immediately higher levels of anxiety induced by the threatening manipulation, and greater intentions and motivation to drink within safer limits. However no between group differences in levels of self-efficacy towards reducing drinking were expected. To reduce the time demands placed on participants and minimise attrition rates none of these secondary outcomes were assessed at the follow up and therefore no hypotheses for the longer term effect of the manipulation on these outcomes could be made.

3. *Drinking*: Immediately lower drinking rates measured objectively via a taste test and reductions in self-reported drinking levels measured at the one week follow-up.

2. Method

2.1 Design

The present study used a mixed 2x2 randomised controlled design. The between group variable was the experimental manipulation (looming vulnerability guided imagery vs a matched control guided imagery task). The repeated measures variable was time of assessment (immediately post manipulation vs one week follow up). The experimental looming and control conditions were compared on the following dependant variables (a) imagery anxiety and vividness; (b), personal risk; (c) comparative risk; (d) intentions to drink within safer recommended limits; (e) motivation to make changes to alcohol use; (f) self-efficacy to reduce alcohol use; (g) immediate drinking rates and (h) short term (one week) drinking rates.

2.2 Participants

Inclusion criteria for recruitment were being: (1) Aged 18 or over, (2) able to understand spoken and written English and (3) reporting hazardous level of drinking on the AUDIT- C (scoring 4 + for men, or 3 + for women).

Exclusion criteria were: (1) currently receiving treatment to reduce drinking, (3) currently receiving treatment for a chronic physical or mental health condition and/ or (4) recent/current involvement in other alcohol-related projects.

Participants were randomly allocated to receive either the looming vulnerability manipulation or the control condition through the use of an online random number table generator (Urbaniack & Plous, 2013).

2.3 Sample size and power

Power analysis for the present study was informed by prior research by Mc Donald et al. (2010). In their study, they used the Heath Risk subscale of the Smoking Consequences Questionnaire (Copeland, Brandon & Quinn, 1995) to assess between group differences in risk perception and found an effect size of $d = .42$ (Cohen's $d =$ small-medium). Power calculations for a repeated- measures between-factors ANOVA were subsequently carried out using G*Power 3.1.2 (Faul, Erdfelder, Lang & Buchner, 2007), specifying $\alpha = .05$ and desired power at 0.80. This estimated a required sample size of 82 (41 per group) to detect a medium effect, or a sample of 34 (12 per group) to detect a large effect from a between group comparison involving two groups, with two levels of measurement. The achieved sample size was 38 (23 experimental, 15 controls) indicating the study was likely powered to detect only large effects.

2.4 Procedure

2.4.1 Participant recruitment

This project was approved by the UCL Research Ethics Committee (Appendices 3 and 4). Potentially eligible participants were recruited via advertisements (see Appendix 5) placed in a range of UCL buildings (e.g. UCL library, UCL student union) as well as on various online sites including: (a) the Call for section of the UCL research newsletter, (b) *Sona* systems

and (c) social media (e.g. Facebook). Emails of the study advertisement were also sent out from the course administrators from different departments across UCL.

Participants who emailed their interest in taking part were emailed an information sheet explaining the study (Appendix 6) as well as a questionnaire link of questions to assess their eligibility (Appendix 7).

2.4.2 Testing procedure

In session experiment

Participants who completed the screening questions and were found to be eligible were given an appointment time (via email) to attend the in session experiment. Individual sessions took place between 5-10 pm on weekdays (one participant was tested at 1pm) and lasted approximately 30 minutes to 1 hour per participant.

Prior to testing participants were given the information sheet to remind them what the study entailed and provided written informed consent (Appendix 8). Data was then collected on their current drinking and drinking history and current anxiety and depression levels. Participants were then randomly assigned to receive either the experimental or control recordings, based on a number provided from a previously generated random number table.

Before hearing the recordings participants were given written instructions (on a computer screen placed in front of them which also played the recordings) to close their eyes and try to vividly imagine the scenarios

they were given. For all participants, the recordings began with the same practice imagery recording to help orient them to the task. This was followed by four further recordings presented one after the other (after a brief 5 second delay) read by a female voice. Participants were instructed to press the space bar on the computer once they had finished each recording to move onto the next one. Each of the experimental looming and control scenarios lasted for approximately 1.5 minutes and were matched as far as possible for length and sensory features, but differed in their content.

After hearing the imaginary scenarios, participants completed measures of their immediate imagery related anxiety and vividness, personal and comparative risk estimates, self- efficacy, intentions and motivation to make changes to their drinking. For the final part of the in session experiment participants took part in a taste test to assess immediate alcoholic drinking rates. Once all post -test assessments were completed participants were paid £7 for their time and were provided with referral sources for reducing/quitting drinking if requested, but were not actively encouraged to make changes to their drinking. A time to conduct the follow up assessment was then arranged.

One week follow up

Participants were contacted via email at the prearranged date one week after experiment to collect follow up data on risk perceptions (personal and comparative), drinking rates (units consumed over the week) and the acceptability of the recordings (experimental group only). At the end of this phase a full debrief about the purpose of the experiment was provided.

Reminder emails were sent out to participants who did not reply within a day of sending out the follow up email.

2.4.3 Apparatus

For the Timeline follow back procedure participants were given a printed out unit guide (Appendix 9) alongside instructions on how to complete the task presented using PowerPoint slideshow (Microsoft) on a laptop computer (Appendix 10). During this task participants were also given a printed out A4 calendar grid with the dates marked for them to write in their retrospective estimated units of alcohol consumed.

The imagery recordings were played through headphones. The instructions (Appendix 11) were presented on a computer screen.

For the post experiment in session taste test precise volumes of each drink before and after the test were measured using a measuring cylinder. A breathalyser was also used in the event that participants consumed all of the 200 ml alcohol to ensure they were not intoxicated prior to leaving the session (BAC < 0.05%).

Both the screening and follow up assessments were collected from questionnaires developed using Opinio software (version 7.0) sent via email links.

2.4.4 Imagery scenarios

Looming vulnerability imagery scenarios

Participants in the experimental condition were given four imaginary scenarios which all referred to the act of drinking and explicitly related this to symptoms (e.g. pain in the abdomen) associated with liver disease. In all of

these scenarios participants were guided to imagine the threat of liver disease growing closer in time or space, and were modelled on similar instructions used by McDonald et al. (2010). At the end of each scenario participants were told they could control the threat (e.g. slow down its progression) by reducing their drinking, but that only by consistently drinking within the safe recommended limits would they eliminate this threat altogether (see Appendix 12). The four experimental scenarios were presented in the following order:

- (1) Conveyer belt: In this scenario, participants are asked to imagine being in a dimly lit factory holding their usual alcoholic drink. Whilst being in this room they find themselves on a conveyer belt that moves faster the more they drink. The scenario goes on to describe how as they continue to drink the conveyer belt speeds up and they become aware that the diagnosis of liver disease awaits them at the end. At this point, they are asked to imagine pains in their abdomen.
- (2) Office building with calendar pages: In the second imaginary scenario participants were placed in an office on their own, watching the pages of a calendar fly off the wall whilst drinking their usual alcoholic drink. As they drink more, and time progresses, participants are asked to imagine developing symptoms of liver disease.
- (3) Doctor approaching: In the third imaginary scenario participants are asked to imagine being in a hospital drinking their usual alcoholic drink. As they drink more, they are told to imagine a doctor approaching at an

increasing speed and realise they are about to be diagnosed with liver disease.

- (4) Changing appearance (jaundice): The final scenario is set in a bar. Whilst drinking their usual alcoholic drink, participants are asked to imagine their skin becoming increasingly yellow and are asked to realise this is a symptom of liver disease caused by their harmful drinking.

Control imagery scenarios

Participants in the control condition were also given four imaginary scenarios which contained elements of movement in time and /or space but made no reference to drinking alcohol or its negative consequences (see Appendix 12). These scenarios were presented in the following order:

- (1) Escalator (matched control version of 'conveyer belt'): In the first control scenario, participants are placed in a mall and are asked to imagine being on an escalator which gradually takes them up to the second floor.
- (2) Office building with magazine pages (matched control version of 'office building with calendar pages'): The second control scenario puts participants in an empty office reading a magazine. They are asked to imagine calendar pages flying off a calendar on the wall and time progressing as they continue to flip through the pages of the magazine.
- (3) Postman approaching (matched control version of 'doctor approaching'): In the third scenario, control participants are placed in a quiet high street

early in the morning. They are told to imagine a postman coming towards them at increasing speed as they are drinking a bottle of water.

- (4) Changing appearing (shadows) (control version of changing appearance (jaundice): In the final control scenario, participants are asked to imagine lying down in a park and watching shadows gradually cover their body as time passes.

These imagery scenarios were created by requesting and subsequently adapting the content from two of the scenarios used in McDonald and colleagues (2010) study (e.g. experimental and control scenarios 1 and 2) to be relevant for drinkers. Two new additional scenarios were also created (experimental and control scenarios 3 and 4) to achieve greater similarity between our imaginary tasks (e.g. to ensure all four experimental scenarios directed participants to think about the link between their drinking and the growing threat of developing liver disease). Feedback was sought regarding the appropriateness of all scenarios for our study aims from a leading expert in the field (J. H. Riskind, personal communication, July 2014). A pilot test was also conducted on three volunteers who took part in the full experiment and gave feedback at the end. This confirmed that the scenarios were vivid enough and the experimental versions were experienced as threatening.

2.5 Measures

2.5.1 Screening pre- test measure

As part of the screening process participants were asked about the following areas:

Demographics: age, gender, ethnicity, education level and employment status (5 items; Appendix 7).

Current treatment: whether they were currently receiving treatment to reduce their drinking, or for a physical or mental health condition (2 items; Appendix 7).

The AUDIT Alcohol Consumption Questions (AUDIT-C; Bush, Kivlahan, McDonell, Fihn, & Bradley, 1998): The AUDIT-C is a brief 3 item questionnaire that was used to screen participants as hazardous or harmful drinkers. It includes questions about the frequency and quantity of a person's alcohol consumption with 5 response options that are awarded from 0-5 points; with higher scores reflecting greater likelihood of harmful drinking or the presence of an alcohol use disorder. The full measure is scored on a scale of 0-12. Scores of 4 or more for men, or 3 or more for women indicate hazardous drinking levels or the presence of an alcohol use disorder. The AUDIT- C is a modified version of the full scale 10 item Alcohol Use Disorders Identification Test (AUDIT; Saunders, Aasland, Babor, de la Fuente & Grant, 1993) and has been found to have good sensitivity and specificity (Bush et al; 1998).

2.5.2 Pre- test measures

The Alcohol Use Disorders Identification Test (AUDIT; Saunders et al; 2001): The full audit was used to obtain more detailed information to confirm participants as hazardous or harmful drinkers. It consists of 10 items covering the areas of alcohol consumption, drinking behaviour, and alcohol related problems. Each item is scored from 0-4 points, with higher scores reflecting greater likelihood of hazardous or harmful drinking. A score of 8 or more is considered to indicate hazardous or harmful alcohol use. The AUDIT has been shown to have good validity and reliability (Babor, Higgins-Biddle, Saunders & Monteiro, 2001).

Other drinking related questions: Single item questions were used to determine the age participants started drinking, their length of continuous alcohol use since having their first alcoholic drink, the length of their current average use, previous reduction and/or quit attempts and the amount of time these lasted (7 items; Appendix 13).

The Hospital Anxiety and Depression Scale (HADS; Zigmond & Snaith, 1983): The HADS was used to screen participants' current levels of anxiety and depression as part of a risk assessment to ensure we did not include people who may have become distressed by the manipulation. The HADS is a 14-item self-report measure designed to briefly assess mood problems among the general adult population. It is comprised of 2 subscales, one measuring depression (7 items) and one measuring anxiety (7 items). The total range of scores from each subscale is from 0-21. Individual items are rated on 4-point Likert scales ranging from 0-3, with

higher scores reflecting greater symptom severity. Scores of 0-7 indicate normal levels of anxiety or depression, scores of 8-10 indicate borderline abnormal levels of anxiety or depression and scores of 11-21 suggest abnormal levels of anxiety and depression. The HADS has been demonstrated to have good reliability and validity (Zigmond & Snaith, 1983) and normative data is available to aid the interpretation of scores from non-clinical adult samples (Crawford, Henry, Crombie & Taylor, 2001).

Timeline Follow Back (TFL; Sobell & Sobell, 1992): Current drinking was assessed using the timeline follow back procedure. In this task, participants are presented with a calendar on which a target interval is selected for them to retrospectively estimate their daily alcohol use. In the present study, participants were asked to recall their daily alcohol use over the 7 days directly prior to coming into the in session experiment. To help make these estimates participants were given a unit guide along with instructions to aid their recall. Specifically, following the procedure used by Sobell and colleagues (e.g. Sobell, Sobell, Klajner, Pavan & Basian, 1986) participants were instructed to first mark down on the calendar any events in the previous week that stood out (e.g. birthdays). They were then told to write on the calendar the amount of alcoholic units they drank on those dates as well as on the days preceding and following the memorable events. If they were unable to accurately recall the exact date that they drank, they were encouraged to make their best guess. After completing the calendar, participants were asked whether the completed 7 day interval was representative of their drinking behaviour over the last year. If it was not, they were asked to complete a second calendar representing their drinking during a typical week over the past year. This procedure has been found to

have high test-retest reliability among non-clinical student drinkers (Sobell et al; 1986) and when administered via computer, rather than face to face (Sobell, Brown, Leo & Sobell, 1996).

2.5.3 Post-test measures

Subjective experience of the imagery recordings: The Imagery Response Form (Morissette, Palfai, Gulliver, Spiegel & Barlow, 2005) was used as a manipulation check. It is comprised of two items: one assesses the extent to which participants felt anxious whilst listening to the recordings. The second item assesses how vividly participants imagined the scenarios by asking how much they felt a part of them. In this study participants made their responses by marking a cross on a 100 mm horizontal line anchored by the phrase “not at all” on the left (0) to “completely” (100) on the right (Appendix 14).

Personal risk: A single question was used to assess participants’ perceived vulnerability to the targeted health threat which was developed based on recommendations from previous research (e.g. conditioning their estimates on their drinking, Van der Pligt, 1998). Participants were asked: “To what extent do you believe that you would be personally at risk of developing liver disease in the future because of your drinking?”. Responses were rated on a 7 point Likert scale from 1 (“strongly disagree”) to 7 (“strongly agree”). Similar wording has been used by other researcher’s (e.g. Wild et al; 2001).

Comparative risk: was assessed using the indirect method advocated by other researchers as it explicitly focuses participants' attention towards the comparison target when making their risk estimate (e.g. Covey & Davies, 2004). Participants were asked "To what extent do you believe that some other person your age and gender who drinks the way you do would be at risk of developing liver disease in the future?" Responses were rated on a 7 point Likert scale ranging from 1 ("strongly disagree") to 7 ("strongly agree"). The precise wording was based on recommendations in the risk perception literature (e.g. specifying the target group by age and gender as recommended in Shepperd, Klein, Waters & Weinstein, 2013). Similar wording has also been used in previous research (e.g. Wild et al; 2001).

Self-efficacy: Participants' confidence in their ability to engage in safer drinking was also assessed using a single item used in previous research (Murgraff, White & Phillips, 1999). Participants were asked to rate their level of agreement (on a 7 point Likert scale from 1 ("strongly disagree") to 7 ("strongly agree")) how strongly they agreed or disagreed with the statement "I am capable of starting and continuing drinking at safe levels".

Intentions: to drink within safe recommended limits was measured by a single item used in previous research (Murgraff et al; 1999). Participants were asked to rate their level of agreement on the same 7 point Likert scale how much they agreed or disagreed with the following statement: "From now on I intend to drink within safe levels as a regular habit".

Readiness to Change Questionnaire (RCQ; Rollnick, Heather, Gold & Hall, 1992): The RCQ is a 12-item scale that was used to assess the stage of change participants were in. It is comprised of 3 scales each consisting of 4 items that correspond to the pre-contemplation, contemplation, preparation and action stages of change from the Transtheoretical model developed by Prochaska and DiClemente (1984). Participants were asked to rate how strongly they disagreed or agreed with each item on a 5 point Likert scale ranging from - 2 ("strongly disagree") to + 2 ("strongly agree"). Total scores for each scale ranged from - 8 to + 8. Higher positive scores reflected stronger agreement with the items. In this study we chose to use the refined scoring system outlined by Heather and Rollnick (1993) to be able to distinguish participants' who were in the preparation stage of change from those in contemplation or action stages. Using this method, participants were classified into one of the four stages of change based on the pattern of positive and negative signs of the scores they had obtained for each scale (see Heather & Rollnick, 1993 for the exact scoring procedure). The RCQ has been tested to have robust psychometric properties (Heather & Rollnick, 1993).

Taste test: Participants' immediate alcoholic drinking rates were assessed through the use of a taste test, using the same procedure outlined by Field and Eastwood (2005). Participants were simultaneously presented with 200 ml of chilled beer (Carlsberg brand, 3.8 % alcohol by volume (ABV)) and 200 ml chilled orange juice (Tesco own brand). They were instructed to consume as much or as little of each beverage as they wanted and were informed that once they had finished tasting each drink, their task was to make value judgements about each beverage along four continuums

(unpleasant-pleasant, tasteless-strong tasting, bitter-sweet, flat-gassy) by marking a line on a 100 mm visual analogue scale provided (Appendix 15). However the only dependant variable of interest in the current study was the amount of alcohol consumed in each drink, which was used as an objective measure of their immediate drinking behaviour (and was measured after participants had left the experiment).

2.5.4 Follow up measures

At the one week follow up, participants were asked to complete the following measures via email:

Alcoholic units consumed over the previous week: was assessed using the same timeline follow back procedure used previously, however on this occasion participants were emailed the calendars and instructions.

Personal and comparative risks: as before

Acceptability of recordings: Participants were finally asked “ How acceptable would you rate the recordings used in this experiment as part of a treatment to help people reduce their drinking.” Responses were rated on a 7 point scale from 1 (“highly acceptable”) to 7 (“highly unacceptable”).

2.6 Data scoring and coding

Single item Likert and questionnaire data: Data from the single item and questionnaire Likert scale measures were scored as continuous variables (except for the motivational data which was scored as categorical data). For the comparative risk measure, scores were computed as the

difference between participants' personal risk ratings subtracted from their other risk ratings (Covey & Davies, 2004). Larger, more positive difference scores on this measure reflected participants' beliefs about having a higher risk than comparable peers (or comparative pessimism), whereas lower or negative scores reflected comparative optimism.

Self-reported current drinking: For the timeline follow back procedure only the summed total units participants reported consuming in the previous weeks were used as a continuous measure of current drinking.

Objective drinking rates: participants' immediate drinking rates assessed via the taste test were computed by subtracting the volume of beer that was left from the original volume they were given.

Visual analogue data: Participants' scores on the imagery vividness and anxiety visual analogue scales were computed as the number of millimetres from the 0 point on the left that they made their cross.

2.7 Data analysis

2.7.1 Data preparation

Data was analysed using the Statistical Package for Social Sciences (SPSS) version 22.0. Prior to analysis, scores from all continuous outcomes were screened (separately for the control and experimental groups) to ensure they met parametric assumptions. First, potential outliers were detected by visually inspecting the histograms for each outcome and computing z scores for any extreme scores that were found. This revealed only one outlying case (defined as any score > 3 standard deviations from

the group mean or $z > 3$) on the immediate comparative risk outcome for the experimental group only. This score was subsequently replaced with the mean immediate comparative risk score (experimental group) + 3 *SD* based on recommendations provided by Field (2005).

Assumptions of normality were checked for all continuous outcomes (for each group separately) via visual inspections of their respective histograms and an analysis of their skewness and kurtosis values. All data from the control group was found to be normally distributed. However for the experimental group, scores on the comparative risk outcomes at follow up were non-normal with significant skewness ($z = -3.29, p < .001$) and kurtosis values ($z = 4.01, p < .001$). Scores from the timeline follow back (TFL) units of alcohol consumed at baseline and follow up from the experimental group also showed significant skewness (TFL time 1: $z = 2.78, p < .01$; TFL time 2: $z = 2.70, p < .01$). Between group residuals for these outcomes were also non-normal (follow up comparative risk z skewness = $-3.61, p < .01$; z kurtosis = $5.26, p < .001$; TFL time 1 z skewness = $2.54, p < .01$; TFL time 2 z skewness = $2.57, p < .01$). Based on these findings, transformations were applied to this data. Specifically, a square root transformation was performed to correct the negatively skewed follow up comparative risk data. As this outcome was to be compared to the immediate comparative risk scores, this data were transformed in the same way to ensure both outcomes had the same units of measurement (Field, 2005). This involved firstly reflecting these scores (by multiplying each score by -1) and then making them positive (to be able to apply the square root transformation) by adding the largest score on this outcome to all of the scores. Overall, this transformation was successful at removing the negative skewness ($z = 2.49, ns$), without

adversely affecting the immediate comparative risk distribution ($z = 2.14$, *ns*). For the timeline follow back time 1 and 2 data, the positive skewness was also effectively removed after the log transformation (TFL time 1: $z = -1.41$, *ns*; TFL time 2: $z = -2.16$, *ns*).

2.7.2 Statistical analysis

Data were analysed in the following stages:

1. Preliminary analyses:

Randomization check: Separate independent *t*-tests and chi-square tests (for categorical data) were performed to ensure the randomisation procedure was successful at creating groups that were equivalent on key demographic and drinking related variables.

Imagery induced anxiety and vividness: Independent *t*-tests were performed separately on the imagery anxiety and vividness data from each group to confirm that both groups found their respective scenarios to be equally vivid, but that the experimental group experienced higher levels of anxiety from their scenarios. Effect sizes were calculated to assess the magnitude of the effects using Cohen's *d* (looming mean - control mean / pooled *SD*) whereby .2 = small, .5 = medium, .8 = large by convention (Cohen, 1992).

2. Primary outcomes

Personal and comparative risk outcomes: Separate mixed model repeated measures ANOVA'S were conducted to determine whether there were any significant differences between experimental and control

participants' personal and comparative risk estimates at the two time points assessed. Effect sizes (eta squared) were calculated (SS between or SS within / SS total; Cohen, 1973) to determine the magnitude of these effects whereby .02 = small, .13 = medium, .26 = large by convention (Cohen, 1988).

3. Secondary outcomes

Intentions, self-efficacy and immediate drinking rates: A series of independent samples *t*-tests (with Cohen's *d* effect sizes) were conducted to assess for any significant differences between experimental and control groups on these outcomes.

Motivation: A chi-square test was planned to assess whether there was a significant association between group condition and stage of motivation. Cramer's *v* was used to test the strength of this association, whereby .10 = small, .30 = medium and .50 = large (Cohen, 1988).

Short term drinking rates: A mixed model ANOVA (with eta squared effect sizes) was conducted to determine whether there were significant differences in the amount of alcohol consumed between the two groups from the week prior to the week following the experiment.

4. Additional analysis

Relationship between imagery related anxiety, vividness and outcomes: A series of bivariate parametric and non-parametric (for the categorical motivation data) correlations were performed (on the experimental group data only) to assess whether each of these variables were related.

Mediation analysis: For significant outcomes, potential mediators were assessed through a series of regression analysis following recommendations by Baron and Kenny (1986). This approach highlights that four conditions have to be met for mediation to have occurred. Firstly the Independent variable (IV) has to predict the mediating variable (MV) (path a); secondly the IV should predict the dependant variable (DV) (path c); thirdly the MV should also predict the DV (path b), and finally the IV should no longer significantly predict the DV when the effects of the potential mediator are controlled (path c'). The Sobell (1982) test was used to assess the significance of indirect effects (via an online calculator developed by Preacher & Leonardelli, 2001).

Acceptability of the manipulation: For the experimental group, the acceptability of the manipulation as part of an intervention tool to help at risk drinker's reduce their drinking was also assessed by exploring the range and overall mean of scores for this outcome.

Due to the exploratory nature of this pilot study no Bonferroni corrections were applied to any significant outcomes.

3. Results

3.1 Recruitment and attrition

A total of 38 participants were recruited who all met full inclusion criteria. Twenty-three participants were randomised into the experimental condition and 15 were randomly assigned to receive the control condition (the imbalance in numbers between groups was due to the fact that participant allocation was based on pre-generated random numbers set for

our desired sample size of 75). There was no missing data for any baseline measures or outcomes assessed immediately after the manipulation. Seven scores from the motivation data had to be excluded from analysis since they provided inconsistent or contradictory responses and could not be correctly classified into one of the four stages of motivation using the procedure outlined by Heather and Rollnick (1993).

Rates of attrition at the one week follow up were minimal and equal across the two groups ($n = 3$ in both groups). An analysis conducted on the missing data for the follow up measures of personal risk, comparative risk and drinking units revealed no systematic differences in the patterns of missing data between groups (Little's MCAR chi square test: $\chi^2(18) = 14.01$, $p = .73$). Scores for this missing data were subsequently estimated from relevant predictor variables (e.g. personal and comparative risk perceptions immediately post -test, baseline weekly units consumed, full audit, years of current drinking). Since differences between the actual values and estimated means created for these missing scores were only minimal, these estimated scores were considered appropriate to be imputed to replace the missing data.

3.2 Participant characteristics

Basic demographic and drinking related characteristics of this sample have been presented separately for the experimental and control groups in Tables 1 and 2, respectively.

3.3 Baseline equivalence of groups

As can be seen from the baseline equivalence tests presented in Table 1, there were no significant between- group differences in any of the demographic variables assessed, indicating that the randomisation procedure was successful at producing equivalent groups in these areas. However, Table 2 shows that audit scores were significantly higher in the control group compared to the experimental group. These scores were subsequently correlated against all of the outcomes which showed only personal risk perceptions and alcoholic units consumed (at both time points for both outcomes) to be significantly positively correlated with audit scores (all r 's $< .5$). However, adding audit scores as a covariate in all subsequent analyses for these variables did not change the main effects of group on these outcomes. As such, the results are presented without controlling for these differences.

Table 1
Participant demographics

| Demographic Variable | Experimental condition (n = 23) | Control condition (n = 15) | Statistic | p value |
|---------------------------------|---------------------------------|-------------------------------|--------------------|---------|
| Age in years: <i>M (SD)</i> | 23.7 (6.1) Range: 18-40 | 25.9 (5.2) Range: 18-34 | $t(36) = -1.14$ | .26 |
| Gender: N (%) | | | | |
| Female | 9 (39.1%) | 3 (20.0%) | $\chi^2(1) = 1.53$ | .21 |
| Male | 14 (60.9%) | 12 (80%) | | |
| Ethnicity: N (%) | | | | |
| White British | 11(47.8%) | 12 (80.0%) | $\chi^2(5) = 6.17$ | .19 |
| Asian | 3 (13.0%) | 2 (13.3%) | | |
| Black | 0 (0.0%) | 0 (0.0%) | | |
| Mixed | 2 (8.7%) | 1 (6.7%) | | |
| Chinese or other | 4 (17.4%) | 0 (0.0%) | | |
| White other | 3 (13.0%) | 0 (0.0%) | | |
| Highest qualification: N (%) | | | | |
| GCSE | 1 (4.3%) | 0 (0.0%) | $\chi^2(4) = 8.88$ | .06 |
| Alevel | 12 (52.2%) | 6 (40.0%) | | |
| Degree | 8 (34.8%) | 2 (13.3%) | | |
| Masters | 1 (4.3%) | 6 (40.0%) | | |
| Further postgraduate | 1 (4.3%) | 1 (6.7%) | | |
| Employment status: N (%) | | | | |
| Full time undergraduate student | 13 (56.5%) | 6 (40.0%) | $\chi^2(3) = 2.98$ | .39 |
| Full time postgraduate student | 7 (30.4%) | 4 (26.7%) | | |
| Full time employed | 3 (13) | 4 (26.7%) | | |
| Part-time student & employed | 0 (0.0%) | 1 (6.7%) | | |

Note. All *t*-tests were two-tailed.

Table 2
Drinking related variables

| Drinking variable | Experimental condition (<i>n</i> = 23) | Control condition (<i>n</i> = 15) | Statistic | <i>p</i> value |
|--|--|---------------------------------------|-----------------------|----------------|
| Age first started drinking: <i>M</i> (<i>SD</i>) | 15.3 (2.2) Range: 7-19 | 15.2 (1.9) Range: 12-19 | <i>t</i> (36)= 0.08 | .93 |
| Years drank: <i>M</i> (<i>SD</i>) | 8.4 (6.3) Range: 1-25 | 10.3 (4.4) Range: 3.5-17.0 | <i>t</i> (36) = -0.98 | .33 |
| Full Audit: <i>M</i> (<i>SD</i>) | 12.1 (4.2) Range: 7-27 | 15.40 (5.2) Range: 5 - 26 | <i>t</i> (36) = -2.11 | .04* |
| Years drank current units: <i>M</i> (<i>SD</i>) | 3.2 (3.5) Range: 0.33-11.00 | 4.4 (4.6) Range: 0.2 - 16.0 | <i>t</i> (36) = -0.87 | .39 |
| Number of Quit attempts: <i>M</i> (<i>SD</i>) | 0.4 (1.2) Range: 0-5 times | 0 | <i>t</i> (36)= 1.68 | .10 |
| Length of longest quit attempt (days): <i>M</i> (<i>SD</i>) | 9.9 (44.3) Range: 0-213.08 | N/A | <i>t</i> (36) = 0.86 | .40 |
| Number of reduction attempts (days): <i>M</i> (<i>SD</i>) | 1.7 (2.3) Range: 0-10 | 0.8 (1.2) Range: 0 - 4 | <i>t</i> (36)= 1.44 | .16 |
| Length of longest reduction attempt (days): <i>M</i> (<i>SD</i>) | 82.3 (252.1) Range: 0 -1216.76 | 39.4 (94.8) Range: 0 - 365.24 | <i>t</i> (36) = 0.62 | .53 |

Note. All tests were two-tailed, * = $p < .05$.

3.4 Effects of the looming manipulation on anxiety and vividness ratings

3.4.1 *Hypothesis for Imagery related anxiety and vividness:* We predicted that participants exposed to the looming manipulation would report higher levels of anxiety compared to the controls, but there would be no differences between the two groups in the vividness ratings obtained from their respective imagery scenarios.

As can be seen in Table 3, the manipulation had a large effect on anxiety and vividness ratings. As expected, experimental participants reported significantly

higher levels of anxiety immediately following the manipulation than controls. However, against our predictions the results showed significantly higher vividness ratings among the control group compared to the experimental group.

Table 3
Post manipulation imagery related anxiety and vividness ratings

| Outcome | Experimental (<i>n</i> = 23) | Control (<i>n</i> = 15) | <i>t</i> (36) | <i>p</i> | <i>d</i> |
|---|----------------------------------|-----------------------------|---------------|----------|----------|
| Imagery related anxiety: <i>M</i> (<i>SD</i>) | 42.82 (28.86) | 16.40 (27.33) | 2.817 | .008** | .94 |
| Imagery related vividness: <i>M</i> (<i>SD</i>) | 46.35 (29.05) | 69.33 (14.59) | - 3.223 | .003** | 1 |

Note. *d* = Cohen's *d* effect size measure whereby .2 = small, .5 = medium, .8 = large (Cohen, 1992). ** = *p* < .01.

3.5 Effects of the looming vulnerability manipulation on personal and comparative risk perceptions

3.5.1 *Hypotheses for personal and comparative risk perceptions:* We hypothesised that compared to controls, participants exposed to the looming manipulation would report greater perceived personal and comparative risks towards developing liver disease in the future immediately after the manipulation, which would remain at the one week follow up.

Table 4 provides the means and standard deviations of the personal and comparative risk estimates from both groups across the two time points.

Personal risks

Contrary to our predictions, there was no significant main effect of group, ($F(1, 36) = 0.38, p = .54, \eta^2 = .01$), indicating that across the two time

points combined experimental and control participants' average personal risk ratings were similar. There was no significant main effect of time ($F(1, 36) = 0.33, p = .57, \eta^2 = .00$), indicating that regardless of group, average personal risk ratings were also similar immediately post-test and at follow up. More importantly, the time X group interaction was not significant ($F(1, 36) = 0.87, p = .36, ns, \eta^2 = .02$), highlighting that changes in average personal risk ratings over time were similar between the experimental and control groups.

Comparative risks

Against our predictions there was no significant main effect of group, ($F(1, 36) = 0.54, p = .46, \eta^2 = .02$), indicating that experimental and control participants' average ratings of comparative risk did not differ significantly across the two time points combined. There was a significant main effect of time ($F(1, 36) = 227.90, p < .001, \eta^2 = .86$), which produced a large effect size. However contrary to our predictions, the results showed that in both groups, comparative risk ratings significantly decreased immediately post-test to the one week follow up. More importantly there was no significant group X time interaction, indicating that the decreases in comparative risk ratings in the experimental and control groups were on average the same ($F(1, 36) = 0.18, p = .67, \eta^2 = .00$).

Table 4
Descriptive data for risk perception outcomes

| Group | Perceived risk measure | Time of assessment | |
|----------------------------------|------------------------|--------------------------------------|-----------------------------------|
| | | Immediate post-test <i>M (SD)</i> | 1 week follow up <i>M (SD)</i> |
| Experimental (<i>n</i> = 23) | Personal risk | 4.52 (1.53) | 4.26 (1.64) |
| | Comparative risk | - 0.40 (0.73) | - 0.44 (0.82) |
| Control (<i>n</i> = 15) | Personal risk | 4.67 (1.59) | 4.73 (1.45) |
| | Comparative risk | 0.00 (0.65) | - 0.19 (0.53) |

Note. Means and standard deviations for comparative risk outcomes from both groups are based on the untransformed data. For clarity, transformed data has been included alongside this original data in Appendix 16. Missing data for the one week follow up personal risk scores consisted of: 2 control group scores and three experimental group scores. Missing data for the one week follow up comparative risk scores consisted of: 2 control group scores and 3 experimental group scores.

3.6 Immediate effects of the looming vulnerability manipulation on secondary outcomes.

3.6.1 *Hypotheses for motivation and intentions:* We hypothesised that participants who received the looming manipulation would report immediately greater motivation and intentions to drink within safer limits, compared to controls.

The motivation data violated the assumption that expected frequencies should be > 5 (75 % cells had expected counts < 5) (Field, 2005), thus Fishers exact test was performed instead to assess whether there was a significant association between group condition and stage of motivation. As can be seen in Table 5, there was a medium to large, but

non-significant, association between group condition and stage of motivation, with more experimental participants being in the preparation and action stages after the experiment compared to controls. Intentions to drink within safer limits were significantly higher among the experimental group compared to the controls following the experiment, with the manipulation found to produce a medium-to large effect on this outcome. Thus the hypotheses for intentions and motivation were partially supported.

3.6.2 *Hypothesis for self-efficacy:* We predicted there would be no difference in levels of self -efficacy between experimental and control participants immediately after the experiment.

The results confirmed this prediction, as Table 5 shows self-efficacy scores among the experimental group were slightly, but not significantly, higher than controls directly after the experiment.

3.6.3 *Hypothesis for immediate drinking rates:* We predicted that participants who received the looming manipulation would drink less alcohol immediately after the experiment as measured objectively via a taste test

This hypothesis was not supported. As can be seen in Table 5, although the experimental group on average consumed 20 ml less beer than controls directly after the manipulation, this difference was statistically non-significant and the overall effect size was small. The amount of orange juice consumed by both groups was more clearly equivalent.

Table 5
Descriptive and significance data for immediate post-manipulation secondary outcomes

| Outcome | Experimental (<i>n</i> = 23) | Control (<i>n</i> = 15) | Statistic | Significance | Effect size |
|-----------------------------------|----------------------------------|-----------------------------|---------------|--------------|-------------|
| | N (%) | N (%) | <i>FET</i> | <i>p</i> | <i>v</i> |
| Stage of Motivation : | | | | | |
| Pre-contemplation | 5 (25.0) | 3 (27.3) | 5.05 | .17 | .40 |
| Contemplation | 5 (25.0) | 6 (54.5) | | | |
| Preparation | 6 (30.0) | 0 (0.0) | | | |
| Action | 4 (20.0) | 2 (18.2) | | | |
| | <i>M (SD)</i> | <i>M (SD)</i> | <i>t</i> (36) | <i>p</i> | <i>d</i> |
| Intentions | 4.30 (1.55) | 3.13 (1.46) | 2.33 | .02* | .77 |
| self-efficacy | 5.43 (1.16) | 5.33 (1.40) | 0.243 | .81 | .08 |
| Immediate drinking rates (beer) | 74.52 (71.71) | 94.53 (79.91) | - 0.804 | .43 | .26 |
| Immediate drinking rates (orange) | 86.81 (66.64) | 87.53 (66.06) | - 0.03 | .97 | .01 |

Note. All *t*-tests were two-tailed. *v* = Cramer's *v*. whereby .10 = small, .30 = medium, .50 = large (Cohen, 1988). *d* = Cohen's *d* effect size measure whereby .2 = small, .5 = medium, .8 = large (Cohen, 1992). * = *p* < .05.

3.7 Longer term effects of the looming vulnerability manipulation on drinking behaviour

3.7.1 Hypothesis for Follow up drinking rates: We hypothesised that participants exposed to the looming threat manipulation would report significantly greater reductions in drinking at the one week follow up compared to controls.

This prediction was not supported. The results showed there was no significant main effect of group ($F(1, 36) = 2.70, p = .10, \eta^2 = .07$). As can be seen in Table 6, average weekly units consumed by experimental and control participants were similar across the two time points. The main effect of time was also not significant ($F(1, 36) = 1.182, p = .28, \eta^2 = .03$), showing that across both groups the amount of average weekly units consumed did not differ to a significant degree from baseline to the 1 week follow up. The time X group interaction was also not significant ($F(1, 36) = 0.17, p = 0.70, \eta^2 = .00$), highlighting that the observed changes in the average units consumed in both groups were also similar.

Table 6
Descriptive data for drinking rates post-manipulation and at one week follow up

| Group | Time of assessment | |
|---------------------------|-------------------------------------|--------------------------------------|
| | Baseline units of alcohol M (SD) | Follow up units of alcohol M (SD) |
| Experimental ($n = 23$) | 26.28 (16.04) | 27.08 (20.79) |
| Control ($n = 15$) | 36.21 (19.22) | 34.13 (17.80) |

Note. Means and standard deviations for units consumed from both groups are based on the untransformed data. For clarity, transformed data for units consumed has been presented in Appendix 17 alongside the original data. Missing data for the one week follow up consisted of: 3 scores missing from controls, 3 from the experimental group.

3.8 The relationship between imagery anxiety, vividness and study outcomes

Due to the unexpected between-group differences in vividness ratings alongside the non-significant effects of the manipulation on the many of our outcomes a series of bivariate parametric and non-parametric (for the categorical motivation data) correlations were performed (on the

experimental group data only) to assess whether vividness and anxiety ratings were related to the outcomes in the way we originally expected.

3.8.1 *Hypothesised relationships between imagery related anxiety and vividness and the primary and secondary outcomes among the experimental group:*

Based on previous theoretical arguments (e.g. Riskind, 1997; 1999; Riskind et al; 2006; Rogers, 1975) we anticipated that imagery related anxiety and vividness ratings would be positively correlated with each other, and with personal and comparative risk ratings; and may also be positively related with intentions and motivations. In contrast, drawing on the rationale that being able to more vividly imagine a threatening event gaining momentum and coming closer and experiencing greater anxiety as a result should activate protective actions (Riskind et al; 2006), we expected vividness ratings would be negatively correlated with drinking rates assessed immediately post- test and at the one week follow up. The results below show that these hypotheses were only partially supported.

Imagery-related anxiety

As can be seen in Table 7, imagery related anxiety and vividness were positively and modestly correlated among the experimental group highlighting that as expected, the more vividly participants could imagine the threatening scenarios, the more anxiety they experienced.

However, unexpectedly, imagery related anxiety was found to be unrelated to personal risks, and negatively correlated with comparative risks measured immediately post- test; suggesting that those who felt more

anxious after imagining the threatening scenarios rated other people's risks as higher than their own.

Despite this, imagery- related anxiety had a strong positive relationship with intentions, indicating that participants who felt more anxious after being exposed to the threatening scenarios had greater intentions to drink within safe limits. The results also showed imagery anxiety to be moderately correlated with motivations, which just fell short of significance ($p = .09$), suggesting the more anxiety participants felt from the scenarios, the greater their motivations were to make changes to their drinking. As predicted, imagery anxiety was also negatively correlated with immediate drinking rates, suggesting that those who experienced greater anxiety from the scenarios drank less alcohol immediately afterwards; but this relationship also failed to reach significance ($p = .08$). However as can be seen in Table 7, imagery related anxiety was found to be unrelated to subsequent drinking rates measured at follow up and was not significantly associated with any of the other study variables.

Imagery-related vividness

In keeping with our predictions, there was a significant moderate positive correlation between vividness ratings and personal risks estimates assessed at both time points among the experimental group. This fits with our hypothesis that participants who could more vividly imagine the threatening scenarios rated their personal risks as higher. In addition, we found vividness and motivation to be significantly positively modestly, correlated, suggesting the more vividly experimental participants could imagine these scenarios the more motivated they were to make changes to

their drinking directly afterwards. However, against our predictions, vividness ratings were moderately positively correlated with drinking rates assessed at the one week follow up, highlighting that the more vividly experimental participants could imagine the scenarios during the experiment, the more alcohol they consumed during the week following the experiment. No other significant relationships were observed (see Table 7).

Table 7
Correlations between imagery related anxiety, vividness and study outcomes

| Outcome | Imagery related anxiety | Imagery related vividness |
|---------------------------|-------------------------|---------------------------|
| Imagery related anxiety | 1 | .45* |
| Imagery related vividness | .45* | 1 |
| Personal risk time 1 | .26 | .57** |
| Personal risk time 2 | .18 | .55** |
| Comparative risk time 1 | -.42* | -.07 |
| Comparative risk time 2 | -.14 | .07 |
| Intentions | .55** | .01 |
| Motivation | .38 | .52* |
| Self-efficacy | .11 | .06 |
| Immediate beer rates | -.37 | -.02 |
| Follow up units consumed | .17 | .36* |

Note. Spearman's rho was conducted on ranked motivation data. All correlations are two-tailed, * = $p < .05$, ** = $p < .01$.

3.9 Assessing the mediating effects of imagery related anxiety on intentions

Given the significant and medium-large effect of the manipulation on intentions, alongside the positive correlation found between imagery anxiety and intentions, we explored whether the effect of group on intentions was mediated by the amount of anxiety induced by the manipulation. This analysis was limited to the role of imagery anxiety since no other relevant demographic, drinking or manipulated variables were found to be significantly correlated with intentions.

As Figure 1 illustrates, the standardised regression coefficient between group condition and imagery anxiety was significant ($\beta = -.43$) as was the standardized regression coefficient between group condition and intentions ($\beta = -.36$). The standardised regression coefficient between imagery anxiety and intentions was also significant ($\beta = .58$). A hierarchical multiple regression was therefore performed with imagery anxiety entered in the first step and group entered in the second step. The results showed that when the effect of imagery anxiety was controlled, the effect of group was no longer significant ($\beta = -.14$, $p = .36$) indicating full mediation of the group effect by anxiety. A Sobell test confirmed the indirect effect was significant (Sobell z statistic = 2.35, $SE = 0.34$, $p = .02$). Table 8 provides full details of the results from this analysis.

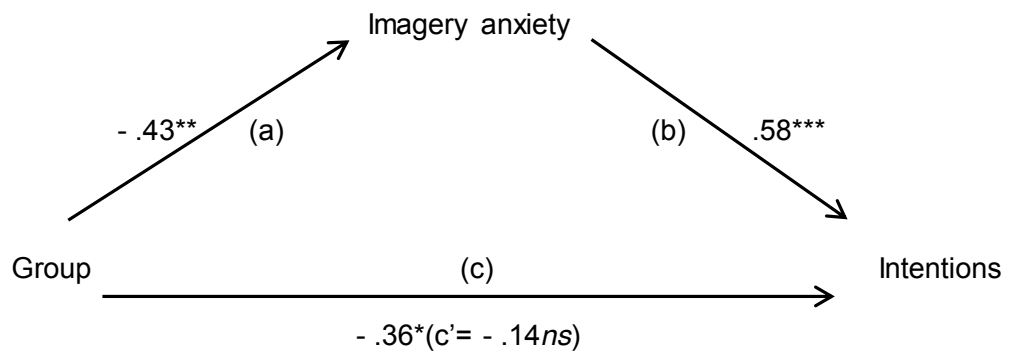


Figure 1. Regression analysis from the looming vulnerability condition showing relationships between looming manipulation, imagery-related anxiety and intentions. Figures represent standardised regression β coefficients. Note: * = $p < .05$, ** = $p < .01$, *** = $p < .001$.

Table 8
Results from the regression analysis

| | <i>R</i> | <i>R</i> ² | <i>adjR</i> ² | ΔR^2 | <i>B</i> | <i>SE B</i> | β |
|---|----------|-----------------------|--------------------------|--------------|----------|-------------|---------|
| Analysis one: Looming manipulation on intentions | .36 | .13 | .10 | | -1.17 | .50 | -.36* |
| Analysis two: Looming manipulation on imagery anxiety | .43 | .18 | .16 | | -26.43 | 9.38 | -.43** |
| Analysis three: <u>Step 1</u> Imagery anxiety on intentions | .58 | .34 | .32 | | .03 | .007 | .58*** |
| <u>Step 2</u> Looming manipulation on intentions (controlling for imagery anxiety) | .59 | .35 | .32 | .02 | -.46 | .49 | -.14 |

Note. * = $p < .05$, ** = $p < .01$, *** = $p < .001$

3.10 Acceptability of the manipulation as an intervention

For the final part of the analysis, descriptive data were used to assess how acceptable the experimental participants found the imagery recordings as part of an intervention targeting at risk drinkers. At the one week follow up, on average the experimental participants rated the imagery recordings as 'slightly acceptable' ($M = 5.11$, $SD = 1.66$; range = 1 - 7).

4. Discussion

4.1 Main findings

The current study explored the effect of inducing a sense of looming vulnerability towards the threat of liver disease among a sample of harmful drinkers. Experimental participants were guided to imagine liver disease rapidly approaching them in time and/or space as they continued to drink alcohol, whereas the control condition received a matched guided imagery task with no threat or drinking related content. We were primarily interested in the impact of this manipulation on drinkers initial and short-term (one week) perceived risks of developing liver disease and on their drinking behaviour. We were also interested in exploring its immediate effects on drinking related intentions, motivations and self-efficacy to make changes to their drinking. To the best of our knowledge, this is the first time this approach has been tested with harmful drinkers specifically for this purpose.

Based on findings from McDonald et al; (2010), who investigated the effects of a similar looming manipulation among a sample of smokers, we hypothesised that participants who received the looming threat would experience more anxiety and would perceive themselves as having a greater personal and comparative risk of developing liver disease in the future, while retaining a sense of self-efficacy to make changes to their drinking. In turn, this was expected to generate greater intentions and motivation to drink within safer limits among the experimental participants, manifesting in lower drinking rates immediately after the experiment and reduced drinking rates at a one week follow up.

A number of interesting findings emerged from this investigation. Firstly, as predicted, the looming manipulation increased participants'

anxiety levels without adversely affecting their self-efficacy to drink within safe limits. However, against our predictions this manipulation had no effect on their immediate or short term (one week) risk perceptions. We found no significant between group differences on personal and comparative risk across the two time points.

Encouragingly, the manipulation was found to have a significant and medium-large effect on intentions, with participants in the looming threat condition reporting higher intentions to regularly drink within safe limits immediately following the experiment, compared to controls. Further analysis revealed that this effect was mediated entirely by the amount of anxiety the manipulation generated. The manipulation also has a moderate to large, but non-significant effect on motivation, with the experimental group reporting greater readiness to make changes to their drinking than controls. The experimental group also consumed less beer than the control group immediately after being exposed to the looming threat, but again this difference was not significant and the overall effect size was small. Despite these promising findings, the manipulation had no short term effects (at one week follow up) on participants' actual drinking behaviour, as the experimental and control group reported consuming similar levels of alcohol before and after participating in this study.

4.1.1 Intentions and motivation

The present results provide tentative evidence to suggest that inducing a sense of looming vulnerability towards the threat of liver disease among at-risk drinkers can help them to develop greater intentions to drink within safer limits; and may also have important effects on their motivation to

reduce their drinking. However, it is not possible to make firm conclusions regarding the manipulations effect on motivation levels since the observed association, although moderate to large, did not reach significance; which could be attributed to a lack of power given the small sample.

Overall, these findings add further support to established theories that emphasise the conditions that are necessary to encourage individuals to develop healthy behavioural intentions. For instance, Protection Motivation Theory (e.g. Rogers, 1975) argues there are three crucial elements that a fear appeal should target in order to be effective in this respect , namely 1) the magnitude of noxiousness of the presented event, 2) the likelihood of the event occurring and, 3) the effectiveness of the recommended coping response (Maddux & Rogers, 1983; Rogers, 1975). Although we developed our manipulation focusing predominantly on increasing the likelihood component, our decision to include a life-threatening disease and ask drinkers to imagine its symptoms actually happening to them whilst also advising them that they could effectively reduce their risk of experiencing this particular threat by drinking within safer limits, would have also satisfied the criteria of noxiousness and response effectiveness. Furthermore, our results build on this research by providing preliminary evidence to suggest that encouraging drinkers to imagine the threat of severe health problems increasing (should they continue to drink harmfully) could be a particularly effective strategy to enhance their intentions to drink less.

What remains unclear from the present results is the precise mechanism by which the manipulation influenced intentions among this sample. Although the effect of the manipulation was found to be mediated fully by the amount of anxiety evoked, imagery related anxiety could only

explain a moderate proportion of the variance in intentions, highlighting that other key variables would have been involved. Furthermore, there is strong evidence that intentions to engage in healthy behaviours, such as safer drinking, are not directly caused by emotions (for a review of this research see Rogers, 1983). Instead intentions are proposed to be influenced mainly by cognitive factors, such as the types of beliefs explicitly targeted by our manipulation, including the perceived susceptibility of the threat (our looming vulnerability manipulation), the perceived severity of the threat (the noxiousness element), the perceived effectiveness of the recommended coping response (response efficacy), and the perceived ability to effectively perform the recommended response (self-efficacy) (Rogers, 1983). Previous studies have shown how each of these beliefs can independently predict healthy intentions and/or actions (see Rogers, 1983 for a review). However, it has been argued that intentions are likely to be highest when all of these beliefs are strong (Rogers 1983).

We predicted that the mechanism through which our manipulation would enhance drinkers' intentions would be by increasing their risk perceptions (e.g. Rogers, 1975). The fact that we did not find any effect of the manipulation on these outcomes, nor did we find any positive associations between perceived risks and intentions, together suggests this was not the path by which the manipulation positively influenced intentions. However, as discussed in the following section, this conclusion is limited by the fact that perceived risks may have been conflated with healthy intentions, which would have masked any significant main effects or relationships regarding these outcomes.

Instead the manipulation may have influenced intentions by activating or increasing beliefs about the severe consequences of continuing to drink harmful levels of alcohol (e.g. Rogers, 1975). In support of this, other studies have shown that increasing perceived severity about other serious threats (e.g. lung disease) can lead to greater negative emotions (e.g. fear), which in turn has been found to indirectly increase intentions by increasing severity appraisals (Rogers & Mewborn, 1976). However since no measure of severity was taken following the manipulation, as we were concerned exclusively with its effect on perceived risks, we were unable to test these assumptions.

Intentions may have also been influenced by beliefs about response efficacy and self-efficacy (e.g. Maddux & Rogers, 1983; Schwarzer & Luszczynska, 2008), both of which were also targeted by the manipulation (e.g. by making it clear they could successfully reduce the threat of liver disease by making changes to their drinking in the imaginary scenes); either independently or in combination with appraisals about the presented threat (Maddux & Rogers, 1983). Again, as response efficacy was also not assessed in this study it is not possible to confirm whether manipulating this variable had any effect on participants' intentions. Nevertheless, we did find self-efficacy to be slightly higher following the manipulation among the experimental group. However, given that these between group differences were not significant, and that there was no significant relationship between self-efficacy and intentions in this sample, it seems unlikely that self-efficacy beliefs on their own were responsible for producing the greater intentions found among the experimental group in this study.

4.1.2 Risk perceptions

The finding that manipulating at risk drinkers to experience the threat of liver disease to be looming closer did not seem to affect their perceptions of risk for this threat contradicts previous theoretical arguments suggesting this should have been a powerful strategy in this respect (e.g. Riskind, 1997, 1999; Riskind et al; 2006). This finding is also at odds with prior research showing that similar approaches have been effective at increasing personal (e.g. Mc Donald et al; 2010) and comparative risks (Ayers & Myers, 2011) among harmful drinkers and smokers.

One likely explanation for this inconsistency could be that our study may have been underpowered to detect similar significant effects on these outcomes given the small sample size we achieved. This is supported by the fact that previous studies reporting positive findings were conducted on larger samples of drinkers and smokers and were only able to produce small to medium effects on these outcomes (for personal risks see McDonald et al; 2010; for comparative risks see Ayers & Myers, 2011).

Another potential explanation for this unexpected finding could be due to problems with the way in which risks perceptions were assessed in this study. In particular, by only asking participants to estimate their risk of contracting liver disease as a result of their current drinking, rather than explicitly instructing them to base this on continuing to drink in the same way, they could have taken their future healthier intentions into account when making these estimates; which would have artificially lowered their perceptions of risk and masked any potential effects (e.g. Brewer et al; 2007; Weinstein, Rothman & Nicolich, 1998). The fact that intentions and

motivations were significantly higher in the experimental group provides some support for this argument.

The manipulation may have also failed to increase risk perceptions because the threatening scenarios were too difficult to imagine. For example, although the results showed that participants who could vividly imagine the looming scenarios rated their personal risks as higher, in line with previous theoretical arguments (e.g. Riskind, 1997, 1999; Riskind et al; 2006), the average vividness ratings from the experimental group was less than 50%. This suggests that the majority of experimental participants would have been limited in the degree to which they could increase their personal risk estimates purely from this manipulation. Some support for this argument comes from the fact that the vividness ratings from the threatening scenarios used in Mc Donald et al's. (2010) study that increased smokers' perceptions of risk were noticeably higher than those obtained from our sample. Furthermore, in our study, participants difficulty in vividly imagining the threatening scenarios would have also (indirectly) limited the manipulations ability to enhance their estimates of comparative risk, since these estimates were based on their perceived personal risks.

The fact that the vividness ratings obtained from the experimental group were much lower than the controls indicates that it was the threatening content used in these scenarios that was harder to imagine. More specifically, it might have been the way in which the threat of liver disease was sped up in the experimental scenarios that was either unbelievable or hard to imagine. This would be particularly problematic since researchers have emphasised that the subjective experience of looming vulnerability, and the value subsequently placed on a threatening event, is

based primarily on the extent to which it appears to be changing or growing from moment to moment (e.g. Riskind, 1997, 1999). According to this view, unless participants were able to imagine the threat of liver disease continuously growing during the scenes whilst they continued to drink, there would be no reason for them to believe their personal risk of developing this disease was also increasing.

Alternatively, it could simply have been overly ambitious to expect a sample of predominately young and otherwise healthy individuals to accept the personal threat of a serious long term condition rapidly approaching them as a result of their current drinking levels. Indeed other studies have also failed to increase student drinkers' perceived risk of liver disease using imagery manipulations alone (Ayers & Myers, 2011). Furthermore, although Mc Donald et al's. (2010) looming manipulation, which included both chronic (lung disease) and immediate health threats (accident), was found to increase smokers risk perceptions in general, it remains unclear precisely how much of an effect this had on smokers' perceived risks for longer term conditions since the majority of their risk items concerned general risks associated with smoking (e.g. "smoking is hazardous to my health"); with only one item tapping into beliefs about their risk of contracting more chronic and severe health conditions ("By smoking I risk heart disease and lung cancer").

Perceptions of comparative risk may have been even harder to influence in our study using the current manipulation alone, since we found the anxiety it induced to be negatively related to these risk ratings among the experimental group. Thus, participants exposed to the looming threat may have reacted defensively to reduce these negative feelings, by exaggerating

their estimates of others risk, compared to their own (e.g. Weinstein & Klein, 1996); which would further explain why only the experimental group, on average, were found to be biased in their comparative risk ratings. To overcome these defensive reactions, others have suggested it may be necessary to supplement threatening messages with accurate information regarding the actual risk status of relevant peers (e.g. Weinstein, 1983).

4.1.3 Immediate and short term drinking behaviour

Despite the positive effects of the manipulation on intentions to drink within safer limits, these effects did not translate into significant reductions in drinking. Although the experimental group drank less beer than controls immediately after the experiment, the overall effect size was small and did not reach significance. The short-term effects were even less promising, as we found the experimental group on average increased the amount of alcohol they consumed (by 1 unit) during the week following the experiment, whereas the controls reported reducing their drinking (by 2 units); however none of these differences were significant.

Together, these findings are inconsistent with the results of McDonald et al; (2010) who reported significant reductions at a one month follow up (yielding a medium effect size) in smoking rates as well as non-significant trends towards greater quit attempts and treatment seeking among smokers who received their looming manipulation.

A number of reasons could be put forward to help explain why we were unable to produce important reductions in drinking using a similar approach. One explanation is that our manipulation may not have increased intentions to a high enough level necessary to produce significant changes

in drinking among our sample. The average intention rating from the experimental group (4.3), was clearly not as high as it could have been, and scores from the motivational data revealed that half of the participants exposed to the looming threat were still not thinking about making changes (pre-contemplative stage) or were just beginning to consider whether this was necessary (contemplation stage) directly afterwards. In addition, any positive intentions and motivations in our sample may have decreased more over time, further minimising their potential influence on subsequent drinking behaviour.

It is also possible that intentions were less important than other factors in determining the drinking behaviour among this sample. For example, other researchers have emphasised the importance of having confidence in the ability to perform the recommended behaviour (self-efficacy) (Bandura, 1977; Schwarzer & Luszczynska, 2008; Strecher, McEvoy, Becker & Rosenstock, 1986), as well as having the belief that the behaviour is under the persons control (perceived control) (Ajzen & Madden, 1986); especially for behaviour changes that are difficult to make such as reductions in drinking (for a previous review see Strecher et al; 1986). By making it clear to participants who were exposed to the looming manipulation that they could control the speed at which the threat was approaching through their own actions (e.g. reducing their intake in the scene) we hoped they would be able to remain confident in their ability to successfully control this threat (e.g. McDonald et al; 2010). However our manipulation was found to have only a trivial effect on the levels of self-efficacy reported.

Situational barriers may have also been present to inhibit participants' positive intentions manifesting into increased reductions in drinking after the experiment (e.g. Janz & Becker, 1984), such as a lack of supportive friends, or the absence of alternative rewarding activities. None of which were addressed in this study. Similarly, participants' may have simply required extra support to successfully enact their positive intentions. According to the Health Action Process Approach, once an individual has developed strong intentions to perform a healthy behaviour, they then need to create a detailed plan about how they will go about putting those intentions into action (Schwarzer & Luszczynska, 2008). In this study, no specific advice or guidance about how participants could reduce their drinking was offered, and even though referral sources were available upon request, none of the participants asked for this information.

Based on the above arguments, it may have been necessary to have spent some time at the end of the experiment encouraging participants to develop 'implementation intentions' (Gollwitzer, 1999) specifying precisely when, where and how they would begin to realise any intentions to drink within safer limits. There is considerable evidence highlighting that such planning can help to 'bridge the gap' between intentions and behaviour (e.g. Sheeran, 2002), by not only helping individuals start and subsequently maintain actions that are in line with their intentions (e.g. for a range of health preventive and other goal directed behaviours see Gollwitzer & Sheeran, 2006; for health risk behaviours only see Schwarzer & Luszczynska, 2008), but also by supporting them to overcome common barriers that can arise during this process; such as forgetting their good intentions, or experiencing unhelpful emotions, or negative contextual

influences that can get in the way of achieving their goals (Gollwitzer & Sheeran, 2006; Gollwitzer, 1999).

4.2 Limitations

There were a number of limitations in the present study worth noting. As already discussed, a key issue was the small sample size achieved. This meant that the study was only powered to detect significant large effects. In addition, the use of multiple tests would have inflated the type1 error rate. However the fact that the effect sizes for key significant findings (e.g. intentions, imagery anxiety) were in the moderate to large range suggests they were unlikely to have been due to chance.

Secondly, to control for practice effects, no baseline measures for any of the immediate outcomes were taken so we were unable to assess any between group *changes* along these variables. This is particularly problematic for the significant between –group difference in intentions that was found since it is possible that this may have arisen due to pre-existing differences in intentions among the two groups, rather than being caused by the manipulation itself.

A third issue was in the measurement of study outcomes. As already stated, our failure to condition risk questions on participants' continued current drinking levels may have been partially responsible for our inability to find any effect of the manipulation on risk perceptions (e.g. Weinstein & Klein, 1998). In addition, overreliance on the use of brief, single report items to assess perceived risks as well as the majority of other immediate outcomes (except immediate drinking rates), would have introduced error into the results and also meant we had less information to explore the effects

of the manipulation in greater detail. Our decision to rely solely on single questions to assess risk perceptions and other cognitions was based partly on our desire to minimise the demands placed on participants and partly due to the lack of available reliable and valid scales for many of these constructs for this group (e.g. for risk perceptions). However, with regards to risk perceptions, recommendations have been made to suggest that more reliable and valid assessments of these constructs can be achieved by including a range of different types of risk questions involving a variety of scales of measurement (e.g. Shepperd et al; 2013).

The use of self-report over more objective measures (except for immediate drinking rates) would have introduced further error into the results as certain biases may have been operating to influence participants' reports. Lack of experimenter blinding may have also influenced participants' responses and confounded the results. These issues would have been compounded by the fact that the initial session was conducted in person whereas the follow up assessment was conducted via email. As a result, participants may have only been biased to provide more agreeable or socially desirable responses during the experiment (e.g. reporting to have drunk less alcohol than their true consumption), whereas at the follow up they may have felt able to respond more honestly; which could partially explain the lack of pre-post changes along some of the study variables. Lack of follow up measures of intentions, and other secondary outcomes, also meant we were unable to assess any longer term effect on these outcomes. Finally, since other manipulated factors were not assessed at all in the present study, namely perceived severity and response efficacy, since the

main focus was on perceived risks, we could not establish their role as potential mediators.

4.3 Implications for research

The present study has highlighted a number of important areas for future research. Firstly, in order to adequately assess the effects of the looming manipulation on key risk and drinking related variables, the limitations of the current study should be addressed by using a larger scale sample of drinkers, whilst assessing and controlling for baseline levels of risk and other motivational variables, using a range of questions to achieve more reliable and valid measurements of these outcomes (whilst making questions conditional on future behaviour), and ensuring adequate blinding and similar testing conditions throughout.

The findings also suggest that more work is currently needed to develop the looming threat scenarios used in this study so they can be vividly imagined by at-risk drinkers; in order to conclusively determine their effects among this group. As part of this work, it would be interesting to tease apart the relative impact of the different scenarios, as well as explore the effects of different types of looming threats (comparing the impact of serious long term (e.g. liver disease) versus short term health threats (e.g. having an accident), and the effects of social threats (e.g. arguing with friends when intoxicated) versus health threats).

What also remains unclear is whether the approach we used is superior, or even equal to presenting health threats in a purely static form. There is preliminary evidence to suggest that guiding drinkers to imagine the risks of drinking related health threats is more effective than simply showing

them the negative consequences that can happen as a result of excessive drinking, at least when focusing on one specific outcome such as having an accident (Ayers & Myers, 2011). However, whether adding a looming element to these threats would have additional positive effects on drinkers' beliefs about their risks, as well as on their motivation and intentions to reduce, remains to be determined by future studies. This could be tested by comparing the looming manipulation with active rather than matched control conditions, which could require drinkers to imagine the threats happening to them (with no threat progression) or to watch the same threats happening to another person (as in Ayers & Myers, 2011). If the addition of a looming component was found to be more powerful than these already established methods of risk communication, further work could address the most effective and feasible way in which to incorporate this method into clinical practice and large scale media campaigns.

4.4 implications for practice

The results of the present study also have important implications for practice. In particular, the findings suggest that to increase at-risk drinkers intentions to drink within the recommended limits, interventions should encourage them to imagine themselves becoming increasingly vulnerable towards developing serious health problems; whilst reassuring them that they can decrease their risks by reducing their drinking. Such an approach differs from methods typically employed in government health campaigns and clinical practice (e.g. Alcohol Health Alliance, 2013; Department of Health, 2015). Traditionally, media health campaigns have tended to rely on the use of graphic visual information to raise drinkers' awareness and overall

fear about the associated risks (e.g. Ruiter, Abraham & Kok, 2001). In terms of clinical practice, Nice recommendations state that harmful drinkers should not only be informed about their risks but should also be helped to acknowledge their current risk status using screening tools such as the AUDIT (NICE 2010).

The results of the present study, alongside the findings from other research (e.g. Mc Donald et al; 2010) tentatively suggest that these approaches might also benefit from incorporating into these messages the idea that drinkers risks are ever increasing. However as previously discussed, whether this would lead to additional benefits on top of those found from simply informing drinkers about their risks, remains an important question for future research.

Despite these encouraging implications, the present study also showed that this manipulation alone is unlikely to lead to important reductions in drinking. This finding is also consistent with other studies showing the gap between people's good intentions and their actual behaviour (Sheeran, 2002).

Together, these findings should perhaps be unsurprising given that behaviour change is widely acknowledged to be a complex process involving a range of distinct and interacting factors (e.g. Maddux & Rogers, 1983).

Even if drinkers truly believe their drinking is placing their health at serious risk and they have strong motivation and intentions to reduce, a whole range of other conditions need to also be met for them to have a reasonable chance of successfully making these changes (e.g. absence of external barriers; Janz & Becker, 1983). Behaviour change is also a dynamic, rather than linear process, and it is therefore common for people

who are initially motivated to change their behaviour to fall back into earlier pre-contemplative stages (e.g. no longer thinking about change) when their initial attempts are difficult or unsuccessful (e.g. Prochaska & DiClemente, 1982). Perhaps one of the largest obstacles faced by drinkers who want to reduce is the fact that this often involves immediate unpleasant costs (e.g. craving, lack of alternative relaxing activities, less socialising with friends), whereas any potential benefits may not be realised until years later. The fact that drinkers cannot be certain that reducing their drinking now will lower their risks of developing problems in the future, could mean that for many, this trade-off may not seem worthwhile.

Nevertheless, the available evidence base suggests that important reductions in drinking can be achieved using multi-element approaches tackling many of the factors known to impede behaviour change. For example, NICE (2010) recommends first offering brief opportunistic sessions, following the FRAMES model. This involves providing feedback about drinkers' current risk status, emphasising their responsibility for making changes, providing clear advice about how they can implement those changes (offering a range of options), whilst communicating genuine empathy for the real difficulties they are likely to face in their efforts to reduce as well as building their confidence that changes are possible. For those who do not respond to brief interventions, extended treatment (of around 20-30 minutes lasting for a maximum of 5 sessions) is recommended to resolve any ambivalence about reducing, using the non-confrontational communication style employed in motivational interviewing and motivational enhancement therapy (e.g. exploring the pros and cons of making changes and staying the same) (NICE, 2010). Alternatively, or as part of these

interventions, another approach that has already shown promise in terms helping individuals enact their positive intentions (e.g. Schwarzer & Luszczynska, 2008) could be to help at risk drinkers' develop precise plans about how to cope in difficult situations involving drinking. However, it remains for future research to determine if including elements of these effective treatments to our looming intervention can lead to further reductions in drinking among harmful and hazardous drinkers.

4.5 Conclusions

Overall this study has found preliminary evidence highlighting the usefulness of inducing a sense of looming vulnerability to the serious threat of liver disease at enhancing at-risk drinkers' intentions to drink within safer limits. However, larger scale studies are required to confirm these findings and to determine whether additional support from established treatments will help translate the positive intentions it is able to elicit into important changes in drinking. Furthermore, by addressing the limitations in this study, future research could help clarify the precise mechanism(s) by which the looming manipulation exerts its influence on drinking related cognitions as well as potential moderators of its effects (e.g. whether it applies only to specific types of threat, and whether the looming components add to the effects of imagination).

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Part 3: Critical Appraisal

1. Overview

The following paper provides a critical appraisal of three key elements of my research project: the design of the experimental manipulation, measurement of outcomes and recruitment of participants. Before reflecting upon the main decisions and challenges that arose in these areas, I will begin by briefly discussing the experiences that sparked my initial interest in risk perceptions.

2. Background

Perhaps unsurprisingly, as a trainee clinical psychologist, I have always been interested in why people behave in the ways that they do. A particular curiosity of mine has been why people engage in behaviours that have the potential to be very dangerous or harmful. Our propensity to take risks (or what Freud famously described as the death wish) begins from the earliest stages of life (Freud, 1920). During my own childhood I can remember often trying to outcompete my siblings in a variety of acts (e.g. who can jump from the highest tree) that had the potential to be highly destructive.

Luckily, such overtly risky behaviour gradually diminishes as we grow older and our brains develop enough for us to begin to conceive of the future consequences of our actions. However for a large majority of people, a childlike sense of invincibility seems to persist well into adulthood. This is highlighted by the fact that the largest causes of morbidity and mortality nowadays are not from uncontrollable events such as environmental hazards or infectious diseases, but are instead due to the risky behaviours

people choose to perform, such as smoking, eating unhealthily and drinking excessively (McKeown, 1979).

During my undergraduate degree I was keen to explore more about the psychological factors that compel people to perform behaviours that are risky to their health. Early theories of learning, such as operant conditioning theory, helped make sense of this by highlighting how behaviour is largely influenced by its immediate rewards rather than any longer term consequences (Skinner, 1938). This is problematic since many unhealthy behaviours often bring about immediately pleasant effects (e.g. smoking can be positively reinforcing by leading to increased feelings of social acceptance, and can be negatively reinforcing by removing unpleasant cravings), and the costs of stopping (e.g. cravings) can often seem to outweigh any potential benefits in the future. Other theories, focusing on health behaviours specifically, further emphasised the role of beliefs about the severity and susceptibility of illnesses (e.g. Janz & Becker, 1984; Rogers, 1975). The idea that people perform unhealthy behaviours because they are unaware of the risks, and will therefore become motivated to stop if they knew how risky their behaviour was, seems to have been what has driven many mass media campaigns to raise public awareness about the severe risks through warning signs and graphic visual images (e.g. Ruiters, Abraham & Kok, 2001). However, intuitively it did not make sense to me why anyone would pay attention to these warnings unless they truly believed they were personally at risk of experiencing these outcomes, having repeatedly heard friends, family and even myself dismiss these messages with statements like, "...yes I know X is a horrible disease, but it's not going to happen to me..."

My awareness of the important role of beliefs about susceptibility in driving healthy behavioural choices grew further as I gained some work experience in a drug and alcohol service prior to training. It was in this setting where I became struck by how many people continued to take drugs and alcohol despite telling me terrible stories about people they had known who had either died or become seriously unwell from doing these very same things. Even more surprising to me was the fact that these clients would be warned on almost a daily basis about the long term damage their drinking and drug taking was doing to their health. Yet all of these warnings, despite coming from highly authoritative sources such as doctors and nurses, were not enough to persuade them to stop.

Although clearly there are a range of factors responsible for why people with drug and alcohol problems find it incredibly difficult to give up (e.g. physical dependence, huge social deprivation leading to a lack of alternative rewarding activities, lack of other meaningful connections with people, lack of supportive friends etc. to name but a few), this experience made me appreciate just how powerful the denial to accept our personal susceptibility to risks can be, and how potentially dangerous this is.

Based on these experiences, when it came to decide on my research project I was eager to explore whether I could develop an intervention that might help bridge the apparent gap between people's risky behaviours and their perceptions about the longer term risks. During the research phase I learnt about a similar piece of research conducted by McDonald and colleagues (2010) with smokers, which seemed to fit my interests and

overall aims. My goal was to see whether this intervention could be successfully applied to a different at risk group, such as harmful drinkers.

3. Designing the intervention

Having already decided on the target population, the next step involved designing the manipulation. As already mentioned in the clinical paper, this was informed largely by the work of McDonald and colleagues (2010) who developed guided imaginary scenarios using the looming vulnerability model of anxiety (Riskind, 1997; Riskind, Williams & Joiner, 2006). This model attempts to offer a more ecologically valid description of the subjective experience of anxiety (in both its state and more enduring forms), by highlighting the dynamic and temporal nature of perceived threats (Riskind et al; 2006). Specifically, it argues that individuals who suffer from anxiety create "...mental scenarios of rapidly intensifying danger..." (Riskind et al; 2006 p. 785) which, once activated is proposed to lead to "...an intense feeling of fear and personal vulnerability, and thus lead to an increased sense of time urgency and imperative need for action, even in the absence of objective danger" (Riskind et al; 2006, p. 785).

In our study, necessary adaptations were made to the looming vulnerability scenarios used in McDonald and colleagues (2010) study to make them relevant to a sample of drinkers, and in keeping with our desire to see whether this approach would help drinkers personalise one particularly serious health risk, such as liver disease. This was attempted by drawing on key elements of the concept of looming vulnerability; specifically the idea that perceived threats are experienced to be '...rapidly rising in risk as they approach through time or space, or move towards dreaded ends.'

(Riskind et al; 2006, p. 781). We also sought further advice when developing the scripts from Professor Riskind, who after reading an earlier draft version of our guided scenes recommended informing participants that the symptoms (e.g. pain in abdomen) were associated with liver disease and were directly caused by drinking harmful levels of alcohol; whilst making it clear that these consequences could be slowed down (or stopped altogether) by drinking within safer limits (J. H. Riskind, personal communication, July 2014).

There were obvious advantages to this approach. Not only was a significant amount of time saved in adapting the ready-made guided imagery scenarios provided by Mc Donald and colleagues (2010) to our specific aims, this also gave me confidence that the manipulation would be effective, given its previous success among a similar at-risk group (Mc Donald et al; 2010). Indeed the fact that manipulating threat in this way did cause participants to feel more anxiety, and had a positive, and almost large effect on their intentions as well as non-significant trends towards greater motivation, suggests that in some important respects it was successful.

Despite these positive findings, our manipulation did not have the main intended effect of increasing drinkers' perceived susceptibility to liver disease. In the empirical paper we discussed how this may have been partly due to difficulties in their ability to vividly imagine the scenarios. Alternative explanations were also put forward to suggest problems with measurement may have obscured any effect of the scenarios on risk perceptions. Regardless of this latter possibility, it is important nonetheless to establish

how our scenarios could have been experienced more vividly given that we found vividness and risk perceptions to be positively related in our sample.

In general it would have been beneficial to have spent more time developing and testing out the scenarios. Although an initial pilot test suggested the scenarios we created were acceptable in terms of their overall vividness (all vividness ratings were greater than 50%) time constraints, alongside other problems with recruitment, meant that we could only pilot them on a limited sample of drinkers ($n = 3$). Issues with the specific content used in the scenes may have made it hard for participants to vividly imagine the presented threat; such as the fact that the threat of liver disease was mentioned only once in each scene and its associated symptoms were described in a relatively benign way (e.g. slight pain in abdomen). This was due to our desire to not overwhelm participants with too much fear since we were aware that this can have the opposite effect of making people more defensive against threatening messages (e.g. Brown & Smith, 2007; Ruiter et al; 2001). However it seems that, at least with our sample, this may have minimised their chance of vividly imagining and personalising these events.

To increase the likelihood of drinkers' being able to vividly imagine a long-term health threat happening to them as a result of their continued drinking it may be necessary for future studies to describe the threats and their associated symptoms in greater sensory detail (e.g. Riskind, 1997). It may also be beneficial to expose participants to the threatening messages for a longer time period; particularly when using a threat that develops after a long period of time, since it may take longer to persuade participants of

this happening compared to other more immediate consequences (e.g. feeling nauseous from drinking too much).

As discussed in the empirical paper it might have also been the process by which the threat of liver disease was sped up in the scenarios that was difficult to vividly imagine. For instance, even if drinkers could imagine developing liver disease in the future from continuing to drink at harmful levels, unless they were able to consider this risk increasing by coming closer towards them as a result of their current drinking levels there would be no reason for them to report being at any greater risk than the controls. This is because they could have appraised the potential threat as being far away in the future, by which point they may have thought they would have already made healthy changes to prevent this from happening.

In our study we tried to manipulate the progression of the threat by adapting Mc Donald and colleagues (2010) scenes, using the same language and format they used. However, in the end we were only able to adapt two of their scenarios (the conveyer belt and office building scenes). We decided against using their train track scenario as this did not depict a severe health threat happening due to participants' unhealthy behaviour (e.g. in this scene smokers were instead asked to imagine a train coming closer towards them at a fast speed as they continued to smoke). We also did not include their clock ticking scenario, which asked smokers to imagine terrible health consequences related to smoking coming closer towards them as they smoked whilst keeping track of time for 3 minutes. This was because we wanted participants' tasks to be similar across all the scenarios. Instead, in collaboration with Professor Riskind (personal communication, July 2014)

we created two new scenes in which we tried to make our participants imagine symptoms associated with liver disease developing within them as they continued to drink; following the same format used in the other two scenarios (e.g. gradually increasing the threat of liver disease in time or space).

Perhaps it would have been easier for drinkers to imagine the negative consequences associated with harmful drinking actually happening to them if they were allowed to choose the specific negative consequences that could happen (similarly to the clock ticking exercise). Alternatively it may have been easier to imagine these risks increasing if the symptoms were described as building up faster over a longer period of time and becoming more and more severe as time passed (e.g. Riskind, 1997).

Other techniques than guided imagery may have also been more effective at inducing a sense of looming vulnerability. For example, the sense of increased threat towards liver disease could have been made more concrete and realistic by using videos depicting the threats growing in time or space, or virtual reality to simulate the experience of these negative consequences (e.g. Song, Kim, Kwon & Jung, 2013). Indeed some promising results have shown that allowing smokers to embody negative consequences of smoking (e.g. simulating the experience of facial ageing as a result of smoking) through an educational game in which smokers played avatars of possible future selves increased their perceptions of risk for these consequences; and made them develop more negative attitudes towards smoking as well as greater intentions to quit (Song et al; 2013). However the choice to use these more technologically advanced approaches in future

studies would have to be balanced against the additional costs and other feasibility and ethical issues (e.g. greater time demands, potential to be too anxiety-provoking).

Future studies would also benefit from more in-depth checks to ensure the manipulation worked as intended. In our study we only asked participants to rate the extent to which they felt a part of the scenarios when listening to the recordings, similarly to the checks used in prior work (McDonald et al; 2010). Upon reflection, it would have been more helpful if we had directly assessed the extent to which a sense of looming vulnerability had been induced among the experimental participants; by exploring the degree to which they felt the presented threat and its associated symptoms were actually increasing through time and/or space, as well as how believable this felt. Specific questions for this purpose that have been used in other studies investigating the looming vulnerability concept include: “How rapidly is the threat of ‘X’ growing? or “How quickly is ‘X’ becoming more dangerous?” (Riskind 1997, p.691). Including questions such as these would further clarify the precise elements that were easier or more difficult to imagine that could be targeted by future developments.

4. Measurement issues

There were also notable limitations in the measurement of our outcomes which could be improved by future research.

4.1. Lack of validated and reliable measures

Risk perceptions

As discussed in the empirical paper, the single item questions we used to assess risk perceptions may not have been sensitive enough to detect any significant effects. More problematically was the fact even though single item questions are commonly used to assess perceived risk (e.g. Wild, Hinson, Cunningham & Bacchioni, 2001) they are less valid and reliable than psychometrically validated tools. Although currently no validated measures of risk perception exist for harmful drinkers, some recommendations have been made to suggest that more reliable and valid assessments of these constructs (for both personal and comparative risks) can be achieved by including a range of different types of risk questions involving a variety of scales of measurement (e.g. Shepperd, Klein, Waters, & Weinstein, 2013). For example, in our study, we could have been more confident that we were reliably measuring drinker's risk perceptions by supplementing our verbal Likert scale risk questions with numerical risk questions such as: "What is the likelihood you will get liver disease?" from 0% to 100% (for details regarding other recommendations see Shepperd et al; 2013; Weinstein, 1998, or refer to the literature review for a summary of these recommendations).

It would have also been useful to assess whether the manipulation affected perceptions of risk in general, rather than focusing solely on its effects on the one risk that we targeted. Not only would this have added useful information regarding the generalizability of any effects, it would have made our results more directly comparable to the work of Mc Donald et al.

(2010). In their study, they included the validated Health risk- subscale from the Smoking Consequences Questionnaire for Adults (SCQ-A; Copeland, Brandon, & Quin, 1995) to measure smokers agreement that they were placing their health at risk in general from smoking (e.g. "Smoking is hazardous to my health") and for being at risk of specific health problems (e.g. lung disease).

For the same reasons as those mentioned above, it would have been interesting to have further explored beliefs about more immediate consequences following the manipulation; using validated scales designed for this purpose such as the Alcohol expectancy Questionnaire (Fromme, Stroebe & Kaplan, 1993). This measure assesses immediate positive and negative expectancies from drinking across a range of areas known to be related to this behaviour. The domain of positive expectancies includes items about sociability, tension reduction, liquid courage and sexuality, whereas the negative domain taps into important areas of cognitive and behavioural impairment, risk and aggression, and self-perception. Drinkers are asked to rate the degree of likelihood they will experience each outcome if they were under the influence of alcohol on a five point Likert scale (1 = disagree to 4 = agree). They are also asked to evaluate each of these consequences on the same scale (1 = bad, 3 = neutral, 5 = good). A similar approach was also included by McDonald and colleagues (2010), who used the Self-Generated Outcome test (SGO; McKee, Wall, Hinson, Goldstein, & Bissonnette, 2003) to assess smokers automatic beliefs about the consequences of smoking which were later categorised into positive reinforcement (e.g. feeling good), negative reinforcement (e.g. less anxiety)

and negative outcomes (burning sensation in my throat), and was shown to have good inter-rater reliability ($\kappa = .80$).

Secondary outcomes: Intentions and self-efficacy

The measurement of intentions and self-efficacy would have been improved by including other more detailed, validated and reliable scales into these assessments. Two questionnaires that could have been useful in this respect are the Alcohol Reduction Strategies – Future Intention Scale (ARS-Future Intentions, Bonar et al; 2012) and the Alcohol Reduction Strategies-Current Confidence Scale (ARS-Current Confidence, Bonar et al; 2011). The ARS-Future Intention scale asks drinkers' to rate the likelihood (from "not at all likely" to "extremely likely") of them using 21 specific behavioural strategies to reduce their drinking over the next 10 occasions in which they would drink in their typical binge drinking situation (e.g. leave 15 minutes in between each drink, avoid drinking with friends who drink excessively). The ARC further establishes their confidence (from "not at all confident" to "completely confident") in employing these reduction strategies by asking them to imagine drinking in their preferred binge drinking location. Both of these measures have proven to hold adequate psychometric properties (e.g. Bonar et al; 2011, 2012).

However, any advantages obtained from including additional measurements in future research (e.g. increased precision, greater detail, less measurement error) would have to be balanced against the potential negative impact on participants (boredom or fatigue), which would reduce the reliability of their responses.

4.2. Issues with self-report

The use of self-report questions for the majority of outcomes we assessed (except immediate drinking behaviour) may have introduced bias into participants' responses. With regard to participants' drinking levels, although self-report measures are generally considered to provide accurate responses, and the time line follow back measure used in our study has consistently been shown to be a valid and reliable approach (Sobell, Brown, Leo & Sobell, 1996; Sobell, Sobell, Klajner, Pavan & Basian, 1986), it is still commonly assumed that the desire to present oneself in a more favourable way often leads drinkers to report lower drinking rates (Del Boca & Noll, 2000). Indeed, participants in our study did appear to be concerned about how their levels of drinking would be perceived (e.g. asking who would have access to the information), and many exhibited signs of embarrassment when handing over their completed drinking calendar (e.g. making comments hoping no one else would see it).

We tried to reduce the chance that they would give social desirable responses by reassuring them that the information they had given would remain confidential throughout the experiment and afterwards (e.g. Nederhof, 1985); however since they were aware the experimenter could still read their responses it was not possible to remove the potential influence of this bias entirely.

Despite this, the overall accuracy of participants self-reported drinking would have been supported in our study by using aided recall techniques in both assessments (e.g. prompting them to recall memorable

events associated with their drinking) (Del Boca & Noll, 2000). To further reduce the demands placed on their memory during this task, and improve the accuracy of their responses even more, we could have asked them to complete drinking diaries during the week prior to the study and throughout the follow up phase. However necessary caution would be required in using this approach given that asking people to monitor their drinking can cause them to reduce, and can therefore act as a form of intervention itself which would confound the results (Del Boca & Noll, 2000).

Immediate self-reported intentions, risk perceptions and motivation could have also suffered from biases influenced by the experimenters' presence in the room, such as the need to provide agreeable responses. Again we attempted to minimise this by emphasising the importance of responding honestly. This type of response bias could have been further minimised if we had included other strategies known to motivate individuals to provide truthful responses. For example, we could have asked participants to sign an explicit agreement to answer honestly or prompted them to do this and reinforced their efforts with gratitude (Del Boca & Noll, 2000).

The decision to remain present in the room when participants were completing the measures was to ensure that any queries could be clarified immediately and help them provide more accurate responses. Further attempts were made to reduce the experimenters' influence by providing clear written instructions on how to complete the questions, and by sitting at a distance away, outside of their view. In the end, however, very few participants needed help in answering the questions. Thus, in future studies

it would be preferable for the researcher to leave the room entirely, or to use a different researcher administering the questions and experiment, who are both blinded to the purpose of the experiment, to reduce the likelihood of the experimenter unintentionally influencing the results.

In general, the accuracy of all of the self-reported measures may have been improved by triangulating responses with other sources, such as information obtained from other informants or from other measures assessing similar constructs (Del Boca & Noll, 2000). Again the decision to include these additional assessments in future research would have to be based on any cost and time limitations.

4.3 Other important outcomes not assessed

As touched upon already in the clinical paper, a number of mechanisms, other than participants' risk perceptions, could have mediated the observed effect of the manipulation on participant's intentions; which were not assessed. This was because we hypothesised that the primary route by which the intervention would have its influence would be by enhancing their risk perceptions, given that this was the main factor manipulated in our experiment. However, as already discussed, our additional manipulations of severity and response efficacy could have also been responsible for producing this effect.

In order to test these competing explanations and tease apart their relative importance as potential mediators it would be necessary for future research to measure these variables and/or include suitable alternative control groups. Similarly to the measurement of risk perceptions, these items

have also typically been assessed via single item questions (e.g. Milne Sheeran & Orbell, 2000). An example question to assess the severity variable could be to ask participants' level of agreement with the statement "Liver disease is a very serious disease" (1= strongly disagree to 5 = strongly agree), whereas response efficacy could be measured with questions such as "If I reduce my drinking I will greatly decrease my chance of developing liver disease in the future", using the same scale (e.g. Milne et al; 2000). However, as already discussed, given the error associated with single item questions it would be important to supplement these questions with additional items, worded slightly differently.

5. Recruitment issues

A major limitation of our study was in the small sample size we achieved which reduced our overall power to detect significant effects. In this section we briefly reflect upon the reasons for our difficulties with recruitment and consider how these issues could have been dealt with more successfully.

At the start of the experiment we were optimistic that we could achieve a similar recruitment rate as that achieved by Mc Donald and colleagues (2010), which would have allowed us to detect significant medium effects on our primary outcomes of interest. However shortly before we started recruitment it became clear that this was not going to be as easy as we had initially anticipated. One of the main barriers that we faced early on was the fact we were not able to advertise to a wide enough audience to be able to achieve the desired sample size within the seven month time

frame we had allocated for this purpose. Specifically, we originally planned to use the UCL wide *Announce* system as our main recruitment strategy, since this had shown to be highly effective at yielded large recruitment rates from other studies involving drinkers and smokers previously conducted at UCL (e.g. other studies conducted by my supervisor and previous trainees received over 100 responses after sending out one message using this system). Unexpectedly however, shortly after we were ready to begin recruitment the university chose to close this system down due to concerns that students were becoming overburdened by emails. As a result we were left relying on more time consuming recruitment methods (e.g. paper advertisements, social media). Other strategies were also tried to increase our recruitment rates (e.g. snowballing techniques) and by using other participant recruitment systems (e.g. Sona systems, Call for participants). Despite this, the interest we received through each of these methods combined remained very low throughout the recruitment process (e.g. at our peak we received five responses back in one week, starting from October 2014 until May 2015).

Further problems we faced in recruitment came from the fact that many other similar studies were being advertised at the same time, across UCL and other universities nearby, which meant we had to compete to gain the interest of potential participants. An added barrier was the fact that our study would have been seen as arguably less appealing than other similar projects that were being advertised which did not require as many time demands (e.g. internet based studies).

As noted by other researchers, the decision to participate in research often involves a cost-benefit analysis on the part of the participant (Patel, Doku & Tennakoon, 2003). With this in mind, we tried to increase our initial response rates by reducing any perceived costs, by making it clear on the advert that the experiment was only 30 minutes and the follow up questions would place even less time demands on them since they would be conducted over email. To further increase the likelihood of people agreeing to take part after registering their initial interest we also tried to be as flexible as possible about when they could attend the experiment, by offering a range of evening appointments. We also hoped that offering a financial reward (£7 immediately after the in session experiment) would add to their perceived benefits about taking part. All of these strategies have been suggested to increase the chance of achieving greater response rates (e.g. Patel et al; 2003). However, as already mentioned, since other studies available at the same time as ours were also offering similar rewards, without the added time demands as ours (e.g. internet based studies), our study would have remained arguably less appealing.

To improve our overall response rates it may have been necessary to have used a larger overall incentive to gain more interest in our study in the first place (e.g. giving participants the chance to win a voucher for a larger overall sum by taking part). Participant numbers may have been further improved had we used the assertive tracking approach advocated by other researchers (Patel et al; 2003). For example, although participants who failed to respond after showing their initial interest were sent a further email asking if they would like to take part, had we followed up all non-responders

a second or even third time we may have been more successful in recruiting larger numbers into the study (Patel et al; 2003).

A key learning curve that has come out of this experience has been the importance of not underestimating the likelihood of unforeseen problems occurring during the research process. Had we accounted for the possibility that we would not be able to use the UCL announce system from the start, we would have started recruitment much earlier to be able to achieve our desired sample size.

6. Conclusions

This critical review has highlighted some important areas for future studies to address. A key focus of this research should be towards developing, and adequately testing, alternative methods to induce a sense of looming vulnerability among at risk drinkers, using an appropriately powered sample; whilst including more valid and reliable measurements and assessing for other relevant mediators.

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Appendices

APPENDIX 1: EXAMPLE SEARCH STRATEGY USED ON PSYCHINFO OVID SP.

1. (smoke* or smoking).ab,ti.
2. (cigarette adj2 (smoke* or smoking)).ab,ti.
3. (binge* adj drink*).ab,ti.
4. (alcohol adj2 (user* or using or consumer* or consuming or consumption or drinker* or drinking)).ab,ti.
5. (tobacco adj2 (smoking or smoker or using or user*)).ab,ti.
6. (drinking adj1 (harmful or hazardous)).ab,ti.
7. *Tobacco Smoking/
8. binge drinking/ or *alcohol abuse/ or *alcohol drinking patterns/ or *alcohol intoxication/ or *social drinking
9. *Alcohol Drinking Attitudes/
10. 1 or 2 or 3 or 4 or 5 or 6 or 7 or 8 or 9
11. (perceived adj susceptibility).ab,ti.
12. (vulnerability adj1 (looming or perceived)).ab,ti.
13. (comparative adj optimism).ab,ti.
14. (unrealistic adj optimism).ab,ti.
15. (optimis* adj bias*).ab,ti.
16. (perceived adj risk*).ab,ti
17. denial.ab, ti.
18. (personal adj3 immunity).ab,ti
19. (protection adj motivation).ab,ti.
20. (negative adj consequence*).ab,ti.
21. *Health Behavior/ or *Health Knowledge/ or *Health Attitudes/ or *Physical Illness (Attitudes Toward)/
22. *Risk Perception/
23. social comparison/ or *self evaluation/
24. *Reality/ or *Threat/ or *Optimism/ or *Self Perception/

25. *Fear/
26. *Denial/
27. *Feedback/
28. *Intervention/ or *Attitude Change/
30. 11 or 12 or 13 or 14 or 15 or 16 or 17 or 18 or 19 or 20 or 21 or 22 or 23 or 24 or 25 or 26 or 27 or 28
31. 10 and 30
32. animal*.mp. [mp=title, abstract, heading word, table of contents, key concepts, original title, tests & measures]
33. 31 not 32
34. limit 33 to (animal and animal)
35. 33 not 34
36. limit 35 to (inpatient or outpatient)
37. 35 not 36
38. limit 37 to ("qualitative (maximizes sensitivity)" or "qualitative (maximizes specificity)" or "qualitative (best balance of sensitivity and specificity)")
39. 37 not 38
40. limit 39 to (bibliography or editorial or encyclopedia entry or letter or obituary or poetry or review-book or review-media or review-software & other)
41. 39 not 40
42. limit 41 to (classic book or conference proceedings or handbook manual or reference book or "textbook/study guide")
43. 41 not 42
44. pregnancy.mp. [mp=title, abstract, heading word, table of contents, key concepts, original title, tests & measures]
45. 43 not 44
46. limit 45 to (100 childhood <birth to age 12 yrs> or 120 neonatal <birth to age 1 mo> or 140 infancy <2 to 23 mo> or 160 preschool age <age 2 to 5 yrs> or 180 school age <age 6 to 12 yrs> or "380 aged <age 65 yrs and older>" or "390 very old <age 85 yrs and older>")
47. 45 not 46

48. (cannabis or marijuana).mp. [mp=title, abstract, heading word, table of contents, key concepts, original title, tests & measures]

49. 47 not 48

APPENDIX 2: EXAMPLES OF STRATEGIES USED WITHIN EACH MANIPULATION CATEGORY (from Portnoy et al; 2014)

1). Deliberative manipulations involve presenting individuals with factual and/or numeric risk information

Examples include:

- presentation of risk information (e.g. presenting written statements about the link between lung cancer and smoking)
- one on-one counselling (e.g. genetic counselling)
- false feedback (e.g. presenting a fixed and not necessarily accurate risk estimate)
- risk calculator (e.g. having the participant input their health history to generate a tailored risk estimate)

(2) Affective manipulations include components that focus on emotion.

Examples include:

- fear appeal (e.g. attempting to explicitly provoke anxiety or fear about risk)
- emotion manipulation (other than fear appeal; e.g. attempting to elicit anticipated regret at inaction, such as by hearing about others personal experiences of health problems as a result of continued smoking)
- incidental emotion manipulation (e.g. attempting to elicit an emotion irrelevant to the decision but that may systematically bias risk estimates)
- presentation of graphic risk images (e.g. presenting pictures, or videos depicting the severe consequences of a risk, such as blackened lungs in the context of smoking).

(3) Decision Science based manipulations are those that incorporate elements informed by decision science theory and research, such as those often employed in lab or field-based decision-making studies.

Examples include:

- new format for risk presentation (e.g. presenting numeric risk in pictograph/interactive visual form)
- message framing (e.g., presenting risk in terms gain vs. loss).

4) Social Psychology-based manipulations are those based on a theory or phenomenon from Social Psychology and/or focused on self-concept.

Examples include:

- vignettes and self-affirmation (e.g. having the participant list values to bolster the self before receiving risk information)
- dissonance enhancing interventions

(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*



Please return completed form to:

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

APPENDIX 4: ETHICS APPROVAL

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 /s/



Professor John Foreman
Chair of the UCL Research Ethics Committee

Cc: Professor Peter Fonagy, Head of Department

APPENDIX 5: RECRUTIMENT ADVERT



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

Do you regularly drink more than the government recommended guidelines for alcohol consumption (which are 2-3 units for women and 3-4 units for men) and/or binge drink (consume over twice the recommended daily amount of units) at least once a week?

If you **drink beer**, are **generally healthy** and **aged between 18-50** you may be eligible to take part in a study that examines the effects of craving and drinking on psychological tasks and questionnaires.

Participants will be required to complete a brief 5-10 minute screening questionnaire over email. Eligible participants will be invited to take part in the 30 minute experiment at the UCL Research Department of Clinical, Educational and Health Psychology. For the final part of the study you will be emailed 1 week later to complete a brief (5 minute) follow up questionnaire. You will **receive £7 for taking part** in the study.

APPENDIX 6: PARTICIPANT INFORMATION SHEET

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-related thoughts and feelings?’’.

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 alcohol-related thoughts and feelings in heavy social drinkers (i.e. those who regularly drink more than the government recommended levels). We are interested in whether thoughts and feelings related to alcohol change 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 or imagination in a particular way). It is not currently known how alcohol-related thoughts and feelings impact on drinking behaviour. By learning more about the mental activities that are involved in drinking-related thoughts and emotions we may be able to develop more effective interventions to reduce alcohol consumption in problem drinkers.

Who can take part?

If you are generally healthy and regularly drink more than the daily government-recommended amount of alcohol (recommended amounts are 3-4 units for men and 2-3 units for women) AND/OR binge drink (consume over twice the 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 need to refrain from drinking alcohol for 12 hours prior to this appointment. You should not eat or drink any caffeinated drink for three hours prior to the appointment and not use any recreational drugs in the last 24 hours. You will then be given some questionnaires to measure your cravings, mood, attitudes about alcohol, drinking history and use of other drugs.

Next you will take part in computerized and pen and paper tasks. The tasks will involve thinking about the negative consequences of drinking.

Part of the experiment may involve tasting different types of alcoholic and non-alcoholic drinks and rating your preference for these. A breathalyser may also be used during this task. All of this will take up to one and a half hours. After this you will be paid for your time.

We would also like to contact you again: either after 24 hours, or one 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 drinking-related thoughts and feelings, 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.

APPENDIX 7: PRE-EXPERIMENT SCREENING QUESTIONS

(sent via email link using opinion software)

"How do verbal and visuospatial strategies modify alcohol-related thoughts and feelings?"

Thank you for your interest in this study. The aim of our research is to examine the effects of psychological task performance on alcohol-related thoughts and feelings in heavy social drinkers (i.e. those who regularly drink more than the government recommended levels).

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

To determine your eligibility to take part you will be asked a series of brief questions. This should take approximately 5-10 minutes in total.

Instructions

The following questions are designed to let us know about you and your drinking patterns. Please read each question carefully before providing a response. Don't take too long over your replies: Your immediate reaction is best.

1. What is your gender?

- Male
- Female

2. How old in years are you?

3. How would you describe your ethnicity?

If OTHER, please specify in the box below:

- White: British, Scottish, Irish, Other
- Mixed: White and Black Caribbean, White and Black African, White and Asian, Other
- Asian or Asian British: Indian, Pakistani, Bangladeshi, Other
- Chinese or Other ethnic group: Chinese, Other

4. What is the highest level of education you have achieved?

- GCSE
- Alevel

- Degree
- Masters
- Postgraduate

5. Are you currently a student?

- Yes
- No

If NO, please state your current employment:

6. Are you currently receiving any treatment for a physical or mental health condition?

If YES and you are prepared to say, please provide details below:

- Yes
- No

7. Are you currently using any kind of treatment to help you reduce or stop drinking alcohol?

If YES, please provide details below:

- Yes
- No

8. How often do you have a drink containing alcohol?

- Never
- Monthly or less
- 2-4 times a month
- 2-3 times a week
- 4 or more times a week

9. How many drinks containing alcohol do you have on a typical day when you are drinking?

- 1 or 2
- 3 or 4
- 5 or 6
- 7 to 9
- 10 or more

10. How often do you have six or more drinks on one occasion?

- Never
- Less than monthly
- Monthly
- Weekly
- Daily or almost daily

11. Have you taken part in any other projects at UCL about your drinking over the past 2 weeks?

If YES, please state the name of the project and/or the researcher(s) in the box below:

- Yes
- No

12. What is your preferred method of contact?

- Email
- Telephone

Please provide details of your email address and mobile phone number in the box below:

END

Thank you for completing the questions.

One of our researchers will contact you shortly to let you know if you are eligible to take part in this research.

APPENDIX 8: INFORMED CONSENT SHEET

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 alcohol-related thoughts and feelings?’’

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.

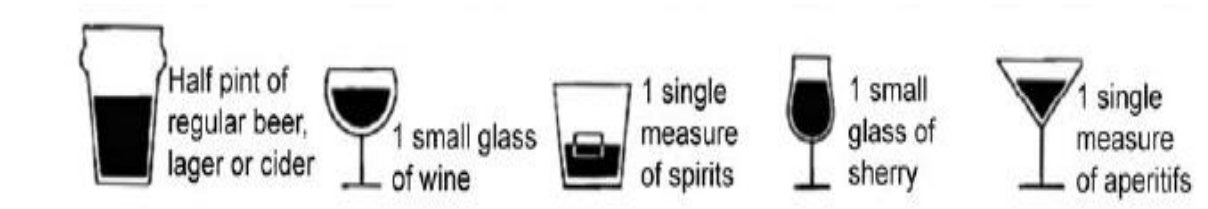
Signed:

Date:

APPENDIX 9: TIMELINE FOLLOW BACK UNIT GUIDE

UNIT GUIDE

Each of the drinks below contains 1 unit of alcohol:



Each of the drinks below contains more than 1 unit of alcohol:



APPENDIX 10: TIMELINE FOLLOW BACK INSTRUCTIONS

(presented on computer screen via PowerPoint)

Instructions

For this task you will be asked to describe your alcohol consumption as accurately as possible in the past week, using the calendar and unit guide provided.

To help you with this, mark down on the calendar any events that stand out (e.g. birthdays, nights out with friends, the weekend)

Next, on the calendar write down:

1. The amount of units of alcohol you consumed during those days.
2. The amount you consumed on the days preceding and following the event(s).

If you are unsure about the exact date you drank alcohol, just estimate as well as possible when you think the event occurred.

Please make sure you have filled in all of the seven days that are marked, even if the amount you drank was 0.

>Please inform the experimenter when you have completed this task<

APPENDIX 11: GUIDED IMAGERY INSTRUCTIONS

(presented on PowerPoint along with recordings)

Instructions

For this task you will be guided through different imaginary scenarios.

The scenarios are obviously fictitious but the task involves asking you to engage vividly in the fantasy.

When listening to the recordings close your eyes to help you imagine what is happening.

You will begin with a practice trial to familiarise you with the task.

To start the recordings, press the space bar and wait for the recording to play.

Once the recording has finished, press the space bar to continue.

When you are ready, press the space bar to start the practice trial.

APPENDIX 12: GUIDED IMAGERY SCRIPTS

(played through headphones attached to a computer)

Practice Imagery (Both Groups)

Movement: Walking through the Park

Imagine that you're walking through a large park.

It is a sunny day and you can see many trees as you move down the path.

You hear people talking in the distance as you walk along.

The smell of fresh-cut grass fills the air.

As you continue, you feel a slight chill from the wind.

You pass by a playground and see children playing.

You walk along and see a bird sitting in a tree.

You keep walking until your legs feel tired and you sit down on a nearby bench.

Looming Imagery 1: Conveyor belt

Movement: Conveyor Belt Progression

Imagine you are in a dimly-lit factory.

As you look around you can see various machines in darkened corners.

Suddenly you realize you are on a conveyor belt.

You feel its tough texture beneath you.

In your hand you are holding your usual alcoholic drink.

As you begin to drink the conveyor belt begins to move.

Faintly you hear the hum of the conveyor belt motor as it slowly carries you along.

While you take another sip of your drink, the conveyor belt moves faster.

You realize the more you drink the faster the conveyor belt becomes.

You can now see that at the end of the conveyor belt are two paramedics holding a stretcher, waiting to take you away.

The conveyor belt moves quicker and quicker as your cup becomes emptier and emptier.

The realization that liver disease lies at the end of the conveyor belt now dawns upon you.

Your throat becomes wet from the drink you are swallowing and you feel a slight pain in your abdomen as you approach your impending fate.

You try to get off the conveyor belt, but it is moving too fast.

The conveyor belt will slow down little by little if you drink significantly less alcohol, but you can only stop it altogether if you stick to drinking alcohol within safe limits.

Looming Imagery 2: Office building with calendar pages

Movement: Accelerated progression of time and symptoms

Imagine you are standing alone in an office.

As you look around, you notice a page-a-day calendar mounted on the wall.

As you pour your usual alcoholic drink into a cup the first of many calendar pages tears off.

You feel the cup between your fingers as you bring it to your lips.

As you take your first sip the days start to go by faster and faster.

As the taste of alcohol fills your mouth you notice that instead of days passing by one by one, now entire weeks begin to tear off quicker and quicker.

Soon pages begin flying off the calendar.

As your cup becomes emptier you feel a slight pain in the upper part of your abdomen as your stomach starts swelling up.

You realise these are symptoms of liver disease caused by drinking harmful levels of alcohol.

While you stand there watching the calendar, you feel your shoulders becoming warmer and colder as the seasons change at an ever-increasing speed.

It's now hard to hear over the flapping of the pages as they fly off the wall.

You see the pages begin to blur together at an incredible rate of speed.

You can slow down the rate at which you develop these symptoms if you drink significantly less alcohol, but can only stop this altogether if you stick to drinking alcohol within safe limits.

Looming Imagery 3: Doctor approaching

Movement: accelerating progression of doctor delivering a diagnosis of liver disease

Imagine that you are standing alone at the end of a long, narrow corridor.

All you can hear is the sound of faint beeping noises coming from behind the doors either side of you.

On your bare feet you can feel the cold hard surface beneath you.

In your hand you are holding your usual alcoholic drink.

Far away across the other end of the corridor you notice a tall figure standing there.

As you take your first sip you see the figure begin to approach you.

As they get closer you recognize the person is a male doctor.

You suddenly realise that you are in a hospital.

As you drink more you hear the doctor's steps coming closer and getting louder as he approaches you faster and faster.

You realise that the more you drink the faster the doctor approaches you.

When your drink is nearly empty you see the doctor is only a few steps away with a look of concern and sympathy on his face.

In his hands he is holding a white piece of paper marked 'liver test results'.

It dawns on you that he is about to diagnose you with liver disease.

You can increase the amount of time until you receive a diagnosis of liver disease if you drink significantly less alcohol, but you only stop this from happening altogether if you stick to drinking alcohol within safe limits .

Looming Imagery 4: Changing appearance (jaundice)

Movement: accelerating growth of symptoms

Imagine you are walking to a bar having just finished work.

As you walk down the busy street you can hear people talking excitedly about their weekend plans.

Once you arrive at the bar you order your usual alcoholic drink.

You sit down on an empty table nearby.

As you take your first sip you turn your head to see all the people that are inside.

Whilst looking you catch a glimpse of your reflection in a mirror hanging on the wall opposite you.

You can see that the colour of your face is suddenly turning yellow.

As you look closer at your reflection you notice that when you take large gulps of your drink your face becomes more and more yellow by the minute.

As you continue to drink you see the whites of your eyes rapidly taking on a dark yellow glow.

You realise that these are symptoms of liver disease that are caused by drinking harmful levels of alcohol.

As your drink is nearly empty you begin to feel more and more nauseous and weak.

You can slow down the rate at which your skin is yellowing and you are feeling unwell if you drink significantly less alcohol, but you can only stop this altogether if you stick to drinking alcohol within safe limits.

Control Imagery 1: Escalator

Movement: Rise of the Escalator

Imagine you are at a mall early in the morning as the stores are beginning to open.

As you walk along you come to the escalator and decide to get on.

The escalator slowly begins to take you upward.

You feel the cold rubber of the handrail as you reach out for it.

The escalator starts to bring you steadily higher and higher.

You are moving at a constant pace.

As the escalator carries you upward you can smell the various foods at the food court.

You faintly hear the hum of the escalator as it carries you along.

You continue to approach the 2nd floor.

As the escalator gradually rises you see a variety of stores.

You are in no hurry, so you stand still on the escalator as it steadily takes you to the second floor.

As you get close to the top, you see someone putting up a sale sign in a window.

You finally get to the top of the escalator, step off, and walk into a store.

Control Imagery 2: Office building with magazine pages

Movement: Progression through Pages

Imagine you are sitting alone in an office, reading a magazine.

As you look around, you notice a page-a-day calendar mounted on the wall.

As you turn the pages of the magazine you notice the first of many calendar pages tears off.

You continue to flip through the pages of the magazine one by one.

The pages feel crisp and cool.

You go through several articles, some of which are interesting or funny to you and some of which are not.

As you turn through the pages you see various advertisements.

You begin to daydream a little as you look at a picture.

As you look up from the magazine you notice on the calendar that instead of days passing by one by one, now entire weeks begin to tear off quicker and quicker.

Soon pages begin flying off the calendar.

While you sit there watching the calendar, you can feel your shoulders becoming warmer and colder as the seasons change at an ever-increasing speed.

It's now hard to hear over the flapping of the pages as they fly off the wall.

You see the pages begin to blur together at an incredible rate of speed.

Control Imagery 3: Postman approaching

Movement: accelerating progression of postman approaching

Imagine that it is early in the morning and you are standing alone outside a shop on a quiet high-street.

Up above you can see the sun slowly rising higher in the sky.
You enjoy the feel of its warm rays as they gently brush your face.
All around you can hear the sound of shopkeepers and workers discussing their plans for the day ahead.
In your hands you are clasping a bottle of water.
As you take your first sip you look across the street and see a figure in the distance.
Whilst you are drinking the water you can see the figure approaching.
He gradually moves closer and then starts to pick up his pace.
As the figure gets closer you recognise the person is a postman.
As you keep watching you can see he is now running across the street.
You realise that he is late for the morning collection.
When he arrives at the post box he hurries to open it up.
Once the post box is open he rushes to fill his sack with the post that is now spilling out onto the street.
When the post box is empty you watch as he dashes off once again running quickly across the street.

Control Imagery 4: Changing appearance (shadows)

Movement: change of appearance

Imagine that it is an early afternoon on a hot summer's day.
You have just finished work and decide to walk to a park nearby.
As you approach the park you see it scattered with families, couples, and the odd person sitting alone.
You can hear the sounds of people chatting and laughing in the distance.
As you enter the park you find a quiet place to lie under a tree that is unoccupied.

Whilst lying there you place your arms and legs out towards the sun.

You enjoy the feel of the soft grass beneath you.

You watch as your legs gradually become covered by the shade as the sun starts to set.

As you lie there longer you see your arms start to become more shaded by the shadows from the tree.

As each minute passes the shadows keep on creeping further upwards until they reach the tip of your fingers.

You continue to watch as your arms and legs become darker and darker.

You lie there until you are completely covered in shade and you hear the far off sounds of talking and playing begin to gradually fade.

It is now getting dark so you decide to get yourself up and walk on home.

APPENDIX 13: PRE-EXPERIMENT SINGLE ITEM DRINKING QUESTIONS

1. What age did you start drinking alcohol?

2. Please select from the options below whether you have been drinking for YEARS, MONTHS or BOTH

- Years
- Months
- Both

3. In the box below please state the approximate number of YEARS, MONTHS or YEARS and MONTHS that you have been drinking for:

4. Please select whether you have been drinking at your current average weekly alcohol units for YEARS or MONTHS or BOTH.

- Years
- Months
- Both

5. In the box below please state the approximate amount of YEARS, MONTHS or YEARS and MONTHS that you have been drinking at your current average weekly alcohol units:

6. Have you tried to quit or reduce your drinking?

- Quit
- Reduce
- Neither

If so, how many times?

7. If YES to 6, please select below whether you have been able to reduce or stop drinking alcohol for YEARS, MONTHS, WEEKS or a combination of these.

- Years
- Months
- Weeks
- Combination

In the box below please state the approximate amount of YEARS, MONTHS, WEEKS or a combination of these that you were able to reduce or stop drinking for:

**APPENDIX 14: VISUAL ANALOGUE SCALE TO ASSESS IMMEDIATE
IMAGERY ANXIETY AND VIVIDNESS**

Instructions

The following questions are designed to assess your experience of listening to the recordings.

1. Please mark with a cross (X) on the line directly below how much anxiety you felt when hearing the recordings.

0 ————— 100

0 = none at all
100 = as much as imaginable

2. Please mark with a cross (X) on the line directly below how much it felt like you were part of the recordings?

0 ————— 100

0 = not vivid at all
100 = extremely vivid

APPENDIX 15: TASTE TEST VISUAL ANALOGUE SCALES

Instructions

For each of the drinks you just tasted, please mark with a cross (X) on the lines provided that best represents your agreement with each of these statements.

Beer

Unpleasant _____ Pleasant

Flat _____ Gassy

Bitter _____ Sweet

Tasteless _____ Strong taste

Orange juice

Unpleasant _____ Pleasant

Flat _____ Gassy

Bitter _____ Sweet

Tasteless _____ Strong taste

APPENDIX 16: TRANSFORMED AND ORIGINAL COMPARATIVE RISK PERCEPTION DATA

Table X
Means and standard deviations for transformed and original comparative risk data

| Group | Perceived risk measure | Time of assessment | |
|----------------------------------|--------------------------------|--------------------------------------|-------------------------------------|
| | | Immediate post test <i>M (SD)</i> | one week follow up <i>M (SD)</i> |
| Experimental (<i>n</i> = 23) | Comparative risk (original) | - 0.40 (0.73) | - 0.44 (0.82) |
| | Comparative risk (transformed) | 1.19 (0.26) | 1.84 (0.21) |
| Control (<i>n</i> = 15) | Comparative risk (original) | 0.00(0.65) | - 0.19 (0.53) |
| | Comparative risk (transformed) | 1.17 (0.21) | 1.78 (0.15) |

Note: For the transformed comparative risk ratings, higher scores reflect lower comparative risk.

APPENDIX 17: TRANSFORMED AND ORIGINAL TIMELINE FOLLOW BACK DATA (AVERAGE UNTIS CONSUMED)

Table X
Means and standard deviations for transformed and original timeline follow back data

| Group | <u>Time of assessment</u> | | | |
|----------------------------------|---|-------------------------|--|-------------------------|
| | Baseline average units <i>M (SD)</i> | | follow up average units <i>M (SD)</i> | |
| | Original TFL data | Transformed TFL data | Original TFL data | Transformed TFL data |
| Experimental (<i>n</i> = 23) | 26.28 (16.04) | 1.35 (0.27) | 27.08 (20.79) | 1.30 (0.38) |
| Control (<i>n</i> = 15) | 36.21 (19.22) | 1.49 (0.27) | 34.13 (17.80) | 1.47 (0.27) |