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Author:	Carly Moses
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The 'Jumping to Conclusions' Bias: A Meta-Analysis and

Empirical Investigation of the Effectiveness of Implementation

Intentions

By Carly Moses

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DECLARATION

I hereby declare that this thesis has not been, and will not be, submitted in whole or in part to another University or institution for the award of any other degree.

EDITED ETHESIS

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STRUCTURE

For university approval of the specified journals see Appendix A.1.

1.   Literature Review:

The 'Jumping to Conclusions' Bias in Delusions: A Meta-Analysis.

Prepared according to current guidelines for contributors to *Clinical Psychology Review* (Appendix A.2).

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2.   Research Report

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ABSTRACT

The reasoning of people with delusions is characterised by a 'Jumping to Conclusions' (JTC) bias. A meta-analysis of the empirical literature retrieved 54 effect sizes of the JTC bias from 30 papers. The magnitude of the effects implied that JTC is a robust phenomenon. The JTC effect was largest ($d_+ = .58$) when measured by the amount of information requested to make a decision on probabilistic reasoning tasks. An analysis of methodological and theoretical factors which moderated JTC found that defining delusions by diagnosis of delusional disorder ($d_+ = .74$) and employing black and white beads ($d_+ = .87$), were associated with the largest effects. The degree of variance in effects indicates caution when interpreting the findings.

An empirical study investigated JTC in eighty-five non-clinical participants divided into high and low paranoia groups. The study aimed to explore the impact of task variations on JTC and demonstrate the effectiveness of implementation intentions ('if-then' plans) in reducing hasty decision-making on probabilistic reasoning tasks. Given there was no evidence of JTC in the present sample, the findings do not support a role for JTC in the formation of delusions. There was a non-significant trend indicating that forming an implementation intention increased the amount of information requested. Task difficulty and bead colour also influenced the amount of information requested. Implementation intentions appeared most effective when the task was difficult and paranoia was high. Caution is required in drawing conclusions from these findings due to the limitations of the study.

ACKNOWLEDGEMENTS

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SECTION 1: LITERATURE REVIEW

The 'Jumping to Conclusions Bias' in Delusions: A Meta-Analysis

ABSTRACT

The reasoning of people with delusions is characterised by a 'jumping to conclusions' (JTC) bias. There has been considerable increase in research pertaining to JTC in recent years. The present study updated and extended previous reviews of the literature by employing meta-analysis to determine the magnitude of the bias. From 30 papers, 54 effect sizes of the JTC bias were retrieved. The amount of information requested on probabilistic reasoning tasks best distinguished the presence of delusions as indicated by a medium-to-large-sized effect of $d_+ = .58$. The review identified methodological and theoretical factors which moderated the JTC bias: definition of delusions; comparison sample; type of task; ratio of stimuli and colour of beads. Of these, defining delusions by diagnosis of delusional disorder and employing black and white beads were associated with the largest JTC effects ($d_+ = .74$ and $d_+ = .87$, respectively). Theoretical implications and limitations of the review are considered. Caution is required when interpreting these findings given the degree of variance in effects and the inability to comment on interactions between moderating variables.

Keywords: jumping to conclusions, reasoning bias, delusions, meta-analysis

INTRODUCTION

The Jumping to Conclusions Bias and Delusions

The formation and maintenance of delusional beliefs involves complex, multifactorial, psychological processes including affect, motivation and perception (e.g., Garety & Hemsley, 1994; Freeman, 2007). Of these, reasoning style has received considerable interest. The 'jumping to conclusions' (JTC) bias refers to the tendency for individuals with delusions to make decisions based on less information than individuals not experiencing delusions, on probabilistic reasoning tasks (Garety & Freeman, 1999; Fine, Gardner, Craigie & Gold, 2007; Freeman, 2007).

The most widely adopted method to investigate JTC is the 'beads task' (Huq, Garety & Hemsley, 1988; Garety, Hemsley & Wessely, 1991). In the classic paradigm, participants are shown two jars containing two different coloured beads (e.g., red and green). In one jar the proportion of red to green beads is 85:15; in the other jar the proportion is 15:85, respectively. The jars are removed from sight and a predetermined sequence of beads, purporting to come from one of the jars, is shown one at a time. Participants are instructed to decide which jar the beads are being drawn from.

Different dependent variables have been employed to examine JTC. To explore the assumption that people experiencing delusions accept hypotheses on the basis of limited evidence, most studies employ the 'draws-to-decision' (DtoD) measure. A JTC bias is deemed present when individuals with delusions make a decision based on less information than a control group. Some authors have

employed the stricter criteria of 'extreme responding' where JTC is defined as making a decision based on one or two beads (e.g., Startup, Freeman & Garety, 2008; Garety et al., 2005). Where 'certainty' ratings are used, the methodology is altered slightly: participants are shown the whole sequence of beads and, for each bead, are asked to decide which jar the bead has been drawn from and how certain they are of that decision. Mean certainty ratings and the number of beads seen before reporting a high level of certainty are recorded (e.g., Colbert & Peters, 2002; Peters & Garety, 2006). Some studies have used a 'graded estimates' variant where certainty is reported on an ordinal scale e.g., definitely, almost certainly, probably or no preference (e.g., Young & Bentall, 1997).

To explore how readily individuals with delusions alter their hypothesis in response to new information two further measures have been employed. 'Response to potentially disconfirmatory evidence' (change in certainty when presented with a single bead which potentially disconfirms the initial hypothesis; e.g., Garety, et al., 1991; Fear & Healy, 1997) and 'response to reversal' (change in certainty when the sequence of beads is predetermined to initially indicate one jar before shifting to indicate the other jar (e.g., Young & Bentall, 1997; Langdon, Ward & Coltheart, 2010).

Potential Mechanisms of JTC in Delusions

The development of different dependent measures was driven by various theoretical accounts of the mechanisms underlying JTC and the relationship with delusions. For instance, DtoD can be employed to explore cognitive accounts. One such account proposes that people with delusions are unable to

integrate new information into existing hypotheses therefore make hasty decisions to avoid sequential information (Young & Bentall, 1997). Others suggest that JTC reduces the amount of cognitive effort needed to complete tasks (John & Dodgson, 1994). Alternatively, people experiencing delusions may have lower thresholds for accepting hypotheses meaning that they require less information to make a decision ('liberal acceptance': Moritz & Woodward, 2005). Measures of response to potentially disconfirmatory evidence are employed to examine motivational accounts. For example, a 'need for closure' suggests that people with delusions cannot tolerate the uncertainty that arises from contradictory evidence and so reduce their discomfort by altering their decision in line with new evidence (Colbert & Peters, 2002).

Previous Reviews of the Literature

Garety and Freeman (1999) examined the empirical JTC literature between 1987 and 1999. Of the eight studies reviewed, they found no support for people experiencing delusions having difficulties making probability estimates (i.e., which jar the bead came from) when presented with a fixed number of stimuli. Studies which employed DtoD as the dependent variable did support the JTC bias, but variations existed in the findings. The choice of comparison group affected the ability to differentiate groups experiencing delusions from control groups. Further, modifying the task to increase the emotional salience of the presented stimuli (e.g., positive and negative adjectives) exacerbated the JTC bias across experimental *and* control groups, but this trend was more exaggerated in delusional groups. Their findings suggest that the dependent measure and type of stimuli employed influence the magnitude of JTC. Garety and Freeman (1999, p. 131) conclude that 'people with delusions [show] a

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tendency to seek less information to reach a decision but not, when presented with information, being unable to use it'. That is, they demonstrate a data-gathering bias.

Two reviews focused specifically on persecutory delusions (i.e., the belief that another person intends to harm the individual; Freeman & Garety, 2000). Bentall, Corcoran, Howard, Blackwood and Kinderman (2001) confirm that JTC represents a data-gathering bias and propose that it is one of a number of biases which contribute to the development *and* maintenance of delusions. Freeman (2007) reported that all of the 10 studies reviewed demonstrated that delusional groups made hastier decisions than control groups. Based on evidence of JTC in individuals at risk from developing psychosis and in those with remitted delusions, he concurs that the data-gathering style is implicated in both the formation and maintenance of delusions. However, neither review was able to draw firm conclusions regarding the contribution of JTC specifically to persecutory delusions, because few studies only employed a sample comprising individuals experiencing persecutory delusions; most studies employed mixed delusional samples and failed to report subtypes.

Fine et al. (2007) employed meta-analysis to quantify the conclusions of previous authors. In total, 47 effect sizes were extracted from 12 studies. Using the Stouffer *Z* method, data from individual studies were pooled to determine whether the combined effects reached significance. This method focuses on joint significance, not the magnitude of the effect (Darlington & Hayes, 2000). Accordingly, Fine et al. (2007) did not report aggregated effect sizes. However, they conducted focused comparisons allowing them to comment on differences

between Z values. They found that the effect size for DtoD was significantly larger than effects sizes for other dependent measures (draws to certainty, $Z = 3.79$; response to potentially disconfirmatory evidence, $Z = 3.14$; and response to reversal, $Z = 4.22$; all $p < .001$) supporting a data-gathering bias in people experiencing delusions. Consistent with Garety and Freeman (1999), Fine et al. (2007) found that when the comparison group was recruited from clinical populations the effect size was smaller than when the comparison group was drawn from non-clinical populations ($Z = 2.16$, $p < .01$).

Fine et al. (2007) also explored whether JTC was specific to delusions or an epiphenomenon of schizophrenia (i.e., the consequence of general cognitive impairment associated with schizophrenia). The presence of delusions did differentiate participants diagnosed with schizophrenia suggesting JTC is not an epiphenomenal effect. Further, delusion-proneness was associated with hasty decision-making, indicating that JTC contributes specifically to the formation of delusions. However, the information needed to analyse the association between JTC and individual symptoms of schizophrenia was not reported in the original studies. Consequently, Fine et al. (2007) concluded that it was not possible to fully support the specificity of JTC to delusions.

Contrary to Garety and Freeman (1999), Fine et al. (2007) found no difference in effect sizes when comparing emotionally salient tasks with either neutral (i.e., beads) tasks or realistic (e.g., children's names) tasks, regardless of whether the comparison group was clinical or non-clinical. They concluded that JTC is pervasive regardless of the type of presented stimuli.

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Moderators of the JTC Bias

Reviews of the literature indicate variables which might impact on the presence of, or ability to detect, a JTC bias in people with delusions. The definition of delusions is important because the extent to which JTC is specific to delusions remains unclear (Fine et al., 2007). The majority of JTC studies have employed cross-sectional designs (i.e., comparison of participants with and without delusions at one time point). Therefore, we can only conclude that JTC is a state factor associated with the presence of delusions (Garety & Freeman, 1999).

The presence of the JTC bias in groups that are not currently experiencing delusions (e.g., in delusion-prone individuals) may indicate that the data-gathering bias is present before delusions occur and is therefore also a trait factor in individuals with a vulnerability to experiencing delusions (Freeman, 2007). Susceptibility to readily accepting anomalous beliefs may indicate that the reasoning bias is implicated in the formation of delusions.

Freeman (2007) also argues that if the bias exists in groups where delusions are remitted there is further evidence that JTC represents a trait factor because the reasoning style is pervasive regardless of the presence of delusions. However, Freeman (2007) neglects to consider the possibility that evidence of JTC where delusions are remitted could also indicate that the bias develops as a consequence of a previous delusional, or psychotic, state or the associated experiences e.g., societal stigma or compulsory admission to hospital. Rather than reasoning style contributing to the formation of delusions, it possible that the experience of delusions impacts on individuals' subsequent reasoning in

that they then make hastier decisions. Given that many studies exploring JTC employ samples of people diagnosed with schizophrenia, it is also possible that the presence of JTC where delusions are remitted also represents some neurocognitive impairment associated with schizophrenia (Lincoln, Ziegler, Mehl & Rief, 2010; Ziegler, Rief, Werner, Mehl & Lincoln, 2008).

The comparison sample employed may also impact on the JTC bias. For example, participants with Obsessive Compulsive Disorder (OCD) are overcautious on probabilistic reasoning tasks possibly due to a disconfirmatory reasoning style (Dudley & Over, 2003; Fear & Healy, 1997). Therefore, larger JTC biases may be expected when people with delusions are compared to people with OCD than when compared with non-clinical samples. Conversely, anxious participants perform more similarly to participants experiencing delusions possibly because they both demonstrate attentional biases to threat-related stimuli and perceive threat in neutral stimuli (e.g., Garety et al., 1991; Bensi & Giusberti, 2007; Dudley & Over, 2003). Consequently, employing a comparison sample comprising people with anxiety may mask a true JTC effect or reduce the size of the effect.

Further, the dependent measure of JTC has been shown to influence the ability to detect the bias (Fine et al. 2007). Related are the type of task adopted (i.e., neutral, emotionally/socially salient or realistic) and task difficulty as determined by the ratios in which stimuli are presented. Both variations have been shown to impact on the hastiness of decision-making, though the findings are not conclusive (e.g., Dudley, John, Young & Over, 1997a; Dudley et al., 1997b; Fine et al., 2007). Further, if JTC is related to general cognitive difficulties

associated with schizophrenia then the presence of a memory aid might reduce the size of the bias (Menon, Pomarol-Clotet, McKenna & McCarthy, 2006).

The colour of the beads employed in the classic paradigm varies across studies, although most have used either black and yellow or red and green beads (e.g., Garety et al., 1991; Moritz, Woodward & Lambert, 2007). Although not previously considered in the literature, the colour of beads potentially influences the JTC bias. Elliot, Maier, Moller, Friedman and Meinhardt (2007) proposed that colours have psychological connotations. Through repeated experiences of perceiving colour, learned associations develop. For instance, red is often associated with danger and therefore caution (e.g., traffic lights, warning signs) while green is associated with openness and space (e.g., fields). Therefore, colour is assumed to impact on motivation and attention. Avoidance motivation, and a narrowed field of attention, is likely to occur where colour is associated with danger, while approach motivation, and a widened field of attention, occurs where colour is associated with more benign environments (Elliot et al., 2007; Mehta & Zhu, 2009). Perhaps unsurprisingly, the impact of colour on motivation and attention has been found to affect performance on cognitive tasks (e.g., Elliot & Maier, 2007; Mehta & Zhu, 2009; Smeesters & Liu, 2011).

The Present Meta-Analysis

This meta-analysis aims to expand upon previous reviews by updating a meta-analysis of experimental studies employing probabilistic reasoning tasks to explore the JTC bias in people experiencing delusions (Fine et al., 2007). The number of studies included in previous reviews has ranged from eight (Garety &

Freeman, 1999) to 12 (Fine et al., 2007) indicating that the evidence base relating to data-gathering biases associated with delusions was modest. Clearly, there are potential difficulties in drawing inferences from such a small literature and therefore an update was deemed timely.

However, to conduct another narrative review of the literature could be criticised for lacking scientific rigour in extracting data and being vulnerable to bias due to inherent subjectivity (Greenhalgh, 2006). Moreover, by using the Stouffer Z method, Fine et al.'s (2007) meta-analysis focused on identifying combined significant effects across studies, but did not calculate the *size* of these effects. Lipsey and Wilson (2001) argue that the advantage of employing meta-analysis is the capacity to reflect on the magnitude and strength of significant outcomes. Therefore, the present review will also expand the previous meta-analysis by considering the size of effects in the JTC literature.

More specifically, the primary aim of the present review was to investigate which measures of reasoning may best distinguish people who do and do not experience delusions. It was of particular interest as to whether the present meta-analysis replicated previous findings suggesting that DtoD, and therefore the amount of information requested, best characterises the JTC bias in individuals experiencing delusions. Further meta-analyses will then examine whether theoretical or methodological characteristics of the studies (e.g., colour of the beads, the type of sample employed) moderate the size of the effect. By systematically exploring such variables, the present study aims to (a) clarify discrepancies identified in previous reviews, (b) consider theoretical implications

of how JTC relates to delusional experiences, and (c) describe implications for the design of future studies examining the JTC bias.

METHODS

Selection of Studies

Figure 1 presents the process of selecting studies for inclusion in the meta-analysis. Eligible studies were identified electronically through PsycINFO and Medline databases up to April 2011. Only English language published studies were included. Search terms had to be present in either the study title or abstract and included *jumping to conclusions, reasoning, delusions, data-gathering bias* and *beads task.* Further, the reference list of recent studies (defined as those published in the last five years) and that of Fine et al. (2007) were reviewed to identify any publications which had not emerged through the electronic search.

The inclusion criteria were: (a) the presence of a delusional experimental group and a non-delusional control group. This criterion was wider than that of Fine et al. (2007) as both clinical and non-clinical, delusion-prone, participants were included as experimental groups. This is consistent with a continuum perspective of psychotic experiences that allows inferences to be made about clinical experiences based on the study of non-clinical samples (Myin-Germeys, Krabbendam & van Os, 2003); (b) employment of the 'beads task' methodology or an adaptation that remained consistent with the probabilistic principles of the paradigm; and (c) employment of at least one of the dependent variables of

interest: DtoD, certainty ratings, response to potentially disconfirmatory evidence or response to reversal.

When reviewing study abstracts, the following exclusion criteria were: (a) duplicate studies; (b) unpublished studies. As published studies are easier to locate, unpublished studies were excluded for convenience. Further, it could be argued that unpublished studies lack methodological rigour resulting in larger individual effect sizes which would artificially inflate the aggregated effect size; (c) studies of reasoning unrelated to the delusional continuum.

Full-text papers were subsequently read. Further exclusion criteria included the method diverging too far from probabilistic principles to be comparable, and review papers. Consistent with the primary aim of determining which dependent variable best distinguishes people with and without delusions, studies without a comparison group were also excluded.

Diagram removed in concordance with copyright legislation.

Figure 1: PRISMA flow diagram of the selection of studies for the meta-analysis (adapted from: Moher, Liberati, Tetzlaff, Altman & The PRISMA Group, 2009)

The search revealed 30 papers eligible for inclusion in the meta-analysis. When compared with the number of studies included in previous reviews, the result of present search is indicative of the considerable interest in the JTC bias and expansion in this area. The increase is particularly notable given that previous reviews included correlational studies, 21 of which were excluded from this review. Of Fine et al.'s (2007) 12 studies, 10 were included in this meta-analysis. One was excluded because there was no comparison group while the second was an unpublished study.

Meta-Analytic Strategy

Coding the Studies

Study characteristics that may contribute to variation in the JTC effect were coded by the author. The coding scheme was informed by both theoretical and methodological factors, and comprised the following variables:

(a)   Sample size for the experimental (N_E) and control (N_C) groups.

(b)   The effect size for differences between the experimental and control groups for each dependent variable (see below for meta-analytic approach).

(c)   Definition of delusions: schizophrenia/schizoaffective disorder; mixed psychotic diagnoses (including schizophrenia/schizoaffective disorder, bipolar disorder, delusional disorder); delusional disorder; first-episode psychosis and delusion-prone.

(d)   Nature of the control group: clinical (including depression, anxiety and psychosis without current delusions) or non-clinical.

(e)   Dependent variable: DtoD, certainty, response to potentially disconfirmatory evidence or response to reversal.

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(f)   Type of beads task: original, socially/emotionally salient or realistic/novel.

(g)   Ratio of stimuli in the task: 85:15, 60:40 or multiple ratios comprising more than two 'jars' of beads e.g., 44:28:28 (Broome et al., 2007).

(h)   Presence of a memory aid.

(i)   Colour of beads where the original paradigm was employed: Red/Green, Black/Yellow and Black/White.

Extracting Effect Sizes

From 30 papers, 54 effect sizes were extracted across the four dependent measures of JTC (Table 1). DtoD was the most widely adopted dependent variable with effect sizes calculated from 28 studies (93.3%). Just under half of the studies (n = 13, 43.3%) employed certainty ratings, while response to potentially disconfirmatory evidence and response to reversal were less frequently adopted with eight (26.7%) and six (20%) studies employing these dependent variables, respectively.

In terms of validity, meta-analysis assumes independence between samples. To avoid violating this assumption and introducing bias into the overall aggregated effect size, attempts were made to ensure data from the experimental groups were used only once when extracting effect sizes. Accordingly, where studies recruited more than one control group, the data from the non-clinical sample were used to calculate the effect size. Non-clinical samples were the most frequently adopted control group in the studies. Furthermore, given that reasoning may vary by clinical presentation, it was assumed that non-clinical samples would be more homogenous therefore introduce less variability into the aggregated effect size.

For the initial meta-analysis, effect sizes were calculated for each dependent measure. However, mean effect sizes were calculated where a number of tasks were completed (e.g., neutral and emotionally salient) and across ratios. Moderator analysis comprised further meta-analyses of individual effect sizes relating to these study characteristics (e.g., type of task, ratio).

Where the information needed to calculate the effect size (i.e., mean and standard deviation) was not reported, an estimate was made based on the reported significance level. If the finding was non-significant a conservative effect size of $d = .00$ was assumed.

Table 1

Study characteristics and effect sizes for measures of JTC included in the meta-analysis

Author(s) (year)	Sample				Effect Size (d)			
	Experimental	N_E	Control	N_C	DtoD ($k = 29$)	Certainty ($k = 14$)	Disconfirm. Evidence ($k = 7$)	Reversal ($k = 6$)
Brankovic & Paunovic (1999)	Schizophrenia	29	Non-clinical	35			-0.72	
Broome *et al.* (2007)	At risk mental state	35	Non-clinical	23	0.50 [a]			
Colbert & Peters (2002)	Delusion-prone	34	Non-clinical	34	0.80	0.29	-0.63	
Colbert, Peters & Garety (2010)	First-episode psychosis	17	Non-clinical	35	-0.26 [a]			
Conway, Bollini et al. (2002)[*]	Delusional Disorder	10	Non-clinical	10	0.92			
Corcoran *et al.* (2008)	Various diagnoses	39	Non-clinical	33	0.71 [a]			
Dudley *et al.* (2011)	First-episode psychosis	35	Psychosis (no current delusions)	49	0.03 [a]			
Dudley, John, Young & Over (1997a)[*]	Various diagnoses	15	Non-clinical	15	0.93 [a]			
Dudley, John, Young & Over (1997b)[*]	Various diagnoses	12	Non-clinical	12	1.39 [a]			0.42 [a]
Fear & Healy (1997)[*]	Delusional Disorder	22	Non-clinical	30	0.94	0.17	-0.82	0.00
Fraser, Morrison & Wells (2006)	Schizophrenia / Delusional Disorder	15	Non-clinical	15	0.50 [a]			
Freeman, Pugh, et al. (2010)	Current persecutory delusions	30	Non-clinical	30	0.56			
Garety, Hemsley & Wessely (1991)[*]	Delusional Disorder	14	Non-clinical	14	0.72		0.95	0.16
Huq, Garety & Hemsley (1988)[*]	Schizophrenia	15	Non-clinical	15	0.97	0.94		
Keefe & Warman (2011)	Delusion-prone	67	Non-clinical	66	0.00 [c]	0.40 [a]		
Langdon, Ward & Coltheart (2010)	Schizophrenia / schizoaffective disorder	35	Non-clinical	34	0.68	0.08	0.97 [b]	0.39

Author(s) (year)	Delusional	N_E	Control	N_C	DtoD	Certainty	Disconfirm. Evidence	Reversal
Lincoln, Salzmann, et al (2011)	Non-clinical high paranoia	32	Non-clinical	31	0.53	0.54		
Lincoln, Ziegler, Mehl & Rief (2010)	Current delusions	44	Non-clinical	68	0.43 [a]			
McKay, Langdon & Coltheart (2007)	Various diagnoses	22	Non-clinical	19	0.37	0.86		
Menon, Pomarol-Clotet, et al (2006)[*]	Schizophrenia	18	Non-clinical	18	0.33 [a]			
Moore *et al.* (2006)	Late-onset psychosis	29	Non-clinical	30	0.27			
Moritz & Woodward (2005)[*]	Schizophrenia	17	Non-clinical	17	0.80	0.49		0.52
Moritz, Woodward & Lambert (2007)	Schizophrenia	37	Non-clinical	37	0.56 [a]	0.47		
Peters & Garety (2006)[*]	Various diagnoses	23	Non-clinical	36	0.43 [a]	-0.73 [a]	0.87	
Peters, Thornton, Siksou, et al (2008)	Psychosis	21	Psychosis (no current delusions)	16	0.39	-0.43	-0.15	
So, Freeman & Garety (2008)	First-episode psychosis	30	Non-clinical	30	0.59 [a]			
Startup, Freeman & Garety (2008)	Current persecutory delusions	28	Non-clinical	30	0.48			
Warman (2008)	Delusion-prone	35	Non-clinical	35	0.15 [b]	0.55		
White & Mansell (2009)	Delusion-prone	17	Non-clinical	22	3.26			
Young & Bentall (1997)[*]	Schizophrenia/ Delusional Disorder	20	Non-clinical	20		0.51 [a]	-0.86 [a]	-0.67 [a]

[*] Included in Fine et al. (2007)
[a] Mean effect size across tasks
[b] Effect size estimated from probability
[c] Assumed conservative effect size

Meta-analytic Approach

The unbiased effect size estimator *d* was employed for the meta-analysis (Hedges & Olkin, 1985). Effect size *d* was calculated using the formula:

$$d = \frac{x_1 - x_2}{(sd_1 + sd_2)/2}$$

where x_1 = mean of experimental (delusional) sample, x_2 = mean of control sample, sd_1 = standard deviation of experimental sample and sd_2 = standard deviation of control sample. The calculations were conducted using the META program (Schwarzer, 1988). A random effects model was employed because it allows for the possibility that there are multiple true effects which differ between studies due to variations in, for example, sample, task or experimental manipulation (Borenstein, Hedges, Higgins & Rothstein, 2009). Sample-weighted average effect sizes (d_+) were interpreted using Cohen's (1992) guidelines: *d* = .20 constitutes a small effect size, *d* = .50 a medium effect size and *d* ≥ .80 a large effect size.

RESULTS

Comparison of the dependent measures of the JTC bias

The initial analysis explored the difference between participants with and without delusions for each of the dependent variables commonly employed to measure JTC. Figure 2 presents the range of effect sizes (*d*) for each dependent measure.

DtoD		Certainty		Disconfirm. Evidence		Reversal	
Stem	Leaf	Stem	Leaf	Stem	Leaf	Stem	Leaf
-0.8		-0.8		-0.8	62	-0.8	
-0.7		-0.7	3	-0.7	2	-0.7	
-0.6		-0.6		-0.6	3	-0.6	7
-0.5		-0.5		-0.5		-0.5	
-0.4		-0.4	3	-0.4		-0.4	
-0.3		-0.3		-0.3		-0.3	
-0.2	6	-0.2		-0.2		-0.2	
-0.1		-0.1		-0.1	5	-0.1	
-0.0		-0.0	8	-0.0		-0.0	
0.0	03	0.0	8	0.0		0.0	0
0.1	5	0.1	7	0.1		0.1	6
0.2	7	0.2	9	0.2		0.2	
0.3	379	0.3		0.3		0.3	9
0.4	338	0.4	079	0.4		0.4	2
0.5	003669	0.5	145	0.5		0.5	2
0.6	8	0.6		0.6		0.6	
0.7	1	0.7		0.7		0.7	
0.8	00	0.8	6	0.8	7	0.8	
0.9	2347	0.9	4	0.9	57	0.9	
1.0		1.0		1.0		1.0	
1.1		1.1		1.1		1.1	
1.2		1.2		1.2		1.2	
1.3	9	1.3		1.3		1.3	
.		.		.		.	
.		.		.		.	
3.2	6	3.2		3.2		3.2	

Figure 2. Stem-and-leaf plots of effect sizes (*d*) for the dependent measures

Stem digits represent the ones and tenths of effect size places. Leaf digits are the hundredths places. Multiple leaf digits reflect multiple effect sizes (e.g., 0.33, 0.37, 0.39).

Table 2 shows the sample-weighted effect sizes (d_+) for the dependent measures. Effect sizes for DtoD ranged from -.26 to 3.26 with a standard deviation (*SD*) of .60. The significant overall sample-weighted effect size of $d_+ =$ .58 (*p* <.001) is considered medium-sized according to Cohen's (1992) criteria. Measures of certainty had a small-to-medium-sized effect ($d_+ = .30$) with effect sizes ranging from -.73 to .94 (*SD* = .46); again the size of the effect was significant (*p* <.01). Effect sizes ranged from -.86 to .97 (*SD* = .89) for response to potentially disconfirmatory evidence and from -.67 to .52 (*SD* = .43) for response to reversal. Effect sizes for both the latter dependent measures were

considered small at $d_+ = -.04$ for response to potentially disconfirmatory evidence and $d_+ = .13$ for response to reversal and neither were significant.

Table 2
Effect sizes for the dependent measures of the JTC bias

Dependent Measure	N	k	Q	d_+	95% Confidence Interval
DtoD	1558	28	64.01*	.58*	.36 to .80
Certainty	770	13	27.85**	.30**	.06 to .55
Disconfirm. Evidence	380	7	56.11*	-.04	-.70 to .62
Reversal	253	6	8.88	.13	-.21 to .47

* $p < .001$, ** $p < .01$

Moderator Analysis

The homogeneity statistic indicated that there was no significant variation in the effect sizes for response to reversal, Q (5) = 8.88, *ns*, suggesting that any variance could be accounted for by sampling error alone (Lipsey & Wilson, 2001). However, the homogeneity statistic was significant for the remaining three dependent measures: DtoD, Q (27) = 64.01, $p < .001$; certainty, Q (12) = 27.85, $p < .01$; response to potentially disconfirmatory evidence, Q (6) = 56.11, $p < .001$. This indicates that the sets were heterogeneous and that variation in effect sizes was due to factors others than sampling error alone. Additional meta-analyses focused on exploring whether a number of theoretical or methodological moderators contributed to the variation.

Although both certainty and response to potentially disconfirmatory evidence sets were heterogeneous, the total number of effect sizes (k = 13 and 7, respectively) were insufficient to allow further moderator analysis. Additionally, it was deemed more relevant to focus on variation in the DtoD set of effect sizes given that this emerged as the measure that best differentiates between the presence and absence of delusions. The following moderators were examined: definition of delusions; type of comparison group; task type; bead ratios and colour of beads.

Unfortunately, due to insufficient data, it was not possible to include memory aids in the moderator analysis. Although eight studies reported the presence of a memory aid and two specifically reported not employing a memory aid, the remainder failed to report any information.

Definition of Delusions

Table 3 presents the sample-weighted effect sizes for the difference between participants with and without delusions according to the definition of delusions employed. Delusion proneness had the largest effect size at d_+ = .88. According to Cohen's (1992) criteria the size of this effect was large. However, the effect only approached significance (p = .06) while the homogeneity statistic, Q (4) = 39.31, p <.001, indicated a large degree of variation within the set. When delusions were defined by first-episode psychosis the effect was small-to-medium-sized, but, again, did not quite reach significance (d_+ = .29, p = .06).

All sets of effect sizes, except delusion proneness, were homogeneous. Accordingly, with a significant effect size approaching large magnitude of d_+ =

.74 (p <.001), defining delusions by diagnosis of delusional disorder appeared to best differentiate between experimental and control groups in terms of JTC. Where the experimental group comprised participants with a diagnosis of schizophrenia or schizoaffective disorder, or mixed diagnoses (including schizophrenia) the effect sizes were of medium magnitude (d_+ = .51 and d_+ = .58; both p <.001).

Table 3
Effect sizes by definition of delusions

Definition of Delusions	N	k	Q	d_+	95% Confidence Interval
Schizophrenia /Schizoaffective	272	5	2.21	.51*	.27 to .76
Mixed psychotic diagnoses	529	10	7.25	.58*	.39 to .76
Delusional Disorder	110	3	.87	.74*	.35 to 1.13
First-episode psychosis	274	5	8.03	.29	-.09 to .20
Delusion-prone	373	5	39.31*	.88	-.26 to 2.02

* p <.001

Type of Comparison Group

Of the 30 papers included, 11 employed both clinical and non-clinical comparison groups (Table 4). Statistical comparison of the control groups was not conducted as this would involve using data from the experimental groups more than once, thereby violating the independence assumption. However, the data indicate that the size of the JTC effect was slightly smaller when the

comparison group was a clinical sample (M = .68) compared to a non-clinical sample (M = .72).

Effect sizes for the clinical comparison groups ranged from d = .05 to d = 1.28. A large effect size was found where participants experienced OCD (d = .96) whereas a small effect was found where participants experienced anxiety (e.g., d = .28). The impact on JTC when the comparison group comprised depressed participants is unclear, because the size of the effect ranged from small (d = .08) to large (d = 1.28).

In the non-clinical comparison group, effects sizes ranged from d = .27 to d = 1.39. With the exception of a comparison with older adults (d = .27) all effects were of medium-to-large magnitude. This size of the JTC bias was largest when participants were recruited from university staff (e.g., d = 1.39; d = 1.02).

Table 4

Effect sizes by comparison group

Author (s) (Year)	Clinical			Non-Clinical		
	Presentation	*n*	*d*	Recruitment	*n*	*d*
Dudley, John, Young & Over (1997a)	*Depression*	15	1.28	University staff	15	1.39
Dudley, John, Young & Over (1997b)	*Depression*	12	.97	University staff	12	1.02
Moore, Blackwood, et al. (2006)	*Major depressive disorder*	30	.08	Older adult lunch club	30	.27
Corcoran, Rowse, Moore, et al. (2008)	Major depression	27	.55	Advertisement	33	.71
Peters & Garety (2006)	Depression or anxiety symptoms	22	1.05	Psychology department participants pool	36	.43
Garety, Hemsley & Wessely (1991)	Anxiety: *GAD, agoraphobia, specific phobia*	14	.28	Hospital staff	14	.72
Fraser, Morrison & Wells (2006)	*Panic disorder*	15	.58	General population, informal contacts	15	.65
Fear & Healy (1997)	*OCD*	26	.96	Hospital staff and family/acquaintances	30	.94
Huq, Garety & Hemsley (1988)	Mixed psychiatric: *Depression, Manic-Depression, phobia, anxiety, eating disorder*	10	.91	Information not available	15	.97
Moritz & Woodward (2005)	Mixed psychiatric inpatients: *Agoraphobia/panic, depression, PTSD, personality disorder, social phobia*	28	.77	Hospital staff, general population	17	.80
Menon, Pomarol-Clotet, et al. (2006)	Schizophrenia (non-deluded)	16	.05	Health care/university staff	16	.68
	Mean Effect Size		.68	*Mean Effect Size*		.72

Italics denote diagnosis using Diagnostic and Statistical Manual of Mental Disorders (DSM)
GAD: Generalised Anxiety Disorder; PTSD: Post-traumatic Stress Disorder

Type of Task

Table 5 presents the 34 sample-weighted effect sizes for three types of tasks employed to explore JTC. The classic beads paradigm had a medium-to-large-sized effect of $d_+ = .60$ ($p <.001$) compared to a small-to-medium-sized effect of $d_+ = .30$ ($p <.05$) for the socially/emotionally salient task. The effect size of $d_+ = .33$ for realistic/novel tasks was not quite significant ($p = .06$) but this was the only paradigm that had a homogeneous set of effect sizes ($Q (3) = 4.49$, *ns*).

Table 5
Effect sizes according to task type

Type of Task	N	k	Q	d_+	95% Confidence Interval
Beads task	1365	24	61.52*	.60*	.35 to .86
Socially/emotionally salient	277	6	10.88**	.30	-.08 to .06
Realistic/novel	202	4	4.52	.33	-.09 to .74

* $p <.001$, ** $p <.05$
$k > 28$ because individual effect sizes were averaged across tasks in the initial analysis; one study was excluded because individual task data were not reported.

Ratio

Sample-weighted effect sizes indicate that the size of the JTC bias was similar when information was presented in ratios of 85:15 ($d_+ = .47$, $p <.001$) and 60:40 ($d_+ = .43$, $p <.001$; Table 6). Multiple ratios had the largest significant effect size ($d_+ = .58$, $p <.001$) and was the only homogeneous set ($Q (3) = .86$, *ns*).

Table 6

Effect sizes according to ratio of stimuli

Ratio	N	k	Q	d_+	95% Confidence Interval
85:15	688	14	29.67***	.47*	.22 to .71
60:40	854	14	25.87**	.43*	.22 to .64
Multiple ratios	278	4	.86	.58*	.34 to .83

* $p <.001$, ** $p <.01$, *** $p <.005$

$k > 28$ because individual effect sizes were averaged across ratios in the initial analysis; one study was excluded because individual ratio data were not reported.

Colour of beads

Of the 24 papers that employed the beads task, the colour of the beads employed was not reported in five papers. Six studies used different colours to those identified for coding (e.g., pink and blue; Keefe & Warman, 2011). However, there was insufficient data to constitute separate groups. In total, 15 studies contributed to the analysis (Table 7).

Table 7

Effect sizes according to colour of beads

Colour of Beads	N	k	Q	d_+	95% Confidence Interval
Red/Green	325	5	5.41	.39**	.13 to .67
Black/Yellow	399	7	6.66	.67**	.46 to .87
Black/White	194	3	6.23	.87*	.27 to 1.48

* $p <.01$, ** $p <.001$

All effect size sets were homogeneous indicating that effect sizes did not vary more than would be expected from sampling error alone. Furthermore, all effect sizes emerged as significant. With the largest effect size, black and white beads had a large JTC effect ($d_+ = .87$) suggesting these colours best distinguished between participants with and without delusions. Employing red and green beads only resulted in a small-to-medium-sized effect ($d_+ = .39$). The effect of JTC was medium-to-large-sized where black and yellow beads were employed ($d_+ = .67$).

DISCUSSION

The initial analysis compared the dependent measures commonly employed in the JTC literature. Consistent with the previous meta-analysis (Fine et al., 2007), DtoD had the largest, significant, effect size. Although Fine et al. (2007) did not report the magnitude of aggregated effect sizes, they presented the distribution, and median, of individual study effect sizes in a box-plot. Visual inspection of these data indicate that the average effect, using DtoD, was approximately double the size (*Mdn g* ≈ 1.00) of the average effect in the present review (*Mdn d* = .56). Because the effect size estimator, *g*, tends to overestimate effect sizes, we might expect the unbiased effect size estimator, *d*, to be smaller (Hedges & Olkin, 1985). However, the discrepancy between effect sizes may also reflect variation between studies, as indicated by the significant homogeneity statistic for DtoD in the present meta-analysis. Variation may be due to the increased number of studies included in the present review.

Comparison of the remaining dependent measures did not reveal such large discrepancies, possibly because there has not been as large an increase in the number of studies employing these measures compared to DtoD. The median certainty ratings in the present study ($d = .44$) were similar to that of Fine et al.'s (2007), $g \approx .40$, while their median effect size for response to potentially disconfirmatory evidence (*Mdn g* $\approx$ .50) was comparable to the present study albeit in the opposite direction (*Mdn d* = -.39). Effect sizes for response to reversal were only slightly smaller in this meta-analysis ($d = .28$ vs. $g \approx .35$).

Overall, the findings suggest that the most prominent characteristic of JTC is the tendency to request less information in decision-making, consistent with Garety and Freeman's (1999) conclusion that JTC represents a data-gathering bias. Further, DtoD is the measure that best distinguishes between participants with and without delusions in their performance on probabilistic reasoning tasks. The dependent measure employed reflects the theoretical account of interest. Although the present findings do not provide direct support for any theoretical account they also do not discredit accounts which rely on DtoD (e.g., impairments in sequential processing or liberal acceptance; Young & Bentall, 1997; Moritz & Woodward, 2005). Small effect sizes for both response to potentially disconfirmatory evidence, and reversal, suggest that people experiencing delusions do not alter their decision in response to contradictory evidence much more readily than controls. Therefore, it is unlikely that a 'need for closure' can account for the JTC bias because it would be expected that people with delusions would alter their decision quickly to reduce the discomfort associated with uncertainty (Colbert & Peters, 2002).

Moderating Variables

The variability in the effect size for DtoD was addressed by an analysis of potential moderators. The samples recruited by studies were considered first. Where the experimental group comprised clinical samples, defining delusions by diagnosis of delusional disorder was associated with the largest JTC effect. When delusions were defined as schizophrenia/schizoaffective disorder *or* mixed psychotic diagnoses there was only a medium-sized JTC effect. Taken together, these findings suggest that the data-gathering bias is specific to delusions and not an epiphenomenon of schizophrenia (Fine et al., 2007). However, because the information was not reported in the original studies, it was not possible to determine whether the JTC exhibited by participants with schizophrenia was related only to delusional symptoms or whether it could also be related to other symptoms (e.g., cognitive impairments).

The JTC effect was large when delusion-prone participants were employed. This is consistent with Fine et al. (2007) who also found a significant effect for JTC in delusion-prone participants (Z = 3.89, p <.01). The finding appears to support the role of the JTC bias in the formation of delusions. It would be tempting to argue that the size of the effect goes some way to address the discrepancies between studies considering JTC in the formation of delusions (Freeman, 2007). However, the effect did not quite reach significance and the large degree of variation in the set indicates caution. It is possible that different measures of delusion proneness could account for the variation. However, if JTC was associated with the formation of delusions, we would expect similar findings among participants with first-episode psychosis. Contrary to this

hypothesis, however, the effect was small-to-medium and, again, did not quite achieve significance.

The type of comparison group was found to moderate the JTC bias. Consistent with previous reviews (e.g., Garety & Freeman, 1999; Fine et al., 2007) the size of the JTC bias exhibited by participants with delusions was smaller when compared to a clinical control group than a non-clinical control group. This indicates that the presence of mental health difficulties, other than delusions, is also associated with reasoning that differs from that of non-clinical samples. Further, it appears that clinical samples reason more similarly to people with delusions, that is, they make hastier decisions.

The hastiness of clinical samples may, however, be an artefact of conservative reasoning in non-clinical samples (Philips & Edwards, 1966). Certainly, the largest effect sizes were found where participants were recruited from non-academic university staff who may have been more motivated to complete the task successfully and therefore requested more information (Fraser, Morrison & Wells, 2006). Despite equivocal findings where participants with depression were recruited, largely the findings from the clinical samples are consistent with existing evidence e.g., more cautious reasoning in people experiencing OCD and more hasty reasoning in people experiencing anxiety (e.g., Bensi & Giusberti, 2007; Fear & Healy, 1997). Although caution is needed in drawing conclusions, this indicates that overall, clinical samples do reason more similarly to delusional samples and this is not an artefact of conservative reasoning in non-clinical samples.

Moderator analysis of task variations revealed several interesting findings. The JTC effect did differ according to the type of task employed. When neutral stimuli were employed the JTC effect was of medium-to-large magnitude and significant. However, only small-to-medium-sized effects were found for both socially/emotionally and realistic/novel stimuli and neither effect was significant, suggesting that people with delusions request less information on neutral tasks than on socially/emotionally salient or realistic/novel tasks. This contrasts with Dudley et al.'s (1997b) assumption that increasing the emotional salience of the task would exacerbate JTC because people with delusions demonstrate attentional and memory biases for self-referent stimuli. Although some studies confirmed this assumption (e.g., Dudley et al., 1997b; Fraser, et al., 2006; Garety & Freeman, 1999), others have failed to find a difference between neutral and emotionally salient tasks (e.g., Colbert, Peters & Garety, 2010; Fine et al., 2007; Menon et al., 2006; Corcoran et al., 2008). If emotional salience does not impact on decision-making, the findings also contradict theoretical accounts which highlight the role of affect in the JTC bias (e.g., Dudley & Over, 2003).

It is possible that participants find socially/emotionally, and realistic/novel, tasks more interesting, therefore put more effort into completing them successfully (i.e., make a more informed decision). Further, other task variations (e.g., ratio) are known to impact on JTC and, therefore, may interact with the type of task. This is consistent with the considerable variation in the set of effect sizes for the beads tasks and the socially/emotionally tasks. Consequently, the findings and theoretical implications must be viewed with caution.

The effect size for all ratios was significant, suggesting that participants with delusions request less information than control participants, regardless of the difficulty of the task. Contrary to expectations, there was little difference in magnitude of the JTC effect between the ratios 85:15 and 60:40 indicating that there was no difference in the amount of information requested when the task was more difficult. The considerable variability in effect sizes reduces the confidence with which inferences can be drawn from these findings. Moreover, the effect was slightly larger for more difficult tasks where multiple ratios were employed, suggesting some sensitivity to task difficulty. However, the findings do indicate that people with delusions are less sensitive to task difficulty than previously assumed. This has considerable implications for theoretical accounts. For instance, based on participants' ability to respond appropriately to variations in task demand, Dudley et al. (1997a) concluded that JTC was not due to impulsivity or indiscriminate responding. Further, theoretical accounts have been discounted due to an inability to explain responsiveness to task difficulty (e.g., accounts positing impairments in integrating sequential information would predict that JTC would occur regardless of task difficulty; Young & Bentall, 1997).

Within the moderator analysis, the colour of the beads was the only variable to comprise all homogenous sets of effects sizes. The largest JTC effect was found where the beads were black and white. Where the beads were red and green the magnitude of the JTC bias was smaller. This is likely because the learned associations with the colours red and green may impact on motivation and attention and, therefore, performance on cognitive tasks (e.g., Elliot et al., 2007; Smeesters & Liu, 2011). However, further research is needed to

understand how colour specifically impacts on JTC. This is particularly important where red beads are employed because red can signify both danger *and* attractiveness, therefore has the potential to inhibit and improve task performance (Mehta & Zhu, 2009).

Limitations of the Present Study

The validity of the findings reported is potentially threatened by only using non-clinical comparison groups. The present study confirmed Fine et al.'s (2007) conclusion that when participants with delusions are compared with non-clinical controls the JTC effect is larger than when compared with clinical controls. Accordingly, the reported effect sizes may be inflated and represent an upward bias. However, it could be argued that this is less of a problem than the variability that would be introduced by employing clinical comparison groups where reasoning is likely to differ according to presentation.

In only including published studies, the present meta-analysis failed to account for publication biases (see Lipsey & Wilson, 2001). The tendency for studies with significant effects to be more readily published, and those without significant effects not to be published, was termed the 'file drawer problem' by Rosenthal (1979). Further, Lipsey and Wilson (1993) note that the effect sizes of published studies are generally larger than unpublished studies. Consequently, it is possible that the reported effect sizes reflect a further upward bias and that the inclusion of unpublished studies may reduce the magnitude of effects. Conversely, it could be argued that studies that are not published, lack methodological rigour which affects the validity of the findings and thereby introduces different biases to meta-analysis. With this in mind, the

present study is also vulnerable to limitations in the methodological quality of the original studies. For instance, many studies fail to report the impact of sample characteristics on dependent variables (Garety & Freeman, 1999). This is important given that we do not fully understand how some characteristics relate to the JTC bias (e.g., intelligence; Garety, et al. 1991; Mortimer et al. 1996). Accordingly, the present study would have benefitted from coding the methodological quality of the original studies and incorporating it into the moderator analysis to determine the impact on the magnitude of the JTC effect.

The present study only employed one rater to code the original studies. The reliability of this procedure would have been vastly improved by having another, independent rater also code the studies. In addition to addressing any coder biases, a second rater would also have been useful in ensuring the accuracy of effect size calculations.

Finally, the amount of variation within the effect sizes for the moderator variables does raise questions about the confidence with which inferences can be drawn from the present findings. The present study is unable to comment on the interactions between moderating variables. For instance, it is possible that the ratio of beads *or* the colour of beads could account for the variation in the effect size for the beads task. Alternatively, the type of task could account for variations in the effect size for different ratios. Consequently, the present findings must be viewed with caution.

Further Research

It is apparent that the ability to detect JTC in people with delusions is influenced by a number of methodological factors, therefore the present review has important implications for the design of future studies exploring the bias. For instance, employing DtoD as the dependent measure is most likely to detect the bias. Further, variables which moderate JTC should be considered when designing methods and analysing data. The latter point is perhaps most important if we are to better understand how methodological variables interact and impact on the JTC bias.

The specificity of the JTC bias remains an area of interest. This review concludes that JTC is not solely an artefact of schizophrenia and may indeed reflect trait and state factors in delusions. However, systematic exploration of the association between JTC and other symptoms (e.g., attentional or memory difficulties) associated with schizophrenia is still required to conclude the bias is specific to delusions. Studies could contribute to this endeavour through more comprehensive reporting about samples with schizophrenia (e.g., illness duration, cognitive impairments, medication).

Further, the JTC literature has neglected to explore how the bias relates to different delusional subtypes (Garety and Freeman; 1999). This is important given that McKay, Langdon and Coltheart (2007) suggest that individuals experiencing persecutory delusions are more wary and *less* hasty in decision-making than people experiencing other types of delusions (e.g., grandiose). If differences exist between delusional subtypes, there are important implications

for understanding discrepancies in the JTC literature and for psychological interventions aimed at generic delusional belief systems.

CONCLUSION

The present meta-analysis of the JTC literature replicated the finding that, with a medium-to-large-sized effect, DtoD best distinguishes the presence of delusions. The size of the bias was large where delusions were defined by diagnosis of delusional disorder ($d_+ = .74$) and where black and white beads were employed on the classic paradigm ($d_+ = .87$). The magnitude of the effects implies that JTC is a robust phenomenon associated with delusional experiences. Further research is needed to disentangle potential interactions between moderating factors to better understand the bias.

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SECTION 2: RESEARCH REPORT

Regulating the 'Jumping to Conclusions' Bias: The Effectiveness of

Implementation Intentions

ABSTRACT

The reasoning of people with delusions is characterised by a 'jumping to conclusions' (JTC) bias. This study investigated the phenomenon in non-clinical participants expressing high levels of paranoia. The aims were to examine the impact of task manipulations on JTC and demonstrate the effectiveness of implementation intentions ('*if-then*' plans) in reducing hasty decision-making. After completing a measure of paranoia, eighty-five participants were allocated to high and low paranoia groups before being randomly assigned to one of three instruction groups: control, goal intention or implementation intention. Probabilistic reasoning was measured by performance on the neutral 'beads task' and the emotionally salient 'survey task'. Given there was no evidence of JTC in the present sample, the findings do not support the assertion that JTC contributes to the formation of delusions or is specific to persecutory subtypes. Task difficulty and bead colour influenced the amount of information requested. The colour red led to more cautious reasoning, particularly when the task was difficult. Although implementation intentions were associated with increases in the amount of information requested this was not significant. Implementation intentions appeared most effective when the task was difficult and paranoia was high. Further research is needed to better understand the impact of task variations on JTC, clarify the role of JTC in the formation of delusions and the specificity to persecutory beliefs, and to more reliably demonstrate the effectiveness of implementation intentions. Caution is indicated when interpreting the findings due to limitations of the study.

Keywords: Jumping to Conclusions, Reasoning, Paranoia, Implementation Intentions

INTRODUCTION

The Jumping to Conclusions Bias and Delusions

The tendency for people with delusions to make hasty decisions has been termed the 'jumping to conclusions' (JTC) data-gathering bias (Garety & Freeman, 1999). Since the seminal work of Garety and colleagues, numerous studies have replicated the finding that people experiencing delusions make decisions on the basis of less information than control groups specifically on probabilistic reasoning tasks (Garety, Hemsley & Wessely, 1991; Huq, Garety & Hemsley, 1988).

In the classic probabilistic reasoning paradigm, the 'beads task', participants are shown two jars containing different coloured beads and informed of the relative proportion of beads in each jar (e.g., jar A contains 15 yellow beads and 85 black beads where Jar B contains 15 black beads and 85 yellow beads). Based on a predetermined sequence of beads purporting to be drawn from one of the jars, participants are instructed to decide which of the jars the beads are being drawn from. The JTC bias in people experiencing delusions is defined as either fewer 'draws-to-decision' than non-deluded comparisons, or as 'extreme responding' (i.e., making a decision based on two or fewer items of information).

Reviews of the literature concur that JTC is associated with the presence of delusions (e.g., Fine, Gardner, Craigie & Gold, 2007; Garety & Freeman, 1999). However, there is less clarity regarding the cause of JTC and the mechanisms by which it is associated with delusional beliefs (e.g., Freeman, 2007). Motivational accounts suggest, for example, that people with delusions are

unable to tolerate uncertainty and so JTC fulfils the 'need for closure' (e.g., Colbert & Peters, 2002; Freeman et al., 2006). Cognitive accounts posit deficits in information processing, such as an inability to integrate sequential information into existing hypotheses (Young & Bentall, 1997), or a lower threshold for accepting hypotheses ('liberal acceptance': Moritz & Woodward, 2005; Moritz, Woodward & Lambert, 2007; Moritz et al., 2009). At present, no one account can fully explain the JTC bias as attempts to evidence the accounts has resulted in inconsistent, discrepant findings.

JTC in Non-Clinical Samples

A continuum approach to understanding psychotic experiences allows inferences to be made about clinical experiences based on the study of non-clinical samples (Myin-Germeys, Krabbendam & van Os, 2003). Non-clinical samples are advantageous because their reasoning is less likely influenced by impairments in cognitive, and social, functioning often found in clinical samples. Evidence of JTC in delusion-prone individuals would implicate the data-gathering bias in the formation of delusions. Unfortunately, the findings are equivocal (e.g., Broome et al., 2007; Colbert & Peters, 2002; Linney, Peters & Ayton, 1998; Ziegler, Rief, Werner, Mehl & Lincoln, 2008). Having reviewed the literature, Freeman (2007) concluded that JTC does exist in delusion-prone individuals, but the bias is more subtle than when delusions are present.

Exploration of JTC in non-clinical samples also presents an opportunity to better understand how the bias relates to specific delusional sub-types. The results of the few studies investigating the association between JTC and persecutory (paranoid) beliefs in non-clinical samples are encouraging. To determine the

prevalence of JTC in the general population, Freeman, Pugh and Garety (2008) found that 20% demonstrated the data-gathering bias. JTC was strongly predicted by conviction in paranoid beliefs, and to a lesser extent, by the distress experienced. Lincoln, Salzmann, Ziegler and Westermann (2011) found that non-clinical participants experiencing high levels of paranoia made significantly hastier decisions than those experiencing medium or low levels of paranoia. Moreover, Lincoln, Lange, Burau, Exner and Moritz (2010) found that attempts to evoke anxiety increased paranoia and a JTC tendency mediated this association. Further studies are required to replicate and determine the reliability of these findings.

Impact of Task Variations on the JTC Bias

Variations in probabilistic reasoning tasks have been employed to explore factors which potentially mediate the JTC bias. Evidence of JTC on the neutral beads task suggests that the data-gathering bias is generalised and unrelated to delusional content (Dudley, John, Young & Over, 1997a). However, people with delusions exhibit attentional and memory biases for self-referent and delusion-referent stimuli. Accordingly, Dudley et al. (1997b) proposed that increasing the emotional salience of probabilistic tasks would exacerbate JTC. They found that participants with and without delusions both demonstrated a greater tendency to make hasty decisions when the stimuli were emotionally salient (i.e., adjectives describing someone similar to themselves), but that this was exaggerated in participants with delusions. Subsequent findings have been less conclusive. Although Young and Bentall (1997) replicated the finding, Warman and Martin (2006) found that the JTC bias was only present when the

first stimulus was weighted negatively. Further, a meta-analysis of the evidence revealed no effect of emotional salience on JTC (Fine et al., 2007).

The difficulty of the task, manipulated by varying the ratio in which stimuli are presented, has also been found to influence the JTC bias. Dudley et al. (1997a) found that participants with delusions performed similarly to controls in that they increased the amount of information requested to make a decision when the task was harder (i.e., when ratios were 60:40 compared to 85:15). This finding is well-replicated (e.g., Menon, Pomarol-Clotet, McKenna & McCarthy, 2006; White & Mansell, 2009). Although Dudley et al. (1997a) found that, regardless of task difficulty, people with delusions demonstrated JTC, this has not been well replicated. Other studies investigating task difficulty have found that the JTC bias was only present when the task was more difficult but not when the task was easy (e.g., Broome et al, 2007; Menon et al., 2006). However, studies that did not manipulate task difficulty have found a JTC bias at the easier ratio of 85:15 (e.g., Conway et al., 2002; Garety et al., 1991) suggesting fatigue or learning across tasks may influence the findings. Further, So, Freeman and Garety (2008) found that the ability to detect a JTC bias was dependent on the measurement: JTC was evident, regardless of task difficulty, when measured by 'extreme responding', but only on difficult tasks when draws-to-decision was employed.

One aspect of the beads task which has not been explored specifically in the JTC literature is the colour of the beads employed. On other cognitive tasks, there is evidence that colour affects performance. For example, Elliot, Maier, Moller, Friedman and Meinhardt (2007) found that performance was impaired

on IQ tests when participants were exposed to the colour red compared to those exposed to green, black or white. They argue that red is evolutionarily associated with danger and unconsciously evokes an avoidance motivation which impedes performance. However, red is also evolutionarily associated with attractiveness and therefore may evoke an approach motivation which enhances performance (e.g., Mehta & Zhu, 2009). Accordingly, it is likely that the colour of beads employed will affect performance on the beads task.

Two studies have investigated the impact of rushing on JTC. White and Mansell (2009) found that delusion-prone individuals, who made hastier decisions, were in more of a rush to complete the tasks than controls. The magnitude of the JTC bias was predicted by how much they were rushing. Keefe and Warman (2011) experimentally manipulated the perception of rushing. Although there was no effect of rushing on the *amount* of information requested, delusion-prone individuals rated themselves as more *certain* of their decision despite having the same amount of evidence as individuals who were not delusion-prone. Further research is needed to better understand how rushing influences JTC.

Cognitive Interventions for the JTC Bias

Garety et al. (2005) argue that targeting the processes underpinning delusional beliefs is fundamental in weakening belief conviction and preventing increased distress, and decreased functioning, associated with relapse. Developing this metacognitive approach to treatment, Moritz and colleagues devised a group treatment programme (see Moritz & Woodward, 2007; Moritz, Vitzthum, Randjbar, Veckenstedt & Woodward, 2010). Two modules of their Metacognitive Training in Schizophrenia (MCT) focus specifically on JTC.

Evaluation of the effectiveness of MCT has shown a non-significant reduction in JTC in both people experiencing acute delusions and those in a more stable, but chronic phase, of psychosis (Aghotor, Pfueller, Moritz, Weisbrod & Roesch-Ely, 2010; Moritz et al., 2010). Briefer interventions have been employed with promising results. Ross, Freeman, Dunn and Garety (2011) found that a 45-minute reasoning training package, partly focussing on data-gathering, was successful in increasing the amount of information used to make subsequent decisions in individuals currently experiencing delusions. These findings are important as they suggest that reasoning biases are amenable to brief, and thereby cost-effective, interventions. Further, contrary to MCT, the brief intervention did not explicitly link the training to delusional experiences which has important implications for the acceptability of the approach in people who do not consider their beliefs anomalous.

Goal Intentions, Implementation Intentions and Goal Achievement

Dudley and Over (2003) propose that people with delusions do not have an inability to follow formal logical principles (e.g., they are sensitive to changes in difficulty on probabilistic tasks). However, difficulties in reasoning arise due to their personal goals. People with delusions assign more threat-related salience to neutral stimuli, therefore their personal goal is self-protection. The JTC bias is a rapid way of confirming threat-related beliefs thereby initiating self-protective behaviour. However, it is the consequence of JTC that lacks rationality because incorrect conclusions can be drawn e.g., an innocuous sound on a telephone line is perceived as evidence of a government conspiracy. In contrast to the desired outcome of self-protection delusional beliefs can actually leave people vulnerable to harm and exploitation because their basic needs are neglected.

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Failure to attain desired outcomes is not unusual. Gollwitzer and Sheeran (2006) propose four reasons to account for the discrepancy between intending to act and failure to attain the desired outcome: failing to initiate goal striving; goal striving becoming 'derailed' by undesired influences; failure to disengage from goal striving which is ineffective or unproductive, and lacking capacity to pursue subsequent goals due to overextension on previous goals. If we are to accept Dudley and Over's (2003) account, it is possible that JTC reflects a failure to disengage from a pattern of goal striving that is ineffective. Certainly, it is considered particularly 'difficult to disengage from an ongoing goal pursuit when self-defensive concerns are activated' (Gollwitzer & Sheeran, 2006, p. 81) as is the case when self-protection is the desired outcome of reasoning for people experiencing delusions.

There is evidence that difficulties in the self-regulation of goal pursuit can be effectively targeted by forming implementation intentions: 'if-then' plans which connect specific anticipated situations with goal-directed responses (Gollwitzer, 1999). Whereas goal intentions focus on the desired end-state (i.e., *what* is to be achieved), implementation intentions define the strategy required to reach the goal by identifying environmental cues that initiate predetermined behavioural or cognitive responses (the *how*, *where* and *when* of goal achievement; Gollwitzer & Sheeran, 2006). Specifying an association between situation and response forms a mental representation that becomes highly activated and accessible, transforming conscious, effortful tasks into automatic processes controlled by situational cues (Gollwitzer, 1999; Webb & Sheeran, 2007; Webb & Sheeran, 2008).

Implementation intentions are effective for a wide variety of goals: health behaviours (e.g., Andersson & Moss, 2011; Sheeran & Orbell, 2000; Armitage, 2008; 2009), environmental behaviours (e.g., Bamburg, 2000; Holland, Aarts & Langendam, 2006) and regulation of emotional and cognitive responses (Schweiger Gallo, Keil, McCulloch, Rockstroh & Gollwitzer, 2009; Stewart & Payne, 2008). Further, there is evidence for the long-term effectiveness of implementation intentions (e.g., Connor & Higgins, 2010; Martin, Sheeran, Slade, Wright & Dibble, 2011).

A meta-analysis of the empirical evidence indicated that forming an implementation intention had a medium-to-large effect on goal attainment (d_+ = .65; Gollwitzer & Sheeran, 2006). The review provided support for the processes underpinning the effectiveness of implementation intentions: the activation of the mental representation of specified cues and automatic responses to said cues. Implementation intentions had at least a medium effect on self-regulatory strategies. In particular, forming an implementation intention had a medium effect on preventing disengagement from ineffective goal striving (d_+ = .47). Accordingly, if JTC was conceptualised as an ineffective approach to goal striving, implementation intentions could potentially reduce the bias and encourage people with delusions to make more informed decisions.

Although implementation intentions have been successful for individuals who experience difficulties persisting with their goals (e.g., adults with schizophrenia and children with ADHD; Brandstätter, Legnfelder & Gollwitzer, 2001; Gawrilow & Gollwitzer, 2008) and in decision-making (Owens, Bowman & Dill, 2008),

studies have yet to determine the effectiveness in reducing the JTC bias associated with delusions.

The Present Study

The present study aims to investigate whether implementation intentions, when compared with basic instructions and goal intentions, are effective in reducing the JTC bias on neutral and emotionally salient probabilistic reasoning tasks in a non-clinical sample with high levels of paranoia. In doing so, the study aims to explore whether the bias exists in non-clinical samples and, in particular, in people experiencing high levels of paranoia (Freeman et al, 2008; Lincoln et al., 2011). Moreover, it examines the impact of the following task manipulations on the hastiness of decision-making in a non-clinical sample: type of task (neutral vs. emotionally salient); task difficulty (according to variations in ratio); colour of the dominant bead (i.e., the colour of the highest proportion of beads in the first jar shown: red vs. green) and rushing (by evoking a perceived time pressure).

Specifically, it was hypothesised that:

(i)   Participants with higher levels of paranoia will demonstrate the JTC bias, as determined by significantly fewer draws-to-decision than control participants, on neutral and emotionally salient probabilistic reasoning tasks.

(ii)   The JTC bias will decrease as the difficulty of the neutral task increases.

(iii)   Perceived time pressure will rush participants' decision-making and reduce the amount of information requested to make a decision on the neutral task.

(iv)   When participants form an implementation intention they will request more information to make their decision than participants who form a goal intention or receive basic instructions only, on both neutral and emotionally salient probabilistic reasoning tasks.

(v)   Implementation intentions will remain effective even when task manipulations exacerbate the JTC bias.

Given the equivocal findings regarding the emotional salience of tasks, no specific predictions were made about the difference in draws-to-decision between tasks. Although colour impacts on task performance, this has not been examined in the JTC literature. Accordingly, no specific predictions were made regarding the influence of dominant bead colour on draws-to-decision, although we might speculate that decision-making will be more cautious (i.e., more information will be requested) where the dominant bead is red (Elliot et al., 2007).

METHODS

Participants

To determine the sample size required to detect an effect of implementation intentions, a priori power analysis was conducted using G*Power (Buchner, Erdfelder & Faul, 1997). Based on Gollwitzer and Sheeran's (2006) meta-analysis of the implementation intention literature, and using Cohen's (1988) criteria, a medium-to-large effect size of $f = .33$ was assumed. With a significance level of $\alpha = .05$ and six groups of participants, a total sample of 78 participants was required to achieve 80% power.

An opportunistic self-selecting sample was recruited from the University of Sheffield via two routes: the participation in psychological research scheme for first-year psychology undergraduates and the university volunteers list comprising undergraduate and postgraduate students, and university staff across all academic disciplines. Figure 1 presents the flow of participants through the study.

Potential participants were contacted by email inviting them to complete an Internet-based survey (Appendix C.1*).* In addition to study information and questions pertaining to demographic characteristics, the survey comprised the Paranoia Scale (Fenigstein & Vanable, 1992; Appendix D.1). As the Paranoia Scale was developed specifically for non-clinical populations no norms are available for clinical samples, though the authors report a mean score of 42.7 (*SD* = 10.2) from a possible range of 20 to 100. A large enough distinction in paranoia was needed between groups to ensure the best possible chance of detecting a JTC bias. Accordingly, high (n = 43) and low (n = 42) paranoia groups were determined, respectively by scores within the top and bottom third of the range (20-79) of actual scores in the sample. Scores in the high paranoia group ranged from 58 to 79 and from 20 to 38 in the low paranoia group. Where participants' scores fell within the mid range they were excluded from the study (n = 181). Failure to complete the survey also resulted in exclusion (n = 82). Further exclusions were determined by failure to respond to a second email invitation to participate in the laboratory-based stage of the study (n = 133), providing invalid contact details (n = 11) and not attending/being unable to attend the second part of the study (n = 7).

Participants in the high and low paranoia groups were randomly allocated to one of three instruction groups: control, goal intention and implementation intention. On probabilistic reasoning tasks, the control group was provided only with the instructions needed to complete the tasks. The goal intention group was provided with task instructions in addition to a standardised goal intention (*'to do this task well you should take your time before making a decision'*). The implementation intention group was given task instructions and a standardised implementation intention (*'If I am faced with a decision, then I will ask myself - have I really gathered enough information to make a good decision?'*). Prior to the survey task (see Figure 2; Measures below) participants were asked to read through the goal intention or the implementation intention from a cue card at least three times before the card was removed from sight. Participants were asked to repeat the goal or implementation intention to the author to check understanding. After completing the survey task, participants were asked to repeat the goal or implementation intention again before proceeding with the beads task (see Figure 2; Measures below).
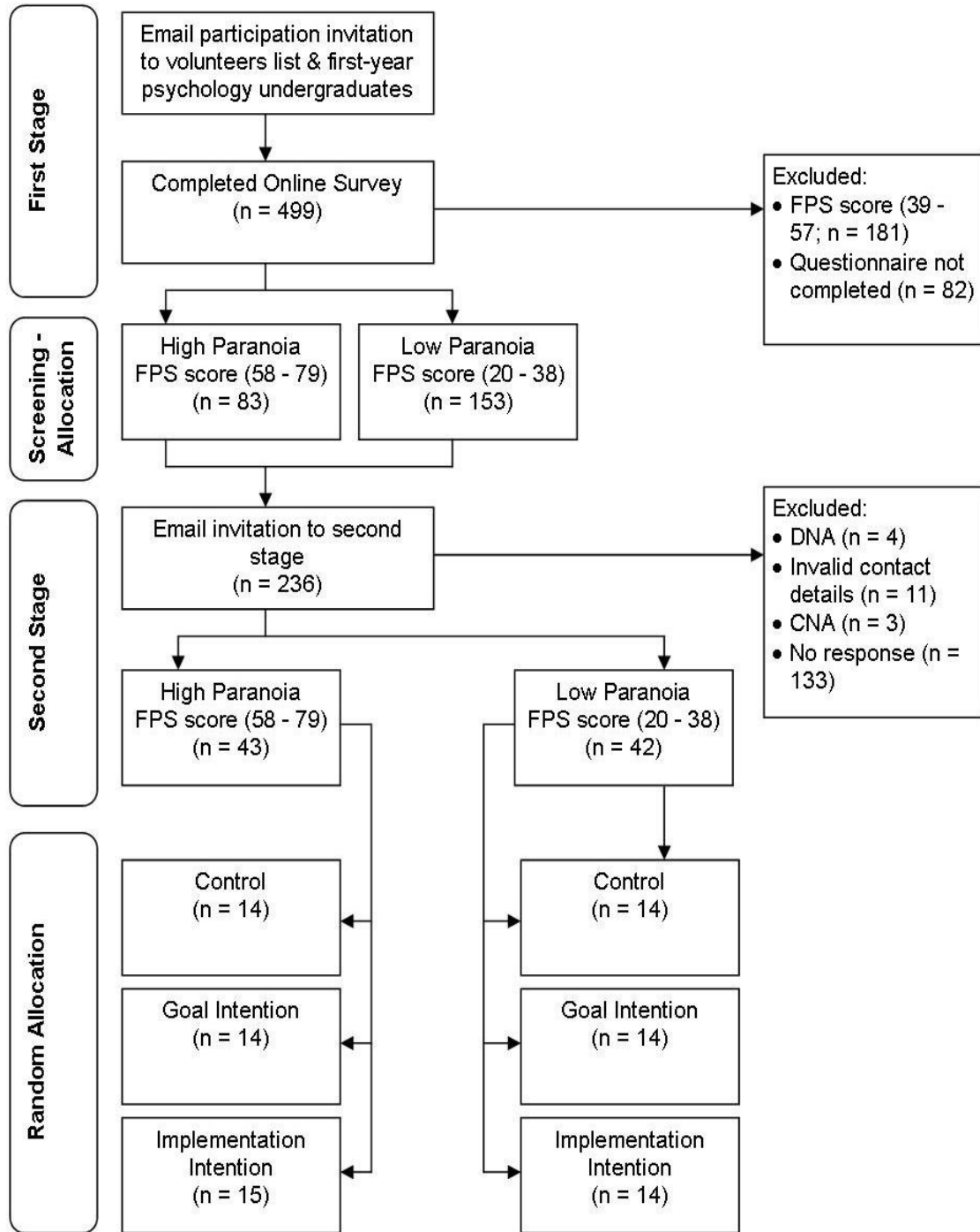
Information regarding the number of potential participants contacted for participation in the first stage is unavailable
DNA = Did not attend
CNA = Could not attend

Figure 1. Flow of participants through the study

Measures

Paranoia Scale (Fenigstein & Vanable, 1992)

The Paranoia Scale (Appendix D.1) is a 20-item self-report instrument developed specifically to measure paranoia in non-clinical populations. Each item relates to at least one of four dimensions of paranoia: (i) belief that behaviour or cognitions are influenced by external people or forces; (ii) belief that others are against the individual; (iii) belief that others' talk about or watch the individual; (iv) mistrust or suspicion relating to others' intentions and feeling bitterness or resentment. Each item is rated on a five-point scale (1 = not at all applicable to me to 5 = extremely applicable to me) resulting in a single total item score ranging from 20 to 100. Higher scores indicate greater paranoia. In the present sample the Paranoia Scale was deemed reliable (α = .95). This is consistent with the original validation which demonstrated internal consistency (α = .84), good test-retest reliability (r = .70) and convergent and divergent validity (Fenigstein & Vanable, 1992). The scale has been extensively employed to explore paranoia in non-clinical samples (e.g., Newman-Taylor, Graves & Stopa, 2009) where the mean score ranges from 39.2 (SD = 12.72; Martin & Penn, 2001) to 42.7 (SD = 14.3; Freeman et al., 2005).

Affect-Arousal Scale (Aarts & Dijsterhuis, 2003)

A modified version of the Affect-Arousal Scale was administered to measure mood and level of arousal at the time of testing (Appendix D.2). The measure comprises six bipolar items rated on a scale of 1 to 10. Three items measure mood: bad-good, sad-happy and pleased-displeased. Three items measure arousal: calm-excited, tired-energetic and sedate-aroused. Aarts and Dijsterhuis (2003) reported good reliability for the mood scale (α = .81) but moderate

reliability for the arousal scale (α = .50). The mood scale was less reliable in the present study (α = .50) whereas the arousal scale was slightly more reliable (α = .62) but both were below the acceptable level of α = .70 needed to assume reliability.

National Adult Reading Test (2nd Edition; Nelson & Willison, 1991)

The National Adult Reading Test (NART-2; Appendix D.3) uses accuracy in the pronunciation of 50 irregularly spelled words (e.g., naive) to measure intelligence (IQ). The test is based on the correlation between IQ and reading ability. The irregularity of items limits the extent to which participants can guess the pronunciation. The test has been standardised on a British sample and developed for use alongside the Wechsler Adult Intelligence Scale (WAIS). Strauss, Sherman and Spreen (2006) reported excellent internal consistency (α = .90), test-retest reliability (r = .98) and high inter-rater reliability (r = .88). In the present study, the test had good internal consistency (α = .77).

Probabilistic Reasoning Tasks

The Beads Task (Huq et al., 1988; Garety et al., 1991)

Participants in the present study completed a modified computerised version of the beads task presented using E-Prime software (Schneider, Eschman & Zuccolotto, 2002). The ratio of beads was varied to manipulate task difficulty. Two sets of five ratios of red and green beads were counter-balanced for dominant colour (i.e., the colour of the highest proportion bead in the first jar presented): 95:05, 85:15, 75:25, 65:35, 55:45 and presented in two conditions. Ratios in the first condition were administered according to the classic paradigm ('no prime'). Music intended to induce a perceived time pressure, and therefore

evoke rushing, accompanied each bead drawn in the second ('prime') condition. The iconic theme tune from the British television game show 'Countdown' was selected as it is used to time contestants on anagram and maths tasks. It was assumed that most participants would recognise the tune and associate it with a limited amount of time to complete the task. There was no actual difference between the prime and no-prime conditions in the amount of time given to complete the task.

In each condition, the predetermined sequence of beads was randomly generated and then presented in a fixed order (Appendix E.1). Each ratio of beads was presented in a random order within each condition ('no prime' and 'prime').

Task instructions and ratios were displayed prior to each change in ratio. On the first screen participants were shown a picture of two jars of beads. The jars were labelled beneath as either 'Jar 1' or 'Jar 2' alongside information regarding the ratio of coloured beads inside. This information remained on the screen until participants responded to the instruction to *'press the space bar to continue'*. The following instructions were provided on the subsequent screen: *'When you press the space bar, the computer will pick one of these jars. You will not be told which jar the computer has picked. The computer will then pick a bead randomly out of this jar. You will be shown this bead and then the computer will put it back into the jar. You will then have the option to ask the computer to pick more beads out of the jar. Your job is to decide which jar the computer is taking the beads out of – Jar 1 or Jar 2. You can ask the computer to show you as many or as few beads as you wish. When you think you know which jar the*

beads are coming from, press the number '1' for Jar 1 and the number '2' for Jar 2.'. The participants were instructed to press the space bar again to begin each trial.

A pictorial representation of the two jars, but not the numerical ratio, remained on the screen throughout each trial. The beads were presented on the screen one at a time and remained there until the participant responded. Unlike Dudley et al. (1997a) and subsequent studies, a memory aid was not provided in that the beads were purportedly replaced in the jar after each draw (i.e., draws were random with replacement) rather than remaining in view (random draws). Participants were unaware that a maximum of 20 beads could be drawn for each ratio, but only 4.7% requested all 20 beads across the conditions. The dependent variable was the number of draws-to-decision.

The Survey Task (Dudley, et al. 1997b)

The survey task is a version of the beads task which employs more emotionally salient stimuli. The task was administered by the author not by computer. Participants were told that two surveys had been conducted about a person 'very much like' themselves. Both surveys comprised 100 comments where one survey was mostly positive (60 positive comments: 40 negative comments) and the other survey mostly negative (60 negative comments: 40 positive comments). Participants were instructed to ask for as many comments as needed to decide which survey the comments were coming from. The predetermined sequence of 20 comments was used in previous research (Dudley et al, 1997b; Appendix E.2). The dependent variable was the number of

draws-to-decision i.e., the number of comments requested before making a decision.

Go/No-Go Task (adapted from Bates, Kiehl, Laurens & Liddle, 2009)

The Go/No-Go paradigm is a measure of inhibitory control. Participants are instructed to respond when a 'go' stimulus is presented and to withhold their response (i.e., do nothing) when a 'no-go' stimulus is presented. The go/no-go task in the present study acted as a 'filler' to minimise learning between the no prime and prime conditions of the beads task. However, it also allowed exploration of group differences and the influence of executive functioning on draws-to-decision. The computerised task was presented using E-Prime software (Schneider et al., 2002). Participants were instructed to respond (press the space bar) as quickly and as accurately as possible when the letter 'X' appeared on the screen and do nothing when the letter 'O' appeared on the screen. Following two practice sequences, two trials were administered. Participants were unaware that in the first trial, there was an equal (50:50) probability that an X or O would be displayed; in the second trial the probability was 75:25, respectively. The letter was displayed on screen for 650 milliseconds. The duration between each presentation of letter was 1000, 2000 or 3000 milliseconds.

Effort Rating

After the tasks finished, participants rated the overall amount of effort they put into completing the tasks on an 11-point scale ranging from 0 (no effort at all) to 10 (the most amount of effort possible; Appendix D.4). They were also asked to comment on what they thought the study was exploring. Most participants did

not make any inferences while others simply repeated the study information provided at the outset. The presence of music in the prime condition led many participants to infer that the study was exploring the impact of stress, or pressure, on task performance. Some participants did identify decision-making which is understandable given both the goal intention and implementation intention instructions focused specifically on improving the decision-making process. No participant made reference to paranoia or JTC. No data were excluded from the analysis given no participants inferred the true nature of the study.

Procedure

The study received ethical approval from the University of Sheffield's Department of Psychology Research Ethics Committee (Appendix B.1). Participants were told that they were participating in a study investigating the relationship between interpersonal beliefs and performance on puzzle tasks (Appendix C.2). The true nature of the study was not disclosed in an effort to reduce demand effects. All participants were required to give informed consent to take part in the study (Appendix C.3). The questionnaires and tasks were administered in a fixed order (see Figure 2). All participants were tested individually and, with the exception of the computerised tasks, in the presence of the author. A proportion of participants from the volunteers list received £5 for taking part while the remainder participated voluntarily; first-year psychology undergraduates received credits towards course requirements. Participants were debriefed verbally and with written information (Appendix C.4).
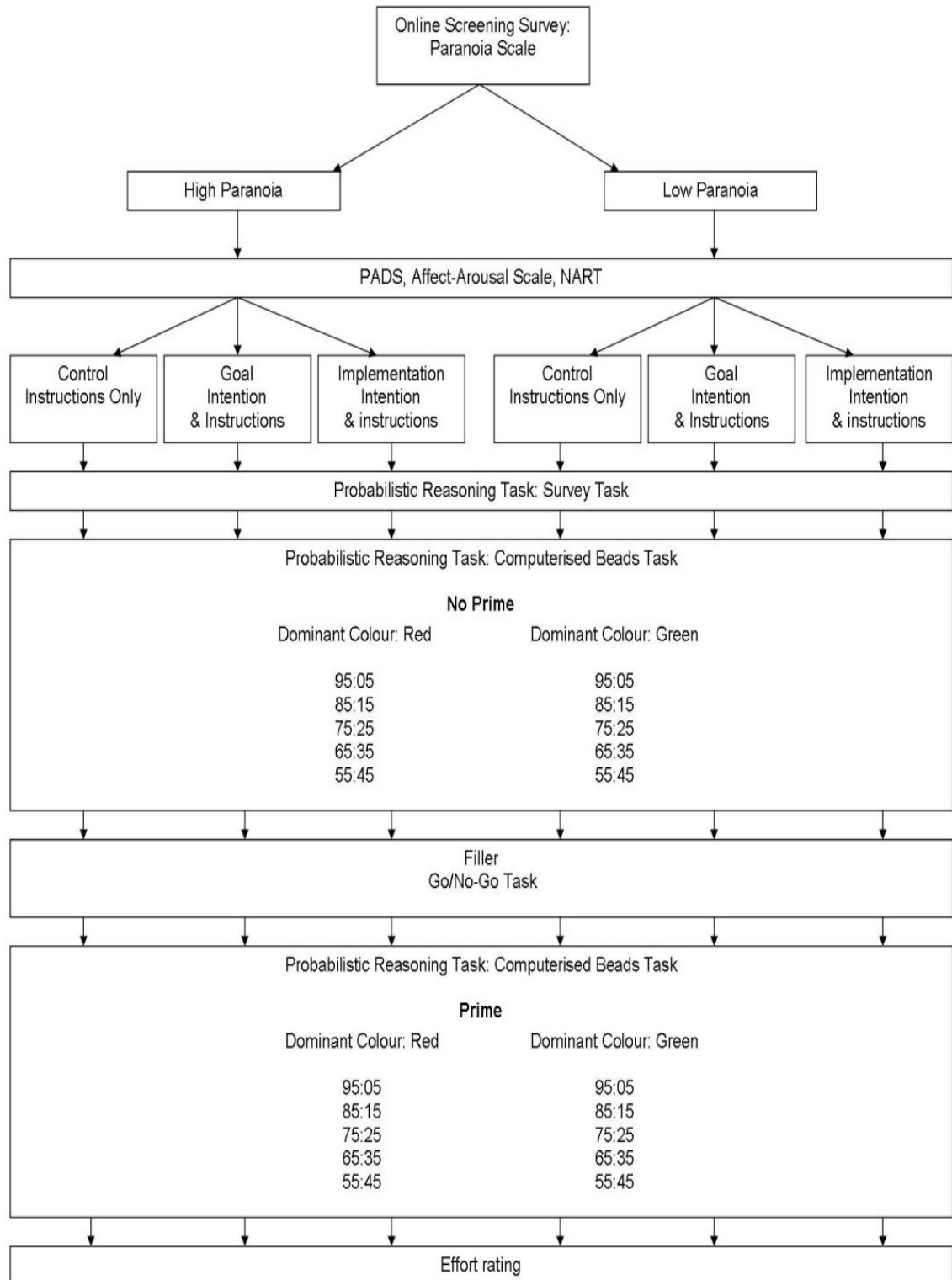
Figure 2. Fixed order procedure

Analysis

Statistical analyses were conducted using SPSS version 16.0 for Windows (SPSS, 2007). Participants were not matched on demographic characteristics during recruitment. Accordingly, randomisation checks involved comparing both the paranoia groups and instruction groups according to demographic characteristics. Chi-square tests were employed for group comparisons where data were categorical. Where assumptions for parametric data were not met, Mann-Whitney and Kruksal-Wallis tests were used to compare groups with two or more independent groups respectively. Associations between group variables and dependent variables were analysed using Spearman's rho as the data were not parametric. The majority of correlations between group variables and the dependent measures were not significant and therefore did not warrant inclusion as covariates in the main analysis. Accordingly, for the beads task, a mixed 2-between (paranoia: high vs. low) by 3-between (instruction: control vs. goal intention vs. implementation intention) by 5-within (ratio: 95:05 vs. 85:15 vs. 75:25 vs. 65:35 vs. 55:45) by 2-within (dominant bead colour: red vs. green) by 2-within (prime: prime vs. no prime) analysis of variance (ANOVA) was conducted. For the survey task, a 2-between (paranoia: high vs. low) by 3-between (instruction: control vs. goal intention vs. implementation intention) ANOVA was conducted. Significant interactions were explored with a series of post-hoc ANOVAs. Finally, a mixed 2-between (paranoia: high vs. low) by 3-between (instruction: control vs. goal intention vs. implementation intention) by 2-within (beads task vs. survey task) ANOVA compared the neutral and emotionally salient tasks. Draws-to-decision was the dependent variable for all analyses.

RESULTS

Participant Demographics and Randomisation Check

The demographics of the participants (n = 85) whose data contributed to the final analyses are presented in Table 1. Randomisation checks, as outlined above, largely revealed no significant differences between both the paranoia groups and the instruction groups. To summarise, differences were indicated between the paranoia groups only for IQ, affiliation with the Department of Psychology and receipt of reimbursement. Further comparisons on other measures found differences in mood/arousal but not effort or performance on the Go/No-Go task (Table 2). The findings are discussed in more detail below.

Table 1

Participant Characteristics by Paranoia Group and Instruction Condition

	Low Paranoia				High Paranoia			
	Control	Goal Intention	Implementation Intention	Total	Control	Goal Intention	Implementation Intention	Total
Occupation (n)								
Undergraduate student *(%)*	8 (57.2%)	9 (64.3%)	13 (92.9%)	30 (71.4%)	13 (86.7%)	14 (100%)	12 (85.8%)	39 (90.7%)
Postgraduate student *(%)*	1 (7.1%)	2 (14.3%)	0	3 (7.2%)	0	0	1 (7.1%)	1 (2.3%)
University Staff *(%)*	5 (35.7%)	3 (21.4%)	1 (7.1%)	9 (21.4%)	2 (13.3%)	0	1 (7.1%)	3 (7.0%)
Psychology (n)								
Yes *(%)*	5 (25%)	6 (30%)	9 (45%)	20 (47.6%)	14 (45.2%)	9 (29%)	8 (25.8%)	31 (72.1%)
No *(%)*	9 (40.9%)	8 (36.4%)	5 (22.7%)	22 (52.4%)	1 (8.3%)	5 (41.7%)	6 (50%)	12 (27.9%)
Reimbursement (n)								
None *(%)*	5 (35.7%)	5 (35.7%)	4 (28.6%)	14 (33.3%)	0	0	1 (6.7%)	1 (2.3%)
Course credits *(%)*	4 (28.6%)	4 (28.6%)	7 (50%)	15 (35.7%)	9 (64.3%)	7 (50%)	8 (53.3%)	24 (55.8%)
Payment *(%)*	5 (35.7%)	5 (35.7%)	3 (21.4%)	13 (31.0%)	5 (35.7%)	7 (50%)	6 (40%)	18 (41.9%)

	Low Paranoia				High Paranoia			
	Control	Goal Intention	Implementation Intention	Total	Control	Goal Intention	Implementation Intention	Total
Mean age (SD)	29.07 (15.26)	26.14 (11.28)	23.36 (10.65)	26.19 (12.48)	21.6 (8.52)	19.29 (1.27)	19.86 (3.16)	20.28 (5.36)
Gender (n)								
Male (%)	3 (21.4%)	3 (21.4%)	5 (35.7%)	11 (26.2%)	2 (13.3%)	4 (28.6%)	5 (35.7%)	11 (25.6%)
Female (%)	11 (78.6%)	11 (78.6%)	9 (64.3%)	31 (73.8%)	13 (86.7%)	10 (71.4%)	9 (64.3%)	32 (74.4%)
Mean NART Score (SD)	114.79 (9.09)	116.30 (8.66)	113.07 (7.86)	114.74 (8.45)	107.4 (6.74)	107.5 (8.20)	109.21 (6.70)	108.02 (7.11)
Mean FPS score (SD)	29.57 (4.35)	27.64 (5.06)	31.71 (4.16)	29.64 (4.74)	64.8 (5.60)	63.43 (4.90)	64.07 (2.26)	64.12 (5.51)

Participants' Age

The median age of 19 years did not differ between paranoia groups (U = 711.5, *ns*) or instruction group (H = 0.33, *ns*). Age did not correlate with the dependent measure, draws-to-decision, on either the beads task (r = -.19) or the survey task (r = -.04); both *ns*.

Participants' Gender

There were no significant differences in gender between the paranoia groups (χ^2 (1) = .00, *ns*) or instruction groups (χ^2 (2) = 2.56, *ns*). There was no correlation between gender and draws-to-decision on either the beads task (r = -.04) or the survey task (r = -.07); both *ns*.

NART Scores

Differences between the paranoia groups existed in NART scores demonstrating that the low paranoia group had significantly higher IQ (*Mdn* = 116.50) than the high paranoia group (*Mdn* = 108.00; U = 471.00, p < .001). There was no significant difference in NART scores between the instruction groups (H = 0.15, *ns*). NART scores (IQ) did not correlate with the dependent measures on the beads task (r = -.18, *ns*) but did on the survey task (r = .22, p < .05). Further analysis of the latter relationship, revealed that higher IQ was associated with more draws-to-decision on the survey task in the high paranoia group (r = 0.35, p < .01), but not the low paranoia group (r = .28, *ns*). Similarly, higher IQ was related to increased information requested on the survey task for the goal intention group (r = .56, p < .001) but not the control (r = .04, *ns*) or implementation intention (r = -.06, *ns*) groups.

Occupational Background

Participants were categorised as either undergraduate students, postgraduate students or university staff. However, there was no difference between paranoia groups (χ^2 (2) = 5.16, *ns*) or instruction groups (χ^2 (4) = 4.29, *ns*) in occupational background. However, the assumption of a minimum number of variables in this analysis was violated reducing confidence in these findings. Occupational background was not correlated with draws-to-decision on either the beads task (*r* = -.09) or the survey task (*r* = -.12); both *ns*.

Did participants have a psychology background?

Of those participants who worked or studied in the Department of Psychology significantly more were in the high paranoia group (60.8%) than the low paranoia group (29.2%; χ^2 (1) = 5.303, p < .05). Participants in the instruction groups did not differ according to whether they came from a psychology background (χ^2 (2) = .86, *ns*). Psychology background was not correlated with draws-to-decision for either the beads task (*r* = .01) or the survey task (*r* = -.04); both *ns*.

Were participants reimbursed?

More participants in the low paranoia group did not receive reimbursement (33.3%) than the high paranoia group (2.3%), while more participants in the high paranoia group received credits (55.8%) or payment (41.8%) than the low paranoia group (35.7% and 31% respectively; χ^2 (2) = 14.14, *p* < .001). Participants in the instructions groups did not differ according to whether they

received reimbursement for participating (χ^2 (4) = .90, *ns*). There was no correlation between reimbursement and the dependent measure on either the beads task (*r* = .07) or the survey task (*r* = -.06); both *ns.*

Mood/Arousal

The paranoia groups significantly differed in mood valence. Participants in the high paranoia group rated themselves lower on the 'Bad-Good' continuum (*Mdn* = 7) compared to the low paranoia group (*Mdn* = 8; *U* = 646.5, *p* < .05). The high paranoia group also demonstrated significantly less positive mood (*Mdn* = 7) than the low paranoia group (*Mdn* = 8) on the 'Sad-Happy' continuum (*U* = 668.5, p < .05). There were no differences between the paranoia groups on the other measure of mood (Pleased-Displeased, U = 891.00) or any of the measures of arousal (Calm-Excited, U = 842.50, Tired-Energetic, U = 740.50 and Sedate-Aroused, U = 791.50; all *ns*). There were no differences between the instruction groups on any of the mood measures (Bad-Good, H = .06; Sad-Happy, H = .30; Pleased-Displeased, H = 3.01; all *ns*) or the arousal measures (Calm-Excited, H = .05; Sedate-Aroused, H = .30; Tired-Energetic, H = .04; all *ns*). There were no significant correlations between scores on the mood/arousal measures and draws-to-decision on the beads task (Bad-Good, *r* = .17; Sad-Happy, *r* = .04; Pleased-Displeased, *r* = .10; Calm-Excited, *r* = -.07; Sedate-Aroused, *r* = .15; Tired-Energetic, *r* = .03; all *ns*) or the survey task (Bad-Good, *r* = .08; Sad-Happy, *r* = .01; Pleased-Displeased, *r* = -.01; Calm-Excited, *r* = -.08; Sedate-Aroused, *r* = .12; Tired-Energetic, *r* = -.08; all *ns*).

Task Effort

There were no significant differences in the amount of effort participants put into the tasks between both the paranoia groups (U = 732.00, *ns*) and the instruction groups (H = .47, *ns*). Effort did not correlate with draws-to-decision on the beads task (*r* = -.08, *ns*) or the survey task (*r* = .10, *ns*).

Go/No-Go Task

On the Go/No-Go task, participants in the high and low paranoia groups did not differ in response inhibition as measured by the number of correct positive responses (U = 733.50, *ns*) and false positive responses (U = 892.00, *ns*). Similarly, response inhibition did not differ between the instruction groups (correct positives: H = 3.70; false positives: H = .81; both *ns*). On the beads task both correct positive responses (*r* = -.03, *ns*) and false positive responses (*r* = -.10, *ns*) did not correlate with draws-to-decision. This was consistent with the survey task where no correlations were found between correct positive responses (*r* = -.15, *ns*) or false positive responses (*r* = .02) and the dependent variable.

Table 2

Summary of significant differences between paranoia groups

	High	Low	Statistic
IQ (Mdn)	108.0	116.5	$U = 471.00^*$
Psychology (%)	60.8%	29.2%	$\chi^2 (1) = 5.303^{**}$
Reimbursement (%)			
None	2.3%	33.3%	
Credits	55.8%	35.7%	$\chi^2 (2) = 14.14^*$
Payment	41.8%	31%	
Mood/Arousal (Mdn)			
Bad-Good	7	8	$U = 646.5^{***}$
Sad-Happy	7	8	$U = 668.5^{**}$

$^* p < .001$; $^{**} p < .05$; $^{***} p < .01$;

Main Analysis

Hypothesis i: Participants with higher levels of paranoia will demonstrate the JTC bias

Table 3 presents the mean draws-to-decision for the paranoia groups and instruction groups averaged across all manipulations of the beads task. On the beads task, the data suggest that there was little difference between high and low paranoia groups on the amount of information requested. This was supported by the mixed ANOVA which indicated no main effect of paranoia, $F(1, 79) = .01$, *ns*, $\eta^2 = .00$.

Table 3
Mean (SD) draws-to-decision on beads task

	Paranoia Group		
Instruction Group	High	Low	Total
Control	5.78 (2.50)	5.82 (3.74)	5.80 (3.10)
Goal Intention	6.40 (3.11)	7.05 (3.26)	6.73 (3.14)
Implementation Intention	7.92 (3.47)	7.48 (3.46)	7.70 (3.41)
Total	6.68 (3.10)	6.78 (3.45)	

The mean numbers of draws-to-decision for the paranoia groups and the instruction groups on the survey task are shown in Table 4. The two-way ANOVA revealed that the main effect of paranoia group, $F(1, 79) = 1.17$, *ns,* $\eta^2 = .00$, was non-significant. Taken together with the findings from the beads task this suggests that JTC was not present in the current sample.

Table 4
Mean (SD) draws-to-decision for the survey task

	Paranoia Group		
Instruction Group	High	Low	Total
Control	8.33 (5.51)	5.92 (5.85)	7.17 (5.71)
Goal Intention	7.86 (6.70)	11.71 (6.41)	9.79 (6.73)
Implementation Intention	12.21 (7.21)	6.57 (3.23)	9.39 (6.19)
Total	9.44 (6.63)	8.07 (5.84)	

Hypothesis ii: JTC will decrease as the difficulty of the neutral task increases

The absence of a main effect of paranoia suggests that the JTC bias was not present in the current sample. However, the ANOVA revealed a main effect of ratio, $F(4, 316) = 72.01$, $p < .001$, $\eta^2 = .48$, indicating that all groups requested more information as the difficulty of the task increased with the variations in ratio: 95:05 ($M = 4.34$; $SD = 2.13$); 85:15 ($M = 5.28$; $SD = 3.53$); 75:25 ($M = 7.07$; $SD = 3.95$); 65:35 ($M = 7.05$; $SD = 3.81$); 55:45 ($M = 9.91$; $SD = 5.28$).

Hypothesis iii: Perceived time pressure will rush participants into requesting less information

On the neutral beads task, the main effect of prime was not significant, $F(1, 79) = .40$, *ns*, $\eta^2 = .01$, indicating that rushing had no impact the number of draws-to-decision ($M = 6.67$, $SD = 3.40$) compared to not rushing ($M = 6.79$, $SD = 3.39$).

Hypothesis iv: More information will be requested by participants who form an implementation intention

On the beads tasks, the main effect of instruction group was non-significant, $F(2, 79) = 2.40$, *ns*, $\eta^2 = .06$, (see Table 3). Similarly, on the survey task, there were no differences between participants based on the instructions that they received prior to completing the task, $F(2, 79) = 1.66$, *ns,* $\eta^2 = .00$. Taken together these findings suggest that implementation intentions were not more effective than goal intentions or basic instructions in increasing the amount of information requested

Colour of the dominant bead

No specific predictions were made regarding the influence of the dominant bead colour on the amount of information requested. However, there was a significant main effect of dominant colour, $F(1, 79) = 38.51$, $p < .001$, $\eta^2 = .33$, suggesting that more information was requested when the highest proportion of beads in Jar 1 was red ($M = 7.08$, $SD = 3.42$) than when the highest proportion was green ($M = 6.38$, $SD = 3.21$).

Interactions between Variables

Hypothesis v: Implementation intentions will remain effective where task manipulations exacerbate the JTC bias

Although the JTC bias was not present in the current sample and implementation intentions did not appear to impact on the amount of information requested on both the beads task and the survey task, the main analysis revealed a number of interactions between variables.

Beads Task

The main effects on the beads task were qualified by a significant three-way interaction between ratio, dominant colour and instruction group, $F(8, 316) = 2.08$, $p < .05$, $\eta^2 = .05$. A series of additional ANOVAs were conducted to further explore this interaction. Separate repeated measures ANOVAs comparing ratio by dominant colour and instruction group revealed that, regardless of whether the dominant colour was red or green, there was a significant effect of ratio for all instruction groups at the $p < .001$ level (see Figure 3). The size of the effect of ratio was larger when the dominant colour was red (control: $F(4, 112) = 20.17$, $\eta^2 = .42$; goal intention: $F(4, 108) = 28.79$, $\eta^2 = .52$; implementation

intention: $F(4, 108) = 37.70$, $\eta^2 = .58$) compared to when the dominant colour was green (control: $F(4, 112) = 9.11$, $\eta^2 = .25$; goal intention: $F(4, 108) = 8.37$, $\eta^2 = .24$; implementation intention: $F(4, 108) = 25.45$, $\eta^2 = .49$). Overall, the more difficult the task became the more information participants requested. Post hoc Bonferroni tests indicated that the difference in draws-to-decision between ratios was significant for the majority of comparisons at the minimum $p < .05$ level.
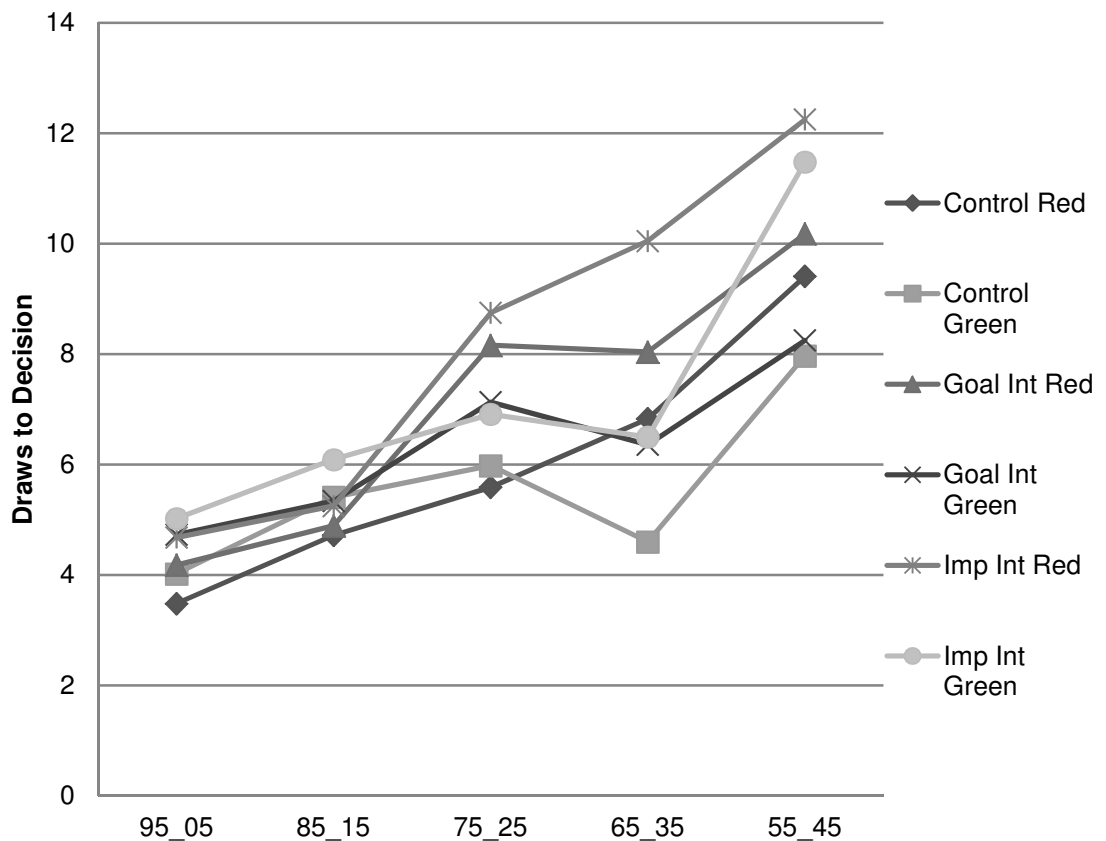

Figure 3. Mean draws-to-decision across ratio by group and dominant colour

To explore the differences between dominant colour by instruction group, initial repeated measures ANOVAs indicated a main effect of colour in the control ($F(1, 28) = 4.54$, $p < .05$, $\eta^2 = .14$), goal intention ($F(1, 27) = 13.85$, $p < .001$, η^2

= .34) and the implementation intention ($F(1, 27) = 25.35$, $p < .001$, $\eta^2 = .48$) groups. These initial findings suggest that within each instruction group, participants requested more information when the dominant colour was red compared to when the dominant colour was green. Further repeated measures ANOVAs comparing dominant colour by instruction group *and* ratio revealed that the main effect of colour held true for the majority of ratios across the instruction groups at the minimum of $p < .05$ level. However, what becomes more apparent through this subsequent analysis is that the direction of the effect changes as the ratios decrease. In Figure 4 the mean draws-to-decision for each ratio by dominant colour have been averaged across the instruction groups to demonstrate the interaction between dominant colour and ratio. Where the task is easier (i.e., where the ratios are more distinguishable) participants requested more information when the dominant bead in the ratio is green. However, when the task becomes more difficult, more information is requested when the dominant bead is red. Specifically, this change occurs when the ratio moves from 85:15 to 75:25.
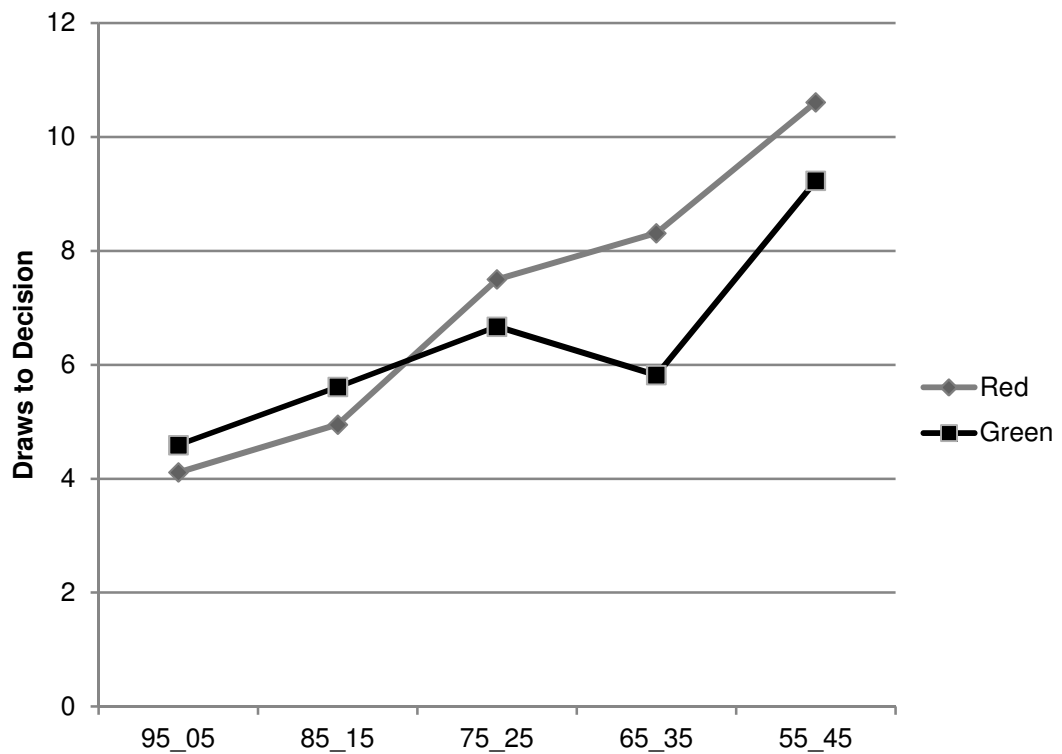
Figure 4. Mean draws-to-decision across ratio by colour

However, this trend must be viewed with caution given that there was no significant difference in draws-to-decision according to dominant colour for ratios 55:45 in the implementation intention group ($F(1, 27) = 1.63$, *ns,* $\eta^2 = .06$), 75:25 in the control ($F(1, 28) = .48$, *ns,* $\eta^2 = .02$) and the goal intention ($F(1, 27) = 2.57$, *ns,* $\eta^2 = .09$), 85:15 in the goal intention group only ($F(1, 27) = 1.01$, *ns,* $\eta^2 = .04$) and 95:05 in the implementation intention group only ($F(1, 27) = 2.81$, *ns,* $\eta^2 = .09$).

Finally, one-way ANOVAs were carried out to explore the differences between instruction groups for each dominant colour and ratio. Where the dominant colour was red, there was no main effect of instruction group for the ratios 95:05 ($F(2, 82) = 2.59$, *ns,* $\eta^2 = .06$), 85:15 ($F(2, 82) = .16$, *ns,* $\eta^2 = .00$) or 55.45 ($F(2,$

82) = 2.16, *ns*, η^2 = .05). Bonferroni post hoc tests for the main effect of instruction group for the 75:25 ratio, $F(2, 82) = 3.80$, $p < .05$, $\eta^2 = .09$, and 65:35 ratio, $F(2, 82) = 4.30$, $p < .05$, $\eta^2 = .10$, signified that participants who received an implementation intention requested significantly more information than those who received basic instructions in the control group (both $p < .05$). Where the dominant colour was green, there was no main effect of instruction group for majority of the ratios: 95:05 ($F(2, 82) = 1.39$, *ns*, $\eta^2 = .03$), 85:15 ($F(2, 82) = .33$, $p = ns$, $\eta^2 = .01$), 75:25 ($F(2, 82) = .68$, *ns*, $\eta^2 = .02$). The effect of the 65:35 ratio reached trend level, but was not significant ($F(2, 82) = 2.45$, *ns*, $\eta^2 = .06$). Consistent with the previous findings, post hoc Bonferroni tests for the main effect of instruction group where the ratio was 55:45 ($F(2,82) = 3.61$, $p < .05$, $\eta^2 = .08$) indicated that significantly more information was requested by the implementation intention group compared to the control group ($p < .05$).

Survey task

Although there were no main effects for either paranoia or instruction group on the survey task, there was a significant interaction between the two variables on the number of draws-to-decision: $F(2, 79) = 4.61$, $p < .01$, $\eta^2 = .10$. Simple main effects revealed no significant differences between the instruction groups for the high paranoia group. However, a main effect of instruction group was found for the low paranoia group: $F(2, 39) = 4.92$, $p < .05$, $\eta^2 = .20$. Consistent with visual inspection of the data, post hoc Bonferroni tests indicated that participants who received the goal intention instructions requested significantly more information than either those who received the implementation intention ($p < .05$) and those who were given basic instructions only in the control group ($p < .02$). Contrary to expectation, there was no significant difference in the amount of information

requested between the control group and the implementation intention group. Further, although there was no main effect of paranoia for either the control or goal intention groups, there was for the implementation intention group, $F(1, 26) = 7.14$, $p < .01$, $\eta^2 = .22$, where high paranoia participants requested more information ($M = 12.21$, $SD = 7.21$) than the low paranoia participants ($M = 6.57$, $SD = 3.23$).

Task Comparison

No specific predictions were made regarding the impact of varying the emotional saliency of the tasks. The mixed ANOVA comparing the beads task against the survey task by paranoia group and instruction group revealed a main effect of task, $F(1,79) = 8.39$, $p < .001$, $\eta^2 = .10$, where more information was requested on the survey task ($M = 8.76$, $SD = 6.25$) compared to the beads task ($M = 6.73$, $SD = 3.27$). The main effect was qualified by a significant three way interaction between task, paranoia group and instruction group ($F(2,79) = 3.09$, $p < .05$, $\eta^2 = .07$). Further exploration of this interaction indicated that the type of task had no impact on the amount of information requested in the low paranoia group ($F(1,41) = 1.62$, *ns*, $\eta^2 = .04$). However, participants in the high paranoia group requested more information on the survey task ($M = 9.44$, $SD = 6.63$) than the beads task ($M = 6.68$, $SD = 3.10$ $F(1,41) = 7.57$, $p < .001$, $\eta^2 = .15$). Further, there was no main effect of type of task for the control group ($F(1,28) = 1.86$, *ns*, $\eta^2 = .06$) or the implementation intention group ($F(1,27) = 1.80$, *ns*, $\eta^2 = .06$). However there was a main effect of task in the goal intention group ($F(1,27) = 4.52$, $p < .05$, $\eta^2 = .14$) where participants also requested more information on the survey task ($M = 9.78$, $SD = 6.73$) than the beads task ($M = 6.73$, $SD = 3.14$).

DISCUSSION

The present study aimed to contribute to growing evidence that the JTC bias robustly found in people experiencing delusions, exists in non-clinical samples and specifically in people who experience non-clinical paranoid or persecutory beliefs. Contrary to expectation, the number of draws-to-decision did not differentiate the high and low paranoia groups on either the neutral or emotionally salient probabilistic reasoning tasks. The finding is inconsistent with previous research specifically focusing on the relationship between paranoia and JTC in non-clinical samples (Freeman et al. 2008; Lincoln et al, 2010; 2011). However, it does reflect inconsistencies in the ability to detect the reasoning bias in relation to generic delusion-proneness (e.g., Colbert & Peters, 2002; Ziegler et al., 2008; Warman & Martin, 2006).

The absence of JTC has implications for our understanding of how the bias relates to delusions. The presence of JTC in non-clinical participants has been used as evidence that hasty decision-making is a trait of people on the delusional continuum and therefore implicated in the formation of delusions (e.g., Colbert & Peters, 2002). However, the present findings do not support this assertion.

Freeman et al. (2008) concluded that the data-gathering bias in the general population is less prevalent and more subtle than in clinical samples so the current finding is perhaps unsurprising. Further, the evidence that JTC is related specifically to persecutory delusions is assumed by the literature given that the majority of delusions are persecutory in nature (Freeman, 2007). However, this

has not been systematically investigated, therefore the association between JTC and clinical persecutory beliefs is not conclusive. Moreover, task manipulations did have a significant impact on performance in the present study potentially masking any inherent differences between the paranoia groups. This latter point is consistent with the difficulties encountered in detecting JTC in delusion-prone samples. For example, studies have failed to detect a JTC bias on the beads task but have on emotionally salient or more realistic tasks (e.g., Ziegler, et al., 2008; Warman & Martin, 2006).

Contrary to expectation, participants did not rush their decision-making when the task was accompanied with music intended to induce rushing. The failure to find a difference between paranoia groups is consistent with Keefe and Warman (2011) who found that the amount of information requested did not differ as a result of attempts to rush. However, the findings differ from White and Mansell (2009), who found that rushing to complete the task was related to delusion-proneness. It is possible that differences in the measures employed can account for the discrepancy with White and Mansell (2009). They employed a self-report measure of rushing, whereas the present study attempted to induce rushing experimentally. It is, however, also possible that the attempt to evoke rushing in this study was not effective. It was assumed that music from 'Countdown' was sufficiently iconic to be associated with a time pressure. Anecdotally participants certainly expressed feeling pressured. Nonetheless, it could be argued that some participants may never have watched the game show and therefore would not associate the music with a time pressure.

To the best of the author's knowledge this was the first study to consider how the colour of the beads employed affected the amount of information reviewed when making a decision. No directional predictions were made regarding the impact on draws-to-decision given that previous research exploring the association between colour and task performance is equivocal (Mehta & Zhu, 2009). However, the findings indicated that participants requested more beads when the highest proportion of beads in Jar 1 was red. The findings are consistent with the assertion that the colour red is associated with caution. Further, it appears that red is related to an avoidance motivation which appears to hinder performance (Elliot et al., 2007; Mehta & Zhu, 2009).

As hypothesised, task difficulty, as determined by varying the ratio in which the beads were presented, had an effect on decision-making. All participants responded to increases in task difficulty by increasing the amount of information requested before making a decision. The failure to find a difference between paranoia groups is consistent with previous studies employing clinical samples (e.g. Menon et al., 2006) but not studies employing non-clinical samples at risk from developing delusions (e.g. Broome et al., 2007). It could be argued that the present finding supports the view that people with higher levels of paranoia do reason similarly to those with lower levels of paranoia (Garety & Freeman, 1999; Maher, 1992).

The colour of the dominant bead interacted with task difficulty although it is unclear why. The findings suggest that when the task was easier the colour red motivated an approach response as indicated by fewer draws-to-decision. However, when the task was more difficult red motivated avoidance and

signalled caution as indicated by more draws-to-decision (Elliot et al., 2007; Mehta & Zhu, 2009). One explanation for this shift is that colour only impacts on task performance when cognitive demands are high, that is, when the task difficulty is greater. Clearly, the impact of the colour of the beads employed has considerable implications for the JTC literature given that it has not been previously investigated as a mediating variable in the bias. It is possible that the choice of coloured beads will impact on the JTC bias, although the present finding needs replicating in samples where there is a clear JTC bias before firm conclusions can be drawn. Further, it is likely that the effect of colour on decision-making is more prominent where colours with more apparent connotations are employed (e.g., red).

This was also the first study to investigate the effectiveness of implementation intentions in reducing the JTC bias. Although the data-gathering bias was not detected in the current sample, implementation intentions did influence performance on the tasks. It was predicted that forming an implementation intention would lead to more cautious decision-making, therefore, the absence of a main effect of instruction on either probabilistic task was surprising. Although it just failed to reach significance, the trend of data on the beads task was, however, consistent with expectations. Where participants formed an implementation intention they requested more information before making a decision than those who received no instructions or simply formed a goal intention. Previous attempts to demonstrate the effectiveness of interventions aimed at increasing the amount of information requested on probabilistic reasoning tasks have had similar results. For instance evaluations of Metacognitive Training (MCT) have demonstrated similar non-significant trends

(e.g., Aghotor et al., 2010; Moritz et al. 2011). Further, others have been unable to demonstrate the effectiveness of Cognitive Behaviour Therapy (CBT) on JTC (Garety et al. 2008).

Alternatively, it is also possible that the interaction between instructions, ratio and dominant colour accounts for the failure to find a main effect of instruction on the beads tasks. Implementation intentions were effective in increasing the amount of information requested, compared with the formation of a goal intention or no instruction, in certain conditions. However, it is unclear why implementation intentions were only effective when the dominant bead was red and the ratio 75:25 or 65:35 or when the dominant bead was green and the ratio 55.45. The limited effectiveness of implementation intentions in altering routine decision-making has been demonstrated elsewhere (Betsch, Haberstroh, Molter & Glöckner, 2004). However, other studies have shown that implementation intentions are effective in facilitating performance on executive functioning tasks where response biases exist (e.g., Cohen, Bayer, Jaudas & Gollwitzer, 2008; Miles & Proctor, 2008).

It is possible that task difficulty can offer an explanation, given that the formation of an implementation intention was not effective when the ratio was 95:05 or 85:15 for either dominant colour. Ross et al. (2011) found that their intervention to address the JTC bias was also less effective on easier tasks. They propose that to withhold a decision where the correct answer is apparent (e.g., when the ratio is 85:15 or 95:05) is not advantageous as it improbable that accuracy will improve. The implementation intention instructed participants to gather 'enough information to make a good decision'. On easier tasks, it might not be expected

that more draws-to-decisions *would* lead to any better a decision than fewer draws-to-decision. Accordingly, it is understandable that the amount of information requested by participants who formed the implementation intention would not differ from those who received no instructions or simply formed a goal intention.

Further, there is also evidence to suggest that the effectiveness of implementation intentions is also sensitive to variations in task difficulty (Sheeran, Webb & Gollwitzer, 2006). When the intended goal is easy to achieve (i.e., there are few obstacles) self-efficacy and strong goal intentions are sufficient factors to promote goal achievement, leaving implementation intentions redundant. However, when the task is more difficult, the formation of implementation intentions overcomes potential obstacles (Gollwitzer & Sheeran, 2006). In the present study, participants appear able to achieve the goal of making a decision without the support of implementation intentions when the task is easy (e.g., a ratio of 95:05). However, when the difficulty increases (e.g., a ratio of 55:45) participants benefit from forming an implementation intention.

That there was no difference between paranoia groups on the more emotionally salient survey task is perhaps not surprising given that it proved difficult to detect a JTC bias in the current sample. Further, evidence of the impact of emotional saliency is equivocal (e.g., Dudley et al., 1997b; Warman, 2008; Warman & Martin, 2006; Young & Bentall, 1997). The role of instructions on the survey task was surprising. In the low paranoia group, simply having a goal intention resulted in more information being requested than when participants formed an implementation intention. Further, the formation of an implementation

intention did not result in more cautious reasoning when compared to task instructions only. It is possible that intelligence might explain this finding given that scores on the NART were positively correlated with draws-to-decision on the survey task in the goal intention group.

However, motivational factors might also contribute to the absence of behaviour change in response to the formation of an implementation intention. Increasingly, the importance of concordance between goal intention and implementation intention is recognised as moderating the effect on behaviour (e.g., Sheeran, Webb & Gollwitzer, 2005). Implementation intentions are less likely to be effective when the individual's underlying goal intention is not concordant with the specified behaviour. Further, the strength of goal intention is important. Implementation intentions need to be supported by strong goal intentions. It could be argued that participants in the present study were unlikely to be invested in performing well on the tasks. Consequently, the strength of their goal intention may have been weak thereby affecting the how beneficial the implementation intention was.

Consistent with this notion, participants in the high paranoia group appeared more receptive to forming an implementation intention than those in the low paranoia group on the survey task. It could be argued that participants with higher levels of paranoia are more likely to feel judged or criticised if they perform badly. Accordingly, the strength of their goal intention to do the task well might have been higher, and therefore more concordant with the implementation intention, than participants with lower levels of paranoia. There is also evidence to suggest that people who experience difficulties pursuing

their goals (e.g., people with schizophrenia; Brandstätter, Legnfelder & Gollwitzer, 2001) are more likely to benefit from forming an implementation intention than those who do not experience difficulties in goal striving (Sheeran, Webb & Gollwitzer, 2006). It is therefore possible that participants in the high paranoia group encountered more difficulties in goal striving (e.g., becoming derailed) and so benefitted more from the formation of implementation intentions. However, these inferences are made with caution given that there is no consistent pattern in the effects of implementation intentions across tasks.

Although the paranoia groups did not differ on the survey task, varying the emotional content did have an impact on reasoning between tasks. However, the findings contrast with the assumption that emotional salience will exacerbate JTC across all participants because decision-making was *less* hasty when the content was emotionally salient (Dudley et al., 1997b). This finding was specific to the high paranoia group thereby potentially undermining theoretical accounts which emphasise the role of affect in JTC (e.g., Dudley & Over, 2003). However, caution is needed in interpreting this finding because it could reflect the effectiveness of implementation intentions in increasing the amount of information requested on the survey task.

It is also possible that the ratio in which stimuli was presented can account for the findings. The mean draws-to-decision across ratios on the beads task was employed for task comparison which, therefore, comprised hastier decision-making when the task was easy and conservative decision-making when the task was more difficult. A more valid comparison might have been to match the 60:40 ratio of the survey task to the nearest ratio on the beads task (i.e., 65:35).

Limitations of the Study

It is possible that limitations inherent within the study can account for the failure to detect a data-gathering bias in the present sample. The power of the study was determined by literature pertaining to the effectiveness of implementation intentions. Consequently, the study may have been underpowered to be able to detect the data-gathering bias, increasing the probability that a true JTC effect has been missed in the present sample.

Further, the relationship between JTC and paranoia in non-clinical samples lacks clarity. Although, Lincoln et al. (2011) found an association between non-clinical paranoia and JTC, Freeman et al. (2008) suggested that the presence of persecutory beliefs alone is insufficient; rather it is the conviction with which such beliefs are held that is related to the data-gathering bias. It is possible that the differences between previous study findings reflect the definition of JTC employed (draws-to-decision vs. extreme responding). Nevertheless, had the present study employed a measure of conviction it may have been possible to detect a JTC bias in the present study. However, the complexity of the study design and the partial effects of the intervention cloud any inferences which can be drawn from inability to detect a JTC bias.

The complexity of the study design reflects those variables identified in the literature as having an impact on the JTC bias, in addition to variables which had yet to be explored. This is important if we are to further our understanding of JTC and whether interventions can successfully address the bias. However, the complexity of the present study, and the interactions between variables, limits the extent to which theoretical and clinical implications can be drawn. For

example, the duration and repetitiveness of the beads task may have led to increased fatigue and boredom which likely impacts on participants' effort and concentration.

More fundamental to the present study is that the large number of variables, in addition to the likelihood that the study was underpowered to explore both JTC and the effectiveness of implementation intentions, meant that there were small numbers of participants in each comparison group. Consequently, the analysis of main effects and interactions between variables was compromised due to the increased the likelihood that a type II error has been made (i.e. incorrectly accepting the null hypotheses that there was no difference between groups). Accordingly, caution is required when interpreting the findings; it is possible that the failure to detect a JTC bias was a consequence of a type II error rather than evidence that the bias does not exist in non-clinical samples. Similarly, the trend in the data indicating the effectiveness of implementation intentions may have reached statistical significance had the study design been less complex and sufficiently powered.

To determine whether a JTC bias was present in the current sample, the study design would have benefitted from having a baseline measure by examining reasoning pre and post intervention. However, this introduces the potential for practice effects which are known to increase the amount of information requested as the tasks progress (e.g., Warman & Martin, 2006).

Randomisation checks revealed that participants in the high and low paranoia groups differed in IQ, affiliation with the Department of Psychology, the receipt

of reimbursement and mood/arousal. The study would have benefitted from matching samples from the outset to reduce the possibility that variations between groups influenced the findings.

Findings relating to the relationship between intelligence and the JTC bias are equivocal within the literature. Some studies have found that the bias is associated with lower IQ (e.g., Freeman et al, 2008; Garety et al, 1991) whereas others suggest that the bias is unrelated to intelligence (e.g. Broome et al, 2007; Mortimer et al, 1996). Although participants in the low paranoia group in the present sample had significantly higher IQ than those in the high paranoia group, it is unsurprising that intelligence across both groups was higher than average given the sample was recruited from a university population. It could be argued that higher intelligence protects against the JTC bias because it is associated with improved reasoning.

Although IQ was correlated with the dependent measure on the survey task, this was only true for the high paranoia group and the goal intention group. Given these findings, and the existing complexity of the analysis, IQ was not included as a covariate. However, if higher intelligence does protect against JTC, IQ may offer a further explanation for the failure to detect a JTC bias in the present study. Further, IQ may have contributed to the unexpected finding that participants who formed a goal intention on the survey task requested more information than those who formed an implementation intention. Accordingly, the study would have benefitted from including IQ as a covariate in the analysis to clarify this finding.

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Certainly the relationship between intelligence and the JTC bias requires further consideration in future research, particularly in clinical samples. Given that higher levels of non-clinical paranoia were found in the present sample in the absence of a JTC bias, the study raises a significant question as to whether JTC is associated with paranoia when intelligence is higher. Should intelligence protect against the JTC bias but not the development of paranoia or persecutory beliefs, there are important implications for our understanding of the role of JTC in the formation of delusions.

Although the tasks employed in the present study were consistent with the original paradigm used to explore JTC and thereby allow comparison with previous research, they lacked ecological validity. Accordingly, the task may have influenced the amount of investment participants had in the study. Although effort ratings were generally high, and did not differ between paranoia or instruction groups, the self-report measurement may have lacked validity and be vulnerable to biases. Therefore, it is possible that participants put less effort into completing the tasks than reported. Further, it is unclear what impact the ecological validity of the task has on the JTC bias. Certainly, the survey task was developed, in part, to address this issue. However, the assumption that increasing the social/emotional saliency of the task would exacerbate JTC has received equivocal support (see Fine et al, 2007; Garety & Freeman, 1999). However, attempts to increase the personal saliency of the survey task by having participants generate a list of positive and negative words that describe themselves has impacted on the bias, albeit only in the confidence with which a decision is made (Warman, Lysaker, Martin, Davis & Haudenschield, 2007). Further attempts to increase the ecological validity, and comprehensibility, of

the task while retaining the underlying probabilistic principles (e.g., making judgements about the plausibility of competing titles to describe classical paintings) have successfully demonstrated the JTC bias (Moritz et al, 2009; Woodward, Munz, LeClerc & Lecomte, 2009). However, the findings have not been compared with findings from the original beads task paradigm, therefore the impact on JTC remains unclear.

In addition to the difficulties associated with the ecological validity of the beads task, the presentation of the beads may have introduced bias into the findings. Although the predetermined sequences of beads were presented in a random order, the sequence themselves were randomly generated but then fixed (Appendix E.1). In some sequences (e.g., Red 95: Green 05 in the no prime condition) the first bead could be perceived as misleading (i.e., a green bead was presented when a red would be expected given the ratio). Given the importance of colour on task performance, and the potential impact an incongruent first bead may have, further analysis of the impact of sequencing on the amount of information requested is needed. Certainly, previous studies have employed both fixed order and counterbalanced sequences and found that the presentation order affects the JTC bias (e.g., Moritz & Woodward, 2005; Dudley et al. 1997a). Furthermore, Warman and Martin (2006) found the JTC bias was present on emotionally salient tasks only when the first stimulus was weighted negatively.

The limited effectiveness of the implementation intentions may reflect the design of the implementation intention itself. To be effective the specified cue to respond (the '*if*' of the implementation intention) needs to be accessible (Webb

& Sheeran, 2007; 2008). The cue in the present implementation intention may have benefitted from increased specificity (e.g., rather than *'if I am faced with a decision'* a more specific cue could be '*if I am asked to make a decision about which jar a bead has come from'*. Further, the behavioural response might have been better matched to the task (e.g., rather than *'then I will ask myself - have I really gathered enough information?'* a more specific response could be *'then I will want to see as many beads as I need to be certain'*). Although, altering the implementation intention may have improved effectiveness in the present study, it would raise questions about the clinical applicability given an intervention needs to be flexible enough to cover a range of scenarios faced by people with delusions.

Clinical Implications

Any clinical implications drawn from this study must be done so with caution given that the findings lack clarity and are vulnerable to the limitations described above. However, the formation of implementation intentions did increase the amount of information requested on the beads task. Although this was not significant, it suggests that implementation intentions may have the potential to reduce the JTC bias in people experiencing delusions. It could be argued that people with delusions do encounter difficulties in goal striving and JTC may reflect difficulty in disengaging from a failing course of action. Certainly, participants with high paranoia benefitted more from forming an implementation intention, albeit only on the survey task.

The impact of colour on task performance has interesting possibilities for how interventions addressing the JTC bias are delivered. It appears that when

cognitive demands are high, the colour red is associated with more caution and therefore less hasty decision-making. If this finding is replicated in further research with clinical samples, then presenting written interventions (e.g., 'flash cards') in red might enhance effectiveness.

Further Research

The potential clinical implications of this study are dependent on further research exploring the effectiveness of implementation intentions in reducing the JTC bias, because the present findings lack clarity. The JTC bias appears to be more pronounced where participants are currently experiencing delusions. Therefore, future studies should explore implementation intentions in clinical samples because there is more scope to demonstrate effectiveness. The present study also emphasises how task manipulations interact and impact on the amount of information requested on probabilistic reasoning tasks. Future studies would benefit from reducing the complexity of the task (e.g., employing the beads task using one ratio only) in the first instance before systemically varying the tasks to better understand the effectiveness of implementation intentions. Should implementation intention prove to be effective on experimental, probabilistic reasoning tasks, consideration should be given to how this brief, cost-effective, intervention can be adapted and developed for use in clinical practice with people experiencing delusions.

The present study demonstrates the complexities involved in investigating the JTC bias. The interactions between task manipulations might account for some of the discrepancies in the JTC literature and should be considered in future research. In particular, the colour of the beads employed surprisingly emerged

as a significant factor. This finding certainly needs replicating to reliably draw any firm conclusions. However, the design of future studies should consider the choice of coloured beads carefully and analyse the impact of colour on the dependent measure employed. Further, studies would benefit from considering the relationship between IQ and performance on probabilistic reasoning tasks.

Although a number of factors could explain the absence of the JTC bias in the present sample, there are still only three studies to date considering JTC in non-clinical, paranoid, samples. If we are to employ non-clinical samples to make inferences about the experiences of individuals with persecutory beliefs it is important that we first understand how the JTC bias relates specifically to persecutory delusions. Comparing the JTC bias by delusional subtype is a neglected area in the JTC literature (Freeman, 2007). This is particularly important given the impact different delusional beliefs may have on reasoning (McKay et al., 2007).

CONCLUSION

The present study failed to replicate the finding that JTC is demonstrated by non-clinical participants with high levels of paranoia. Therefore, the study does not support the assertion that JTC contributes to the formation of delusions or is specific to persecutory subtypes. Further, the study failed to replicate the exacerbation of JTC in response to rushing. However, task difficulty did impact on performance with more information requested as the difficulty of the task increased. Interestingly, where the colour of the dominant bead in the first jar

presented was red more information was requested, indicating that red is associated with more cautious reasoning, particularly where cognitive demands are high (i.e., when the task is more difficult). Varying the emotional salience of the task did affect performance. More information was requested on the emotionally salient task compared to the neutral task only in the high paranoia group. However, it is likely that the effectiveness of implementation intentions can account for this finding.

The effectiveness of implementation intentions was not conclusive. The non-significant trend in the data suggested that forming an implementation intention did result in reduced hastiness, but only on the beads task. Implementation intentions appeared to most effective when the task was difficult or where participants may experience difficulties in goal striving (i.e., when paranoia is high). The findings should be viewed with caution given the limitations inherent within the study. Further research is needed to better understand how task manipulations influence the JTC bias, clarify the role of JTC in the formation of delusions and the specificity to persecutory beliefs, and to more reliably demonstrate the effectiveness of implementation intentions.

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SECTION 3: APPENDICES

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Appendix A.1: University Approval of Specified Journals

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Appendix A.2: Guidance for Authors: Clinical Psychology Review

Appendix removed in concordance with copyright legislation.

Appendix A.3: Guidance for authors: Behaviour Research and Therapy

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Appendix B.1: Ethical Approval

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THE JTC BIAS: META ANALYSIS & EFFECTIVENESS OF IMPLEMENTATION INTENTIONS

Appendix C.1:   Email invitation to the study

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Appendix C.1:   Email invitation to the study

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Appendix C.2:　　Information sheet

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Appendix C.3: Consent Form

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Appendix C.3: Consent Form

Appendix C.4: Debriefing Information

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APPENDIX D

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Appendix D.2: Affect-Arousal Scale (adapted from Aarts & Dijksterhuis, 2003)

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Appendix D.3: The National Adult Reading Test 2nd Edition (NART-2; Nelson & Willison, 1991)

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Appendix D.4: Effort Rating

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APPENDIX E

Appendix E.1   Sequence of stimuli – beads task

Appendix E.2   Sequence of stimuli – survey task

Appendix E.1: Sequence of beads

Appendix removed in concordance with copyright legislation.

Appendix E.2: Sequence of comments; reproduced with kind permission from Robert Dudley (personal communication)

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