Attachment theory and paranoid cognitions: An experimental investigation

A thesis submitted to the University of Manchester for the degree of Doctor in Clinical Psychology (ClinPsyD) in the Faculty of Medical and Human Sciences.

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School of Psychological Sciences

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Total word count: 22, 786

Thesis Abstract

This thesis has been prepared in paper based format. The thesis focusses of the use of experimental manipulations in the investigation of paranoia and extends the use of these to an empirical investigation of the role of attachment theory in paranoia. Papers 1 and 2 have been prepared for submission to Clinical Psychology Review and Schizophrenia Bulletin respectively.

Paper 1 provides a comprehensive overview of experimental paradigms that aim to induce or manipulate paranoid thinking in both clinical and analogue samples. Twenty-seven studies were identified that satisfied inclusion criteria for the review. The strengths, limitations, effectiveness of individual paradigms, as well as of the literature as a whole, are considered throughout the review and recommendations for future research are made. Theoretical and clinical implications are also discussed.

Paper 2 reports an experimental analogue in which participants (N=60) were randomised to a secure attachment prime (or neutral/positive affect control) condition before being exposed to a paranoia induction paradigm. Dispositional levels of insecure attachment were associated with both trait and state paranoid thinking. Contrary to predictions, the secure attachment prime did not appear to buffer paranoid thinking. The secure attachment prime was indicated to have a negative impact for people with high levels of attachment anxiety, who experienced higher levels of paranoia following the paranoia induction.

Paper 3 is a critical reflection of the submitted papers and research process as a whole. The strengths and limitations of the presented research, methodological considerations and implications for clinical practice and theory are discussed and directions for future research are highlighted.

Declaration

No portion of the work referred to in the thesis has been submitted in support of an application for another degree or qualification of this or any other university or other institute of learning.

Copyright and Ownership

Acknowledgements

I'd like to acknowledge all who have contributed to the completion of this thesis. Particular thanks to all the people who took part in the research and to my supervisors Dr Katherine Berry and Dr Lyn Ellett for their time and patience and contribution throughout the research process.

I'd like to thank my wonderful friends, family, Mum, Dad and fiancé for their constant support, kindness, fun times and laughs along the way.

Section 1: Systematic Review

Title

Experimental manipulations of paranoid thinking: a systematic review

The following paper has been prepared for submission to 'Clinical Psychology Review'. The guidelines for authors can be found in appendix A

Word Count:

Total (excluding references and tables):	12, 746
Abstract:	178

Highlights:

1) A comprehensive overview of experimental studies that induce or manipulate paranoid thinking is provided.

2) Twenty-seven studies were included in the review and the strengths, limitations and effectiveness of individual approaches are considered throughout.

3) The importance of factors such as ambiguity, failure, high self-awareness, stress, exclusion, interpersonal context and baseline vulnerability was indicated across the included studies.

4) Future research should include both pre- and post-measures of state paranoia, assessment of the mechanisms targeted by paranoia inductions, and consideration of the role of existing vulnerability in paradigm effectiveness.

Abstract:

Experimental methodologies have contributed significantly to the development of theoretical models of persecutory and paranoid thinking. The current review provides a comprehensive overview of experimental paradigms that aim to induce or manipulate paranoid thinking in both clinical and analogue samples. Twenty-seven studies were identified that satisfied inclusion criteria for the review. The methodologies identified were varied and included stress vulnerability paradigms, virtual reality and computer based approaches and manipulations targeting attentional focus and interpretations of negative events. The importance of factors such as ambiguity, failure, high self-awareness, stress, exclusion, interpersonal context and baseline vulnerability, was indicated across the included studies. The reviewed studies were generally well designed with the majority (N = 23) demonstrating the effective manipulation of paranoid thinking. Effect sizes were calculated where possible, and these ranged from small to large, with the majority of studies achieving medium to large effects. The strengths, limitations, effectiveness of individual paradigms, as well as of the literature as a whole, are considered throughout the review and recommendations for future research are made. Theoretical and clinical implications are also discussed.

Key words: Experimental manipulation, paranoia, persecutory delusions

1. Introduction

The term paranoia is often used to describe a particular form of thinking, in which a person falsely believes themselves to be under intentional threat of harm from others (Ellett, Lopes, & Chadwick, 2003; Freeman & Garety, 2000). While paranoid thinking can be commonly experienced in nonclinical populations (Johns et al., 2004), persecutory delusions are distinguished by their fixed and rigid nature as well as the distress and disruption caused to the person's life or to those around them (Freeman & Garety, 2000). Persecutory delusions are often cited as the most commonly experienced form of delusional thinking (e.g. Garety, Everitt, & Hemsley, 1988) and, along with hallucinatory experiences, are a hallmark of psychosis.

A number of psychosocial models of persecutory thinking have been proposed and place varying emphasis on the importance of, for example, the developmental, cognitive, behavioural, affective and interpersonal factors involved in the formation and maintenance of such phenomena. Cognitive models emphasise the importance of the interpretation of anomalous events, and how these interpretations are influenced by factors such as previous life experience, attentional and attributional biases, and affective states (Bentall, Corcoran, Howard, Blackwood, & Kinderman, 2001; Freeman, Garety, Kuipers, Fowler, & Bebbington, 2002; Morrison, 2001). Continuum models of psychosis, stemming from the observed prevalence of psychotic-like-experiences (PLEs) in the general population (Strauss, 1969; van Os, Hanssen, Bijl, & Ravelli, 2000), recognise the overlap between psychopathology and everyday experience. Some have argued that experiences like paranoia are relatively common because they confer an evolutionary advantage (e.g., it is 'better to be safe than sorry') (Ellett, et al., 2003). Such explanations may offer a more normalising framework for the understanding of clinical levels of paranoia.

Improving the understanding of paranoia in the general population may provide important insights into the more distressing and disabling forms of paranoia seen in psychosis. Experimental studies involving nonclinical populations have contributed a great deal to both

the development and testing of psychological models of psychopathology (e.g. Kinderman & Bentall, 2000; Koriat, Lichtenstein, & Fischhoff, 1980). A more recent line of research has involved experimentally manipulating and measuring paranoid-like experiences. These paradigms offer an exciting avenue for the exploration of paranoid experiences and have begun to offer potential insights into our understanding of processes associated with paranoid thinking.[1] Both the application and divergence of such methodologies has increased over recent years. The purpose of the current review is to contextualise and evaluate these emerging findings, examining potential clinical implications and offering guidance on the future applications of these experimental paradigms. With the exception of a recent narrative review of the virtual reality literature (Freeman, 2008), to the authors' knowledge no other systematic review of paranoia induction in a nonclinical or clinical population has been conducted. The key aims of the review are as follows:

- To provide an overview of the different paradigms used to induce paranoia in nonclinical and clinical samples, including evidence in relation to their effectiveness and utility.
- To evaluate the strengths and weaknesses of the paradigms.
- To provide recommendations about future research in this area.

2. Method of Searching

The aim of the literature search was to identify studies that attempted to experimentally induce or manipulate paranoid thoughts. An initial scoping exercise identified several key papers. An analysis of key words in the abstract and titles of these papers helped determine the selection of search terms. Six databases (PsycINFO, PubMed, EMBASE, Web of Science, Medline and AMED) were searched up until January 2013. Abstracts and titles were searched for the following:

[1]The author recognises the diversity surrounding definitions and measurement of paranoid thinking, however for brevity, terms such as paranoia, paranoid ideation and paranoid thinking will be used interchangeably throughout the review.

"(persecution OR persecutory OR paranoid OR paranoia OR suspiciousness OR suspicious thoughts) AND (experimental OR manipulation OR manipulated OR induction OR induced OR paradigm)"

Studies employing paradigms that aimed to experimentally induce or manipulate paranoid thinking, and incorporated a direct measure of paranoia, were eligible for inclusion in the review. Definitions of paranoia vary across the literature, for example, Freeman and Garety note that; '*terms such as paranoia, delusions of persecution and delusions of reference have been used interchangeably and to refer to different concepts'* (Freeman & Garety, 2006 p.405). In line with recent, more stringent, definitions of paranoid thinking in clinical (Freeman & Garety, 2000) and nonclinical (Freeman, 2006) populations, studies had to include measures of paranoid thinking with elements relating to intentional harm or persecution by others. Studies assessing suspiciousness of experimental procedures in the absence of these elements were not included (e.g. Cook & Perrin, 1971; Horvat, 1986; Martin, 1970). Studies that measured factors associated with paranoid thinking, such as reasoning biases, other forms of delusional thinking (ideas of reference, magical thinking) or hallucinations, in the absence of a direct measure of paranoid thinking, were also excluded. Experimental studies of drug (e.g. Couzoulis-Mayfrank et al., 2005; Mason, Morgan, Stefanovic, & Curran, 2008) and sleep (e.g. Kahn-Greene, Killgore, Kamimori, Balkin, & Killgore, 2007) induced paranoid thinking were also excluded as the focus of the current review was on psychological paradigms. Of the studies that fulfilled these criteria, only English language and peer-reviewed articles were included in the review. No restriction was placed on year of publication. Additional search strategies, such as reference list cross-checking, were also employed.

Following the exclusion of duplicate articles, remaining results were assessed at either title, abstract or full text level to determine suitability for the current review. All articles were assessed by the first author and any instances of uncertainty were discussed with the second and third authors. This resulted in 23 papers (27 individual studies) being included in the current review. Only four of these studies included clinical samples. Two were reported

in papers also involving nonclinical samples (Freeman, Pugh, Vorontsova, Antley, & Slater, 2010; Moritz et al., 2011) and two were reported in individual papers (Ellett, Freeman, & Garety, 2008; Valmaggia et al., 2007). The findings of Valmaggia et al. (2007) were re-reported in an additional paper included in the current review (Freeman et al., 2007). The search process is summarised in Figure 1.

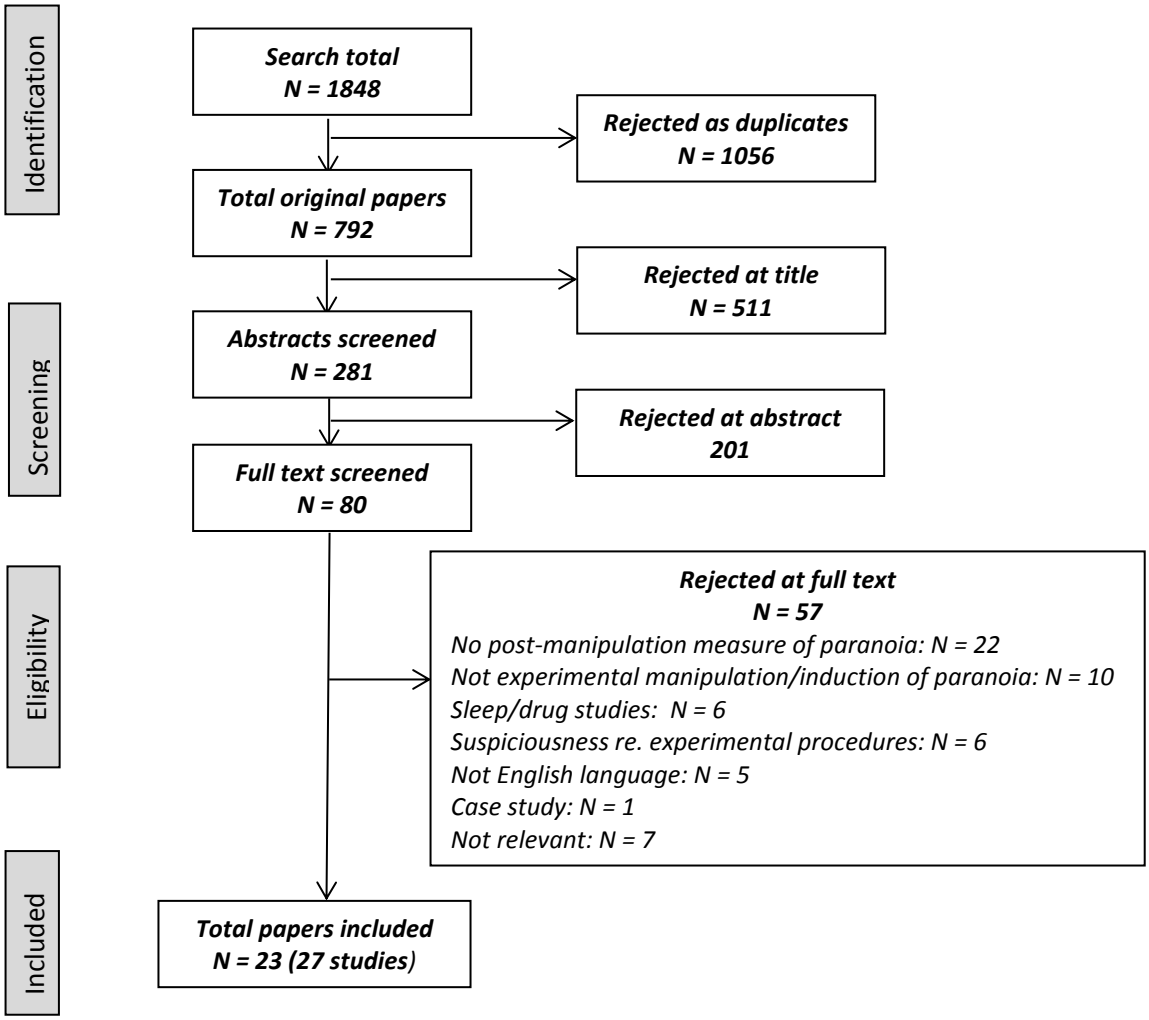

Fig.1. Search results:
(Figure adapted from guidelines set out by the PRISMA group (Moher, Liberati, Tetzlaff, & Altman, 2009))

3. Summary of studies

3.1. Overall summary

Table 1 provides a summary of the 27 studies included in the review. Preliminary data extracted from studies included: (a) sample type; (b) sample size; (c) design (number of groups, conditions, use of randomisation procedures); (d) paradigm/manipulation; (e) primary paranoia outcome measure; and (f) country. Date of publication ranged from 1975 to 2013. Studies were organised into five groups based on the type of experimental paradigm used:

1. Stress vulnerability paradigms: five papers (five studies)
2. Manipulation of attentional focus paradigms: five papers (seven studies)
3. Virtual games: three papers, (five studies)
4. Virtual reality paradigms: six papers (six studies)
5. Other paradigms: four papers (four studies)

The majority of studies (N = 15) incorporated a randomised between-subject design, demonstrating the generally well controlled nature of the studies included in the review. Of the remaining 12 studies, four used a repeated measures design in which the order of conditions was counterbalanced. A minimum time period of one day was left between the completion of conditions in order to minimise any carryover effects, again showing a high standard of design integrity across studies. The majority of the uncontrolled studies (N = 9) came from the virtual reality group. Twenty-five studies involved either exclusively nonclinical (N = 22) or mixed clinical and nonclinical (N = 3) samples. Only two studies involved clinical populations (Ellett, et al., 2008; Valmaggia, et al., 2007). Of the 25 studies including nonclinical samples, 14 recruited from student populations, 10 studies used general population samples and one study (Freeman, et al., 2007) included both a student (N = 64) and general population (N = 100) sample.

Table 1: Characteristics of studies

Author	Sample	Design	Manipulation	Measure	Country
Stress vulnerability paradigms: N = 5					
Lincoln et al. (2009)	Nonclinical (student) N = 64	Randomised, repeated measures (4-6 day interval). 2 conditions: Stress condition & control	Stress: difficult task + building site noise. Control: easier tasks, no noise. Ambiguous experimenter behaviour in both groups.	PCL (State adapted)	Germany
Moritz et al. (2011)	Mixed: N = 35; General population (n = 15), Clinical (N = 20)	Randomised, repeated measures (1-7 day interval). 2 conditions: stress condition & control	Stress condition: Cognitive task + building site noise. Control: identical task, no noise.	PCL (State adapted)	Germany
Lincoln et al. (2010a)	Nonclinical (student): N = 90	Randomised, between subjects. 2 conditions: anxiety condition & control	Anxiety: Dark room + anxiety images (International affective picture system) + imagery task: personal anxiety provoking situation. Control: neutral images/neutral imagery task	PCL (State adapted)	Germany
Palmier-Claus et al. (2011)	Nonclinical (student): N = 70	Randomised, repeated measures (1-2 day interval). 2 conditions: stress/anxiety & Control	3 tasks per condition; 1) Difficult cognitive task + building site noise (control: easy task/no noise). 2) Anxiety images from International affective pictures system (control: neutral images) 3) Imagery: social stress (control: neutral imagery task)	PCL (State adapted)	UK
Lopes & Pinto-Gouveia (2012)	Nonclinical (general population): N = 84	Randomised: between subjects 3 groups: high paranoia, high anxiety & neutral control 2 conditions: success & failure	Failure: difficult task plus failure feedback Success: easy task plus success feedback	PCL & PS	Portugal
Manipulation of attentional focus paradigms: : N = 5					
Bodner & Mikulincer (1998; Study 1)	Nonclinical (student): N = 177	Randomised, between subjects 2 (attentional focus: type) x 3 (task) x 3 (attentional focus: technique). 18 conditions (n = 9-10 per condition).	Failure (Personal, Universal, Control) X attentional focus (Experimenter, self) and focussing techniques (Mirror, Video camera, mirror & camera)	PDS	Israel
Ellett & Chadwick (2007)	**Study 1:** nonclinical (student): N = 60	Randomised, Between subjects: 6 conditions	Task (Failure/ambiguous) X attentional focus (HSA,LSA, control)	PS & PDS	UK
	Study 2: nonclinical (student): N = 40	Randomised: Crossover design. ABAB or BABA	Ambiguous task plus switching manipulation of attentional focus (HSA/LSA)	PDS	UK
	Study 3: nonclinical (student): N = 30	Randomised, between subjects. 2 conditions: Paranoia induction plus buffer or control.	HSA plus failure task with positive- (buffer) or negative- (control) self-cognition prime.	PDS	UK

Author	Sample	Design	Manipulation	Measure	Country
Prevost et al. (2011)	Nonclinical (general population): N = 34	1 group/condition.	Cognitive task during EEG plus suggestion that experimenter can change brain activity plus observed via one way mirror.	PDS	Canada
Boden & Berenbaum (2007)	Nonclinical (student): N = 118	Randomised, between subjects 2 conditions: High or low emotional awareness.	Negative mood induction plus High EA manipulation (story designed to increase awareness of emotions and antecedents (vs. low EA control).	PS	USA
Locascio et al. (1975)	Nonclinical (student): N = 60	Randomised, between subjects 3 conditions: selected attention to threat & non-threat/unbiased attention controls.	Exposure to threatening & non-threatening personal evaluations: Experimental: attention directed to threat related material. 2 Controls: Attention directed to non-threatening statements & no attentional manipulation	VAS	USA
Virtual games: **N = 5**					
Westermann et al. (2012)	Nonclinical (general population) N = 116	Randomised, between subjects 2 conditions: social stress & control	Excluded during virtual ball tossing game. Control: inclusion during same game	PCL (State adapted)	USA
Kesting et al. (2013)	Nonclinical (general population) N = 76	Randomised, between subjects 2 conditions: social stress plus criticism & control	Excluded during virtual ball tossing game plus negative feedback. Control: inclusion during same game plus Neutral feedback.	PCL (State adapted)	Germany
Ellett et al. (2013)	**Study 1:** nonclinical (student) N = 175	1 group/condition.	PDG	SPS	UK
	Study 2: nonclinical (student) N = 111	Randomised, between subjects 2 condition: computer vs. person opponent	PDG: 2 conditions: opponent as person or computer.	SPS	UK
	Study 3: nonclinical (student) N = 152	1 group/condition.	PDG plus measure of reasoning: trust or greed	SPS	UK
Virtual reality paradigms: N = 6					
Freeman et al. (2003)	Nonclinical: (student/university staff) N = 24	1 group/condition.	Exposure to neutral VR environment: library. Ambiguous avatar behaviour reported.	VR- Q	UK
Freeman et al. (2005a)	Nonclinical (general population; low – high trait paranoia) N = 30	1 group/condition.	Exposure to neutral VR environment: library. Ambiguous avatar behaviour reported).	VR- Q	UK
Freeman et al. (2007)	Mixed: N = 185 Student (N = 64), general population (N = 100), Clinical (ARMS; N = 21).	3 Groups (Student, General Population, ARMS). 1 condition.	Virtual reality train journey (London underground)	SSPS	UK
Freeman et al. (2008)	Nonclinical (general population) N = 200	1 group/condition.	Virtual reality train journey (London underground)	SSPS	UK
Freeman et al. (2010)	Mixed: N = 90 General population: low & high trait paranoia (N = 30), plus clinical (persecutory delusions N = 30)	1 group/condition.	Virtual reality train journey (London underground)	SSPS	UK

Author	Sample	Design	Manipulation	Measure	Country
Valmaggia et al. (2007)	Clinical (ARMS) N = 21	1 group/condition.	Virtual reality train journey (London underground)	VR-Q	UK
Other nonclinical studies: N = 2					
Green et al.	Nonclinical (general population) N = 58 Recruited from N = 323; range of trait paranoia	Randomised, between subjects 2 groups: Low/high state paranoia scores. 1 condition.	Participants experienced Interruption & recorded laughter while completing a filler task. Group differences compared.	VAS/ interview	UK
Marr et al. (study 1b; 2011)	Nonclinical (general population) N = 93	Randomised, between subjects 3 Conditions: 1x experimental & 2 x control conditions	Manipulation of motivational goal (1 of 3 conditions): 1) relationship threatening information 2) positive information 3) information about electrical products	PS (work Adapted)	Canada
Casanova et al. (1988)	Nonclinical (students) N = 80	Randomised: between subjects. 2 (evaluation type) x 2 (clarity of recording), N = 20 per condition.	Listening to taped personal evaluations: evaluation type (positive or negative) X clarity (intelligible or partially unintelligible)	PAC	USA
Other clinical studies: N = 1					
Ellett et al. (2007)	Clinical (persecutory delusions) N = 30	Randomised, between subjects 2 conditions: urban exposure & Control	Experimental: 10 minutes exposure to busy shopping street Control: 10 minutes mindful relaxation	SSPS	UK

*ARMS findings (N = 21) re-reported in Valmaggia et al. (2007). Acronyms: PDS: Paranoia & Depression Scale, CIQ: Cognitive interference Questionnaire, PC: Paranoia Checklist, PS: Paranoia Scale, SSPS: State Social Paranoia Scale, PAC: Paranoid Reactions Adjective Checklist, VAS: Visual Analogue Scale, SPS: State paranoia Scale, MMPI: Minnesota Multiphasic Personality Inventory, GPTS: Green Paranoid Thought Scale.

LSA: Low self-awareness, HSA: High self-awareness, EA: Emotional awareness, VR; Virtual reality,

3.2. Measures

The majority of the studies used valid and reliable measures of state paranoia, summaries of which are given in Table 2. Of the nonclinical and mixed design studies, the most frequently used measure was the Paranoia Checklist (PCL; Freeman et al., 2005b) a version of which was used in seven of the included studies. The Paranoia and Depression Scale (PDS; Bodner & Mikulincer, 1998) was used in five studies, the State Social Paranoia Scale (SSPS; Freeman, et al., 2007) was used in four studies and the State Paranoia Scale (SPS; Ellett, Allen-Crooks, Stevens, Wildschut, & Chadwick, 2013) and Paranoia Scale (PS; Fenigstein & Vanable, 1992) were each used in three studies. The Virtual Reality Questionnaire (VR-Q; Freeman et al., 2005a) was used in 2 studies and the Paranoid Reactions Adjective Checklist (PAC; Katkovsky, 1986) was used in one study. While a number of studies incorporated Visual Analogue Scales (VAS) into their design; only two relied solely on such measures (Green et al., 2011; Locascio & Snyder, 1975). Of the clinical studies, Ellett et al. (2008) used the SSPS, and Valmaggia et al. (2007) used the VR-Q.

There is limited information regarding content, reliability and validity of the PAC, therefore findings from this measure should be viewed with caution. The remaining measures have adequate to excellent internal consistency and moderate to large convergent validity correlations with other measures of paranoia. While these measures all contain items relating to intentional harm or persecution by others (Freeman & Garety, 2002), some (PCL, PS and PDS) also include items relating to what Freeman et al. (2007) refer to as the 'hierarchy of paranoia'; that is, items relating to ideas of reference and negative social evaluation. Three measures (SSPS, VR-Q and SPS) provide a clearer assessment of persecutory thinking as defined by Freeman and Garety (2002). While the PCL offers a multidimensional assessment of paranoia, the studies included in this review used a state-adapted, one-dimensional version of the scale. Lincoln, Peter, Schafer & Moritz (2009) reported good internal consistency and convergent validity of a German state-adapted version of this measure.

Table 2. Summary of measures

Measure	Author (year)	Description/Subscales	State /Trait	Items/scoring	Sample items (paranoia)	Psychometric properties
Paranoia Checklist (PCL)	Freeman et al. (2005b)	Developed based paranoid thoughts in nonclinical populations. Items based on clinical experiences of paranoia. Measure multidimensional experiences of paranoia	Trait	18 items rated on a 5-point Likert scale for each domain (frequency, severity, distress)	*"'People would harm me if given an opportunity', 'People communicate about me in subtle ways*', '*Strangers and friends look at me critically'*	General population sample (UK; N = 1202; Freeman et al., 2005b): Excellent internal consistency (Cronbach's alpha ≥ .9) Large convergent validity correlation (r = .71, p < .001) with another measure of paranoia (PS)
Paranoia scale (PS)	Fenigstein & Vanable (1992)	Developed to measure trait paranoia in college students. Derived from existing measures of clinical paranoia.	Trait	20 items rated on a 5 -point Likert scale. Total score ranges from 20-100.	*'I believe that I have often been punished without cause', ''People have said insulting and unkind things about me', 'I am bothered by people outside, in cars, in stores etc. watching me'*	Student sample (USA; total N = 581; Fenigstein & Vanable, 1992): Good internal consistency (Cronbach's alpha ≥.8). Good 6-month test re-test reliability correlation (*r* = .7, *p* not reported). Reasonable construct validity with measures of trust, anger and control by power full others.
Paranoia & depression scale (PDS)	Bodner & Mikulincer, (1998)	Developed to measure nonclinical paranoid and depressive cognitions within the experimental context. Derived from existing measures of clinical paranoia.	State	17 items rated on a 6 -point Likert scales. Paranoia subscale (7 items) ranges from 7-42.	*'I feel that my behaviour is being analysed' 'I feel that people are hostile to me' 'I feel that others influence my performance' 'I do not trust other people's intentions'*	Student sample (Israel; N = 149; Bodner & Mikulincer, 1998). Good internal consistency (Cronbach's alpha = .84) Large convergent validity correlation (r = .67, p < .01) with trait measure of paranoia.
Virtual Reality Questionnaire (VR-Q)	Freeman et al. (2003)	Developed to measure paranoid views of virtual characters within the virtual reality context. Derived from definitions of persecutory thinking.	State	15 items rated on a 4 point scale. 3 subscales (5 items each); persecutory positive and neutral views.	*'Someone in the room was hostile towards me', 'Someone in the room would have harmed me in some way if they could'*	General population (UK; N = 30, Freeman et al., 2005a) Adequate internal consistency (Cronbach's alpha = .66). Moderate-large convergent validity correlations on interview (*r* = .55, *p* = .002) and VAS (*r* = .48, p = .008) ratings of paranoia
State social paranoia scale (SSPS)	Freeman et al. (2007)	Derived from definitions of persecutory thinking. Used in clinical and nonclinical samples. Similar items to VR-Q with 5 additional paranoia items.	State	20 items, Persecutory subscale (10 items), Neutral & friendly subscales (5 items each).	*'Someone wanted me to feel threatened', 'Someone had it in for me'*	Clinical & nonclinical (UK; N = 185, Freeman et al., 2007) Excellent internal consistency (Cronbach' alpha = .91), adequate test-retest reliability and moderate convergent validity correlations (*r* = .38, *p* < .001).
State paranoia scale (SPS)	Ellett et al., (2013)	Designed to asses paranoid thinking 'vis-à-vis' another person. Derived from definitions of persecutory thinking.	State	4 items, rated on a 7 point scale with 2 opposing statements. Range; 4-28.	*'Is friendly towards me' or 'Is hostile towards me', 'Wants to help me' or 'Wants to harm me'*	Student samples (UK; N = 131 & N = 286; Ellett et al., 2013). Excellent internal consistency (Cronbach' alpha = .91), moderate convergent validity correlation (*r* = .415, *p* < .001).

Measure	Author (year)	Description/Subscales	State /Trait	Items/scoring	Sample items (paranoia)	Psychometric properties
Paranoia Reactions Adjective Checklist (PAC)	Katkovsky (1986);un –published	Little known information given unpublished nature. Casanova et al. (1988) stated developed to tap *'emotional & defensive'* reactions associated with paranoia.	Trait	92-items, 8 subscales including reference, grandiosity, hostility.	Unknown	Casanova et al. (1988) state that the developers have the measure have demonstrated the test –retest reliability and construct validity of the PAC. Details unavailable.

3.3. Effect size calculations

To aid the overall evaluation of different studies, effect sizes were calculated wherever possible using Review Manager (The Cochrane Collaboration, 2008) and Comprehensive Meta-Analysis Software Version 2 (Borenstein, Hedges, Higgins, & Rothstein, 2005) and with reference to The Cochrane Handbook (The Cochrane Collaboration, 2009). Hedges's *g* effect size was chosen as this calculation helps to take account of small sample sizes as evident in some of the current literature. Effect size interpretations of Hedges's *g* are comparable to those suggested for Cohen's *d*. Cohen (1992) suggested that, for Cohen's *d* values, a small effect = 0.2, medium = 0.5 and large = 0.8. Nineteen effect sizes associated with 13 of the included studies were calculated (Table 3). For the majority of studies (N = 12) these were derived from mean and standard deviation data either directly reported or from data extracted from graphical plots (N = 1, Moritz, et al., 2011). Casanova, Katkovsky and Hershberger (1988) did not report means and standard deviations and only reported means and inexact p-values for group differences, and only for the PAC subscales. To estimate an overall effect, upper estimates of each p-value were first used to compute a conservative estimate of the effect size for the group differences (The Cochrane Collaboration, 2009). The average of each estimate and associated standard error was then taken to provide an estimate of the overall effect and associated 95% confidence intervals. As recommended by Dunlap, Cortina, Vaslow, & Burke (1996) studies incorporating repeated measures designs were treated in the same way as between subject designs with regards to effect size calculations. While recognising the limitations of this approach, the more accurate calculation requires access to raw data, which was not possible. Effect size estimates are provided in Table 3.

Effect size calculations were not possible for studies that did not report adequate data to allow this[2] (N = 4; Kesting, Bredenpohl, Klenke, Westermann, & Lincoln, 2013; Lopes & Pinto-Gouveia, 2012; Palmier-Claus, Dunn, Morrison, & Lewis, 2011; Westermann, Kesting,

[2] When not available, data was requested from corresponding authors, however, this data was not supplied at the time of writing.

& Lincoln, 2012) or for uncontrolled designs (N = 10, Ellett et al., 2013, Studies 1&3, Freeman et al., 2003, Freeman et al., 2005a, Freeman et al., 2007, Freeman et al., 2008, Freeman et al., 2010, Valmaggia et al., 2007, Green et al., 2011 & Prevost et al., 2011).

The power of each study to detect small, medium and large effects was also calculated using G*power software (Faul, Erdfelder, Lang, & Buchner, 2007). As shown in Table 3, only eight studies had adequate power (80%) (Cohen, 1988) to detect large effects of the experimental manipulation. Only one study (Lincoln et al., 2009) had adequate power to detect a moderate effect, and no studies were adequately powered to detect small, yet potentially important, effects.

Table 3. Effect size summary

Study	Comparison	N (Exp:control)	Measure	Hedges's g	95% CI	Size (direction)	Power 0.2 (small effect)	Power 0.5 (medium effect)	Power 0.8 (large effect)
Stress vulnerability paradigms:									
Lincoln et al. (2009)	Stress vs. no stress	64**	PCL	0.25	-0.1, 0.59	Small (P)	35%	97.6%***	99.9%***
Moritz et al. (2011)	Clinical group: Stress vs. no stress	20**	PCL	0.10	-0.51, 0.70	Marginal (P)	13.6%	56.5%	92.4%***
Moritz et al. (2011)	Nonclinical group: Stress vs. no stress	15**	PCL	-0.10	-0.80 - 0.60	Marginal (N)	11.2%	43.8%	82.1%***
Lincoln et al. (2010)	Stress vs. no stress	45:45	PCL	0.66	0.23 - 1.08	Medium (P)	15.5%	65%	96.4%***
Manipulation of attentional focus paradigms:									
Bodner & Mikulincer	Personal failure/experimenter focus vs. neutral/experimenter.	10:10*	PDS	0.71	-0.02 - 1.62	Medium (P)	7.1%	18.5%	39.5%
(Study 1; 1998):	focus (technique: mirror plus camera)								
	Personal failure/experimenter focus vs. neutral experimenter	10:10*	PDS	0.95	0.02 - 1.89	Large (P)	7.1%	18.5%	39.5%
	focus (technique mirror plus camera)								
Ellett & Chadwick	Study 1: Failure plus Camera vs. Neutral task alone (time 1)	10:10*	PS	1.04	0.09 - 1.99	Large (P)	7.1%	18.5%	39.5%
(2007)	Study 1: Failure plus Camera vs. Neutral task alone (time 1)	10:10*	PDS	0.15	-0.73 - 1.03	Marginal (P)	7.1%	18.5%	39.5%
	Study 2: experimenter vs. self-focus (time 1)	20:20*	PDS	1.07	0.40 - 1.74	Large (P)	9.5%	33.8%	69.3%
	Study 3: Negative vs. Positive affirmation prime (time 1)	15:15*	PDS	1.07	0.3 - 1.85	Large (P)	8.3%	26.2%	56.2%
Boden & Berenbaum	Males: High EA vs. Low EA	27:23	PS	0.99	0.40 - 1.58	Large (P)	10.6%	40.8%	78.9%
(2007)									
Virtual games:									
Kesting et al. (2013)	Social stress + negative feedback vs. control (endpoint data)	39:37*	PLC	0.28	-0.168 - 0.73	Small (P)	13.8%	57.6%	93.1%***
Ellett et al. (2013)	Study 2: Computer vs. person opponent	55:55*	SPS	0.40	0.06 - 2.16	Medium (P)	18.0%	73.8%	98.6%***
Studies of auditory ambiguity:									
Casonova et al.	Normal vs. impaired hearing (p-value data)	40:40	PAC	0.51	0.06-0.95	Large (P)	14.3%	59.8%	94.2%***
(1988)	Positive vs. negative feedback (lower estimate: p-value data)	40:40	PAC	0.33	-0.11-0.77	Medium (P)	14.3%	59.8%	94.2%***
	Positive vs. negative feedback (upper estimate:(p-value data)	40:40	PAC	0.38	-0.58-0.82	Medium (P)	14.3%	59.8%	94.2%***
Other nonclinical studies:									
Marr et al. (2011)	Study 1b. MARTI vs. Neutral control	31:31*	PS	0.54	0.04-1.05	Medium (P)	12.1%	49.1%	87.3%***
	Study 1b (replication). MARTI vs. Neutral control	29:29*	Unknown	0.58	0.05-1.11	Medium (P)	11.6%	46.5%	84.9%***
Other clinical studies:									
Ellett et al. (2008)	Post exposure paranoia SUD ratings	15:15	SUDS	1.51	0.69-2.33	Large (P)	8.3%	26.2%	56.2%

CI = Confidence Interval; P = positive effect, N = Negative effect. *sample size estimated on basis of total N and number of conditions, not stated in paper
**repeated measures design. ***adequate power (≥80%)

3.4 Summary of Results

3.4.1 Stress vulnerability paradigms (five studies)

Based on stress-vulnerability theories of clinical paranoia (Freeman, et al., 2002; Nuechterlein & Dawson, 1984) a series of papers sought to experimentally investigate the relationships between stress and paranoia. Drawing on established research paradigms that have been shown to reliably increase state anxiety, methodologies included exposing participants to anxiety provoking images (Lincoln et al. 2010a, Palmier-Claus et al., 2011) , playing building site noise during a cognitive task (Lincoln et al. 2009, Moritz et al. 2011) and manipulating experiences of task failure (Lopes et al., 2013). All of the studies incorporated randomised designs but only two out of the five (Lincoln, et al., 2009; Lincoln, Lange, Burau, Exner, & Moritz, 2010a) were able to clearly demonstrate the effectiveness of stress based paradigms in the induction of paranoid thinking when using valid and reliable measures of paranoia.

The most convincing findings were reported by Lincoln et al. (2010a) whose paradigm was associated with a moderate to large effect size in one of the most adequately powered studies included in the current review (N = 90). The study used a between group design in which participants were randomly allocated to an anxiety or neutral control condition, thus adequately controlling for confounding variables. Anxiety was induced using an established technique (Smith, Bradley, & Lang, 2005) in which participants were shown anxiety-provoking images taken from the International Affective Picture System (IAPS; Lang, Bradley, & Cuthbert, 2005). Participants were also asked to imagine a personally relevant anxiety-provoking situation for five minutes. In the neutral control, participants viewed neutral images and were asked to imagine eating a lemon for the same amount of time. Significantly higher levels of paranoid thinking were observed in the experimental (anxiety) group. Additional analysis of the anxiety group indicated that increased paranoia was only found in a subset of people with high levels of baseline vulnerability. Similar findings were reported by Lincoln et al. (2009) who again reported the effective application of their stress

manipulation in eliciting paranoid responses and highlighted the mediating role of baseline vulnerability and moderating role of state anxiety in these effects. Although the effect size derived for the purposes of the current review indicated only a small and non-significant effect, this may be a consequence of the reduction in statistical power introduced by treating the groups as if they were independent rather than dependent, and so may underestimate the true effect.

The same building site paradigm was also used by Moritz et al. (2011) who were unable to demonstrate an effect in a small nonclinical sample (N = 15). The authors did, however, report a positive effect of the paradigm in a clinical population, again suggesting that existing vulnerability may play a role in the effectiveness of these paradigms. Effect sizes calculated for the current review indicate that this was a marginal and non-significant effect, however this was derived on the basis of graphical inspection and should be viewed with caution. There are other noteworthy differences between Lincoln et al.'s (2009) study and Moritz et al's (2011) study which might account for the significant findings reported in the former study. Lincoln et al. (2009) reported the incorporation of scripted ambiguous experimenter behaviour into their paradigm, no such behaviour was reported by Moritz et al. (2011). It is not clear whether the addition of 'ambiguous' experimenter behaviour reported by Lincoln et al. (2009) was necessary for the observed increase in paranoid thoughts. In addition, Moritz et al. (2011) did not vary task difficulty across conditions, whereas Lincoln et al. (2009) used a difficult task in the stress condition and an easy task in the neutral control condition.

The other studies in this group did not convincingly demonstrate the effectiveness of stress-vulnerability paradigms in eliciting paranoid cognitions using validated measures of paranoia. Palmier-Claus and collegues (2011) used a combination of the above paradigms with the addition of a guided imagination task in which participants were asked to imagine themselves in either a socially stressful or neutral situation. Exposure to the stress condition was found to be a significant predictor of ambulant feelings of paranoia as measured by visual analogue scales (VAS), providing some limited support for this manipulation. The impact of incorporating personally relevant material into paradigms, as introduced by Lincoln

et al. (2010a) and again used by Palmier-Claus et al. (2011) also remains unclear. Whilst perhaps increasing the ecological validity of the methodology, such personal memories may vary significantly across participants and may be more difficult to control for than more artificial sources of stress. In the interest of developing rigorous, efficient and ethical paradigms for inducing paranoid thinking that do not involve unnecessary burden and potentially unnecessary recall of personally distressing memories, these issues require further investigation.

Lopes and Pinto-Gouveia (2012) focussed on the effectiveness of task failure, versus a neutral control, at inducing paranoid thinking. Although significant effects of the failure condition on paranoia were reported, these were again limited to the VAS ratings of paranoia. These effects were not observed when more valid and reliable measures of paranoid thinking were used (PLC and PS). Participants with high levels of baseline paranoia proneness had been deliberately recruited for this study, indicating that a lack of vulnerability did not adequately explain the null findings. The paradigm did, however, appear to have successfully induced paranoid feelings in a small sub-set of people with elevated levels of hostility, suggesting some utility for the paradigm for a subset of people at least. It is also noteworthy that Lopes and Pinto-Gouveia (2012) reported elevated levels of baseline depression in the pre-defined paranoia group which may have introduced a potential confounding factor. Experiencing poor performance in an otherwise neutral context may lead to more depressive processing of events, such as making internal rather than external attributions for the experience of failure (Abramson, Metalsky, & Alloy, 1989). An adequate level of situational threat is suggested to increase the likelihood of more defensive attributions, such as those associated with paranoid thinking, to being made (Campbell & Sedikides, 1999). For the majority of individuals, task failure alone does not appear sufficient to create such a response.

The nature of the designs used across these studies makes it difficult to delineate which aspects of the manipulations, or combination thereof, are necessary for the observed effects. The findings reported by Lincoln et al. (2009), Lincoln et al. (2010a) and Moritz et al.

(2011) highlight the central role of vulnerability in the effectiveness of stress based inductions. However, Lopes & Pinto-Gouveia (2012) found only a limited impact of their manipulation in a pre-defined high-paranoia group, suggesting that this is only one element in these designs that needs consideration. While failure alone may not be sufficient to trigger paranoid thinking (Lopes and Pinto-Gouveia, 2012) the discrepancies observed between Lincoln et al. (2009) and Moritz et al. (2011) suggest that, it may be a necessary ingredient to the success of some paradigms at least.

An additional strength of the studies reported by Lincoln et al. (2009) and Moritz et al. (2011) is their consideration of the specificity of the paradigms' effects. The activation of multiple negative states has implications when attempting to draw inferences about the presumed impact of experimentally induced paranoia as this may not be acting in isolation of other important variables. Given the demonstrated links between vulnerability and paranoia, it could be that stress inductions activate whatever negative states individuals may be most prone to, rather than specifically inducing paranoid thoughts per se. It may therefore be misleading to consider these paradigms as paranoia inductions. While the above studies provide some evidence for the effectiveness of these paradigms in triggering paranoid thinking in some contexts and for some individuals, further research may help to identify which aspects of stress-laden situations are more or less likely to produce such effects across a wider range of individuals, for example those with lower degrees of 'paranoia proneness'.

3.4.2. Manipulation of attentional focus paradigms (five papers, seven studies)

These studies investigated the impact of manipulations of attentional focus in the experimental induction of paranoid cognitions in student populations. With the exception of a much earlier study by Locascio and Snyder (1975), which involved the manipulation of participants' attention to threat laden material, all studies produced positive effects. In the largest study of this group, Boden and Berenbaum (2007) demonstrated the successful induction of paranoid thinking in a randomised controlled design and achieved a large effect

size. This study involved inducing paranoia via rude and hostile experimenter behaviour and tested the hypothesis that emotional awareness would moderate the effects of the paradigm, with previous research demonstrating associations between emotional awareness and paranoia in clinical (Cedro, Kokoszka, Popiel, & Narkiewicz-Jodko, 2001) and nonclinical samples (Berenbaum et al., 2006). All participants (N = 118) received the negative mood induction and were randomised to either a low or high emotional awareness condition. In the high emotional awareness condition participants read stories in which the protagonist is mistreated by somebody in a position of power and is angry because they are taken advantage of. It was hypothesised that the parallels between this and participants' experiences during the negative mood induction would facilitate emotional awareness. The low emotional awareness control group were given a neutral story with no such parallels. As hypothesised, paranoia was elevated in the low emotional awareness condition. However, this effect was found in male participants only. Female participants were hypothesised to have pre-existing levels of emotional awareness that would protect them from the effects of the negative mood induction regardless of condition allocation, although this was not measured directly. The specifics of the stories used or the gender of the researcher may also account for the observed gender differences and requires further investigation.

The Boden and Berenbaum (2007) study introduces unique elements to the paradigm, specifically, the role of emotional awareness in the context of negative events in the elicitation of paranoid thinking. However, the complexity of the mood induction which was stated to require 'extensive training' to implement limits the utility of this approach. A manipulation that is so heavily dependent on interpersonal interactions may have limited experimental control and consistency. Additionally, it could be argued that the emotional awareness manipulation activated paranoid thoughts directly by increasing awareness of the negative actions of others generally rather than by increasing emotional awareness to their own negative mood per se. Measuring the direct effect of the emotional awareness manipulation on paranoid thinking in the absence of a negative mood induction would help to delineate these possible effects and perhaps result in a more streamlined paranoia

manipulation in which paranoid thoughts could be primed via exposure to stories of hostility in others.

The five studies presented by Bodner and Mikulincer (1998), Ellett and Chadwick (2007) and Prevost et al. (2011) drew on theories suggesting that heightened self-consciousness, especially in the context of adverse events, can lead to paranoid thinking (Fenigstein, 1984; Fenigstein & Vanable, 1992). The manipulations used by Bodner and Mikulincer (1998) and Ellett and Chadwick (2007) involved the completion of unsolvable puzzles for which overtly negative or ambiguous feedback was received. The study by Prevost et al. (2011) required the completion of a cognitive task during electroencephalography (EEG). Attentional focus was manipulated in a number of ways, including completing the task whilst being video recorded (Bodner & Mikulincer, 1998; Ellett & Chadwick, 2007) and / or whilst being in front of a one-way mirror (Bodner & Mikulincer, 1998; Prevost et al. 2011), with the aim being to increase levels of self-conscious awareness. Bodner and Mikulincer (1998) and Ellett and Chadwick used a between subjects randomised design. Although Prevost et al. used a weaker, within subject design, their study benefitted from the use of pre- and post-measures of paranoia. All studies reported significant effects for the manipulations with Bodner and Mikulincer (1998) and Ellett and Chadwick (2007) demonstrating medium to large effects. However the reliability of these effect sizes may be limited due to the small sample sizes in which these effects were found (N = 9 – 20 per group) (Button et al., 2013).

In the Locascio and Snyder (1975) 'attention to threat paradigm' participants were led to believe that men with 'psychopathic tendencies' were viewing them behind a one-way screen. Participants were then presented with false personal evaluations that had ostensibly been made by the men. Attention to threat was manipulated by asking participants to select the most threatening or least threatening statements or to just read the statements without any selection and participants were randomised to one of these three conditions. No group differences were observed on measures of malevolence and fear that were developed for the study. It is possible that the experimental manipulation induced a ceiling effect of paranoia in all participants making the addition of an attention to threat manipulation

redundant. On a similar note, given the level of overt hostility that was expressed towards participants via statements such as *'I might inflict serious physical injury on this person if I had the chance'*, feelings of justified fear may be quite distinct from concepts of paranoia in which ideas of falseness or exaggeration of threat are embedded. This highlights the need to carefully consider what exactly is being triggered and measured in studies in this area. Although not the case here, the demonstrated association between hyper-vigilance to threat and paranoid thinking (Fear, Sharp, & Healy, 1996) suggest that paradigms involving selective attention to threat, perhaps utilising subliminal priming techniques, may provide a useful avenue of enquiry in the experimental induction of paranoia. The paradigm used by Locascio and Snyder (1975) however may entail legitimate ethical issues and, combined with the inconclusiveness of its effectiveness, makes the current utility of this paradigm minimal.

While both Bodner and Mikulincer (1998) and Ellet and Chadwick (2007) found medium to large effects, there is some disparity in the findings of the two papers with respect to the role of focus of attention in eliciting paranoia. Bodner and Mikulincer (1998) reasoned that, in the context of personal failure, directing one's attention toward an external agent (i.e. the experimenter) would lead to the experienced failure being attributed to the experimenter, rather than the self, thus creating a paranoid response. In addition, they predicted that directing attention to the self under the same conditions would lead to a depressive response, as failure would be more likely attributed to the self. In contrast, Ellett and Chadwick (2007) argued that, in the context of failure, focussing attention on the self would *'increase the experience of the self as the target of others' thoughts and actions'* and thus lead to paranoid thinking. Both papers reported results supporting these contrasting predictions, despite using similar methodologies. Ellett and Chadwick's findings benefit from replication across three studies as well as a larger sample size (N = 20 per condition) in their second and third studies, and so perhaps allow for additional confidence in their results. Models of paranoia suggest that the attributions that people make regarding experienced events are central to determining paranoid responses (Bentall, et al., 2001), as such a direct measure of such attributions may further our understanding of the effect observed using

these paradigms. Indeed, internal and external focus of attention may both have a role in triggering external attributions via different mechanisms. Additionally, the role of task failure in the observed results is a little unclear. Unlike the findings of Bodner and Mikulincer (1998) who found significant interactions between task failure and attentional focus in the elicitation of paranoid thinking, Ellett and Chadwick found no differences in paranoid cognitions as a function of task type. While Ellett and Chadwick, (Study 1; 2007) categorised tasks as 'neutral' or 'failure' conditions, it does not necessarily follow that this is how they were experienced by participants. Both tasks involved the use of the same unsolvable puzzles with participants receiving either overt failure or no feedback depending on condition. Ellett and Chadwick (2007) went on to suggest that ambiguity about task performance may be sufficient to trigger paranoid cognitions in the context of high self-awareness and would therefore explain the lack of impact of task type observed here. Alternatively, the lack of effect of task type on paranoia may indicate that high self-awareness alone can account for increases in paranoia. However, taken with the findings of Bodner and Mikulincer (1998), the experience of failure, be it overt or assumed, appears to be important in the elicitation of paranoid responses.

The manipulations across this group are anchored in theory and seem relatively easy to employ. The randomised nature of all but one (Prevost, et al., 2011) of the designs allows for more confidence to be had in any conclusions drawn from the papers and is a strength of this group of studies. However, none of the studies provide baseline measure of paranoia and possible confounding variables such as trait paranoia or mood were not considered in the design. This is considered to be a major weakness of this group of studies as a whole, especially in light of the importance of such factors having been demonstrated by Lincoln and colleagues (Lincoln, et al., 2009; Lincoln, et al., 2010a; Moritz, et al., 2011). Studies in the attentional focus group also indicate that certain subtleties of experimental design, such as being in the presence of an experimenter and perceptions of failure as personal, universal or indeed ambiguous, may each have differential effects of the elicitation of paranoid and depressive cognitions. However the precise nature of the effect these subtleties may have remains unclear. While Bodner and Mikulincer (Study 1; 1998)

controlled for gender and found no impact, Boden and Berenbaum (2007) found this to be a key factor in the effect of their paradigm. It is perhaps not surprising that gender may differentially influence paranoid reactions in different paradigms. This again highlights the possible complexity involved in the induction of paranoid thinking in experimental contexts and indicates the need for a more comprehensive understanding of the mechanisms underlying the observed effects. Additional idiosyncrasies of the papers, such as population characteristics, may account for some of the variance in the findings. For example Bodner and Mikulincer conducted their study with Israeli participants (Bodner & Mikulincer, 1998) whereas Ellett and Chadwick recruited British students (Ellett and Chadwick, 2007). Replication in other populations and in larger samples is clearly needed.

3.4.3. Virtual games (five studies)

Five studies, taken from three papers, demonstrate the use of virtual games in the experimental manipulation of paranoid thinking. Drawing from studies demonstrating the impact of experimentally induced stress on paranoia, two studies (Kesting, et al., 2013; Westermann, et al., 2012) investigated the impact of social stress on paranoia thinking using a cyber-ball paradigm. Ellett et al. (2013) demonstrated the utility of a virtual version of the Prisoner's Dilemma Game (PDG), a methodology extensively used in social psychology research, in the experimental investigation of paranoia. All of the studies in this group emphasised the importance of interpersonal context in the elicitation of paranoid thinking. Each paper included a study with a randomised design and all involved nonclinical populations. Unfortunately the calculation of effect sizes for the cyber ball paradigm was limited by study design and data reporting. For example, the significant effect reported by Kesting et al. (2013) was based on mean change data. Unfortunately, summary data was not reported and so effect size calculations for the current review were limited to end point scores which suggested only a small and non-significant effect in favour of the induction. It was also not possible to calculate effect sizes for the Westermann et al. (2012) study due to limited data reported. Ellett et al. (Study 2, 2013) compared two versions of their paradigm that simulated playing either against a computer or another person. A medium effect in

favour of the person-opponent condition was achieved, indicating the importance of interpersonal context in the elicitation of paranoid thinking. Additional confidence can be had in this effect due to the relatively large sample (N = 110) in which it was found.

Both of the studies using the cyberball paradigm, randomly allocated participants to either a social stress or neutral control condition. In the social stress condition, participants were excluded during an online, virtual ball tossing game. Such 'cyber-ostracism' has been reliably shown to increase negative emotion in participants (Williams, Cheung, & Choi, 2000). In addition, participants in the Kesting et al. (2013) study were given a cognitive task to complete for which they received either neutral or negative feedback as a function of group; that is, the experimental group received negative feedback and the control group neutral feedback. Under these conditions, while significant group differences in state paranoia were found in the predicted direction, paranoia proneness was not found to be a significant mediator or moderator of these effects suggesting that social stress plus negative feedback were able to trigger paranoid thinking regardless of baseline vulnerability. Unfortunately the independent effects of negative feedback and social stress cannot be established due to the nature of the design used. However, given the limited impact of failure alone demonstrated by previous research (Ellett & Chadwick, 2007; Lopes & Pinto-Gouveia, 2012) it appears that this alone would not have produced the observed effects. In the Westermann et al. (2012) study, the cyber-exclusion or cyber-inclusion conditions were used in the absence of an additional failure condition. While group differences in paranoia were not reported, interactions between cyber-ostracism and paranoia proneness in relation to levels of state paranoia were observed. Cyber exclusion alone therefore appears to be effective at increasing state paranoia for some individuals and, like the previously reported work of Lincoln and colleagues (Lincoln, et al., 2009; Lincoln, Peter, Schafer, & Moritz, 2010b), points to the importance of individual variability in the effectiveness of some paranoia manipulations and the impact of this can vary substantially over very similar experimental designs.

The PDG used by Ellett et al. (2013), involves two players who each make a choice to either co-operate or compete against each other. The dilemma faced by both players is that each can maximise outcomes by competing; yet when both players choose to compete, their outcomes are lower than those that can be achieved by mutual cooperation. The authors suggest that competing behaviour belies distrust in your opponent's intentions and the PDG therefore provides an objective behavioural measure of paranoia. This was supported in an initial correlational study (N = 175) in which decisions to compete (N = 61, 35%) on the PDG were significantly associated with higher levels of state paranoia. In the second study, in which participants were randomised to play the PDG against either another person or a computer, state paranoia was again found to be positively correlated with decisions to compete, but only when playing against another person and not when playing against a computer, supporting the inherently interpersonal nature of paranoia. It is of note, however, that decisions to compete did not appear to vary between groups [N = 24 (49%) versus 25 (51%)] suggesting factors other than paranoia may influence such decisions. This was addressed in the third, correlational, study reported in the paper, which incorporated a validated measure of choice reasoning to disentangle the motives of distrust versus greed for the completion choice. Only distrust-based competition was associated with paranoia; distrust-based competition was therefore proposed as a behavioural marker of nonclinical paranoia.

The computerised nature of the paradigms allows for a stringent control of the interpersonal context and offers the opportunity to modify this in a controlled and systematic way and suggests that their potential utility requires further attention. Virtual games also benefit from being inexpensive, transportable and internet-based making them superior to other paradigms in terms of ease of administration and perhaps allow for relatively high levels of recruitment. Given the substantial possible benefits associated with computer-based methodologies, further research should focus on addressing the direct effectiveness of these approaches in eliciting paranoid thinking. The effectiveness of the PDG is particularly promising as a medium effect was observed when comparing two very similar versions of

the game. Investigating this effectiveness using a more neutral comparison group may reveal even stronger effects.

3.4.4. Virtual reality paradigms (six studies)

The use of virtual reality (VR) in the investigation of paranoia has been demonstrated in a series of correlational studies involving both clinical (N = 2) and nonclinical samples (N = 7). The initial feasibility of VR paradigms was demonstrated in two general population samples of N = 24 (Freeman et al., 2003) and N = 30 (Freeman, et al., 2005a). In both studies, participants were exposed to a four minute, VR library scene with five virtual characters (or avatars). As predicted, both studies found that, while positive views of avatars were most frequently reported, a small number of people experienced persecutory and referential thoughts in relation to avatar behaviour. While referred to as neutral, avatars were described as demonstrating some 'ambiguous' behaviour such as smiling, talking and looking at one another. This appears to be similar to the scripted experimenter behaviour described by Lincoln et al. (2009), however the impact of this on the experience of paranoia is yet to be determined. The 2003 study failed to find an association between trait and post-VR levels of paranoia. This was suggested to be the result of low levels of trait paranoia present within the 2003 sample. Freeman et al. (2005a) addressed this by recruiting participants to reflect the full range of nonclinical paranoia and found a significant association between trait paranoia and VR-paranoia. A real strength of the 2005 study, and subsequent studies in this group, is the inclusion of post manipulation measures of both anxiety and paranoia along with comprehensive baseline assessment. As demonstrated by Lincoln et al. (2009), experimental paradigms can have effects beyond those directly targeted and the control of these allows for a more accurate interpretation of results. The authors suggest that this is the first study to look at the differential predictors of anxiety and persecution and suggest that anxiety plus anomalous experiences, such as hallucinations, predicts the occurrence of persecutory thinking rather than anxiety alone. This is in line with theories of paranoia that hypothesise a causal role for such experiences in the elicitation of paranoid thinking (Freeman, et al., 2002; Maher, 1974).

While Freeman et al's 2005 findings were limited due to low power and multiple comparisons, their findings were replicated by Freeman et al. (2008) who conducted the largest (N = 200) and most comprehensive study of nonclinical paranoia in the VR-environment to date, this time using a London underground scenario. Of note, females who regularly used the underground reported lower levels of paranoia indicating the importance of gender in responses to certain VR environments. Additionally, the authors highlight that the experiments took place approximately one year after the 2005 London underground bombings and as such the generalisability of the observed affects warrants further investigation. The effectiveness of what is assumed to be a controlled and neutral environment may be more influenced by contextual factors, outside of the manipulation's control, than expected. Visual analogue scale scores again indicated that the general view of avatar behaviour was neutral or friendly, however persecutory views of avatar behaviour were also present in a subset of participants. These findings were again replicated by later applications of the methodology in clinical (Valmaggia, et al., 2007) and mixed (Freeman, et al., 2007; Freeman, et al., 2010) samples demonstrating the utility of this paradigm to both clinical and nonclinical populations. Valmaggia et al. (2007) established that VR exposure appears to be acceptable for use in clinical samples and did not appear to cause distress. Freeman et al. (2010) found no difference in reported levels of sickness (a possible side effect of VR exposure) following exposure to a VR environment in clinical versus nonclinical samples. Of more general interest to all papers discussed in the current review, Freeman (2003) investigated whether completion of a trait measure of paranoia may prime persecutory responses within the VR environment by counterbalancing the order of the administration of questionnaire (pre or post VR exposure). Results indicated no such priming had taken place.

Taken as a whole, these papers consistently demonstrate the presence of paranoid-thinking within VR environments and the utility of these paradigms for investigating factors predicting paranoid thinking in nonclinical and clinical samples. A key strength of VR paradigms is that they allow for social interactions within the VR environment to be controlled for and to be

consistently applied across participants. VR environments may additionally offer more ecological validity than the more artificial methodologies used in other paradigms. Indeed evidence suggests that people do react to VR environments as if they were real (Emmelkamp et al., 2002) although this may require further investigation. The lack of pre-exposure state paranoia data combined with the correlational nature of the experimental designs used again makes it difficult to evaluate the direct impact of the VR environment on paranoid thinking or to compare the effectiveness of these studies to other methodologies covered in the current review. It may be that, in some individuals at least, state paranoia is elevated before entering the VR environment, making the benefits of the stringent control offered by the use of VR less convincing. The potential benefits of VR may lie in the possible versatility of its application which has not yet been fully demonstrated. Freeman (2008) suggests seven possible applications of the approach, including the identification of environmental predictors of paranoid thinking, establishing causal factors in paranoid experiences and the eventual treatment of these in clinical populations. The flexibility to systematically manipulate the experimental environment may be where the added value of this technique in the investigation of paranoia lies and offers an exciting avenue for future research in this area. The potentially costly nature and inaccessibility of VR equipment however, will likely limit the wider application of this particular methodology.

3.4.6. Other paradigms (four studies)

An additional four papers (Ellett, et al., 2008; Green, et al., 2011; Marr, Thau, Aquino, & Barclay, 2012) introduce a further three approaches. Green et al. (2011) and Marr et al. (2012) again utilised general population samples. The Green et al. (2011) study involved exposing participants to two ambiguous events, in which visual analogue measures of paranoia and anxiety were taken pre- and post-manipulation. No pre-to-post differences were found in levels of paranoia or anxiety, however 15.5% (N = 9) of the sample were rated as giving *'paranoid explanations'* for the experimental events. The nature of the experimental design used, such as the lack of control group and reliance on non-validated measures of paranoia, make firm conclusions difficult. Ambiguous behaviour has been repeatedly

incorporated into experimental designs without direct assessments of its effects (Lincoln et al., 2009; Freeman et al., 2003). The ambiguous behaviour was experienced while completing a neutral writing task. Ambiguous behaviour may have been interpreted in a more threatening way if experienced in a stress-laden context, as it is here that attentional and interpretational biases may be more readily activated (Keinan, 2002; Mogg, Mathews, Bird, & Macgregor-Morris, 1990). Indeed, Lincoln et al. (2009) and Bodner and Mikulincer (1998) found that otherwise benign manipulations, such as shifting attention focus, activated paranoia when experienced in stressful, rather than neutral contexts. While promising in its isolation of elements previously utilised by paranoia paradigms, the paper provides insufficient evidence for, or indeed against, the contribution of ambiguous behaviour to the induction of paranoid thinking.

Marr et al. (2011) used a computer based design to investigate the impact of manipulating participants' motivation to collect relationship threatening information on the occurrence of paranoid thinking. They reported two studies which incorporated well controlled randomised designs and successfully demonstrated that asking people to collect information about others meaning them harm significantly increased paranoia compared with neutral and positive informational goal control conditions. Both studies had reasonable sample sizes and both achieved medium effect sizes. This paper demonstrates that encouraging people to think about the possible malevolent intentions of others can trigger paranoid thinking. Acknowledging the interpersonal quality of paranoia provides greater ecological and face validity. Use of a randomised design and replication of findings using different measures of paranoia is a particular strength, and its computer based nature increases the potential utility of this approach.

Casonova et al. (1988) investigated a possible link between experimentally manipulated auditory ambiguity and paranoid thinking and achieved a medium effect. Participants (N = 80, females only) were randomly allocated to listening to one of four audio-tapes that they were led to believe were self-referent personality evaluations. The conversations varied on two domains; content (positive or negative) and audibility (completely intelligible or partially unintelligible). The impact of these domains was found to be independent and additive; the

biggest increase in negative affect was observed in the partial inaudibility plus negative feedback condition. This pattern of results was found in all subscales of the paranoia measure (PAC), apart from the grandiosity scale, in which a reversed pattern was observed, and the hostility scale, which increased as an effect of inaudibility but not content. The unique impact of inaudibility rather than content on hostility perhaps suggests that it is this element of the manipulation alone that actually influences paranoia. The remaining subscales of the PAC represent thoughts of rejection, reference, vigilance, rigidity, anxiety and depression and arguably do not, in themselves, measure paranoid thinking as defined by Freeman and Garety (2002). Under this conceptualisation of the results, varying content of evaluations adds little to the manipulation's impact on paranoid thinking. The lack of effects of audible negative personal statements on hostility supports the argument that ambiguity is more potent to the experience of paranoid thinking than is overt negative feedback. Indeed, Ellett & Chadwick (2007) found paranoid thinking increased in relation to both ambiguous and overt experiences of failure. The gender specific nature of the sample makes the generalisability of findings unclear, especially given the gender effects subsequently observed in other experimental manipulations of paranoid thinking (Boden & Berenbaum, 2007; Freeman, et al., 2005a). However the study again highlights the importance of anomalous experiences in the elicitation of paranoid thinking.

Finally, Ellett et al. (2008) randomised individuals with current persecutory delusions (N = 30), to either an urban exposure condition (10-minute exposure to a busy shopping street), or a mindfulness control condition of matched length. Subjective units of distress (SUDs) taken pre- and post-manipulation indicated a significant increase in anxiety and paranoia following urban exposure. No change was observed in the mindfulness control condition. Paranoia scores (SSPS), taken post-manipulation only, were significantly higher in the urban exposure condition compared with the control. The endpoint paranoia SUD ratings indicate a large effect of the manipulation of paranoid thinking (Table 3). Given the lack of baseline administration of the SSPS, it is possible that group differences could be accounted for by a decrease in paranoia in the control condition rather than an increase of the same in the urban exposure condition. Brief mindfulness has been shown to be effective in reducing

stress in nurses (Mackenzie, Poulin, & Seidman-Carlson, 2006) and in experiences of experimentally induced pain (Zeidan, Gordon, Merchant, & Goolkasian, 2010). The changes observed in the SUD ratings however do suggest an increase in negative affect in the urban exposure group. Similarly, urban exposure was also associated with increases in other factors linked with paranoia, including an exacerbation in the jumping to conclusions bias (Garety & Freeman, 1999) and negative views of others (Trower & Chadwick, 1995) providing additional support for the claim that urban exposure elicited paranoid thinking. Ellett et al.'s methodology is well anchored in theory and its effectiveness at inducing paranoia in a clinical sample is suggested. While high ecological validity is indicated, the disadvantage of the approach is the lack of control over the experimental environment that is inherent in such real life situations. Replication, perhaps including a more neutral control condition, as well as the possible extension to nonclinical populations would further help to investigate the value of this paradigm.

4. Discussion

4.1. Overview of results

A comprehensive review of 28 studies has been provided and the strengths, weaknesses and effectiveness of individual manipulations have been considered throughout the review. The majority of studies (N = 24) demonstrate the successful application of experimental paradigms to the investigation of paranoid thinking, with these applications largely, but not exclusively, occurring in nonclinical populations. Four of the reviewed studies produced negative or limited findings (Green, et al., 2011; Locascio & Snyder, 1975; Lopes & Pinto-Gouveia, 2012; Moritz, et al., 2011). Interestingly each of these approaches incorporated elements that were well anchored in theories of paranoia and perhaps serve to highlight factors, such as baseline vulnerability or contextual stress, that may influence effectiveness of paradigms in this area. The strongest evidence is considered to come from studies using larger samples, with replicated findings, and incorporating well controlled, randomised experimental designs that use valid and reliable measures of paranoid thinking. Four papers

(Boden & Berenbaum, 2007; Ellett & Chadwick, 2007; Lincoln, et al., 2010a; Marr, et al., 2012) in particular stand out as satisfying these criteria. The Lincoln et al. (2010a) stress-vulnerability paradigm achieved a medium to large effect size in a relatively large sample. The effectiveness of such paradigms, however, may be limited to people with elevated levels of existing vulnerability. The attentional focus paradigm presented by Ellett and Chadwick (2007) produced moderate and large effect sizes, again in studies with a high level of design integrity. While including relatively small samples, the paradigm benefits from three successful replications, however this has yet to be achieved independently. Given the reported discrepancies between this study and the work of Bodner and Mickulincer (1998), further investigation of the contexts in which external versus internal focus of attention trigger paranoid thinking is indicated. The randomised controlled study by Marr et al. (2011) demonstrated the impact of directing attention to relationship threatening material and again benefits from replication, with moderate effect sizes being achieved across two studies reported in the same paper. Finally, the emotional awareness paradigm presented by Boden and Berenbaum (2007) appears promising, having demonstrated a large effect size in a sample adequately powered to detect this. However this approach involves more complex procedures and its effectiveness appears to be limited to male participants only, perhaps restricting its wider application and utility.

Of the remaining paradigms, the virtual reality and virtual game approaches have particular potential in this area of research given the tight control they can offer over the experimental environment and of interpersonal interactions within it. The virtual reality paradigms are the most frequently used of all the reviewed approaches, and have been successfully implemented in both clinical and nonclinical samples. The uncontrolled nature of these studies (i.e. the lack of control group and / or lack of pre-manipulation measure of paranoia), however, has made an assessment of their effectiveness at inducing, or even controlling factors that that trigger, paranoid thinking unclear. The limited availability and costly nature of VR equipment may further limit the utility of this approach. In contrast, the virtual game studies, presented by Ellett et al. (2013), Kesting et al. (2013) and Westermann et al. (2012), are perhaps the most easily employable approaches included in the review. Such

methodologies have the added benefit of being relatively economical, transportable and can be administered online potentially allowing for the recruitment of large samples. The medium effect found by Ellett et al. (2013) is particularly promising.

The only methodology that has been exclusively applied in a clinical sample was the urban exposure paradigm introduced by Ellett et al (2008). The applicability of this methodology to nonclinical populations warrants investigation. While achieving large effect sizes, comparison to the effects sizes reported in nonclinical samples may be misleading, as the sample used may have been more reactive to paradigms inducing paranoia.

4.2. Theoretical and clinical implications

The findings of the review have implications for our understanding of paranoia and the included studies provide support for current models in this area. For example, paranoia is suggested to arise from attempts to make sense of ambiguous or anomalous experiences (Freeman, et al., 2002; Maher, 1974; Morrison, 2001). Ten of the reviewed studies clearly include elements of ambiguity such as experimenters or 'Avatar' behaviour (Lincoln et al., 2009) (Freeman & Garety, 2003), uncertainty over task performance (Ellett and Chadwick, 2007) and audio ambiguity of personal evaluations (Casanova, et al., 1988), providing support for the role of ambiguity in experiences of paranoia. However, the only study to look at the role of ambiguity in isolation suggested only a minimal effect of this on paranoid thinking (Green et al., 2011), when experienced in an otherwise neutral context. It may be that additional factors such as contextual stress or existing levels of vulnerability (Lincoln, et al., 2009), or high self-awareness (Ellett & Chadwick, 2007) are necessary for the influence of ambiguity on paranoid ideation to be observed. Casanova et al.'s finding (1988) that inaudible, rather than unpleasant, personal evaluations were more strongly linked to perceived hostility lends supports to the unique contribution of ambiguity, over and above that of negative experience alone. In line with stress-vulnerability models of paranoia (Freeman, et al., 2002; Nuechterlein & Dawson, 1984), thirteen studies measured trait paranoia and the majority of these (N = 9) noted significant associations between this and

post–manipulation levels of paranoia. In line with the Freeman et al. (2002) model of paranoid thinking, a number of studies identified state anxiety as an important mediating or moderating factor in the experience of paranoia. The findings of Bodner and Mickulincer (1998) and Ellett and Chadwick (2007) provide some, indirect, support for the role of causal attributions in the occurrence of paranoid thinking as suggested by Bentall et al. (2001), however neither study measured causal attributions directly. Interpretational accounts of paranoia (Morrison, 2001) are further supported by the limited impact of negative events alone on paranoid thinking observed in the Lopes and Pinto-Gouveia (2012) and by the role of increased emotional awareness in reducing paranoid responses in the Boden and Berenbaum (2007) study. The effectiveness of social exclusion (Kesting, et al., 2013; Westermann, et al., 2012) and urban exposure (Ellett, et al., 2008) in the experimental elicitation of paranoia provides support for a causal role of such factors in the occurrence of paranoid thinking. Such findings may be important in the understanding of the increased prevalence of paranoia often observed in urban environments (Krabbendam & van Os, 2005) and in ethnic minority groups (Boydell et al., 2001).

The majority of the studies involved nonclinical samples. Given the distress associated with nonclinical experiences of paranoia (e.g. Ellett et al, 2003; Freeman 2005b), these investigations are of independent importance in their own right. Additionally, the ease with which paranoia can be triggered in a nonclinical population adds weight to the conceptualisation of paranoid responses as adaptive strategies that are vestiges of our evolutionary past in which characteristics such as hypervigilance to threat may have been an important survival strategy (Ellett et al, 2003). However, they also provide some insight into the dimensionality of clinical and nonclinical experiences of paranoia. The effectiveness of the reviewed approaches in eliciting paranoid cognitions in nonclinical samples provides further support for continuum models of psychosis (Strauss, 1969; van Os, et al., 2000). The demonstrated role of vulnerability in the elicitation of paranoid thinking across the paradigms provides support for vulnerability-based understandings of the relationship between clinical and nonclinical paranoia which argue that nonclinical experiences serve as vulnerability, or ‘at-risk’ (Yung et al., 2005) markers for future diagnosis. The nature of continuity between

clinical and nonclinical experiences of paranoia is less clear and different approaches to this, such as the phenomenological approach and vulnerability approach, have been suggested (Costello, 1994). The reviewed papers generally support the idea of continuity in the processes governing clinical and non-paranoia, with factors highlighted in clinical models of paranoia also being of relevance to nonclinical experiences. However this was not exclusively the case. For example, subsequent analysis reported from the Lincoln et al. (2009) study suggested that, while stress generally impacted nonclinical paranoia in the predicted direction, and in line with theories of clinical paranoia (Freeman et al., 2002), it did not influence all indices of paranoia in line with what would be expected in clinical populations (Lincoln, et al., 2010b). Nonclinical participants demonstrated a more cautious reasoning style following the paranoia induction, whereas the opposite of this, a tendency referred to as a 'jumping to conclusions bias' (Garety & Freeman, 1999) is thought to characterise clinical paranoia. Freeman et al. (2010) similarly found that this bias was not associated with levels of paranoia experienced within the virtual reality environment. These findings highlight both the utility of analogue experimental investigations of paranoia as well as the need for appropriate caution when extrapolating such finding to clinical populations.

The reviewed studies have a number of clinical implications, which could inform the development of clinical interventions and provide a context in which to test them directly. For example, Ellett and Chadwick (2007) showed that it is possible to attenuate paranoid experiences within a nonclinical population, via the activation of positive self-representations. This supports a recent focus in psychological interventions for psychosis for developing methods that elicit and consolidate positive self-schematic representations (Chadwick, 2006). There is also scope for the reviewed paradigms to be used directly as intervention strategies in their own right. Virtual reality methodologies may be particularly amenable to the treatment of paranoid delusions and such approaches have already been applied to the treatment of social anxiety (Anderson, Rothbaum, & Hodges, 2003) and phobias (North, North, & Coble, 1998) and has been used as an educational tool in relation to the understanding of visual hallucinations (Leff, Williams, Huckvale, Arbuthnot, & Leff, 2013; Yellowlees & Cook, 2006). Additionally, paradigms such as the Prisoners' Dilemma

Game used by Ellett and Chadwick (2013) may provide an alternative to self-report measures of paranoia, with a reduction in distrust-based competition signifying a reduction in paranoia. Similarly, the paradigms could be used therapeutically to demonstrate the role of factors such as interpretational biases in the occurrence of paranoid thinking. Furthermore, the ease with which paranoia can be induced in nonclinical samples may provide useful normalising information for people experiencing psychosis.

4.3. Recommendations for future research

Taken as a whole, this body of research provides important information that can be used to guide future research in this area. Future studies should include adequately powered samples and should consider and measure the range of baseline vulnerability. Measures of paranoia should be valid and reliable and should be administered both pre- and post-manipulation. Including a combination of measures that capture both a range of paranoia experiences such as the Paranoia Scale (PS; Fenigstein & Vanable, 1992) as well as one that is more focused on clinical definitions of paranoia such as the State Social Paranoia Scale (SSPS; Freeman, et al., 2007) may help the interpretation of findings in this area. Studies may also benefit from including measures of distress and conviction in paranoid thinking, rather than focussing on the occurrence of such thoughts alone. Additional post-manipulation measures of the possible unintended effects of paranoid inductions such as anxiety and depressive thinking, is also indicated to help the interpretation of observed results. Direct measurement of the mechanisms by which inductions are hypothesised to have an effect should also be included.

It is difficult to recommend any one paradigm for use in future studies at the current time. The available literature suggests that certain factors such as stress, failure, ambiguity, social exclusion, heightened self-awareness, interpersonal context and baseline vulnerability are important to the experimental manipulation of paranoid thinking. It is also indicated that the effectiveness of these factors when used in isolation is more limited. Paradigms should therefore ensure that well-validated combinations, for example stress plus vulnerability

(Lincoln, et al., 2010a), failure plus social exclusion (Kesting, et al., 2013) or heightened self-awareness (Ellett & Chadwick, 2007), are used wherever possible. Future research should try to refine, and better identify the active ingredients, or combination thereof, in the successful manipulation of paranoid thinking. The inherent ambiguity perhaps embedded within the experimental context should too be considered and investigated with the use of more thorough baseline assessments. The stringent control offered by virtually reality methodologies may be misleading if experimental factors outside this control actually activate paranoid thinking before entry into the virtual reality environment.

In terms of future research agendas more broadly, the reviewed paradigms have scope to be used to target a number of areas. An area of particular clinical interest is the identification of factors that buffer the effects of paranoia inductions as these can be used to inform intervention strategies. Ellett and Chadwick (2007) have already extended the use of the paradigms in this way and demonstrated that priming either positive or negative self-cognitions before exposure to the paranoia paradigm leads to lower or higher post-induction levels of paranoia respectively. The further extension of the paradigms to the investigation of factors that can alleviate paranoia, once it is activated, may also be of additional clinical interest. The use of mindfulness techniques have been successfully demonstrated to alleviate clinical experiences of paranoia (Ellett, 2013), and the investigation of such approaches in experimental contexts would further help to evaluate their effectiveness in alleviating both clinical and nonclinical experiences of paranoia. There is also scope to better elucidate the relationship between clinical and nonclinical experiences of paranoia, which will contribute to both the development and testing of theoretical models in this area.

4.4. Concluding comments

The ability to experimentally induce paranoid thinking is of both theoretical and clinical importance. The reviewed studies are generally well designed and effective and constitute a strong body of literature. However, limitations of individual studies, such as small samples and uncontrolled designs, have been noted throughout the review. Although it is difficult to

make firm conclusions about which paradigm is the most optimal, the strengths and weaknesses of each have been considered. It is hoped that the review has highlighted some of the major considerations that should be taken into account in future research. The review makes an important contribution to the experimental investigation of paranoia and highlights a number of issues that are pertinent for research groups to consider when using experimental paradigms to study paranoia.

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Section 2: Research Paper

Title

Adult attachment and paranoia: an experimental investigation

The following paper has been prepared for submission to 'Schizophrenia Bulletin'. The guidelines for authors can be found in appendix B

Word Count:	5,557
Total (excluding references and tables):	4023
Main text:	3819
Abstract:	204

Abstract

Introduction: Associations between paranoia and insecure attachment have been demonstrated in both clinical and analogue samples. Attachment theory may provide a theoretical framework for understanding the occurrence and maintenance of persecutory delusions. The current study investigates the role of dispositional attachment and contextually primed secure-base attachment representations in the occurrence of paranoid thinking in an analogue sample.

Method: Sixty participants were randomly allocated to one of three conditions; a secure attachment priming condition, a positive affect condition or a neutral control condition. Following priming, all participants were exposed to a paranoia induction. State paranoia was measured pre- and post-manipulation.

Results: Dispositional levels of insecure attachment were associated with both trait and state paranoid thinking. Contrary to predictions, the secure attachment prime did not appear to buffer paranoid thinking. The secure attachment prime appeared to have a negative impact for participants with high levels of attachment anxiety, who experienced higher levels of paranoia following the paranoia induction.

Conclusions: The study provides further evidence for the association between insecure attachment and paranoia. It demonstrates the potentially aversive effects of exposure to secure attachment material in those with existing insecure attachment styles. Clinical and theoretical implications, limitations and considerations for future research in this area are discussed.

Key words: Paranoia, Attachment, Persecutory delusions, Analogue study, Priming

Introduction

In clinical settings, the term paranoia is used to describe thinking of a persecutory nature in which a person may believe themselves to be under serious and intentional threat of harm from others.[1] Such thinking, when of delusional severity, is commonly associated with diagnoses such as schizophrenia and has been identified as the most commonly experienced form of delusional thinking in psychosis.[2]

Continuum models of psychosis[3,4] suggest that paranoid thinking is not unique to those meeting criteria for serious mental health conditions, and rather can be experienced, in varying levels of severity, in the general population.[5] As such, research using analogue samples has been of particular utility in the understanding of the processes and mechanisms underlining clinical paranoia.[6,7] Over the last 10 years, a number of experimental strategies to induce paranoid thinking have been developed allowing for more stringent tests of models of paranoia. In particular, these have allowed the experimental investigation of possible mediating and moderating factors, such as baseline vulnerability and state anxiety, associated with the generation of paranoid thinking.[8-13] Ellett and Chadwick[10] recently extended the application of these approaches by investigating possible buffering influences to the elicitation of paranoid thinking and demonstrated that generating positive self-cognitions protected participants from the subsequent negative effects of a paranoia induction task. Identifying such protective mechanisms may be of particular relevance to the development of clinical interventions for people experiencing persecutory delusions.[10]

Attachment theory[14,15] provides an important theoretical framework for understanding the cognitive, affective and interpersonal aspects of psychosis, and may be particularly relevant to understanding paranoia.[16] Attachment theory emphasises the significance of positive early experiences with primary care givers in the development of affect regulation, and the internalisation of positive working models of the world, that guide interpersonal experiences throughout the life-span. Insecure attachment, suggested to develop as a result of unresponsive, inconsistent or neglectful early experiences,[17] is argued to be crucial in the

understanding of various forms of psychopathology.[18] While different models of adult attachment have been proposed,[17,19,20] they broadly suggest that insecure attachment operates along two key dimensions; avoidant (dismissive or fearful) and anxious (ambivalent/pre-occupied). Anxious attachment is typified by a preoccupation with establishing and maintaining interpersonal relationships, in the context of being fearful of rejection, whereas avoidant attachment is thought to be associated with fear and distrust of others and the avoidance of interpersonal relationships.[19,21] Both anxious and avoidant attachment have been found to be associated with paranoid thinking in clinical[22-24] and nonclinical[25-27] samples.

There is a growing body of research that consistently demonstrates the positive effects of priming 'secure-base' representations across a range of situations, for example, in increasing empathic responses to others[28] and decreasing negative response to psychological pain.[29] The observed effects are often found to occur independently of participants' existing attachment styles; that is, the activation of secure-base representations can be temporally and contextually activated in people with both secure and insecure dispositional attachment. However, a number of studies have demonstrated interactional effects of dispositional attachment with secure-base priming,[28-31] the specific nature of which appears to be dependent on both contextual factors and on the methodologies used. For example, Mikulincer and colleagues[30] demonstrated that although problem solving skills were enhanced by subliminal (i.e. below the threshold of conscious awareness) secure-base priming procedures, interactions with attachment style were not observed. However, when primes were present at a supraliminal, or conscious, level of awareness, people with anxious attachment styles were found to be less susceptible to the beneficial effects of the secure base prime as demonstrated by poorer task performance. Additionally, a series of seven studies successfully demonstrated the effects of a secure base prime in increasing positive evaluations of otherwise neutral stimuli. However, the independent and interactional effects of dispositional attachment were only observed in stress laden, rather than neutral contexts.[31]

The current study combines methodologies taken from the established attachment priming literature with the paranoia induction paradigm described by Ellett and Chadwick to investigate the effects of dispositional attachment and a secure attachment prime on experimentally activated paranoid cognitions in an analogue population. Dispositional attachment insecurity is expected to be positivity correlated with elevated baseline levels of trait and state paranoia (Hypothesis 1). Exposure to a secure attachment prime is expected to reduce, or buffer, the subsequent activation of paranoid cognitions following exposure to the paranoia induction (Hypothesis 2). Given the stress laden nature of the experimental context, insecure attachment is predicted to have an independent effect on state paranoia, with those scoring high on dimensions of attachment anxiety and avoidance being more susceptible to the effects of the paranoia induction (Hypothesis 3). Finally, an interaction between dispositional attachment and secure base priming in relation to paranoid responses is predicted; with the buffering effects of the secure prime being lower in people who have high levels of insecure attachment (Hypothesis 4).

Method

Participants

Sixty participants were recruited from a UK university via poster and internet advertisement. The sample comprised of 12 (20%) males and 48 (80%) females. Participants were 18-35 years old [mean (M) = 21, standard deviation (SD) = 3.5].

Measures and manipulations

Paranoia and Depression Scale[32] (PDS): This 17-item scale was designed to measure paranoid (7 items) and depressive (10 items) cognitions experienced within the experimental context. Participants rate each item using a 6-point scale (1 = *not at all*, 6 = *very often)* allowing for scores to be calculated for both subscales. The PDS has been shown to have excellent convergent validity with trait measures of paranoia and the discriminant validity and

internal consistency (Cronbach's α = .84) of the measure have also been established.[32] In the current study the Cronbach's α for the paranoia and depressive subscales of the measure, as completed at time 2, were .79 and .87 respectively, indicating appropriate internal consistency.

Experiences in Close Relationships Scale Revised[33] (ECR-R): The ECR-R is a 36-item self-report scale designed to measure dispositional levels of attachment avoidance (18 items) and attachment anxiety (18 items) in adults. Participants rate how much they agree with each item on a 7-point scale (1 = *strongly agree*, 7 = *strongly disagree)*, allowing for the calculation of subscale scores ranging from 18 – 126. The measure has been shown to have good test-retest reliability and convergent validity.[34] Again, the internal consistency of the measure in the current sample was indicated by a Cronbach's α of .93 for the anxious attachment subscale and .95 for the attachment avoidance subscale.

Paranoia Scale[35] (PS): This 20-item measure was designed to measure trait levels of paranoia in nonclinical populations. Participants rate how much they agree with 20 statements on a 5-point scale (1 = *Not at all applicable to me*, 5 = *extremely applicable to me*), allowing for a total trait paranoia score to be calculated ranging from 20 - 100. The measure has been shown to have good internal consistency and test-retest reliability in a number of student samples.[35] For the current sample, Cronbach's α was .90.

Attachment priming task

Guided imagery is a frequently used attachment priming methodology.[28,30,36-38] An approach which has previously demonstrated moderate to large effect sizes[30] was adapted for use in the current study. In the secure base priming condition, participants received the following instructions: *"Imagine yourself in a problematic situation that you cannot solve on your own, and imagine that you are surrounded by people who are sensitive and responsive to your distress, want to help you only because they love you, and set aside other activities in order to assist you."* In addition to the attachment prime, two control conditions were included consisting of a neutral and positive affect prime. Secure attachment is thought to have a

positive affective component,[39] therefore the latter control condition was included to help delineate the impact of this from the broader activation of secure base representations, thought to be associated with attachment priming. Similar scripts were provided for the positive affect and neutral control conditions in which participants were asked to imagine themselves either winning the lottery or completing a mundane supermarket shopping task, respectively.

In all three conditions participants were asked to close their eyes and picture the faces of the people they imagined in the described situation. An audio recording with prompts was used to guide participants through the task which lasted for 2 minutes. Following this, participants were asked to write down any thoughts and feelings elicited by the exercise. This task was intended to give a plausible justification for the imagination task and ensure that the manipulation had triggered expected responses. After they received the prime, participants were also asked to rate their current mood across 4 domains (good, bad, happy, sad) on a 7-point Likert scale. This allowed for a total mood score (reversing negative domains) to be calculated and later controlled for, if group differences were present.

Paranoia induction

The paranoia induction was the same as that used by Ellett and Chadwick[10] and is based on theories highlighting the role of high self-awareness in the generation of paranoid thinking.[35,40] The paradigm's effectiveness has been demonstrated in analogue populations, achieving large effect sizes across three studies.[10] In the current study, participants completed an unsolvable task for which they received overt failure feedback under conditions of high self-awareness. Specifically, participants were filmed using a video recorder whilst completing the task, with their recorded image being clearly visible to them on a monitor screen. After completing the unsolvable task, all participants received a failure message (*'that is the wrong answer'*). Further details of this task are described in Ellett and Chadwick.[10] The task was presented on E-Run software (Psychology Software Tools, Inc. www.pstnet.com/eprime).

Design and procedures

Ethical approval for the study was gained from the hosting University's research ethics committee. Half of the sample (N=30) participated as part of their course requirements and the remaining participants received a small monetary reimbursement (£5).

Following written consent, participants were randomised, using a computer-based random number generator, to one of the three priming conditions, resulting in 20 participants per condition. One participant was excluded from the attachment prime group due to language difficulties and incorrect completion of the experimental procedures.

The experiments were conducted by the first author who was blind to group allocation. Participants were informed that part of the study may involve the induction of a negative mood state; however the term paranoia was not referred to. Following completion of baseline measures, participants completed the guided imagery prime and post-imagery ratings. Following this they were prompted to turn on the video camera, resulting in their image appearing on a monitor to their left-hand side, before completing the paranoia induction task. Finally, the PDS and dispositional attachment measures were completed. Following completion of the experimental procedures participants were fully debriefed and informed written consent was retaken from all participants.

Analysis plan

Pearson's correlations were used to test for an association between dispositional attachment and baseline measures of paranoia (Hypothesis 1). In order to investigate the possible buffering effects of the guided imagery task on state paranoia (Hypothesis 2), a one-way repeated measures analysis of variance (ANOVA) was conducted using time (PDS time 1, PDS time 2) as a within group factor and group (secure attachment prime, positive affect prime and neutral prime) as a between subject factor.

A hierarchical regression analysis was used to test the hypotheses that attachment insecurity would be independently associated with post-manipulation levels of paranoia (Hypothesis 3) and that there would be an interaction between dispositional attachment and the attachment prime (Hypothesis 4) in relation to this. To reduce the possible effects of multicollinearity, scores for continuous predictors were centred around their respective means and key assumptions of regression analysis, such as linearity, homogeneity of variance and independence and normality of residuals were also checked before conducting the analysis.[41] Two dummy variables representing the three priming conditions were created. The attachment prime and positive affect prime conditions were used as the two reference categories. In the first step of the regression, the two dummy variables and the mean centred attachment variables (anxiety and avoidance) were entered as predictors, with time 2 PDS paranoia scores as the outcome variable. In the second step, product terms representing interactions between group and both attachment anxiety and attachment avoidance were entered into the model.

A power calculation indicated that 15 participants per group would provide 80% power to detect the effect sizes previously reported by Ellett and Chadwick (2007) (Cohen's d = 1.09), at the $p < 0.05$ level.[42] Increasing the target sample size to 60 allowed for a sufficient participant to predictor ratio for the regression analysis.[43] All variables were screened for normality, with only trait paranoia (PS) significantly varying from the normal distribution. This was successfully transformed using a log 10 transformation. The groups did not differ significantly in relation to age ($f(2)$ = 1.5, p = 0.23) or gender (X^2 = 1.09 df = 2, p = 0.58). Baseline measures and post-prime mood ratings similarly did not vary significantly between groups (Table 1).

Table 1. Sample characteristics and independent variables

Variable	Total Mean (SD) (N = 59)	Attachment prime Mean (SD) (N = 19)	Neutral prime Mean (SD) (N = 20)	Positive affect prime Mean (SD) (N = 20)	Statistics (f / X^2 values) (df = 2)
Age	21.22 (3.46)	22.17 (1.73)	21.37 (1.85)	20.25 (1.85)	f = 1.5, p = 0.23
Gender (male:female)	11:48	5:14	3:17	3:17	X^2 = 1.09, p = 0.58
Time 1 Paranoia Scale	35.00 (11.91)	34.32 (13.80)	36.00 (11.25)	34.65 (11.15)	f = 0.24, p = 0.79
Time 1 PDS (Paranoia)	17.52 (6.31)	18.11 (7.22)	18.5 (6.11)	16 (5.56)	f = 0.90, p = 0.41
Post Prime Mood	5.30 (1.25)	5.16 (1.37)	5.1 (1.42)	4.28 (0.65)	f = 1.06, p = 0.35
Time 2 ECR-R Avoidance	2.74 (1.36)	2.60 (1.31)	3.00 (1.31)	2.60 (1.46)	f = 0.57, p = 0.57
Time 2 ECR-R Anxiety	2.68 (1.14)	2.42 (0.90)	2.93 (1.20)	2.69 (1.26)	f = 0.97, p = 0.39

SD = standard deviation; PDS = Paranoia and Depression Scale; ECR-R = Experiences in Close Relationships Scale Revised

Results

Association between dispositional attachment and baseline paranoia (Hypothesis 1)

Trait paranoia was positively correlated with attachment anxiety (r = .27, p = .04) but not with attachment avoidance (r = .17, p = .19). State paranoia was found to be significantly positively correlated with both attachment anxiety (r = .46, p < .001) and attachment avoidance (r = .33, p = .01).

Effect of attachment prime on paranoia (Hypothesis 2)

Mean and standard deviations for the PDS time 2 scores can be seen in Table 2. The repeated measures ANOVA revealed a significant main effect of time on state paranoia (f (1, 56) = 13.03, p = .001), with paranoia significantly reducing from Time 1 (M = 17.53, SD = 6.31) to Time 2 (M = 15.17, SD = 6.20). A significant main effect of group (f (1, 56) = 0.96, p = .39), was not observed and interactions between group (f (1,56) = 1.40, p = .26), and time were not significant. Contrary to Hypothesis 2, these results indicate that the primes did not differentially impact participants' responses to the paranoia induction. Furthermore, the sample as whole experienced a decrease, rather than the expected increase, in paranoia following this induction task.

In order to assess the specificity of the observed effects, group differences in the depressive thinking subscale of the PDS were investigated using a one-way ANOVA. No main effect of group was found (f (2,56) = 1.17, p = .33).

Table 2. Paranoia and Depression Scale (PDS) scores (Time 2)

Variable	N	PDS Total Mean (SD)	PDS (Paranoia) Mean (SD)	PDS (Depression) Mean (SD)
Total sample	59	38.39 (13.35)	15.17 (6.29)	23.05 (9.11)
Attachment prime	19	40.63 (13.06)	16.89 (6.94)	23.74 (8.11)
Neutral prime	20	40.05 (14.35)	14.70 (6.44)	24.85(9.80)
Positive affect prime	20	34.6 (12.41)	14.00 (5.06)	20.60 (9.21)

SD = standard deviation; PDS = Paranoia and Depression Scale

Effects of dispositional attachment style on post-manipulation paranoia (Hypotheses 3 and 4)

The regression model is summarised in Table 3. The first step of the regression model achieved a weak overall significance at the $p < .1$ level ($p = .081$) with attachment anxiety observed to be the only significant predictor in the model ($\beta = 3.10$, $p = .046$), providing some initial support for the role of insecure attachment in responses to the paranoia induction task (Hypothesis 3).

Table 3: Summary of hierarchical regression with PDS paranoia (Time 2) as the outcome variable

		B	SE *b*	*β*	95% CI	
					Lower bound	Upper bound
Step 1						
	Constant	14.26	1.35		11.56	16.95
	Dummy Variable 1	3.09	1.94	.24	-0.80	6.88
	Dummy Variable 2	-0.25	1.90	-.02	-4.05	3.56
	Attachment anxiety	1.69	0.83	.31*	0.03	3.35
	Attachment avoidance	0.10	0.69	.02	-1.28	1.49
Step 2						
	Constant	14.67	1.31		12.03	17.31
	Dummy Variable 1	3.60	1.90	.27	-0.21	7.41
	Dummy Variable 2	-0.73	1.84	.06	-4.42	2.96
	Attachment avoidance	0.31	1.22	.07	-2.15	2.76
	Attachment anxiety	-0.20	1.33	-.04	-2.88	2.48
	Attachment avoidance X Dummy variable 1	0.08	1.69	.01	-3.32	3.49
	Attachment anxiety X Dummy variable 1	5.24	2.17	.44*	0.90	9.60
	Attachment avoidance X Dummy variable 2	-0.74	1.62	-.10	-3.99	2.52
	Attachment Anxiety X Dummy variable 2	2.11	1.82	.25	-1.54	5.65

Note: N = 59; Step 1 R^2 = .41 (*n.s*), for Step 2, R^2 = .27, *p < .05

In step 2, the model was significant at the $p < .05$ level and explained 27% ($R^2 = .27$, 95% confidence interval [CI]: .10, .43) of the outcome variance, which corresponds to a medium effect size.[44] A significant interaction between attachment anxiety and group was also observed ($\beta = 4.38$, $p = .019$). Attachment anxiety no longer made a significant independent contribution to the model. Taken together, this suggests the impact of attachment anxiety on post-manipulation paranoia was better accounted for by an interaction between this and the secure attachment prime, therefore providing support for Hypothesis 4, but less so for Hypothesis 3. Simple slope tests suggested that higher levels of paranoia were observed in those who received the secure attachment prime but only for those with high levels of dispositional attachment anxiety (Fig. 1).

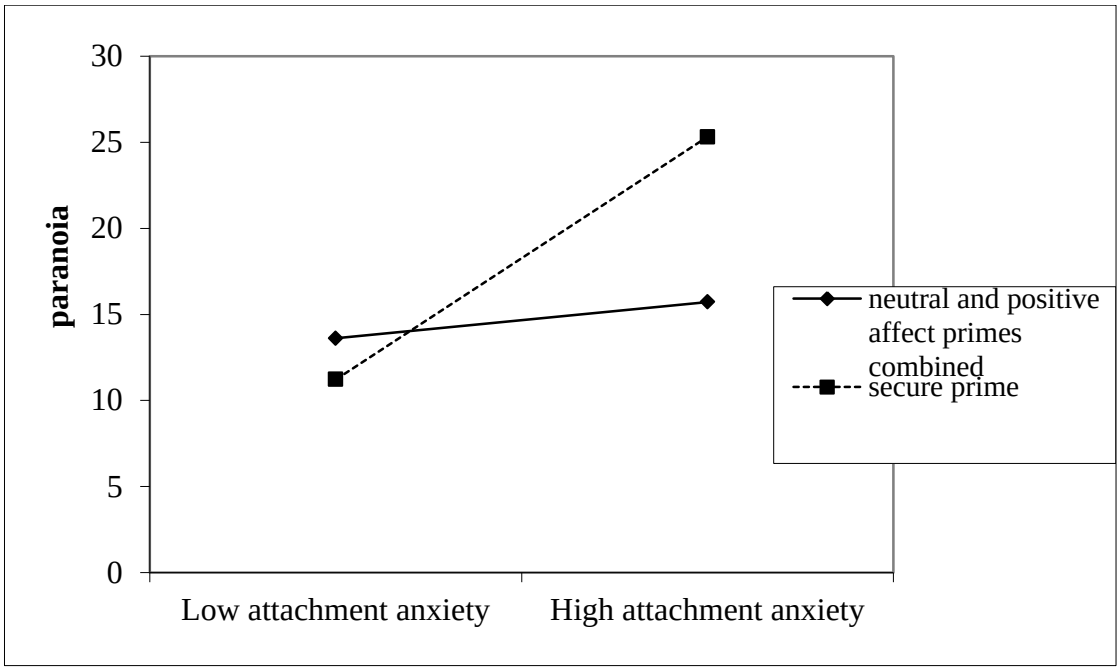

Fig. 1. Interaction effect of group and attachment anxiety on state paranoia

Discussion

The current study provides further evidence for an association between insecure attachment and both trait and state measures of paranoid thinking. It also demonstrates the potentially aversive effects of exposure to secure-base material in those with elevated levels of attachment anxiety.

The observed association between attachment anxiety and both state and trait paranoia (Hypothesis 1) is in line with the findings reported by Berry and colleagues,[25] who found attachment anxiety had a stronger association with paranoia in a nonclinical sample, than did attachment avoidance. Attachment anxiety is thought to lead to hypervigilance for interpersonally threatening information, which may in turn leave people vulnerable to paranoid thinking.[45]

Avoidant attachment has been more readily associated with clinical experiences of paranoia.[23]. In the current study, attachment avoidance was found to be positively correlated with state, but not trait, measures of paranoid thinking. Attachment avoidance is thought to be accompanied by defensive coping strategies such as the suppression of negative affect.[46] In nonclinical populations, such strategies may successfully protect against the development of paranoid beliefs in the short term at least.[45] Such strategies have, however, been associated with deteriorations in mental health in the context of life stress[47] and the short term effects may be limited to self-report, rather than physiological indicators of distress.[48] Such findings perhaps help explain the more commonly observed association between attachment avoidance and paranoia in clinical populations. State measures of paranoia, particularly when assessed in a low threat situation, may reveal momentary instances of paranoid thinking that may otherwise be suppressed.

Increased paranoia in response to experimentally induced anxiety has previously been demonstrated,[8] however the current study is the first to report a potential role for attachment anxiety in this (Hypothesis 3). The elevated levels of state paranoia observed in participants with high levels of attachment anxiety following exposure to the secure prime are in line with Hypothesis 4 and with the findings reported by Mikulincer and colleagues,[30] who demonstrated that the positive effects of a guided imagery prime were not observed in people with high dispositional attachment anxiety. It was suggested that the overt processing of attachment-related material may have detrimental effects on those with high levels of attachment anxiety, possibly via the activation of negative attachment experiences.[30] Qualitative statements recorded by participants in the current study support this explanation,

in that a number of participants reported feeling 'distressed', 'upset' or 'hopeless' following the secure prime. These findings fit with the concept of a *'fear of compassion',*[49] in which individuals may respond negatively to compassion received both from others and the self. Fear of compassion has been shown to be associated with insecure attachment.[50]

The predicted buffering effects of the secure attachment prime (Hypothesis 2) were not supported by the observed pattern of results as post-manipulation paranoia did not vary as a function of prime type. It could be that the priming procedures had a buffering effect on paranoia regardless of the type of prime received. This explanation seems unlikely, however, given the specific and independent effects of the same three priming conditions previously demonstrated over a range of experimental contexts.[28,30,31] Similarly, it may be that the priming procedures were not effective. However the extensiveness of their application elsewhere and observed interactions between the secure prime and attachment anxiety in this study, suggest that the primes were, at least in part, having an effect.

The observed reduction in state paranoia at time 2 (i.e. following the paranoia induction) was contrary to expectations and there are a number of possible explanations for this. The paranoia induction involved task failure in the context of high self-awareness. A similar approach has been shown to activate depressive, rather than paranoid, thinking.[32] However this does not appear to explain current findings, as group differences in depressive thinking were not observed. Baseline state paranoia was particularly high when compared to even the post-paranoia induction scores reported by other studies.[8,10,11] This was not mirrored by levels of trait paranoia which was comparable with other findings in similar populations,[10,35] suggesting that the high levels of baseline state paranoia may have been associated with the experimental context rather than existing vulnerability. The results may reflect initially elevated levels of paranoia that reduced, naturalistically, as the experiment progressed and the paranoia induction may have re-activated paranoia, following an initial reduction. While possible, previous findings suggest that once activated, experimentally induced paranoia may remain so even when contextual threat is diminished.[10] An additional measure of

paranoia following the prime, but before the paranoia induction, may have helped to elucidate these issues further.

The reliance of a self-report measure of attachment may be a limitation of the study as these may not adequately tap covert attachment dynamics[10,35]. However, the measure was completed in a stress laden context (i.e. following the paranoia induction) which may have increased the accessibility of attachment related schemas,[14,52] therefore providing a more accurate measure of attachment. While the attachment prime may have confounded the subsequent completion of the dispositional attachment measure, via the activation of secure attachment representations, this seems unlikely as no group differences were observed for the attachment measure.

There are a number of strengths to the current study including the use of randomisation procedures and experimenter blinding. A particular strength is the inclusion of pre- and post-measures of state paranoia, as this is often neglected in experimental manipulations of paranoid thinking. While the analogue nature of the study may limit its generalisability to clinical populations, such research, when treated with appropriate caution continues to offer great utility for this area of psychological research.

Future studies involving paranoia inductions should consider inclusion of a non-paranoia induction control group, or incorporation of a longer pre-paranoia induction baseline period. Such designs would allow for firmer conclusions to be drawn in relation to the effectiveness of the paradigm used, as they would control for the effects of increased state paranoia attributable to being in an unfamiliar and mildly threatening social situation.[53] Future research might also consider using subliminal attachment primes and the use of both self-report and interview-based measures of attachment such as the Adult Attachment Interview.[54]

Conclusions

The key findings of this study are that, firstly, paranoia was associated with levels of insecure attachment. This is consistent with previous research and supports the hypothesis that attachment is important in paranoia. Secondly, this study provides important evidence that asking people with an anxious attachment style to think about secure attachment experiences has the potential to increase, rather than decrease, paranoia.

The current study has a number of important clinical implications. In particular, people who are high in attachment anxiety might be vulnerable to guided imagery or other therapeutic interventions which attempts to expose them to positive attachments. The current findings suggest that this might be particularly important in any therapeutic work done in the context of paranoia.

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Section 3: Critical Reflection

Word Count (excluding references): 7303

1. Introduction

Both the human and economic costs associated with psychosis are well documented (Department of Health, 2012) and the importance of developing and providing evidenced based treatments for people experiencing psychosis is receiving increasing government attention (Department of Health, 2011; The Schizophrenia Commission, 2012). Experimental research in this area has contributed to the development of theoretical models from which psychological interventions, the provision of which is now recommended for people experiencing psychosis (NICE, 2009), have been derived. While advances have been made over recent years, there is still much work needed in the development of effective psychological treatments for psychosis (Jones, Hacker, Cormac, Meaden, & Irving, 2012).

Persecutory delusions are often cited as the most commonly experienced form of delusional thinking (e.g. Garety, Everitt, & Hemsley, 1988) and, along with hallucinatory experiences, are a hallmark of psychosis. Attachment theory (Bowlby, 1969, 1980) has been suggested as a useful framework for understanding persecutory or paranoid thinking (Berry, Barrowclough, & Wearden, 2007). The current thesis provides a review of experimental methods for manipulating paranoid thinking (Paper 1) as well as the first known experimental investigation of paranoia within the context of attachment theory (Paper 2). This focus is considered timely and constitutes an independent contribution to this area of research, with implications for theory, clinical practice and future research.

The systematic review (Paper 1) provides a comprehensive overview of the methodologies used previously to experimentally manipulate paranoia. It is hoped that the discussion of the strengths, weaknesses and effectiveness of these methodologies, as well as the literature as a whole, will help to guide and improve future research in this area. The paper also makes reference to the theoretical implications of the reviewed studies, such as their relationship to existing models of paranoia and their implications for dimensional views of psychosis. In addition, the review considers the possible utility of the discussed paradigms for clinical practice.

The empirical findings presented in Paper 2 constitute a novel contribution to the evidence base. Data suggesting a possible aversive reaction to secure attachment priming in participants with high levels of dispositional attachment anxiety is of particular clinical relevance. Paper 2 also highlights important methodological considerations in this area of research, including the value of pre- and post-manipulation measures of state paranoia as well as the need for careful consideration of the nature of attachment primes used in the experimental context.

These issues are considered in further detail throughout this critical reflection. The strengths and limitations of the presented research, methodological considerations and reflections on the research process as a whole are also discussed and directions for future research are highlighted. The focus will be on the empirical paper, as many issues relating to the systematic review have already been discussed within the report.

2. The systematic review (Paper 1)

Experimental investigations of paranoia have important theoretical and clinical implications and the introduction of methodologies that manipulate and induce paranoid thinking has broadened the scope of research in this area. Given the complexity involved in these approaches, as well as the paucity of reviews covering such methodologies, this was considered an important topic for systematic evaluation. The review aimed to provide an assessment of the strengths, weaknesses and effectiveness of existing methods as well as guidance for future research.

The process of writing the review provided a number challenges. The aims of the review and the nature of the studies included indicated that attention to micro-level detail of the individual paradigms would be important. It was, at times, difficult to achieve a balance between giving due consideration to these factors whilst at the same time providing a useful overview and integration of the included papers.

The calculation and interpretation of effect sizes and the importance of factors such as sample size, power and replication was a particular area of learning. Difficulties were encountered on a number of occasions in relation to inadequate reporting of primary outcome data, which was not possible to attain despite contacting a number of relevant authors. This was surprising and suggests that the literature may be subject to biases arising from selective reporting of outcomes (Chan, Hróbjartsson, Haahr, Gøtzsche, & Altman, 2004). While the calculation of effect sizes for individual studies is considered a strength of the review, meta-analysis techniques were not employed, which perhaps limits the conclusions that can be drawn. Given the variation in the designs and methods employed across studies, the use of meta-analysis was not considered appropriate in the context of the current review (Lipsey & Wilson, 2001).

The use of a quality assessment tool to aid the evaluation of the included studies was considered. A tool was identified that appeared to fit the aims of the current review, (Quality assessment tool for quantitative studies; Thomas, 2003). This was used to guide the evaluation of the included studies and is considered a further strength of the review. Overall study quality scores were not calculated as this has been shown to be associated with biased ratings and poor inter-rater reliability and is not generally advocated (The Cochrane Collaboration, 2009).

The literature, and therefore the review, may also be subject to publication biases. Positive findings, as opposed to negative findings, are more likely to be published (Button et al., 2013), yet both are important when considering the effectiveness of an intervention or experimental technique. More effort could perhaps have been made to search the *'grey literature'* (Auger, 1989). Even then, the absence of a formal register of protocols for experimental studies makes it difficult to quantify the extent of negative or null findings. The inclusion of unpublished findings may have resulted in a more representative and informative review.

The exclusion of studies involving sleep or drug inductions omits potentially effective approaches to paranoia induction, however the focus of the review was on psychological approaches to experiences of paranoia. While there is inherent overlap between psychological and physiological aspects of paranoia, sleep and drug based methodologies were considered to be conceptually distinct from the papers included in the current review and were therefore omitted.

Overall, the process of conducting this systematic review provided a rich learning experience with skills and knowledge being developed throughout. It felt difficult at times to provide opinions and conclusions in what was initially an unfamiliar area of research, however giving in-depth consideration to issues as they arose, combined with regular supervision, enabled the completion of what is felt to be a comprehensive and valuable review of the targeted literature.

3. The empirical study (Paper 2)

3.1. The attachment prime

A great deal of consideration was put into the choice of the attachment priming procedures used in Paper 2. The literature relating to attachment priming is broad and includes a range of methodologies. As such, a representative selection of this literature was reviewed and a range of subliminal and supraliminal techniques were considered. Effect size calculations were completed to help evaluate the effectiveness of different priming procedures. Pearson's *r* effect sizes were derived and interpreted based on recommendations outlined by Cohen (1992) in which $r = .1$ represents a small effect, $r = .3$ a medium effect and $r = .5$ a large effect.

3.1.1 Summary of attachment priming review

Mikulincer & Shaver (2001) employed different priming techniques across five studies investigating the impact of attachment priming on reactions to in-group and out-group members. Study 1 used the same prime (subliminal presentation of attachment related words; love, trust, closeness) in two different conditions (in-group vs. out-group judgements) and found very different effect sizes (r = .01 and r = .45 respectively), demonstrating the importance of context on the effectiveness of attachment primes. In study 2, participants received a guided imagery supraliminal priming technique (similar to that used in Paper 2 of the current thesis), before being exposed to the same conditions as those used in study 1 and similar effect sizes were again observed (r = .02 and .48 respectfully), indicating some consistency between supraliminal and subliminal techniques when applied to the same experimental contexts. Study 3 used a more idiosyncratic guided imagery task, in which people were asked to visualise a person identified as representing a secure base for participants. A smaller effect size than that observed in studies 1 and 2, for the out-group judgement condition was observed (r = .33), indicating that an idiosyncratic version of the attachment prime was not more effective than the generic versions used in studies 1 and 2. However various methodological differences across the studies in this paper made it difficult to interpret any direct comparisons of effect sizes.

A series of studies presented by Mikulincer, Hirschberger, Nachmias, and Gillath (2001b) looked at the impact of picture primes presented both at a subliminal and supraliminal level on affective responses to otherwise neutral stimuli. This paper provided useful information relating to prime effectiveness as the primes were used in very similar experimental contexts. Subliminal presentation of attachment images (e.g. a Picasso mother and child sketch) achieved consistently medium-to-large effects (r = .27-.49) over ten applications of the prime. Additionally, the largest of these effects was observed when the primes were experienced in a stress laden context. Of note, when the same picture prime was presented at a supraliminal level, it was no longer found to be effective.

Finally, a technique used by Mikulincer et al. (2001a) in which participants were presented with names of individuals identified as being idiosyncratic attachment figures was used in the context of measuring empathic responses to the distress of others. Ten effect size calculations based on ratings of compassion and willingness to help others, produced effect sizes ranging from *r* = .29 to .42. Computing the average effects across the studies indicated that very similar effects were observed for both the subliminal and supraliminal presentation of the primes [Mean (M) r = .26 and .28 respectively]. The procedures by which idiosyncratic names were identified added an additional layer of complexity to the experimental design for little apparent gain when compared to the effect sizes observed in response to the more generic attachment images used by Mikulincer et al. (2001b).

3.1.2 Prime choice and pilot studies

On the basis of the above review, which included consideration of the effectiveness but also the potential costs and benefits of different priming procedures, the subliminal technique used by Mikulincer et al. (2001b), in which participants were subliminally presented with a Picasso mother and child sketch, was initially chosen. As well as appearing to be one of the most consistently applied primes, subliminal techniques have the additional benefit of reducing possible demand characteristics associated with the experiment. However, the development of the subliminal priming procedure proved to be more complicated than anticipated and a number of difficult methodological issues were encountered. Ensuring that primes are presented at a level subliminal (or subconscious) awareness can be contingent on a vast array of factors including prime presentation time, the colour, background, size and position of the prime, the choice of masking procedures and individual differences in thresholds for conscious processing (Epley, 2005). Given the demonstrated null effects of the chosen picture prime when presented at a supraliminal level (Mikulincer, et al., 2001b) this issue was of particular concern. Via consultation with experienced priming researchers and with reference to a subliminal priming manual (Epley, 2005) optimal priming conditions were investigated and a design was established. Despite this, concerns relating to the priming procedures persisted. A pilot study was therefore conducted to attempt to provide

some validation of the methodology. Twenty participants were exposed to either an attachment prime (Picasso mother and child) or neutral prime (geometric shape) condition during a lexical design task as described by Mikulincer et al. (2001b). Both subjective and objective measures of prime awareness were established by free and forced choice procedures and prime detection levels were in line with those reported by Mikulincer et al. (2001b). This provided some reassurance that, for the majority of participants, the prime had been presented at a level that was outside of their conscious awareness. What remained unclear however was whether this level was in fact too low for the required unconscious processing of the prime to occur.

Stage 2 of the pilot study assessed whether subliminal exposure to the Picasso mother and child picture had activated secure attachment concepts. Following exposure to the primes, participants were asked to rate how much they currently felt love, warmth (attachment concepts), happy and good (positive affect concepts), on a six-point Likert scale. While recognised as a crude measure, these ratings had been used by Mikulincer et al. (2001b) to validate the categorisation of their picture primes and significant differences between neutral and attachment pictures had been observed. In our pilot however, no such significant differences for either the attachment ($t(18) = 0.15$ $p = 0.88$) or positive affect ($t(18) = 0.12$, $p = 0.91$) were found (Table 1). Thus there was no strong evidence from the pilot study that the prime had successfully activated attachment schemas in the targeted way.

Table 1. Pilot study post-prime Likert ratings

	Attachment prime (N = 10)	Neutral prime (N = 10)
	Mean (SD)	Mean (SD)
Attachment Likert ratings	13.70 (2.95)	13.5 (2.99)
Positive affect Likert ratings	20.70 (3.59)	20.5 (3.98)

SD = standard deviation

It was not clear whether this was due to limitations of the pilot study or because the prime procedures were truly ineffective. At this stage, two options were considered: 1) continue to validate the prime using a more robust measure of priming effects and, if necessary, adapt the priming methodology until confidence in the manipulation could be had or 2) investigate alternative priming options. At this stage in the research process, option 1 was deemed to have limited feasibility as it was associated with a high level of risk and additional time costs. The use of supraliminal techniques (option 2) was re-considered. The possible demand characteristics associated with such techniques were judged to be outweighed by the benefits of knowing that the intended prime had reached participants' conscious awareness. Additional confidence in this approach was provided by the positive, and similar, effects demonstrated by both subliminal and supraliminal techniques across the literature. The most commonly used supraliminal technique appeared to involve the use of guided imagery (Mikulincer & Shaver, 2001; Mikulincer, Shaver, Gillath, & Nitzberg, 2005; Mikulincer, Shaver, & Rom, 2011) and this technique was therefore chosen for use in the current study.

The chosen methodology (fully described in Paper 2) was piloted on another small sample (N = 3) with the primary aim being to practice and refine the experimental procedures. Qualitative data from this study indicated that the primes were operating in line with expectations. The participant who received the secure prime reported that *'…..It made me feel lucky, happy, safe and supported'.* The participant in the positive affect condition simply reported feeling *'happy'* and the person in the neutral condition reported feeling *'little or no arousal, normal daily activity so not associated with strong emotion.'*

3.2. The Paranoia induction

Consideration was also given to the choice of paranoia manipulation used in Paper 2. The medium to large effect sizes achieved by the attentional focus paradigm used by Ellett and Chadwick (2007) combined with the replication of these effects across a further three studies (unpublished) provided a strong rationale for the use of this technique in the current research. Additionally, training in the implementation of the paradigm was available from the

first author of the original paper allowing for further confidence in the use of this methodology. The results reported in Paper 2, however, suggest some uncertainty about the effectiveness of the chosen manipulation, as evidenced by an apparent reduction, rather than increase, in paranoia following exposure to the paranoia induction task. A number of possible explanations for this were considered in Paper 2, which will be expanded upon here.

The findings of the systematic review, completed after the empirical study, suggested that self-focused attention in the context of task failure can induce depressive, rather than paranoid, cognitions (Bodner & Mikulincer, 1998). However group differences in depressive thinking following exposure to the induction task were not observed in Paper 2, suggesting that this explanation alone is unlikely to account for the observed effects. A small number of participants appeared to guess that the failure task was actually an impossible puzzle, perhaps making them less susceptible to its negative impact. Although not measured systematically, verbal feedback suggested this was more prevalent in psychology students who would perhaps be expected to have an increased awareness of the use of such 'cover tasks' across psychological research. An exploratory subgroup analysis of non-psychology students (N=25) was therefore conducted. Group differences in state paranoia remained non-significant ($f(2,23) = 1.98$, $p = .17$) and paranoia was again lower following the paranoia induction [M =16.67, standard deviation (SD) = 7.23] than it was at baseline (M = 18.10, SD = 7.98). Significant differences were however found in relation to post-manipulation depressive cognitions ($f(2,23) = 4.91$, $p = 0.02$). Non-psychology students who had been exposed to the positive affect prime had significantly lower depressive cognition scores than those in the secure prime or neutral conditions. However, as depressive cognitions were not measured at baseline it is not clear whether this increased or decreased following the paranoia induction. It is possible that the 'paranoia' induction may have elicited depressive thinking in non-psychology students and that this effect was buffered by exposure to the positive affect prime. An alternative explanation is that the positive affect prime merely improved the mood of these participants; however, post-prime mood Likert-ratings did not differ between groups suggesting that the previous explanation is most viable. While

recognising the limitations of the small sample size (N = 25) and exploratory, post hoc nature of the analysis, this highlights need for further clarity around the interplay between self-focused attention and failure in triggering paranoid or depressive cognitions. The interpretation of the observed results would have been aided by (a) systematic recording of participants' awareness of the covert nature of the failure task, (b) measures of causal attributions made in response to task failure, (c) baseline measures of state depressive thinking, and (d) an additional assessment of state paranoia following the attachment prime but before the paranoia induction.

3.3. Measures: The Paranoia and Depression Scale (PDS; Bodner & Mikulincer, 1998)

The Paranoia and Depression Scale (PDS) (Bodner & Mikulincer, 1998) was initially chosen due to its demonstrated psychometric properties (Bodner & Mikulincer, 1998), relevance to paranoia experienced within the experimental context and successful use in studies using paradigms very similar to the paranoia induction described in Paper 2 (Bodner & Mikulincer, 1998; Ellett & Chadwick, 2007; Prevost et al., 2011). On reflection, this may not have been the most appropriate choice. While the reliability and validity of the measure was demonstrated in an Israeli student sample (Bodner & Mikulincer, 1998), the cross cultural validity of the scale has not yet been established. Furthermore, the measure is perhaps limited in scope as it does not closely reflect definitions of paranoia in which intention of the persecutor to cause harm is central (Freeman & Garety, 2000). Additionally, Ellett and Chadwick (Study 1; 2007) failed to find a positive effect of their paranoia induction using the PDS, despite finding such an affect when using a different measure of paranoia, indicating that the measure may be limited in its detection of paranoid thinking. A combination of the PDS with another, more clinically relevant, measure of paranoid thinking, such as the State Social Paranoia Scale (SSPS; Freeman et al., 2007) may have provided a broader and more valid assessment of paranoid thinking.

3.4. Administration of the global attachment measure (ECR-S)

i) When to administer the attachment measure

The attachment priming literature was consulted when considering wider design issues, such as deciding on control conditions, determining the data analysis strategy and the choice of measures used. One issue, that was difficult to resolve, was deciding when to administer the global attachment measure (Experiences in Close Relationships-Revised; ECR-R; Fraley, Waller, & Brennan, 2000). Across the literature, such measures have been administered at various time points. In Mikulincer et al. (2001a) participants completed a measure of global attachment after completing priming procedures. While the authors recognised the possible effects of exposure to the prime on the completion of this measure, they argued that the use of a distractor task between the prime and completion of the measure would minimise the impact of this. Additionally, there were no group differences in global attachment scores, which was taken to imply that exposure to different primes (attachment, neutral or positive affect) had not differentially influenced completion of the global attachment measure. In other studies (Cassidy, Shaver, Mikulincer, & Lavy, 2009) participants have completed global attachment measures immediately before prime exposure and have successfully demonstrated priming effects. Priming effects have therefore been found in studies administering global attachment measures both pre- and post-priming procedures. In addition, instances of primes interacting with global measures of attachment have been observed in both study designs (Cassidy, et al., 2009; Mikulincer, et al., 2011).

For the current thesis, a counterbalanced methodology, in which half of the participants would complete the attachment measure before the experimental procedures, and half after, was initially considered to help assess the possible impact of completing the attachment measure at different time points. On the basis of the above research, combined with findings suggesting the relative stability of attachment measures over time (Lopez & Gormley, 2002), differences in pre- and post-manipulation completion of the measure were not expected.

However, if such differences were observed, a counterbalanced methodology would have significantly reduced the power of the study as only one half (pre- or post-) of the attachment data would have been suitable for inclusion in subsequent analyses. It was therefore decided that the attachment measure would be completed at two time points (pre- and post-manipulation) for all participants.

Although we did not have any explicit a priori hypotheses about changes in attachment over time, we assumed on the basis of previous literature that the global attachment measure would be relatively stable. However, post hoc analyses using a repeated measures ANOVA indicated that there were significant differences in trait attachment scores across the two time points ($f(1,56) = 8.96$, $p = 0.004$), with time one being associated with significantly higher levels of attachment insecurity than time two. This did not vary as a function of prime type ($f(2,56) = 0.55$, $p = 0.58$), which again suggests that the prime itself had not differentially influenced ratings of global attachment. Further testing indicated that the change was significant across the range of attachment scores, i.e. for those with both high and low levels of attachment insecurity.

ii) Why might attachment have decreased over time and what are the implications of this?

One possible explanation for the reduction in attachment insecurity is that the experimental context influenced participants' completion of the measure. Threat laden contexts are thought to activate attachment schemas thus making them more available to conscious processing (Bowlby, 1969; Mikulincer, Shaver, & Pereg, 2003). It was initially reasoned that the paranoia induction may have increased contextual threat, and therefore the accessibility of attachment schemas, and so may have provided a more accurate assessment of attachment. However, given the elevated levels of paranoia observed at time one, rather than time two, it may be that the baseline measure of attachment was in fact more accurate. The stability in the ECR-R has been demonstrated over a 6-month period (Lopez & Gormley, 2002) however in this case the measure was administered in similar and neutral contexts.

Further investigations into the impact of context on the completion of attachment measures may be indicated by the findings of the current study.

Fluctuations in attachment classifications have been demonstrated over longer time periods with attachment instability suggested to be more likely associated with attachment insecurity than attachment security (Davila, Burge, & Hammen, 1997). The fluctuations observed in the current study however were noted across the range of attachment scores suggesting that attachment insecurity did not differentially influence the observed fluctuations in attachment scores.

It is possible that the secure attachment prime inadvertently primed people to answer more positively on the attachment scale, resulting in an apparent decrease in attachment insecurity. This however seems unlikely as the reduction in attachment insecurity scores was observed regardless of prime type received and not all participants experienced the secure prime to be a positive experience. It is also possible that the reduction in attachment insecurity scores are the result of a regression to the mean, that is, the phenomenon by which extreme initial ratings naturally gravitate towards an average score upon repeated measurement. While possible, the attachment scores observed in the current study did not appear to be elevated at baseline when compared to normative ECR-r data (Wei, Russell, Mallinckrodt, & Vogel, 2007).

It is not possible from the findings to know if either administration of the attachment measure provides an adequate assessment of dispositional attachment as this would not be expected to change over such a short time period. It is therefore difficult to make firm conclusions about any of the findings derived from this measure. However, the observed fluctuations in reported attachment may not be at odds with views of attachment as being a stable, trait-like construct. It may be that attachment schemas are both stable and enduring but are also more or less activated and/or available to conscious processing in different contexts (Mikulincer, Shaver, & Pereg, 2003). With this understanding in mind, it is possible that self-reports of attachment may change while the underlying attachment construct remains stable.

It is clear however, that attempts to measure this construct in the current study were limited. Self-report measures of attachment are often criticised for being unable to adequately capture attachment processes (Mikulincer, et al., 2003). Davila, Burge & Hammen (1997) recommend that a combination of interview based and self-report assessment provides a better assessment of latent attachment dynamics. The current study may well have benefitted from adopting such an approach.

iii) Problems with administering the measure at both time points/ how this could have been approached differently

As stated above, the decision to administer the attachment measure at two time points was made in response to a lack of consensus in the literature about when such measures should be administered and is considered to be a legitimate methodological concern. On reflection, the decision to administer the measure at two time points was problematic for a number of reasons. Exploring the stability of the attachment measure was not an explicit aim of the current study and as such was not given adequate consideration at the design stage of the project. In hindsight this would have been an interesting and novel research topic in its own right. No a priori planning was made in relation to which administration of the measure should be used in subsequent analyses leaving this open to the possible biases associated with post hoc decision making. Furthermore, no predictions were made about changes in the attachment measure over time, again making it difficult to make firm conclusions about why such changes may have occurred and an analysis plan regarding how to investigate this was not pre-specified or incorporated in to any power calculations made. The repeated assessment of trait phenomenon may also be conceptually flawed, as changes in such phenomenon should arguably not be expected within such a short space of time. While this may be a valid criticism, the above findings demonstrate that assumptions regarding the nature of such concepts, and/or the ability of questionnaire measures to capture them, can be problematic.

This issue may perhaps have been better addressed at the piloting stage of the study. Should pre- and post-differences in attachment have been highlighted at this stage, further consideration could have been given about how to address these in the eventual study. For example, by using an interview-based measure of attachment such as the Adult Attachment (AAI; Main, Kaplan, & Cassidy, 1985) or by administering the attachment measure 1 -2 weeks prior to completion of other experimental procedures as has been done in other studies (Mikulincer, et al., 2005). Alternatively, an awareness of this issue at a piloting stage would have allowed for a priori consideration of how to incorporate differences in attachment into any analyses conducted. An awareness of the potential instability of the attachment measure may also have influenced the research question being undertaken in the current study, specifically in relation to the conceptualisation of 'trait' attachment and its influence on paranoia.

iv) Interpretation and reporting of findings in response to unexpected change attachment;

Initially, all analyses reported in Paper 2 were conducted using both the pre- and post-measure of attachment. The regression analysis reported in Paper 2 refers to the second administration of the attachment measure. This found that participants with elevated levels of attachment anxiety, as measured at this time-point, reported more paranoia following the secure attachment prime, than did other participants. However, when this regression was completed with the pre-manipulation measure of global attachment, the interactions between global attachment and group were not significant.

The decision to report only the second administration of the attachment measure in Paper 2 initially seemed appropriate. It was reasoned that the second administration of the attachment measure may have provided a more accurate measure of attachment because the second administration of the measure occurred in a threat-laden context (i.e. after the paranoia induction). It was reasoned that the change in attachment overtime was a separate and interesting finding that might be better reported elsewhere.

The decision to report only the second administration of the measure is now deemed to be problematic for a number of reasons. Firstly, the assertion that the second administration of the measure offered a more accurate measure of attachment is not supported by the higher levels of state paranoia found at baseline. Secondly, as we did not have any a priori hypotheses about changes in attachment over time, our argument that time two data were more valid was made post analysis, leaving it susceptible to unconscious reasoning biases. For example, if the first administration of the measure had yielded significant findings, then the authors may have reasoned that this administration of the measure should be reported. Thirdly, not reporting both administrations of the measure has implications for the interpretation of the reported findings, both in terms of their significance and their conceptualisation. The fact that one analysis was significant and one was not brings into question the validity of both findings. In effect, conducting the analysis with both attachment measures separately inadvertently increased the likelihood of a chance finding and therefore of type 1 error; however this is not clear to the reader if only one administration of the measure is reported. Similarly, the change in attachment observed over time is important for the interpretation of the findings as representing a stable, trait-like concept. By reporting only one administration of the measure, readers are not offered the opportunity to make informed conclusions regarding the validity of the reported findings. While attempts were made to ensure that decisions regarding data reporting were made on the basis of sound theoretical reasoning, this process has highlighted the importance of a priori decision making and the possible issues that can arise from selective data reporting.

On reflection it is now felt that both administrations of the measure should have been reported in Paper 2, and the authors will ensure that this is done before any journal publication. In brief, this will include amendments to the method, results and discussion sections of the paper. In the method section, it will be made clear that the attachment measure was administered at two time points and the reasons for this will be outlined. In the results section, it will be made clear that there was an unexpected change on the attachment measure over time and that only the second administration of the measure was associated with significant findings. Finally, the discussion section will consider the limitations of the

results based on the inconsistent findings associated with the different administrations of the attachment measure e.g. the increased likelihood of chance findings. It will also consider the possible reasons for the apparent reduction in attachment over time and the implications of this for the presented research as well as for attachment research and theory more broadly. The unexpected change in attachment may be an interesting finding in its own right and it is hoped that these that this additional data will improve the paper and better contextualise the reported findings.

3.5 Strengths and limitations

The empirical research reported in Paper 2 has a number of important strengths. Hypotheses and methodology were all pre-specified, and all analyses were carried out as planned. Randomisation and blinding provided further rigour. Great consideration was given to the choice of procedures used including the addition of pilot investigations. While previous studies had successfully used similar procedures to those described in the current thesis, the findings of the current study highlights the importance of replication in the area of experimental psychological research. Pashler and Wagenmaker outline the crisis in confidence which has followed some high-profile failures to replicate supposedly robust findings (Pashler & Wagenmakers, 2012). Equally, however, additional piloting of both the paranoia induction and the priming procedures may have led to a more robust experimental design.

The reduction in state paranoia following exposure to the paranoia induction reported in Paper 2 creates some uncertainty about the effectiveness of the paranoia induction. While this is recognised as a limitation in the current study, the use of both pre- and post-measures of state paranoia is considered a particular strength. As discussed in Paper 1, this is often neglected by other studies that use similar techniques (Bodner & Mikulincer, 1998; Ellett & Chadwick, 2007) and the reported findings may be of particular relevance for future research. As discussed in Paper 2, levels of paranoia may be naturally elevated in the experimental context and potentially reduced via processes such as habituation and a

reduction in ambiguity as the experimental expectations become clear. Similarly, the interpersonal interactions between the participants and researcher, if positive, may alleviate initial feelings of paranoia. This may have been a particular limitation of the application of the paradigm reported in Paper 2, as the researcher experienced positive and friendly interactions with participants. Consideration of scripted experimenter behaviour such as that described by Lincoln, Peter, Schäfer and Moritz (2009) should be considered if this, or other such paradigms, are used again.

The homogeneity of the sample in terms of age and educational status may limit the external validity of the research. The sample did however have significant variance in relation to the ethnicity and nationality of participants perhaps adding to the wider applicability of the research findings. The analogue nature of the sample limits the generalisability of the findings; however such research has contributed significantly to the development of psychological models of paranoia (e.g. Kinderman & Bentall, 2000; Koriat, Lichtenstein, & Fischhoff, 1980) and has been relevant to the study of attachment and paranoia (Berry, Band, Corcoran, Barrowclough, & Wearden, 2007; Berry, Wearden, Barrowclough, & Liversidge, 2006; Pickering, Simpson, & Bentall, 2008). Additionally, the occurrence of distressing levels of paranoia in the general population (Freeman et al., 2005), makes this an important research endeavour in its own right. While caution needs to be applied to the extrapolation of these findings to clinical experiences of paranoia, as Borkovec and Rachman (1979) point out *'describing an experiment as an analogue is a description and not a criticism'.* Additionally, the aversive reactions that were observed in response to the guided imagery task in a nonclinical sample may be of particular clinical significance. Clinical populations have been demonstrated to have heightened levels of insecure attachment (Dozier, Stovall-McClough, & Albus, 2008) and so responses to attachment material in clinical samples may be more exaggerated, and more damaging, then those reported here. This hypothesis requires investigation.

A power calculation was originally completed in consultation with a statistician however in hindsight this may have been flawed for a number of reasons. The power calculation for the

analyses of variance conducted in Paper 2 was made on the basis of the large effect size achieved in the Ellett and Chadwick (2007) paper. However this did not take into account factors such as the increased possibility of overinflated effect sizes in small samples or the phenomenon known as the *'winner's curse'* in which initial published findings can reflect overinflated effects (Button, et al., 2013). Basing power calculations on such findings can therefore result in studies that are inadequately powered to detect smaller, yet significant, effects (Button et al., 2013). Additionally the initial regression analysis was based on having a maximum of 6 predictors in the model and therefore the sample size of 60 (10 participants per predictor) was deemed appropriate (Wilson VanVoorhis & Morgan, 2007). However the use of dummy variables in the regression analysis to allow for the inclusion of categorical predictors led to the inclusion of 8, rather than 6 predictors in the regression model. Additional consultation with a statistician indicated that the sample size was, nevertheless, appropriate for the conducted analysis. Furthermore Field and Miles (2010) present guidelines for regression sample size based on expected effect sizes. From this, the sample used in Paper 2 was suggested to be sufficient to detect large effects as observed in previous research in this area (Ellett & Chadwick, 2007) at the recommended 80% power level (Cohen, 1988). Although additional recruitment may have been possible, confidence in the power calculation at that time indicated that this would have been unnecessary, and therefore constituted unethical research practice (Altman, 1980).

4. Implications and future research

The implications of the current thesis for theory and practice, as well as recommendations for future research have been noted both within the individual papers, and throughout the present critical review of the work. A summary of these is presented below.

4.1 Implications for theory

The systematic review provides an overview and evaluation of the ways in which paranoia can be experimentally manipulated. This, along with the consideration of the applications of

such methodologies provided by the review, has important implications for current models of paranoia. For example, the influence of factors such as existing vulnerability, ambiguity, and the interpretation of negative events in these manipulations provides further support for current theories of paranoid thinking (e.g. Morrison, 2001) and provides insight into the context in which such factors may be more or less likely to have an effect. The ease with which paranoia can be activated in nonclinical populations provides further support for continuum models of psychosis (Strauss, 1969; van Os, Hanssen, Bijl, & Ravelli, 2000) and speaks to the potentially adaptive function of paranoid thinking as highlighted by evolutionally approaches to paranoia (Ellett, Lopes, & Chadwick, 2003).

The review also has implications for discussions around the dimensionality of clinical and nonclinical experiences of paranoia, with both similar and divergent processes being noted in clinical and nonclinical samples. The findings from Paper 2 support a particular association between anxious attachment and paranoia in nonclinical populations, whereas associations between paranoia and avoidant attachment have been more readily noted in clinical samples (Mickelson, Kessler, & Shaver, 1997). Such discrepancies could be due to a number of factors and may indicate inherent differences in the experiences of paranoid thinking in clinical and nonclinical populations. A similar pattern has been found when investigating the prevalence of Trower and Chadwick's concept of 'poor me' and 'bad me' paranoia (Trower & Chadwick, 1995), with the former appearing to be more commonly found in clinical paranoia (Fornells-Ambrojo & Garety, 2005) and the later in nonclinical paranoia (Melo, Corcoran, Shryane, & Bentall, 2009). Paper 2 discusses the possible adaptive functions of coping strategies associated with attachment avoidance, which may help to explain some of the discrepancies observed in clinical and nonclinical populations.

Paper 2 provides further support for the role of insecure attachment in both trait and state experiences of paranoia and has potential implications for attachment theory more generally. The aversive reactions to attachment material in those with high levels of attachment anxiety, but not those with attachment avoidance, is consistent with the hypothesis that differing coping strategies may be associated with these attachment domains. The possible

role of threat in the activation of attachment related material is also discussed in Paper 2. The implication of this for the assessment of attachment requires further investigation.

4.2 Implications for clinical practice

The aversive reactions to the attachment prime indicated in Paper 2 may be of particular clinical relevance. The importance of the therapeutic relationship in clinical practice is well established (Norcross & Wampold, 2011), with many psychotherapeutic approaches recognising the parallels between a positive therapeutic relationship and aspects of secure attachment such as warmth, empathy and containment (Farber & Metzger, 2009). The findings reported in Paper 2 suggest that some individuals may experience aversive reactions to these aspects of the therapeutic relationship. Similar concerns are highlighted by the concept of a 'fear of compassion' (Gilbert, McEwan, Matos, & Rivis, 2011), in which people may respond negatively to both self-compassion and compassion from others. Fear of compassion has itself been associated with insecure attachment (Gilbert, et al., 2011). These findings may be of particular importance to therapeutic approaches that attempt to activate secure base representations more overtly, especially for those that incorporate experiential techniques such as guided imagery. The use of compassionate imagery, involving the generation of 'caring other' images, is a core component of Compassion Focused Therapy (Gilbert, 2009). Given the high levels of relational trauma (Varese et al., 2012) and insecure attachment (Mickelson, et al., 1997) associated with psychosis, this issue may be of particular relevance to the suitability of such approaches to this client group.

4.3 Suggestions for further research

The systematic review provides direct guidance for future experimental designs that involve the manipulation of paranoid thinking and makes specific recommendations in relation to this. These include the use of pre- and post-measures of state paranoia, assessment of the

mechanisms targeted by paranoia inductions, consideration of the role of existing vulnerability in paradigm effectiveness as well as the combination of elements such as ambiguity, failure or social exclusion, to be included in such methodologies. In addition, Paper 2 further highlights important methodological considerations for research in this area in relation to thorough baseline assessment, the choice of procedures both for attachment priming and the manipulation of paranoid thinking as well as the importance of measuring the direct impact of these on the targeted processes. Replication using different methodologies, for example, by looking at the impact of subliminal, rather than supraliminal priming techniques and using different paranoia paradigms, is indicated. Similarly, given the noted concerns relating to the power of the sample used in Paper 2 to detect medium or small effects, replication of the reported findings in a larger sample is also indicated.

More broadly, future research examining the role of insecure attachment in paranoia may improve understandings of the mechanisms underlying this relationship, for example, by looking at the role of affect regulation strategies and models of self and other in relation to the observed effects. Additionally, focusing on identifying and testing possible buffers to the impact of paranoid thinking could be of particular clinical importance and should remain a primary focus for research in this area.

5. Concluding comments

The critical review has provided an evaluation of the research conducted and suggests a number of considerations for future research in this area, as well as a reflection on things that might be done differently if the research was to be conducted again. While consideration of the limitations of the current research have been noted, the process as a whole has offered a rich learning experience and the resulting research is thought to constitute an important and timely contribution to the literature.

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- Book: Talairach J, Tournoux P. *Co-planar stereotaxic atlas of the human brain*. New York, NY: Thieme Medical Publishers; 1998.
- Book chapter: Goldberg TE, David A, Gold JM. Neurocognitive deficits in schizophrenia. In: Hirsch SR, Weinberger DR, eds. *Schizophrenia*. Oxford, England: Blackwell Science; 2003:168-184.
- Journal article: Thaker GK, Carpenter WT. Advances in schizophrenia. *Nat Med* 2001;7:667-671.
- Journal article with more than 6 authors: Egan MF, Straub RE, Goldberg TE, et al. Variation in GRM3 affects cognition, prefrontal gluatamate, and risk for schizophrenia. *Proc Natl Acad Sci USA* 2004;101:12604-12609.
- Article published on Advance Access only: Gilad, Y. and Lancet, D. March 5, 2003. Population Differences in the Human Functional Olfactory Repertoire. *Mol Biol Evol* doi:10.1093/molbev/msg013.
- Article first published on Advance Access: Gilad, Y. and Lancet, D. 2003. Population Differences in the Human Functional Olfactory Repertoire *Mol Biol Evol* 2003;20:307-314. First published on March 5, 2003, doi:10.1093/molbev/msg013.

Journal names should be abbreviated in accordance with *Index Medicus* (www.nlm.nih.gov/tsd/serials/lji.html).

Manuscripts in which the references do not follow this format will be returned for retyping. References to meeting abstracts, material not yet accepted for publication, or personal communications are not acceptable as listed references and instead should be listed parenthetically in the text. It is the authors' responsibility for obtaining the necessary permissions from colleagues to include their work as a personal communication.

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Full length manuscripts including regular and invited theme articles should contain no more than a combined total of 5 tables and figures. Theme introductions and special features are limited to 2 tables or figures (total). Figures and tables must be referred to using arabic numbers in order of their appearance in the text (e.g., Figure 1, Figure 2, Table 1, Table 2, etc.).

Tables should be created with the table function of a word processing program; spreadsheets are not acceptable. Include only essential data, and format the table in a manner in which it should appear in the text. Each table must fit on a single manuscript page and have a short title that is self-explanatory without reference to the text. Footnotes can be used to explain any symbols or abbreviations appearing in the table. Do not duplicate data in tables and figures.

Please be aware that the figure requirements for initial online submission (peer review) and for reproduction in the journal are different. Initially, it is preferred to embed your figures within the word processing file or upload them separately as low-resolution images (.jpg, .tif, or .gif files). However, upon submission of a revised manuscript, you will be required to supply high-resolution .tif files for reproduction in the journal (1200 d.p.i. for line drawings and 300 d.p.i. for color and half-tone artwork). It is advisable to create high-resolution images first as these

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All material to be considered as supplementary material must be submitted at the same time as the main manuscript for peer review. It cannot be altered or replaced after the paper has been accepted for publication. Please indicate clearly the material intended as supplementary material upon submission. Also ensure that the supplementary material is referred to in the main manuscript where necessary.

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Appendix C: University of Manchester letters of ethical approval

Secretary to Research Ethics Committee 5
Faculty Office - Devonshire House

Tel: 0161 275 0288

Email: jared.ruff@manchester.ac.uk

Ms Jane Owens
School of Psychological Sciences

10th February 2012

Dear Jane

Research Ethics Committee 5 (Flagged Humanities) - Project Ref 11323

I am writing to thank you for submitting your research project application to the University Ethics Committee which met on 19th December 2011 and providing follow up material to address the issues that I raised with you in January 2012. I can now confirm that by way of chair's action your project has now been formally approved by the University Ethics Committee 5 (flagged Humanities).

This approval is effective for a period of five years and if the project continues beyond that period it must be submitted for review. It is the Committee's practice to warn investigators that they should not depart from the agreed protocol without seeking the approval of the Committee, as any significant deviation could invalidate the insurance arrangements and constitute research misconduct. We also ask that any information sheet should carry a University logo or other indication of where it came from, and that, in accordance with University policy, any data carrying personal identifiers must be encrypted when not held on a university computer or kept as a hard copy in a location which is accessible only to those involved with the research.

Finally, I would be grateful if you could complete and return the attached form at the end of the project.

I hope the research goes well.

Yours sincerely

Jared Ruff
Senior Research Manager
Faculty of Humanities and Secretary to URC 5 (Flagged Humanities)
0161 275 0288
Jared.ruff@manchester.ac.uk

Secretary to Research Ethics Committee 5
Faculty Office - Devonshire House

Tel: 0161 275 0288

Email: jared.ruff@manchester.ac.uk

Ms Jane Owens
School of Psychological Sciences

24th September 2012

Dear Jane

Research Ethics Committee 5 (Flagged Humanities) - Project Ref 11323

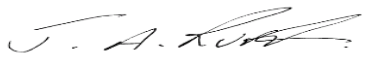
Further to my original letter to you of 10th February 2012 I am writing to acknowledge that the subsequent changes to the above project have been approved by way of chair's action in July 2012.

This approval is effective for a period of five years and if the project continues beyond that period it must be submitted for review. It is the Committee's practice to warn investigators that they should not depart from the agreed protocol without seeking the approval of the Committee, as any significant deviation could invalidate the insurance arrangements and constitute research misconduct. We also ask that any information sheet should carry a University logo or other indication of where it came from, and that, in accordance with University policy, any data carrying personal identifiers must be encrypted when not held on a university computer or kept as a hard copy in a location which is accessible only to those involved with the research.

Finally, I would be grateful if you could complete and return the attached form at the end of the project.

I hope the research goes well.

Yours sincerely



Jared Ruff
Senior Research Manager
Faculty of Humanities and Secretary to URC 5 (Flagged Humanities)
0161 275 0288
Jared.ruff@manchester.ac.uk

Appendix D: Letter from statistician (power calculation)

University Hospital of South Manchester NHS
NHS Foundation Trust

Wythenshawe Hospital
Southmoor Road
Wythenshawe
Manchester
M23 9LT
Tel: 0161 998 7070

26th October 2011

The effects of secure attachment priming on experimentally activated paranoid cognitions

I can confirm that I have discussed the proposal with Jane Owens and provided statistical advice and the following sample size calculations.

Hypothesis 1:
The calculations were based on a simple t-test with conventional two-sided 5% significance level and 80% power, comparing two groups of participants. The sample size calculations used data from two published papers and were performed in nQuery Advisor version 7.0.
The study will have 80% power to detect a mean difference in PDS Scores of at least 5.3 with 13 participants in each group, assuming a common standard deviation of 4.5.
The study will have 80% power to detect a mean difference in PDS Scores of at least 4.2 with 15 participants in each group, assuming a common standard deviation of 3.85.

Hypothesis 2:
The analysis models will contain additional predictors, thus the conventional 10:1 rule for number of participants to number of predictors should be applied.

With 60 participants (20 in each group) the study will have reasonable power to detect differences for a maximum of 6 predictors in the model, which include group, confounders, predictors of interest and interactions.

Sigrid Whiteside
Medical Statistician
Honorary Research Assistant
Education and Research Centre
Tel: 0161 291 5800

Appendix E: Participant information sheet

The University of Manchester

Participant Information Sheet

You are being invited to take part in a research study aimed at investigating the effects of mood on task performance. The study is part of a clinical psychology doctorate. Before you decide it is important for you to understand why the research is being done and what it will involve. Please take time to read the following information carefully and discuss it with others if you wish. Please ask if there is anything that is not clear or if you would like more information. Take time to decide whether or not you wish to take part. Thank you for reading this.

Who will conduct the research?

Jane Owens
Department of Clinical Psychology
University of Manchester
Doctorate in Clinical Psychology Programme
2nd Floor, Zochonis Building
Brunswick Street
Manchester M13 9PL

Title of the Research

Study investigating the effect of mood on task performance

What is the aim of the research?

To assess the effects of mood on task performance.

Why have I been chosen?

The study is open to students based at the University of Manchester. It is hoped that a total of 66 students will take part in the research.

What would I be asked to do if I took part?

If you decide to take part you will be asked to meet with the researcher (Jane Owens) at the University of Manchester for between 45 and 60 minutes. During this time you will be asked to complete a number of questionnaires, some of which ask about personal things such as your mood and how you feel in social situations (e.g. you would be asked to rate how much you agree with statements like 'I felt down-hearted and blue' or 'When mixing socially, I am uncomfortable'). It is possible that you may find answering these sorts of questions upsetting, however these

questionnaires are often used in psychological research and do not cause any distress in the majority of cases. Part of the study involves inducing a negative mood state, however the effects of this are expected to be short lived. Other studies using very similar techniques are not known to have caused any lasting effects in participants. You can stop the study at any time should you feel upset. You will also be asked to complete tasks. One of these is a visualisation task in which you will be asked to imagine yourself in a particular situation. The other is a computerised puzzle. You will be video recorded while doing this task.

What happens to the data collected?

The data collected from the study will be will be entered into a database to be analysed once the study is completed. None of this data will contain any identifiable information. Once the data is analysed the study will be written up for submission for publication in a scientific journal. Again, no identifiable information will be included in this write up.

How is confidentiality maintained?

Any data collected during the study will be kept strictly confidential. Only the research team will have access to your data. All your data from the study will be identifiable by a personalised number only and will be kept in a securely locked filing cabinet in The University of Manchester. Anonymised data (i.e. data that does not contain any personally identifiable information) will be stored on the secure drive on University of Manchester computer. All files will be password protected.

Any Video recordings taken during the study will be stored in a securely locked filing cabinet in The University of Manchester.

What happens if I do not want to take part or if I change my mind?

It is up to you to decide whether or not to take part. If you do decide to take part you will be given this information sheet to keep and be asked to sign a consent form. If you decide to take part you are still free to withdraw at any time without giving a reason and without detriment to yourself.

Exclusion Criteria

The current study excludes anybody who has experienced Psychosis and/or have received a diagnosis of any psychotic illness e.g. Schizophrenia. Please contact the researcher, Jane Owens if you wish to discuss this further (details below).

Will I be paid for participating in the research?

Psychology undergraduates will have a choice between receiving course credits (depending on availability) or a £5 reimbursement towards time and travel expenses.

All other participants will be offered the £5 reimbursement only.

What is the duration of the research?

45 -60 minutes: including the completion of 6 questionnaires (2 of these will be completed on 2 occasions during the study) and 2 computerised tasks.

Where will the research be conducted?

The research will be conducted in a computer lab at the University of Manchester.

Will the outcomes of the research be published?

The findings will be submitted to a peer reviewed journal with the hope of being published. Participants with be asked if they want a copy of the findings and this will be circulated once the study has been written up.

Contact for further information

If you require any further information, please contact the researcher via email on jane.owens-2@postgrad.manchester.ac.uk.

What if something goes wrong?

If you experience any distress after taking part in the study you should contact your GP or The University of Manchester counselling service on counsel.service@manchester.ac.uk or 0161 275 2864 . You should also inform the researcher (Jane Owens) by email (jane.owens-2@postgrad.manchester.ac.uk).

If you decide to make a formal complaint about the conduct of the research you should contact the Head of the Research Office, Christie Building, University of Manchester, Oxford Road, Manchester, M13 9PL.

Appendix F: Consent forms

CONSENT FORM

Title of Project

Study investigating the effect of mood on task performance

Chief Investigator: Jane Owens

ID _____

		Please initial box
1.	I confirm that I understand the nature of the study proposed, having read and understood the information sheet provided. I have had opportunity to ask questions, and I am satisfied with the answers I received.	☐
2.	I understand that my participation in the study is entirely voluntary and that I am free to withdraw at any time without giving a reason and without any depriment to any service/treatment	☐
3.	I agree that part of the study will involve the use of a video recorder.	☐
4.	I agree that if I decide to withdraw from the study then the researchers can continue to use the data and information I have already given them unless I ask for this to be destroyed.	☐
5.	I agree to take part in the study.	☐

Name of participant	Date	Signature
……	… / … / ……	……

Name of person taking consent	Date	Signature
……	… / … / ……	……

NB. This consent form will be stored separately from the anonymous information you provide.

MANCHESTER 1824

The University of Manchester

Post-experiment Consent Form

Title of Project: The effects of attachment priming on experimentally activated paranoid cognitions

Chief investigator: Jane Owens

ID _____

Factors Affecting Task Performance

You have taken part in a study investigating the relationship between task performance and cognitions, which was carried out by Jane Owens. Have you (please circle yes or no):

Been fully debriefed regarding the purpose of the study?	Yes	No
Been informed of your right to withdraw your data from the study without giving a reason and without it affecting your education?	Yes	No
Had an opportunity to ask questions?	Yes	No
Got satisfactory answers to your questions?	Yes	No
Do you still agree to your data being used for the purposes of this study now that you are aware of the full aims of the experiment?	Yes	No

Name of participant	Date	Signature
……	… / … / ……	……..

Name of person taking consent	Date	Signature
……	… / … / ……	……..

NB. This consent form will be stored separately from the anonymous information you provide.

Appendix G: Demographic information sheet

Demographic Information Form

Please provide the following information:

Age _____

Gender Male ☐ Female ☐

Status: Staff ☐ Student ☐

If Student, Please state course & year of study: _____

Nationality: _____

Ethnicity

1. White
   - 1.1 British ☐
   - 1.2 Irish ☐
   - 1.3 Other White background ☐
2. Mixed
   - 2.1 White and Black African Caribbean ☐
   - 2.2 White and Black African ☐
   - 2.3 White and Asian ☐
   - 2.4 Other mixed background ☐
3. Black or Black British
   - 3.1 Caribbean ☐
   - 3.2 African ☐
   - 3.3 Other Black background ☐
4. Asian or Asian British
   - 4.1 Indian ☐
   - 4.2 Pakistani ☐
   - 4.3 Bangladeshi ☐
   - 4.4 Other Asian background ☐
5. Other ethnic Groups
   - 5.1 Chinese ☐
   - 5.2 Other ethnic Group ☐
6. Not stated ☐

Appendix H: Paranoia and Depression scale

Please rate on a 6-point scale (1 = not at all, 6 = very often) the degree to which you

Experienced the following during the last experiment

	Not at all					**Very Often**
1. I'm disappointed from my performance.	1	2	3	4	5	6
2. I feel that I do not have energy to perform other tasks.	1	2	3	4	5	6
3. I feel ashamed of my task performance.	1	2	3	4	5	6
4. I do not have the appropriate abilities to perform the tasks.	1	2	3	4	5	6
5. I have doubts about my abilities and skills.	1	2	3	4	5	6
6. I'm critical of my task performance.	1	2	3	4	5	6
7. I feel guilty about my task performance.	1	2	3	4	5	6
8. I feel that I'm less competent than others.	1	2	3	4	5	6
9. I feel weak and tired.	1	2	3	4	5	6
10. I feel helpless.	1	2	3	4	5	6
11. I feel that my behaviour is being analysed.	1	2	3	4	5	6
12. I feel that people talk about me.	1	2	3	4	5	6
13. I feel that people are hostile to me.	1	2	3	4	5	6
14. I feel that others are picking on me.	1	2	3	4	5	6
15. I feel that others are examining my actions.	1	2	3	4	5	6
16. I feel that others influence my performance.	1	2	3	4	5	6
17. I do not trust other people's intentions.	1	2	3	4	5	6

Appendix I: Experiences in Close Relationships scale (revised)

experiences in close relationships questionnaire – revised (ecr-r)

your name _____ relationship(s) described _____ today's date _____

The statements below concern how you feel in emotionally intimate relationships. You can use them to assess how you tend to feel in close relationships generally, or you can use them to focus on a particular relationship or type of relationship. Typical examples include your relationship with your current romantic partner, romantic partners in general, your mother, your father, your best friend, or friends in general. With adaptations, the statements are also relevant to therapeutic relationships. Using the 1 to 7 scale, after each statement write a number to indicate how much you agree or disagree with the statement.

1 2 3 4 5 6 7

strongly disagree *strongly agree*

1.	I'm afraid that I will lose this person's/others' love	
2.	I prefer not to show this person/others how I feel deep down	
3.	I often worry that this person/others will not want to stay with me	
4.	I feel comfortable sharing my private thoughts and feelings with this person/others	
5.	I often worry that this person/others don't really love me	
6.	I find it difficult to allow myself to depend on this person/others	
7.	I worry that this person/others won't care about me as much as I care about them	
8.	I am very comfortable being close to this person/others	
9.	I often wish that this person's/others' feelings for me were as strong as my feelings for them	
10.	I don't feel comfortable opening up to this person/others	
11.	I worry a lot about my relationship(s)	
12.	I prefer not to be too close to this person/others	
13.	when this person/others are out of sight, I worry that they might become interested in someone else (and leave/exclude me)	
14.	I get uncomfortable when this person/others want to be very close	
15.	when I show my feelings for this person/others, I'm afraid they will not feel the same about me	

16.	I find it relatively easy to get close to this person/others	
17.	I rarely worry about this person/others leaving me	
18.	it's not difficult for me to get close to this person/others	
19.	this person/others make me doubt myself	
20.	I usually discuss my problems and concerns with this person/others	
21.	I do not often worry about being abandoned	
22.	it helps to turn to this person/others in times of need	
23.	I find that this person/others don't want to get as close as I would like	
24.	I tell this person/others just about everything	
25.	sometimes this person/others change their feelings about me for no apparent reason	
26.	I talk things over with this person/others	
27.	my desire to be very close sometimes scares this person/others away	
28.	I am nervous when this person/others get too close to me	
29.	I'm afraid that once this person/others get to know me, they won't like who I really am	
30.	I feel comfortable depending on this person/others	
31.	it makes me mad that I don't get the affection and support I need from this partner/others	
32.	I find it easy to depend on this person/others	
33.	I worry that I won't measure up to other people	
34.	it's easy for me to be affectionate with this person/others	
35.	this person/others only seems to notice me when I'm angry	
36.	this person/others really understands me and my needs	

Appendix J: Paranoia Scale

Please rate how much each statement applies to you on the scale provided.

	Not at all applicable to me				Extremely applicable to me
1. Some people have it in for me	1	2	3	4	5
2. I sometimes feel as if I'm being followed	1	2	3	4	5
3. I believe that I have often been punished without cause	1	2	3	4	5
4. Some people have tried to steal my ideas and take credit for them	1	2	3	4	5
5. My parents and family find fault with me more than they should	1	2	3	4	5
6. No one really cares about me	1	2	3	4	5
7. I am sure I get a raw deal from life	1	2	3	4	5
8. Most people will use somewhat unfair means to gain profit or an advantage, rather than lose it	1	2	3	4	5
9. I often wonder what hidden reason another person may have for doing something nice for you	1	2	3	4	5
10. It is safer to trust no one	1	2	3	4	5
11. I have often felt that strangers were looking at me critically	1	2	3	4	5
12. Most people make friends because friends are likely to be useful to them	1	2	3	4	5
13. Someone has been trying to influence my mind	1	2	3	4	5
14. I am sure I have been talked about behind my back	1	2	3	4	5
15. Most people inwardly dislike putting themselves out to help other people	1	2	3	4	5
16. I tend to be on my guard with people who are somewhat more friendly than I expected	1	2	3	4	5
17. People have said insulting and unkind things about me	1	2	3	4	5
18. People often disappoint me	1	2	3	4	5
19. I am bothered by people outside, in cars, in stores etc. watching me	1	2	3	4	5
20. I have often found people jealous of my good ideas just because they had not thought of them first	1	2	3	4	5

Appendix K: Study prompts for participants

STEP 1	STEP 2	STEP 3	STEP 4	STEP 5	STEP 6
Questionnaires 1 Please start by opening the envelope named 'T1' and complete the questionnaires inside. Please let me know when you have done this	**task instructions/practice** Next, I will explain the computer tasks to you and you can practice one of them while I am in the room	**Imagery task** Please take the instructions out of the guided imagery envelope. Read the instructions and complete the guided imagery task.	**Post Imagery task ratings** Please complete the ratings sales on '*Sheet 1; Post Imagery task ratings*' before completing the computer task	**Complete computer tasks** Before completing the task, please make sure the video camera is set up properly. Please let the experimenter know if you have any problems	**Questionnaires 2** When you have finished both computer tasks, please complete the questionnaires inside the envelope marked T2 .

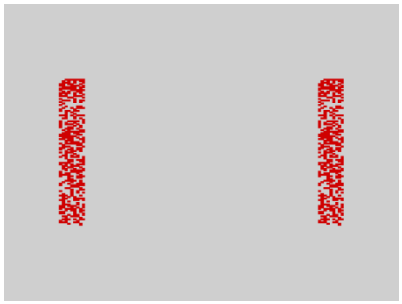
Appendix L: Paranoia induction instructions

COMPUTER TASK INSTRUCTIONS

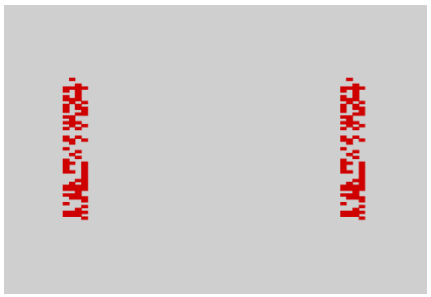
Here are a set of 10 stimuli. They are organised into five different dimensions (i.e. flankers, trapezium, field shape, coloured square and stripe orientation). Each dimension has two values associated with it (i.e., the flankers dimension can either be “coarse” or “fine”).

See below for the 10 possible combinations of dimension and value

FLANKERS

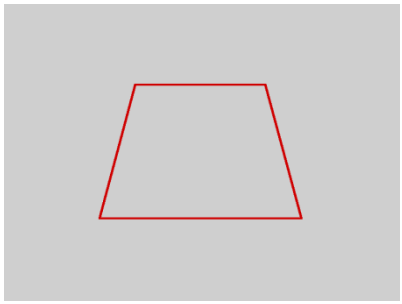


Fine

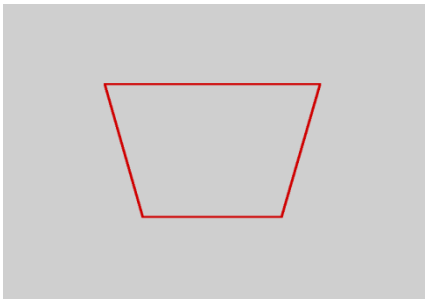


Coarse

TRAPEZIUM

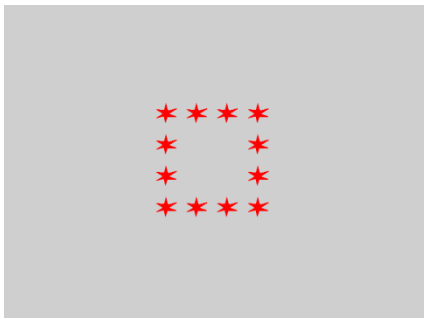


Upright

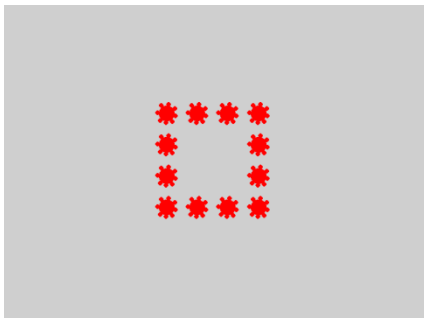


Inverted

FIELD SHAPE

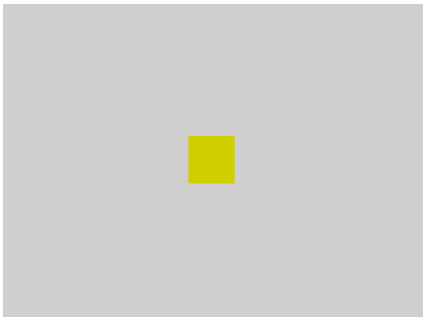


Stars

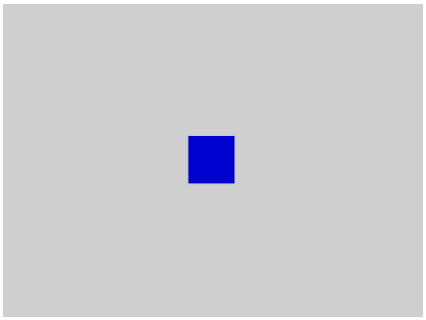


Flowers

COLOURED SQUARE

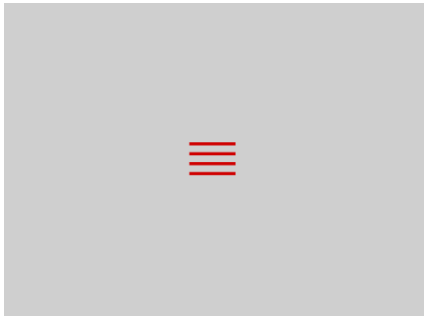


Yellow

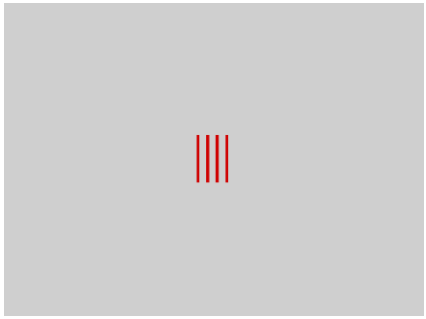


Blue

STRIPE ORIENTATION

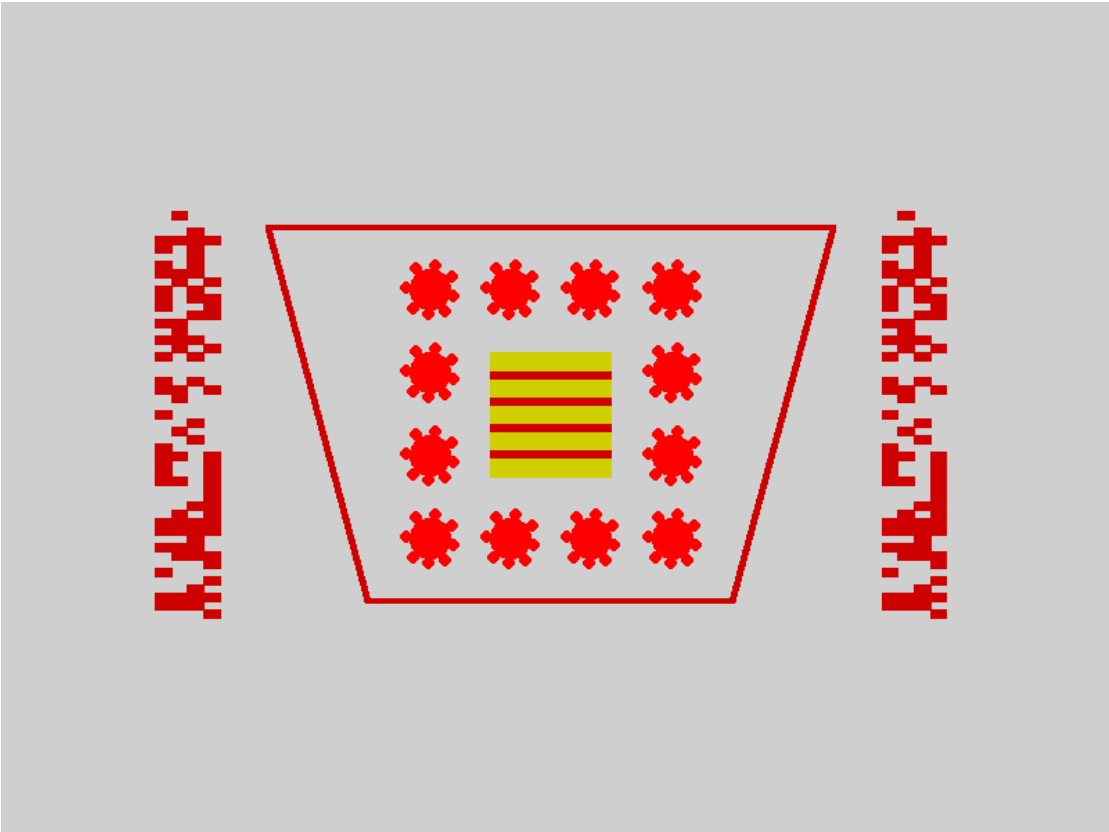


Horizontal



Vertical

During the experiment you will see a number of pictures. Each picture consists of a different combination of the 5 dimensions and values shown above. Here is an example of one of these pictures (show over leaf). As you can see, this picture contains the following dimensions and values: - coarse flankers, inverted trapeziums, flowers field shape, yellow coloured square, horizontal stripes.



The experimenter has randomly chosen one of the values of one of the dimensions as ‘correct’ (e.g., coarse flankers) and your task is to work out which one it is. You will be shown 10 pictures like the one above and your task is to guess whether or not each picture contains the ‘correct’ value.

You will indicate your decision by pressing one of the two shift keys on the keyboard:

Left arrow key = ‘correct’ value is present

Right arrow key = ‘correct’ value is not present

Please note that you need to respond quickly as each picture will only be on the screen for 5 seconds. If you do not respond within 5 seconds, the computer will generate a random response for you.

Having made your guess, you will then be told whether the response is correct or incorrect. You should use this feedback to help you figure out what the correct value is

After you have seen all of the 10 pictures, the following message will appear on the computer screen: “Which of the dimensions contains the correct value?” and you will be asked to select one of the following five options: flankers, trapezium, field shape, coloured square, and stripe orientation. You will then be told whether your answer is correct or incorrect.

Appendix M: Attachment prime scripts

PLEASE READ THE FOLLOWING DIRECTIONS;

DESCRIBED BELOW IS A DESCRIPTION OF A SITUATION THAT YOU WILL BE ASKED TO IMAGE YOURSELF IN AS CLEARLY AND IN AS MUCH DETAIL AS YOU POSSIBLY CAN FOR APPROXIMATELY 2 MINUTES. THE AUDIO CASSETTE WILL HELP GUIDE YOU THROUGH THE TASK.

PLEASE READ THROUGH THE DESCRIPTION CAREFULLY, TRYING TO BRING CLEARLY TO MIND THE SITUATION AND THE FACES OF THE PEOPLE WHO ARE IN THE SITUATION WITH YOU.

ONCE YOU HAVE READ THE DESCRIPTION AND ARE READY TO START THE TASK, PLEASE PRESS PLAY ON THE CASSETTE PLAYER;

Situation description *[Neutral]*

"Imagine yourself going to a supermarket and buying products you need for your house. Imagine other persons who are also buying products, talking among themselves about daily issues, examining new brands, and comparing different products."

Situation description *[Positive affect]*

"Imagine yourself receiving a notice that you win a large amount of money in the national lottery, and imagine other students or colleagues in your class hearing about this notice, approaching you, congratulating you, and telling others about your good fortune."

Situation description *[Secure prime]*

"Imagine yourself in a problematic situation that you cannot solve on your own, and imagine that you are surrounded by people who are sensitive and responsive to your distress, want to help you only because they love you, and set aside other activities in order to assist you."

PLEASE PRESS PLAY ON THE CASSETTE RECORDER WHEN YOU ARE READY TO COMPLETE THE TASK.