Outcomes of psychological interventions designed to change unhelpful illness perceptions in people with coronary heart disease (CHD): A Meta-analysis.

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Abstract

Background

Previous research demonstrated that maladaptive illness perceptions contribute to poor psychological outcomes in CHD. Cardiac interventions are effective in changing illness perceptions. However, it is unclear whether interventions targeting illness perceptions can also contribute to positive changes in symptoms of depression and anxiety. It is unclear whether interventions with psychological components lead to more reduction in inaccurate illness perceptions.

Objective

The current thesis aims to determine if psychological interventions are more efficacious in changing illness perceptions compared to interventions without these components. Another goal is to assess whether cardiac interventions can also contribute to positive changes in anxiety and depression. Finally, the present investigation assesses the impact of type of illness and age on the efficacy of cardiac interventions.

Methods

Using meta-analytic design English databases, relevant journals and references lists were searched for randomised controlled trials of interventions designed to change illness perceptions. The outcomes included illness perceptions, and symptoms of depression and anxiety.

Participants with CHD were included in the current meta-analysis. Effect sizes were expressed as *Hedges's g*.

Results

All cardiac interventions yield a small but consistent effect in reduction of maladaptive illnesses perceptions. However, interventions with psychological component are not significantly more efficacious in changing maladaptive illness perception (*Hedges's g* = .248) compared to interventions that do not contained psychological components (*Hedges's g* = .224). Interventions designed to change illness perceptions contribute to significant positive change in symptoms of anxiety (*Hedges's g* = .204), but not symptoms of depression (*Hedges's g* = -.089). Participants with chronic illness report larger reduction in inaccurate illness representations compared to participants with acute illness.

Conclusions

All components (psychological and non-psychological) of cardiac interventions can lead to small but positive reduction in maladaptive illness perceptions and symptoms of anxiety. While some interventions components (e.g. information giving) might work on the cognitive level, other techniques (e.g. active listening) might be more efficacious in addressing issues in emotional processing of CHD. Further, depressive symptoms and acute nature of illness might complicate process of change because of higher levels of emotional distress.

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1. Introduction

1.1. Chapter Overview

This chapter aims to present a theoretical and empirical context of the current investigation. In the first instance, a general overview of the research field is presented. This is followed by a discussion of theoretical context of the current meta-analysis. Next, the findings from previous studies linking illness perceptions and psychosocial outcomes are critically evaluated. The consecutive section contains critical evaluation of individual studies of interventions designed to change illness perceptions. Following from this, previous systemic reviews and meta-analyses are discussed. Finally, conclusions are drawn and resulting research gaps identified. The final section of this chapter contains research questions of the current meta-analysis.

1.2. General Overview

Coronary Heart Disease (CHD) is an umbrella term for medical conditions that involve narrowing of coronary arteries through gradual build-up of fatty material (atheroma) within their walls (Capewell et al., 2008). The accumulation of these fats leads to a narrowing of arteries, restricting blood flow into the heart. This can cause angina and consequently lead to myocardial infarction (Townsend et al., 2012). CHD typically includes medical conditions, such as angina (stable and unstable), coronary artery disease (CAD), myocardial infraction (MI), and conditions that require revascularisation procedures (e.g. coronary artery bypasses

graft (CABG) and percutaneous coronary interventions (PCI). PCIs include stenting and coronary angioplasty which are procedures designed to improve blood supply to the heart.

Despite CHD being largely a collection of preventable diseases, CHD is believed to be the biggest UK's killer. CHD contributes to approximately 80,000 UK deaths (most of these premature) and 7 million deaths worldwide every year (Townsend et al., 2012; World Health Organisation, 2013). CHDs put substantial pressures on the National Health System (NHS) services in the UK For instance, there were 469,800 inpatient episodes related to CHD and it is estimated that around 81,000 PCIs are carried out every year in the UK- three times more than a decade ago (Townsend et al., 2012). CHD also contributes to considerable economical and societal costs. For example, it has been estimated that CHD costs the UK health care system around £8.7 billion and the UK economy £19 billion (Liu, Maniadakis, Gray, & Rayner, 2002; Townsend et al., 2012; Vilahur, Badimon, Buguardini, & Badimon, 2014).

Given the large societal and economic costs of CHD, there has been an increased scientific interest in exploring risk and protective factors that might contribute to reducing the rates of CHD and associated adverse outcomes, such as premature death, poorer quality of life and/or engagement with treatment (e.g. Bajekal et al., 2012; Smolina, Wright, Rayner, & Goldacre, 2012). For decades researchers have focused their attention on physical health factors, such as obesity, smoking, hypertension, hyperlipidemia, genetic predispositions and/or diabetes (Blumenthal, 2005). These more traditional factors, however, are

insufficient to explain mechanisms involved in development and treatment of CHD. This is mainly because CHD involves a more complex than previously thought interplay between biological and psychosocial mechanisms (Luengo-Fernandez, Leal, Gray, & Rayner, 2006). In addition to physical health risk factors, many individuals with CHD have a wide range of psychosocial vulnerabilities, such as high levels of negative affect (e.g. trait hostility, depression, anxiety) and/or poor social support (Smith & Ruiz, 2002). These factors play an important role in emergence and maintenance of CHD, for example, through maladaptive stress response (Smith & Ruiz, 2002). These maladaptive stress responses can reduce blood flow in arteries and/or increase inflammation (i.e. by increasing cortisol to harmful levels) (Kanel, 2012; Pereira, Cerqueira, Palha, & Sousa, 2013). Individuals with emotional and cognitive vulnerabilities associated with CHD are more likely to appraise contextual stressors more negatively and to respond to them with greater reactivity (e.g. Smith & Ruiz, 2002). Prolonged over-reactivity can perturb physiological mechanisms underlying stress responsiveness (e.g. cortisol production) and consequently lead to CHD (Smith & Ruiz, 2002). The experience of CHD (e.g. MI), in turn, might reinforce pre-existing psychosocial vulnerability factors, such as depression, anxiety, greater social isolation and poorer quality of life (Reid, Ski, & Thompson, 2013).

Researchers and clinicians, therefore, have been showing more interest in the role of psychosocial factors in a development and maintenance of CHD (e.g. Blumenthal, 2005; Platt, Green, Jayasinghe, & Morrisey, 2014). These factors

include social support, uptake of healthy life-style, self-efficacy, locus of control, coping strategies, mood and/or representations of illness (Helgeson, 2003; Lett et al., 2004; Smith & Ruiz, 2002).

A number of psychological factors, such as representations of illness, are thought to be particularly important in CHD because high percentages of individuals with CHD (approximately 83%) develop maladaptive illness cognitions (Foxwell, Morley, & Frizelle, 2013). Dysfunctional representations about illness develop in response to the nature of cardiac events, which often are sudden and unexpected (Petrie & Weinman, 2012). As such the unpredictable nature of CHD often leads to catastrophic and maladaptive appraisal of illness and apprehension of vulnerability. When maladaptive illness cognitions are left unchallenged they may adversely impact psychosocial outcomes and treatment adherence. For example, participants who believe that their illness have serious personal consequences might find it more difficult to return to their life before CHD episode (Foxwell et al., 2013).

Maladaptive illness representations can be modified via focused psychological interventions, such as cognitive behavioural therapies and/or motivational interviewing (Peterson & Kim, 2011). These treatment approaches are shown to be effective in a wide range of chronic and acute health problems, including diabetes, asthma, hypertension, chronic pain and cancer (e.g. Halm, Mora & Leventhal, 2006; Horne &Weinman, 2002). It is beyond the scope of this thesis to review studies that evaluate psychological interventions targeting illness representations in other disease. However, it is recognised such interventions can

substantially aid the adjustment process, improve recovery and treatment adherence, reduce mortality and morbidity and increase utilization of rehabilitation (Hagger & Orbell, 2003; McAdrew et al., 2008; Petrie, Jago, & Devcich, 2007; Weardon & Peters, 2008).

Research into the efficacy of psychological interventions for individuals with CHD has shown significant improvement in mental and physical health. Several large-scale research programmes have been designed to investigate the effects of psychological interventions on changing maladaptive illness representations and improving outcomes for participants with CHD. For example, Enhancing Recovery in Coronary Heart Disease (ENRICH) or Angina Plan (ENRICH Investigators, 2000; Lewin, Furze, Robinson, Griffith, Wiseman, Pye, & Boyle, 2002). Research into these programmes (as well as other smaller scale interventions) is promising because it broadens patients' treatment options, offers more holistic approach to treatment and improves their recovery outcomes.

The findings from the individual studies, however, have so far been inconsistent. While some studies have demonstrated that psychological interventions have the potential to successfully change maladaptive illness cognitions and improve psychosocial outcomes in CHD, other studies have failed to do so (e.g. Cooper, Lloyd, Weinman, Jackson, 1999; O'Rourke & Hampson, 2010; Saab et al., 2009). Additionally, some studies have reported negative results, demonstrating that other treatments conditions are more beneficial in a controlled trial environment (e.g. Bolman, Brug, Bar, Martinali, & van den Borne, 2005). Mixed and contradictory results from individual studies hamper

development of conclusions that can be made about the efficacy of psychological interventions. These contradictory findings may be related to a number of factors, such as methodological differences between studies, differences in the content and theoretical frameworks of the interventions and/or variations in characteristics of research participants.

Inconsistent results from individual studies are to be expected, but nonetheless this level of variation in approach and outcome does little to achieve a scientific consensus over the benefits of psychological interventions. While single studies are still important they are also prone to a number of methodological limitations, such as sampling error, low statistical power and/or measurement error (Schmidt, 1996). One way to overcome these difficulties is by aggregating findings by combining individual studies using the methodology of meta-analysis. By successfully addressing methodological shortcomings of individual studies the methodology of meta-analysis enables the researcher to be more confident in making scientifically sound conclusions (Schmidt, 1996).

Meta-analysis methodology might be particularly helpful in making sense of conflicting findings within the research into the effectiveness of psychological interventions targeting illness representations in CHD. This is because there are discrepancies in findings from the individual studies. These discrepancies make it difficult to ascertain what interventions might work for which group of individuals with CHD and under what circumstance. Additionally, applying a meta-analysis to accumulate findings from single studies can contribute to better

understanding of CHD and development of more effective models of treating CHD.

1.3. Aims of the Current Meta-analysis

The overall aim of the current study is to assess the efficacy of psychological interventions in changing maladaptive illness perceptions in participants with CHD by using the methodology of meta-analysis. Another goal is to determine whether interventions designed to change illness perceptions contribute to positive changes in symptoms of depression and anxiety. The findings of the present meta-analysis may have implications for practice of health professionals working with individuals with CHD. The findings may also contribute to increasing understanding of psychological mechanisms involved in changing illness perceptions. Consequently outcomes may have some implications for improving already existing treatment programmes for individuals with CHD. The overview of the strategy adopted for the present literature search can be found in Appendix A.

1.4. Theoretical Link between Illness Perceptions and Psychological Outcomes

Psychological factors have become increasingly important in understanding and guiding adjustment and recovery of individuals with CHD. Amongst these factors, illness beliefs have been identified to be particularly important in CHD (Petrie & Weinman, 2012). Positive participants' beliefs about

their illness improve their mental and physical health outcomes, treatment adherence and/or reduction of emotional distress (Petrie & Weinman, 2012). In addition, factors that were previously believed to be important have been recently showed to be relatively poor predictors of recovery and less susceptible to change through interventions (Blumenthal, 2005; Leventhal & Cameron, 1987). These factors include disease complexity, treatment duration or participants' demographic and social characteristics (Blumenthal, 2005).

The link between illness perceptions and psychosocial outcomes has strong theoretical fundaments in self-regulation theories (Maes & Karoly, 2005). The relationship between psychosocial factors and illness perceptions has been explained by a wide range of models, including Health Beliefs Model or Theory of Planned Behaviour (Ajzen, 1991; Rosenstock, 1974). While these models have been widely applied in several health-related problems (e.g. smoking and /or breastfeeding), they have been found to be less relevant in explaining recovery and adjustment in participants with CHD (Harvey & Lawson, 2009). For example, the Theory of Planned Behaviour (TPB) upholds that six constructs (attitudes, behavioural intention, subjective norms, social norms, perceived power and perceived behavioural control) contribute to health related behaviours (Ajzen, 1991). In this model, the person's decision to engage in a particular behaviour (e.g. smoking) is directly linked to the intention of engaging in this behaviour (Ajzen 1991). Intention, on the other hand, is influenced by attitudes towards an outcome, the beliefs about behavioural control over the outcome, and normative beliefs about the outcome (e.g. social norms about smoking) (Ajzen 1991). While

this model is helpful in predicting and understanding a range of health behaviours in public health, its utility in more complex medical health problems is limited (Munro, Lewin, Swart, & Volmink, 2007). There are a number of reasons for this limitation, including not accounting for emotional processing that person engages in while making sense of their illness and a linear understanding of the relationship between the variables, which does not reflect a dynamic nature of behaviours (Harvey & Lawson, 2009; Munro et al., 2007).

More recent models, however, have considered cognitive *and* emotional processes in explaining the link between illness representations and psychosocial outcomes (Harvey & Lawson, 2009). Specifically, these models recognised that there is a *dynamic* relationship between emotional responses to the illness, illness representations and outcomes (Harvey & Lawson, 2009). Addressing this dynamic relationship allowed for explicitly recognising the impact of illness perceptions in recovery (Petrie & Weinman, 2012). Leventhal et al. (1987), for example, proposed the Common Sense Model (CSM) of understanding how people make sense of their illness. In this model Leventhal et al. (1987) propose that individual's response to the illnesses is aimed at minimizing fear and avoid danger (Diefenbach & Leventhal, 1996; Leventhal & Cameron, 1987). In trying to achieve these two goals the individual construct 'lay' understandings of different dimensions of illness (called illness representations or perceptions). These illness representations guide patients' coping and engagement in recovery process (e.g. adherence to medical treatments). In the CSM (see Figure 1.1) Leventhal et al. (1987) distinguish six dimensions of illness:

1) I*dentity*: the name or the label given to the illness and the symptoms that are associated with it
2) *Cause:* beliefs about the cause of the illness. These may include genetic, environmental and lifestyle factors. These beliefs may not always be medically accurate.
3) *Timeline:* beliefs about the duration of the illness. This dimension includes patients' beliefs about how acute and chronic their illness is. It is suggested that the beliefs about timeline of illness can change in response to changes in symptoms.
4) *Consequences:* the beliefs about the consequences of patients' illness and how these consequences impact on different aspects of their lives, including social, financial and psychological consequences.
5) *Curability/controllability:* this is a set of beliefs about the extent to which patients perceive to have control over their illness and whether their condition can be cured. Controllability includes personal control (e.g. belief about a degree to which patients' self-management might have an impact on the illness/symptoms) and treatment effectiveness.
6) *Emotional representations:* negative emotions associated with an illness.

In the CSM people actively construct and re-construct beliefs about their illness (Leventhal & Cameron, 1987) (see Figure 1.1). This helps them to make sense of their illness (Leventhal & Cameron, 1987). In doing this people continuously relate dimensions of illness representations to their previous

personal and environmental experiences (Leventhal & Cameron, 1987). They also utilize currently available information, such as public health campaigns or hospital leaflets (Harvey & Lawson, 2009). In order to make sense of different features of illness they draw upon their experience of how they feel (emotional processing) and what they know about the different aspects of the disease (cognitive processing) (Harvey & Lawson, 2009). Figure 1.1 depicts a dual processing involved in making sense of illness. The dual (emotional and cognitive) processing takes place in three stages: 1) illness representations, 2) the coping/action plan to minimize this threat and 3) appraisal stage when the coping and progress are evaluated and modified if necessary (Diefenbach & Leventhal, 1996).

The five dimensions of illness representations (see Figure 1.1) become a guide for health related decisions, behaviours and coping styles which are later evaluated (Diefenbach & Leventhal, 1996). If the person appraises these coping styles and behaviours as helpful in minimising danger and fear they are likely to be perpetuated (Diefenbach & Leventhal, 1996). For example, if an individual holds a belief that angina is caused by stress and worry than he is likely to adopt a maladaptive strategy of avoiding stress-inducing situations, paradoxically maintaining heightened levels of stress and anxiety. Avoidance in a short term may reduce stress levels and consequently the person is likely to appraise this strategy as helpful. Over time, however, avoidance may lead to increased social isolation, withdrawal, high levels of stress and anxiety with a significant negative impact on quality of life (Harvey & Lawson, 2009).

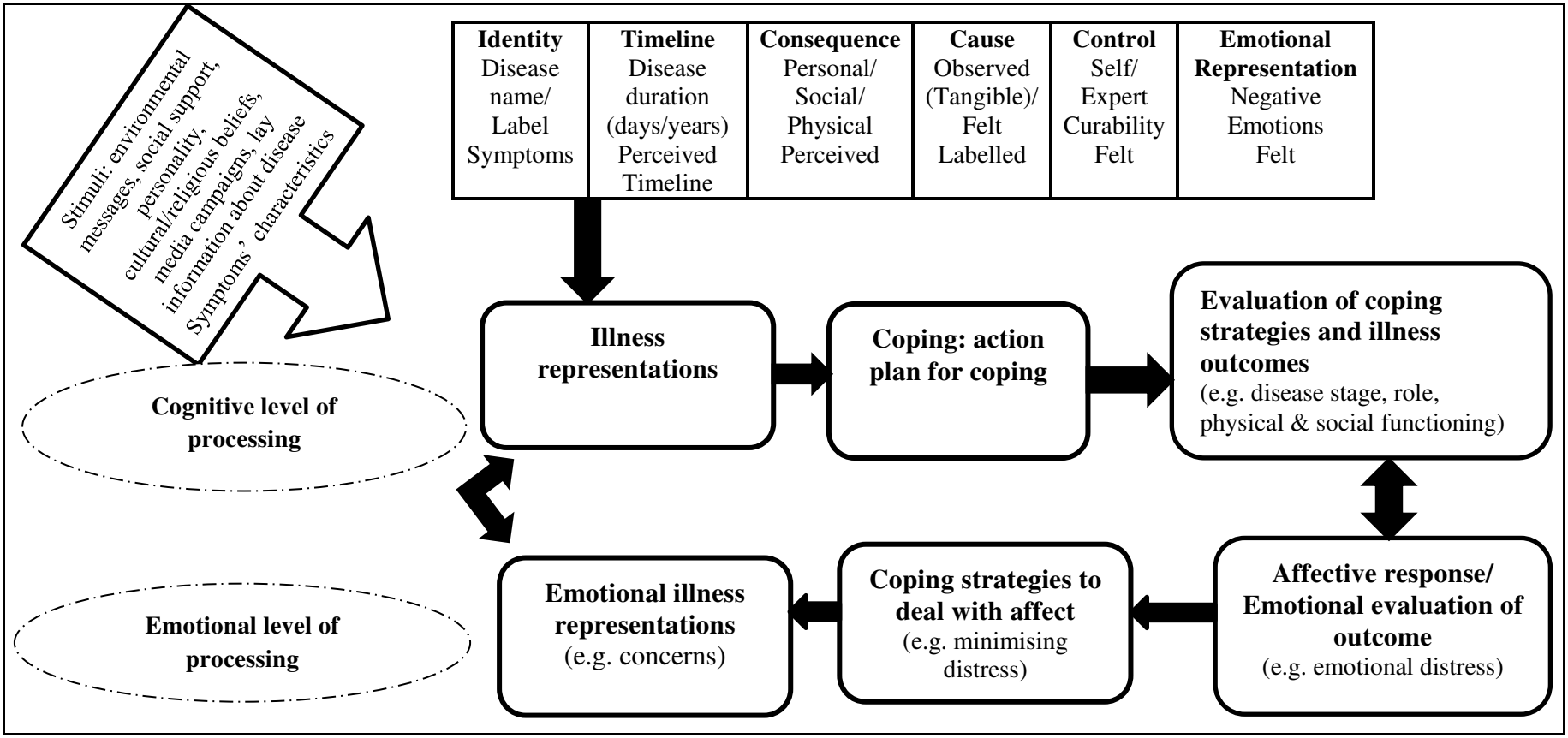

Figure 1. 1 Common Sense Model of Illness Representations.
Adopted from Diefenbach &Leventhal (1996) and Harvey & Lawson (2009)

In the CSM coping mechanisms play central role because they are shaped by how individuals represent their illness. Emotional responses to illness (e.g. avoidance, denial or expressing emotions) form part of passive coping mechanisms (Harvey & Lawson, 2009). These emotional coping mechanisms are linked with poorer psychosocial outcomes, including more symptoms of depression and anxiety (Harvey & Lawson, 2009; Maes & Karoly, 2005). On the other hand, active coping mechanisms, such as problem solving and seeking support are thought to improve outcomes (Harvey & Lawson, 2009).

The CSM model is particularly relevant for participants with cardiac problems because individuals with CHD can hold particularly maladaptive illness perceptions due to the unpredictable nature of the illness (Leventhal & Cameron, 1987; Rigel, 1993). Some of the examples of these unhelpful illness representations might include: 'it is dangerous for people with heart problems to argue', 'angina is caused by stress and worry', 'angina is caused by worn out heart', or 'it is not advisable for people with angina to exercise' (Furze, Bull, Lewin, & Thompson, 2003; Goulding, Furze, & Birks, 2010).

The CSM has also strong empirical support (Diefenbach &Leventhal, 1996). For example, Petrie, Weinman, Sharpe, & Buckley (1996) demonstrated support for the CSM model in participants who suffered heart attack. They found that beliefs about consequences and identity of the illness explained 20 percent of change in disability in social interactions. Participants who thought that they had less control over their illness and who thought that their illness has less serious consequences for their future were also less likely to attend cardiac rehabilitation

programmes. Interestingly, participants' maladaptive beliefs about their heart attack were predictive of poor outcomes independently of psychological distress. These results might indicate that illness perceptions might be better predictors of outcomes than emotional distress itself (Petrie et al., 1996).

Subsequent studies have also found a link between illness representations and psychological and social outcomes in participants with CHD, further supporting the utility of the CSM model. For instance, Cooper et al. (1999) demonstrated that participants who believed that their symptoms of cardiac disease are controllable and caused by lifestyle factors were more likely to engage in treatment. The findings of this study are valuable, because the authors demonstrated that illness representations are important in treatment of individuals with different types of CHD. Additionally, Cooper et al. (1999) have shown that participants' knowledge of risk factors is insufficient to predict attendance at cardiac rehabilitation. This finding is in line with results of other studies, for example, Zerwic, King, & Wlasowicz (1997) found that although participants with diagnosed and suspected CAD were able to identify risk factors for CAD in general, they failed to relate these factors to their own illness or identify risk factors that were documented in their medical records. These findings suggest illness perceptions play a specifically important role in CHD and that knowledge of risk factors by itself is insufficient to facilitate positive coping mechanisms.

A more recent study by Platt et al. (2014) evaluated the CSM model by comparing its utility with the Transtheoretical Model of Change in health-related behaviours (Prochaska & DiClemente, 1983). Platt et al. (2014) found specific

dimensions of illness representations, such as illness consequence, emotional representation and timeline were predictive of adherence to exercise regimes and medication adherence in participants with CHD. Overall, Platt et al., (2014) showed that both models can be equally beneficial in facilitating realistic and achievable treatment plans and outcomes. The CSM, however, was particularly useful in treatment adherence (specifically for those participants whose illness representations were more likely to be guided by emotional processes, such as avoidance or denial) (Diefenbach & Leventhal, 1996; Platt et al., 2014).

The CSM was chosen as a theoretical framework for the current investigation because it is a useful framework for capturing the relationship between psychosocial outcomes and illness beliefs in complex and unpredictable diseases, such as CHD. CSM, however, has been criticised in a context of self-regulation theories as underestimating the role of broader social context in a development of health outcomes (Jakcson, McKenzie & Hobfoll, 2000). As a cognitive processing model the CSM is also difficult to apply in individuals with cognitive impairments because the ability to regulate their behaviour might be affected by cognitive difficulties (Vohs & Baumeister, 2011). Another criticism of CSM is that the model implicitly assumes that coping mediates the outcomes. However, the empirical evidence to support this is limited because a majority of previous research was correlational in nature (Hagger & Orbell, 2003).

Summary. The CSM model can add substantially to our understanding of how people with CHD make sense of their illness, recover and engage in treatment. This model is particularly useful because it represents the dynamic and

multidimensional nature of how people make sense of their illness and treatments. In addition, the empirical evidence supporting the utility of this model in individuals with CHD is relatively strong. The empirical findings demonstrate that what makes a difference to patients' prognosis and treatment is *how they make sense of* their illness rather than just *what they know* about it.

1.5. Illness Perceptions, Psychological Outcomes and Socio-demographic Moderating Factors

Numerous empirical studies demonstrated that maladaptive representations of CHD are linked with poor psychosocial outcomes, such as anxiety and depression (e.g. Cooper et al., 1999; Grace et al., 2005). A selection of these studies relevant to the current meta-analysis is presented in this section (a brief narrative summary can be also found in Table A1.1, Appendix B). For example, Whitmarsh, Koutantji, & Sidell (2003) in a cross-sectional study provide evidence that depression, anxiety and illness perceptions predict attendance at cardiac rehabilitation programmes in participants with MI. In addition, individuals who attended cardiac rehabilitation perceived fewer symptoms of their illness and were more likely to underestimate their seriousness. They were also more likely to use emotionally-focused coping mechanisms, such as denial and avoidance (Whitmarsh et al., 2003). These results corroborate the findings from previous studies which found that individuals who attended cardiac rehabilitation are more likely to feel anxious and depressed (e.g. Petrie et al, 1996). This interesting matrix of results might indicate that participants who experience more distress and

believe that their illness is serious and/or that they experience a lot of symptoms are more likely to engage in treatments. On the other hand, one needs to exercise caution when drawing conclusions about the nature of the relationship between psychological outcomes and illness representations based on the Whitmarsh et al. (2003) study. This is because Whitmarsh et al. (2003) did not investigate a direct relationship between illness representations, depression and anxiety. Instead Whitmarsh et al. (2003) focused on investigating whether depression and anxiety predicted attendance at cardiac rehabilitation. Another limitation of this study is its cross-sectional and retrospective design. A longitudinal design might have shed more light on how the relationship between depression, anxiety and illness representations develops over the course of illness.

Grace et al. (2005) address some of the limitations evident in the study by Whitmarsh et al. (2003). Grace et al.(2005) demonstrated a direct relationship between symptoms of illness representations and depression in participants with CHD. In addition, Grace et al. (2005) also showed that the relationship between illness representation and depression was affected by socio-demographic factors, including age and ethnicity. For instance, participants identified in the study as non-white and male reported more severe symptoms of depression, more chronic perception of illness and lower control over illness and treatment. Younger participants, on the other hand, were more likely to perceive their condition as more debilitating and reported more symptoms of depression (Grace et al., 2005).The findings of this study support the relationship between depression and illness perceptions. Additionally, Grace et al. (2005) also demonstrated that the

association between illness perceptions and psychological outcomes is moderated by other factors, such as age.

Results produced by Grace et al. (2005) are important because of a number of strengths in the study design and sample stratification. Firstly, the study used well validated measures of psychological distress (Hospital Anxiety and Depression Scale-HADS) and illness perceptions (Illness Perception Questionnaire-Revised) (Moss-Morris, Weinman, Petrie, Horne, Cameron, & Buick, 2002; Zigmond & Snaith, 1983). Secondly, Grace et al. (2005) selected a sample of participants with different types of CHDs, including MI, angina and participants post PCI (Percutaneous Coronary Intervention) or CABG (Coronary Artery Bypass Grafting). This is a definite strength of this study as it indicates that the relationship between illness perceptions and depression is shared across all types of CHDs. Finally, the conclusions from Grace et al. (2005) study were strengthened by recruiting a relatively large sample (N= 661).

Despite these advantages, the study by Grace et al. (2005) has also some important limitations. Although the sample drawn in this study is fairly large, there are substantial inequalities in distribution of gender and depressive symptoms. For example, the data analysis was conducted on 504 male participants but only 157 female participants. These are potentially meaningful group differences that could have skewed the results, particularly in the context of statistical methods adopted to analyse the data (i.e. group analysis of variance based on gender) (Field, 2005). Another significant shortcoming of this study was that the majority of the participants (over 80%) did not report any symptoms of

depression. Hence, it is possible that the relationship between depressive symptoms and illness representations could be a false positive related to a small sub-sample of participants with depressive symptoms.

Despite the above-mentioned limitations, the study by Grace et al. (2005) corroborate the findings of other studies indicating the importance of a relationship between participants' maladaptive illness perceptions and poor psychosocial outcomes. For example, Furze, Lewin, Murberg, Bull and Thompson (2005) found that participants with angina who reported unhelpful illness beliefs were also more likely to report an increased number of symptoms of depression and anxiety. Furze et al. (2005), however, went further in their investigation than Grace et al. (2005) by demonstrating participants' illness beliefs changed over time. Participants who reported decrease in unhelpful maladaptive illness representations also reported fewer symptoms of anxiety and depression. Unlike Grace et al. (2005), however, Furze et al. (2005) were unable to demonstrate that any socio-demographic factors were linked to changes in illness representations and depression. This is important because it indicated a dynamic nature of illness perceptions which suggest that illness perceptions are changeable. This in turn concurs with an idea shared by many researchers and clinicians that illness perceptions could be changed via targeted interventions (e.g. Cooper et al., 1999; Petrie & Weinman, 2012; Whitmarsh et al., 2003). In addition, the findings of the study by Furze et al. (2005) are strengthened by the use of relatively well validated and reliable measures of psychological distress

(e.g. HADS) and illness perceptions (York Angina Beliefs Questionnaire) (Furze et al., 2003; Zigmond & Snaith, 1983).

There are, however, a number of limitations evident in the study by Furze et al. (2005). Firstly, the study was relatively low powered- it included a small sample of 105 participants with angina. The small sample might be one of the reasons why Furze et al. (2005) did not detect an association between socio-demographic variables (i.e. age, gender, social class) and changes in illness perceptions. Secondly, the participants in this study had received a targeted intervention (Angina Plan) prior to baseline data collection. It is possible that reported association between illness perceptions and psychological outcomes might be an artefact of residual effects of the intervention and/or small sample size.

Other studies have also attempted to address limitations identified in previous research (e.g. Stafford, Berk, & Jakson, 2009; Juergens, Seekatz, Moosdorf, Petrie, & Rief, 2010). For instance, Stafford et al. (2009) conducted a longitudinal study demonstrating that negative illness perceptions were associated with higher levels of depressive symptoms. The longitudinal design of this study and a moderately sized sample (N = 193) make a relatively strong case for the relationship between illness representations and psychosocial outcomes.

In terms of socio-demographic factors Stafford et al. (2009) found that the relationship between illness perceptions and psychosocial outcomes was associated with income and age, but not with gender. Thus, participants who were older and poorer were more likely to perceive their illness as out of their control.

This is an interesting finding which is only partially consistent with previous studies. It is also contradictory to some of the traditional views about socio-demographic factors associated with CHD, such as that CHD mainly affects men (Lockyer & Bury, 2001). There could be a number of theoretical and methodological explanations for the inconsistency in findings between Stafford et al.(2009) study and previous research. Firstly, participants who are older and poorer might be more likely to have less social and financial resources and support and this might contribute to their perceptions that they have less control over their illness. Secondly, it is possible that the relatively small sample in this study contributed to insufficient statistical power to detect gender differences.

Similarly to Stafford et al. (2009), Juergens et al. (2010) also adopted a longitudinal design by assessing participants before and after heart surgery. The findings of this study further support the idea that illness representations are associated with a range of psychosocial outcomes. Specifically, Juergens et al. (2010) showed that participants with maladaptive representations of their heart condition reported more symptoms of depression pre and post surgery. In line with previous research, in the Juergens et al. (2010) study depressive symptoms were associated with beliefs about chronic duration of illness and perception of symptoms as more serious (e.g. Grace et al., 2005). There are several possible explanations for this, including a relatively small sample (N = 56), use of statistical method (i.e. correlation) that makes it difficult to detect significant results with small sample size. Despite the aforementioned shortcomings, the findings of the study by Juergen et al. (2010) provide further support for the

conceptual premise that illness representations are linked with psychosocial outcomes.

Not all research, however, points towards the importance of illness representations in predicting psychosocial outcomes. In particular findings from research investigating the link between illness representations and health-related behaviours is more ambiguous. Byrne, Walsh, and Murphy (2005), for instance, were unable to demonstrate a direct link between illness perceptions and health-related behaviours. In their study, exercise uptake was associated with higher levels of perceived control over illness and treatment, whereas medication adherence was linked with perception of illness chronicity (Byrne et al., 2005). Interestingly, the strongest association was found between emotional representation of illness and health-related behaviours. These findings may be explained by a number of different factors. For example, the fact that some of the findings do not corroborate with previous studies could be due to methodological limitations, such as self-report on behavioural data and cross-sectional design (French, Cooper, & Weinman, 2006; Podsakoff, MacKenzie, Lee, & Podsakoff , 2003). On the other hand, it is possible that the results of the study by Byrne et al. (2005) reflect a complex and dynamic nature of the relationship between illness representations and psychosocial outcomes. That is, psychological distress may mediate the relationship between illness perceptions and health-related behaviours (Platt et al., 2014). This could explain lack of the empirical support for the relationship between illness representations and health-related behaviours but stronger support for the link between illness perceptions and psychological

outcomes (e.g. Platt et al., 2014). It is difficult, however, to drawn firm conclusions solely from Byrne et al. (2005) study, because the authors did not assess psychological distress.

More recent studies have tried to explore the complex relationship between illness representations and a wide range of psychosocial outcomes (e.g. Greco, et al., 2014; Steca et al., 2013). Specifically, these studies have undertaken an effort to empirically validate a comprehensive model depicting interplay between different illness-related factors, including illness perceptions and psychosocial outcomes, such as depression (e.g. Greco et al., 2014). Steca et al. (2013), for example, demonstrated that illness representations play an important role in maintaining depression (i.e. they found small link between illness representation and depression). These findings corroborate a great deal with previous work in this field, confirming that participants who perceive their illness as less negative are also less likely to suffer from depression (Barth, Schumacher, & Herrmann-Lingen, 2004). Steca et al. (2013) in explanation of the results suggested that participants who felt less depressed and had more positive illness perceptions might have been more capable of engaging in constructive health-related behaviours (Steca et al., 2013). There are, however, a number of limitations evident in this study that suggest the caution in interpreting the results. For example, the sample of the study was relatively small (N = 172) in the context of a number of statistical comparisons made. Additionally, the sample included a broad spectrum of cardiovascular diseases with a different range of illness severity. The inclusion of a variety of heart diseases might have potentially

limited the generalizability of findings because other cardiovascular diseases (e.g. heart failure) have different aetiology, prognosis and treatment (French et al., 2006). Due to these differences individuals with heart failure develop illness perceptions that are qualitatively different (Goulding et al., 2010). Furthermore, cross-sectional design in the Steca et al. (2013) study makes it difficult to draw certain conclusions about how the relationship between illness representations and depression develops over time.

In a partial replication of Steca et al. (2013) study, Greco et al.(2014) investigated the role of illness perceptions in maintaining depression . Their findings replicate results from study by Steca et al. (2013) and were elaborated by demonstrating that the relationship between illness perceptions and depression is sustained over time, although Greco et al. (2014) included a relatively short follow up period of 2 months. In particular, Greco et al. (2014) found participants who had a more adequate perception of their symptoms were less likely to be depressed (Greco et al., 2014). While there are strengths between the studies of Greco et al. (2014) and Steca et al. (2013) as they share many methodological similarities, they also share many similarities in limitations. In terms of strengths both studies adopted similar robust psychometric indices to assess depression and illness perceptions. Nevertheless, the use of depression measure here makes it difficult to relate the findings to previous studies that used different measures of depression. Although the samples were similar in characteristics the sample in Greco et al. (2014) study was significantly smaller (N = 75). This makes the conclusions by Greco et al. (2014) more prone to false positive error. The small

sample size is particularly relevant for the generalizability of the findings for these studies, because a statistical analysis in both studies included a relatively large number of comparisons across different variables (Field, 2005).

Based on the studies evaluated above, a fair amount of inconsistencies and gaps across studies investigating the relationship between illness perceptions and socio-demographic factors is noticeable. These gaps and inconsistencies may be related to methodological differences between studies, such as sample size and application of different measurement tools.

One relatively recent study, however, that explicitly addressed some of these inconsistencies was a study conducted by Aalto et al. (2005). In this study nearly 3000 participants with CHD were asked about their illness beliefs as well as socio-demographic and illness related factors. Aalto et al. (2005) investigated the link between socio-demographic variables, such as age and gender, and illness perceptions. Aalto et al. (2005) found that younger participants in comparison to older participants reported more negative illness perceptions. Interestingly, although the study demonstrated that there were some qualitative differences in illness perceptions between genders, this was not found to be significantly related to illness perceptions.

While the findings of this study make a relatively strong case for the importance of socio-demographic and illness related factors in CHD, it is still difficult to draw certain conclusions about which socio-demographic and illness related factors are particularly important in illness perceptions about CHD.

As the evidence across individual studies is inconclusive and lacks consistency it might be necessary to accumulate the findings from different studies. Foxwell et al. (2013), for example, recently recognised the need for systematic review of studies looking at the relationship between illness perceptions and psycho-social outcomes. Subsequently, they conducted a systematic review of 21 empirical studies examining the relationship between illness perceptions, mood and quality of life (Foxwell et al., 2013). Overall, they demonstrated that participants with CHD who reported negative illness perceptions are significantly more likely to suffer from depression and anxiety (Foxwell et al., 2013). Specifically, the results of this systematic review corroborate the findings of individual studies indicating that participants who have a poorer understanding of their symptoms, perceive their illness as more serious in consequences and more chronic, and themselves as being less in control of their illness are also more likely to experience from depression and anxiety (Foxwell et al., 2013). Although this systematic review provides consistent evidence for the relationship between illness perceptions and psychosocial outcomes, there are a number of reasons why these conclusions need to be treated with caution. Firstly, the review was limited to a narrative synthesis of findings. Therefore, the reader cannot make any conclusions about the theoretical and clinical importance of the relationship between illness perceptions and psychosocial outcomes (Schmidt, 1996). Secondly, this systematic review has some important methodological shortcomings. For example, the authors included only published studies which are likely to significantly increase the publication bias. In addition to this, the authors

did not take into consideration any moderating variables. This is likely to be a significant shortfall because previous studies have shown that socio-demographic markers are likely to be important moderators of the relationship between illness perceptions and psychosocial outcomes (e.g. Aalto et al., 2005; Cooper et al., 1999).

Summary. The findings from the individual studies and systematic review outlined above indicate that individuals with CHD who report maladaptive representations of their illness are more likely to suffer from depression and anxiety. Individuals with CHD who appraise their illness as more serious in consequences and chronic are particularly prone to experiencing psychological distress. Furthermore, some of the studies also showed that socio-demographic factors (e.g. gender and age) can influence the relationship between illness perceptions and psychosocial outcomes. In addition, individual studies also indicated that illness perceptions can change over time. This is an important finding because it suggests that maladaptive illness representations might be malleable to interventions. The studies outlined above, however, differ in design, sample characteristics, number of participants recruited, measured used to assess variables and types of statistical analysis applied. These shortcomings make it difficult to generalize the findings and draw certain conclusions about the importance of the relationship between illness perceptions and psychosocial outcomes.

1.6. Interventions Designed to Change Illness Perceptions

In light of the findings indicating that illness representation can change over time, there has been an increased interest in designing interventions that target illness perceptions. The fundamental assumption behind such interventions is that changing negative or inaccurate illness perceptions may directly improve psychological well-being. This is conceptually consistent with the Leventhal's CSM (Diefenbach & Leventhal, 1996).

Although research into the effectiveness of interventions targeting illness representations is in relatively early stages, there has been steady and consistent increase in published trials that have attested the efficacy of such interventions (French et al., 2006). A range of these studies is critically discussed in the current section (a succinct narrative summary of these studies can also be found in Table 1.1).

Table 1.1.

Individual studies designed to change illness perceptions in CHD (in alphabetical order).

Study authors, publication year, country	**Sample characteristics (size, sex, age, CHD type)**	**Details of the intervention**	**Design/ Length of Follow-up (FU)**	**Outcomes Assessed**	**Results (brief summary)**
Bengtsson, 1983 Sweden	N = 87 M age = 55.3 74M/13F Patients with MI	Cardiac rehabilitation programme involving family members and physical training	RCT design/ 14 months FU	Patients' physical functioning Social factors, e.g. finances, employment Psychological functioning-depression & anxiety (Minnesota Multiphasic Personality Inventory) Questionnaire testing patients' knowledge about illness (unspecified)	No improvements in participants' knowledge about illness and psychological well-being, Improvements in physical functioning, e.g. blood pressure
Broadbent et al., 2009 New Zealand	N = 103 52/51 (intervention/ control) M age for intervention group=54.9 M age for control group = 54.6 91M/12F Acute MI	4 brief inpatients session that included psycho-education and debunking illness misconceptions	RCT design/ 3 & 6 month FU	Illness perceptions & Casual perceptions (Brief Illness Perception Questionnaire & Illness Perception-Revised) Health behaviours (smoking, exercise and diet)	Participants in the intervention group reported improved illness coherence & increase in reuptake of exercise.

Furze et al., 2009 UK	N = 204; 100/104(intervention/control) 164F/40M M age = 55.61 Patients waiting for CABG	'The HeartOp Programme' targeted to change illness perceptions based on CBT principles + relaxation	RCT/ ~8 weeks post baseline data collection	Cardiac misconceptions (York Cardiac Beliefs Questionnaire) Anxiety (State Trait Anxiety Scale) Depression (Cardiac Depression Scale)	Significant improvements in cardiac beliefs and depression, but not in anxiety in the intervention group
Janssen et al. 2013 Netherlands	N = 158 No comparison group M age = 58 127F/31M Broad category of CHD	Cardiac rehabilitation programme	Cross-sectional design Pre and post data	Illness perception (Brief Illness Perception Questionnaire) Health-related quality of life (MacNew Heart Disease Health Related Quality of Life Questionnaire)	Improvements in the following domains of illness perceptions: perception of fewer consequences & fewer symptoms of their cardiac disease, improved understanding of their illness, decreased emotional impact of the illness. Improvements in illness perceptions have also contributed to significant changes in emotional, social and physical quality of life.

Pozen et al 1977 USA	N = 102 M age = 58 79M/23F Patients with acute MI	Inpatient Cardiac rehabilitation programme aiming at increasing knowledge of patients about the illness, reducing anxiety and encouraging reintegration to everyday life	RCT design/ 1 month FU after discharge	Anxiety (IPAT anxiety scale), Physical functioning Knowledge Questionnaire derived for the purpose of the study	Participants attending the programme demonstrated increased knowledge about the illness and social functioning, but no evidence of improvements in symptoms of anxiety
O'Rourke and Hampson, 1999 UK	N = 70 M age for intervention group = 57.7. M age for control group = 59.4 52M/18F Patients with MI	Edinburgh Heart Manual	Longitudinal/ 6 month FU	Illness perceptions (IPQ) Anxiety and Depression (HADS) Utilisation of healthcare services	Participants in programme based on the Heart Manual improved perceptions of their illness, e. g. greater sense of personal control and reported reduced symptoms of anxiety and depression. But no reduction in visits to GP.

CHD-Coronary Heart Disease; MI-Myocardial Infraction; RCT-Randomised Controlled Trial ; IPQ-Illness Perception Questionnaire; HADS-Hospital Anxiety and Depression Scale

Early studies into the efficacy of interventions focused on illness perceptions have produced a mixed pattern of results into the efficacy of multifaceted cardiac rehabilitation programmes in changing participants' illness perceptions (e.g. Bengtsson, 1983; Pozen et al. 1977). Bengtsson (1983), for instance, demonstrated that comprehensive interventions consisting of physical examinations and counselling did not improve participants' medical and psychological well-being. Pozen et al. (1977) showed that a multifaceted rehabilitation programme for participants with acute MI, improved their social functioning and knowledge about illness, but did not lead to positive changes in symptoms of anxiety.

These early studies, however, have a number of significant limitations that hinder the generalizability of their findings (Bengtsson, 1983; Posen et al., 1977). These limitations include lack of clear and consistent theoretical grounding of interventions, focusing primarily on physical functioning, use of different and often less stringent psychometric tools to assess psychological distress, poorly operationalized variables (primarily illness representations) and/or insufficiently described statistical analysis (e.g. lack of intention to treat analysis).

More recent studies have attempted to address some of these shortcomings. For instance, Furze et al. (2009) evaluated a comprehensive intervention programme, challenging participants' unhelpful misconceptions about CABG, provided information about post-operation care, offered relaxation strategies and encouraged participants to set risk reducing treatment goals. The

multifaceted intervention by Furze et al. (2005) was compared against education and counselling intervention which did not directly address participants' illness misconceptions. Overall, the findings of this trial were mixed, indicating that targeted intervention had positive impact on improving participants' physical functioning and reducing depressive symptoms, but did not reduce anxiety. More importantly, Furze et al. (2009) found that participants in the intervention group reported significantly less misconceptions about their illness. This is an interesting set of results, because typically changes to depressive symptoms are associated with changes in anxiety, but this was not the case here. This combination of results could be due to a number of methodological characteristics of the study by Furze et al. (2009). Specifically, a lack of positive change in anxiety might have been due to recruitment of insufficient number of participants. Another factor might be related to the type of outcome measures used, specifically with regards to the measurement of anxiety (i.e. State Trait Anxiety Inventory). This measure has been shown to be less sensitive compared to other measures in identifying anxiety in participants with CHD (Bunevicius et al., 2013). Although Furze et al. (2009) study has also some important methodological strengths (e.g. adopting a random allocation of participants), the shortcomings listed above might have significantly contributed to the mixed pattern of results.

In contrast to the study by Furze et al. (2009) other research has demonstrated that interventions targeting maladaptive illness perceptions can reduce anxiety. Broadbent et al. (2009) evaluated the efficacy of brief intervention targeted at changing maladaptive illness representations against standard hospital

care. The target intervention focused around addressing participants' personal illness misrepresentations and designing idiosyncratic recovery plans. Overall, Broadbent et al. (2009) showed that participants in the intervention group had a greater understanding of their illness and more accurate beliefs about the causes of their illness. The study by Broadbent et al. (2009), however, did not find any effect of the target intervention on other domains of illness perceptions (i.e. illness timeline, consequences and control over illness). It is difficult to make sense of the findings from this study because there are limited statistical data available. There are particularly sparse statistical data on a relationship between illness perceptions and psychological distress. In turn, Broadbent et al. (2009) reported the results on the impact of the intervention on life style changes, such as uptake of exercise and rate of returning to work. While these findings are important, there is limited scope for a reader to make judgments about how targeted interventions can contribute to changes in illness perceptions. Foremost, it is also difficult to make judgments about how any potential changes in illness perceptions might be associated with improvements in psychological well-being. Establishing a link between illness perceptions and psychological distress, however, seems particularly important for two reasons. Firstly, such link might have a potential to be a bridge between illness perceptions and health behaviours. Secondly, it can aid our understanding of mechanisms involved in development and maintenance of maladaptive illness perceptions.

Janssen, Gucht, Exel and Maes (2013) overcome some of the shortcomings outlined above in the studies by Broadbent et al. (2005) and Furze et

al. (2009). A direct effect of multifaceted cardiac rehabilitation programme on illness perceptions in participants with myocardial infraction was examined by Janssen et al. (2013). By using well-validated measures of illness perceptions the authors demonstrated that comprehensive cardiac rehabilitation programme, incorporating psycho-education, physical exercise, relaxation and psychological consultation contributed to positive changes in illness perceptions. Specifically, participants in the interventions group perceived their illness as less severe in consequences and having reduced emotional impact on their live. Participants in the intervention group also had better understanding of their illness and greater perception of control over their illness and treatment. The findings from this study are particularly meaningful, because of its methodological and theoretical strengths. These strengths include: intervention grounded in self-regulation theory and use of measures with satisfactory psychometric properties and well-matched to the measured concepts. Nevertheless, methodological shortcomings significantly reduce validity in the study by Janssen et al. (2013). This study could benefit from the target intervention being compared to treatment as usual or another intervention. Including a comparison group would have enhanced the quality of conclusions about the utility of cardiac rehabilitation programme. Employing a randomized design would have also reinforced the conclusions and make the findings more clinically relevant. It would also have been helpful to understand better which components of cardiac rehabilitation programme are more likely to be linked with changes in illness perceptions. This information could be helpful because the content of cardiac rehabilitation programmes vary

vastly across settings (and studies) (Reid et al., 2013). It is possible that this variability explains a certain degree of inconsistencies across findings from different studies. These inconsistencies make it difficult to gather the findings and to make judgements about what types of interventions are likely to be the most effective.

Research conducted by O'Rourke and Hampson (1999) compared the utility of self-help cardiac rehabilitation programme (Edinburgh Heart Manual) against exercise and educational programme in participants with MI. The Edinburgh Heart Manual consisted of three core components: education, exercise and stress management (Lewin, Robertson, Cay, Irvine, & Campbell, 1992). Including a comparison group enabled O'Rourke and Hampson (1999) to show the target intervention was significantly more effective in changing participants' maladaptive illness perceptions and improving their psychological well-being than usual care. The participants in the intervention group reported having greater sense of personal control over their illness and thought that their illness had fewer consequences in comparison to participants receiving standard care. Participants in the intervention group also reported feeling less anxious and less depressed. These findings demonstrate interventions that directly address illness perceptions can improve psychological well-being. These improvements, however, were not followed by reductions in utilisation of healthcare services (e.g. visits to GP). It is difficult to make broader conclusions about the clinical utility of specific intervention because a number of shortcomings of this study. Firstly, the study did not employ a randomised control design, which is considered to be a gold

standard of intervention research (e.g. Torgerson & Torgerson, 2008). Secondly, the effect sizes for anxiety and depression were small (.08 and .11 respectively). Thirdly, the overall sample size was relatively small (N = 70) and the subsample on which the data for psychological distress were analysed was too small for this purpose (less than 8 participants). The subsample was so small because the authors excluded from the analysis all participants whose scores on the HADS did not reach clinical significance. Such small sample size is a significant disadvantage of the study increasing the likelihood of the type I error and subsequently substantially reducing the quality of findings.

Summary. Individual studies attesting the efficacy of interventions designed to change illness perceptions provide mixed and inconsistent results. These inconsistencies are due to a variety of methodological and conceptual differences across trials, such as different designs (e.g. RCT and non-RCT trials), use of different psychometric tools to measure the same concept (i.e. illness perceptions, depression or anxiety), use of different populations and/or interventions consisting of different components. These differences hamper the generalizability of the findings and subsequently their clinical usefulness. Despite these differences majority of the studies indicate that explicitly addressing illness perceptions through targeted interventions can enhance participants' recovery. Based on single studies, however, it is difficult to make specific conclusions about what interventions or components of interventions might work best in changing unhelpful illness perceptions. It is also difficult to make conclusions about how

any changes in illness perceptions might be related to changes in psychosocial outcomes.

1.7. Previous Reviews Pertinent to the Current Meta-analysis

One way of overcoming methodological shortcomings of individual studies is to aggregate data and analyse findings from multiple individual studies. Meta-analysis is the means by which an aggregated outcome can be determined for a particularly area of study. Meta-analysis calculates effect sizes that can be interpreted and can inform health professionals and policy maker about empirically supported treatments in making judgements about the efficacy of interventions across different settings and populations with more confidence. Aggregating and systematically assimilating findings from a range of individual studies can add more scientific and clinical significance (Schmidt, 1996). Meta-analysis have been used to produce the National Institute for Health and Care Excellence (NICE) guidelines on provision of secondary care for patients after MI (NICE, 2013). In the guideline number 172 NICE recommends that exploring patients' illness representations should form a consistent part of any cardiac rehabilitation programmes (NICE, 2013).

In spite of meta-analysis contribution to clinical and research practice, the meta-analytic design is not free from weaknesses. For instance, the conditions under which meta-analysis can be conducted are still subject of scholars' discussions. Some argue that combining studies that use different measures introduces too much heterogeneity and error (Bartolucci & Hillegas, 2010).

Others suggest that aims of meta-analyses are to answer broader research question than individual studies and therefore it is unavoidable and almost desirable that there is some level of heterogeneity (Borenstein et al., 2009). Another criticism is that the exact source of heterogeneity is almost impossible to detect because it might be linked with a number of unknown factors (Bartolucci & Hillegas, 2010). Finally, it can also be difficult to locate all the relevant publications and therefore the results of meta-analysis often represent only a small subsample of studies (Bartolucci & Hillegas, 2010).

Despite some of the weakness in meta-analytic design, it has been chosen as a design in the current investigation because there have so far been limited efforts to systematically gather and analyse findings from studies investigating efficacy of psychological interventions in changing maladaptive illness perceptions in CHD. To the author's knowledge only two relevant articles were published: one systematic review and one meta-analysis (Goulding et al., 2010; French et al., 2006). Other publications were concerned with integrating findings from studies attesting the importance of psychological factors or other psychologically informed interventions in CHD (e.g. Dusseldorp, van Elderren, Maes, Meulman, & Raaij, 1999; Foxwell, et al., 2013; Reid et al., 2013, McGilion, et al., 2014). Although the findings of these meta-analysis and systematic reviews are important contributions to the field, they did not explicitly consider illness perceptions. These reviews, therefore, will not be discussed in the present thesis. A brief narrative summary of previous systemic reviews and meta-analysis pertinent to the current meta-analysis are presented in Table 1.2.

Table 1.2.

Narrative summary of previously published reviews in relation to illness perceptions (presented in alphabetical order).

Authors Year of publication, country Type of review (meta-analysis vs systemic review)	**Overall aim of the review (as described)**	**No of studies included/ Total Sample (N)**	**Time period covered by the review**	**Type of CHD**	**Characteristics of intervention (if applicable)**	**Brief outline of results**
Dickens et al., 2013; UK; Systemic review and meta-analysis	To assess which components of psychological treatments are most effective in improving depression	62 N = 17,397	Unspecified Studies included in the review from 1983 to 2011	Broad category of CHD	All included studies were RCTs; Interventions varied in mode of delivery, number of sessions, most of the interventions delivered by MDTs	Interventions with CBT component contributed to greater improvements in depression (SMD = .23; N =11); psycho-education, relaxation and problem solving components also significantly improve depression (SMD = .19; .15 and .34, respectively); There was no relationship between age, type of CHD and improvements in depression.

Foxwell et al. 2013; UK; Systemic review	To examine the relationship between illness perceptions, QoL and mood	21; N = not calculated, Range from = 49 to 3130	Unspecified; studies included in the review from 1996 to 2011	Broad category of CHD	Not applicable	Participants with more negative illness perceptions are more likely to report symptoms of depression and anxiety; poor illness understanding, more serious perception of the consequences of illness and perception of more chronic outcome are specifically linked with increased reports of symptoms of anxiety and depression
French et al., 2006; UK; Systemic review and meta-analysis	To examine which domains of illness perceptions contribute to attendance at cardiac rehabilitation programmes	8; N = not calculated, Range from 65 to 194	Search period from 1970 to 2005;	Acute MI	Not applicable	Positive illness perceptions contribute to higher rates of attendance at cardiac rehabilitation. Participants with acute MI who have a greater sense of control over their illness and symptoms and greater understanding of their illness are more likely to attend at the cardiac rehabilitations programmes.

Goulding et al., 2010; UK; Systemic review	To assess if interventions can change maladaptive illness perceptions	13; N = not calculated Range from 40 to 243	Unspecified; studies included from 1977 to 2007	Broad category of CHD	RCTs; Multifaceted interventions designed to change illness perceptions, knowledge and attitudes; Various modes of delivery	Interventions with CBT component led to significant positive changes, but it was impossible to determine if they were more effective than other interventions. There was no clear indication of whether interventions deigned to change illness perceptions contribute to positive changes in psychosocial outcomes.
Taylor et al., 2011; Scotland; Systemic review	To examine the impact of sociodemographic and psychological factors on adherence to cardiac rehabilitation programmes	18; N = 8842	Search period from 1990 to 2009	Broad category of CHD	Not applicable	Studies were inconsistent in the reporting of socio-demographic variables. Age & gender were the most commonly reported variables; ethnicity least commonly reported. Younger participants were least likely to attend cardiac rehabilitation. For older participants the decision to attend was linked with a greater sense of control over their illness. Participants who perceived their illness as more debilitating and serious in consequences were more likely to attend unless they reported more symptoms of depression.

CHD-Coronary Heart Disease; RCT-Randomised Controlled Trial, MDT-Multidisciplinary Team; CBT-Cognitive Behavioural Therapy; SMD-standardised mean difference; QoL-Quality of Life; MI-Myocardial Infraction

The findings from the French et al. (2006) and Goulding et al. (2010) reviews are inconclusive, incomplete and ambiguous. Goulding et al. (2010) statement reflects the tentativeness of the findings relatively well: '(…) Overall this suggests that it is possible to devise interventions which significantly and positively change maladaptive illness cognitions.' (Goulding et al., 2010, p.995). This statement only tentatively indicates that psychological interventions may be effective in changing inaccurate illness perceptions in CHD. Furthermore, the interpretation of results on the basis of narrative synthesis of individual studies has been criticized as it is more likely to be prone to subjective bias of researchers who may be presenting a partisan view (Bartolucci & Hillegass, 2010). Narrative summary of findings from multiple studies is prone to a range of biases that are difficult to assess (Bartolucci & Hillegass, 2010). It is also difficult in systemic reviews to appropriately consider and make sense of conflicting findings. This challenge is relevant for a review by Goulding et al. (2010) who identified eight studies that yielded positive results, three studies that found no effect and one study with negative effects on changing illness perceptions. These narrative findings would have been enhanced by deriving effect sizes and confidence intervals to understand better effects of different treatments and the conditions under which interventions might work (Higgins & Green, 2011).

Furthermore, Goulding et al. (2010) concluded that studies testing out multifaceted interventions based on cognitive-behavioural model were also effective. It is not clear, however, whether such interventions were more effective than other types of interventions, such as psycho-education only or counselling.

Again the methodological shortcomings of systemic review do not permit any conclusions about the type of interventions that might be more effective in changing maladaptive illness perceptions.

Another significant limitation of the data produced by Goulding et al. (2010) is that it did not consider the effects of changing illness perceptions on other psychosocial outcomes, including depression and anxiety. The authors explained that this was because of the heterogeneity among studies. At the very least, however, the results of the review might have been enhanced by providing more detailed narrative summary of the findings from psychosocial outcomes. Goulding et al. (2010) pointed out heterogeneity among studies contributed to a decision to omit statistical data analysis. Borenstein et al. (2009), however, suggest that use of different measure to assess the same variable does not exclude quantitative summary of data. It might, therefore, have been possible to derive individual effect sizes of each component study contained within systemic review without focusing on the magnitude of the overall effect size.

In addition, by including only studies with random allocation design, Goulding et al. (2010) managed to control the heterogeneity among studies to some degree. However, including only studies with one design is considered controversial by some (Higgins & Green, 2011). On one hand, inclusion of only RCTs allows the reader to make fairly confident conclusions about the kind of interventions that might work best. This is because it is more likely that all studies maintain similar levels of methodological homogeneity. On the other hand, the lack of studies with a different design (non-RCT) can sometimes be considered a

weakness as the authors potentially might have excluded a substantial number of studies that could lead to different conclusions (Borenstein et al., 2009).

A strengths of the systemic review by Goulding et al. (2010), however, is that it used a formal measure to assess the methodological quality of each study. Quality assessment is an important component of any systemic review and meta-analysis because it enables the reader to make judgments about the quality of individual study (Higgins & Green, 2009).

The review by Goulding et al. (2010) is one of a few attempts to systematically accumulate studies within this field. It also provides initial evidence that psychological interventions might be effective in changing maladaptive illness perceptions in CHD. Furthermore, this review provides important information about the potential ways of improving research within this field, including using measures with satisfactory psychometric properties. Based on the systemic review by Goulding et al. (2010), however, it is difficult to ascertain which component of interventions might work best in changing maladaptive illness perceptions and negative psychosocial outcomes. This is mainly because of the methodological shortcomings of the narrative review which have been described above.

French et al. (2006) overcome some of the limitations identified in Goulding et al. (2010). Primarily, French et al. (2006) adopted a meta-analysis methodology to synthesize and analyse eight studies that examined the relationship between different dimensions of illness perception and attendance at cardiac rehabilitation programmes in participants with MI. Adopting meta-

analysis as a methodology is a substantial improvement in attempts to make sense of findings from individual studies. It allowed French et al. (2006) to derive effect sizes and confidence intervals. This is important because it allowed for identifying patterns across studies and to account for sources of heterogeneity among studies (e.g. sampling error) and measurement errors (Borenstein et al., 2009; Schimdt, 1996).

The meta-analysis by French et al. (2006), however, contains some substantial limitations. Firstly, its scope is limited to one outcome only-attendance at cardiac rehabilitation. Secondly, it focuses on the impact of illness perception on the cardiac rehabilitation attendance rather than considering which aspect of cardiac rehabilitation programmes might contribute to positive changes in maladaptive illness perceptions. Despite this somewhat narrow focus, French et al. (2006) found a small effect size for the relationship between positive illness perceptions and increased attendance at cardiac rehabilitation. Specifically, participants who believed that their illness is controllable and symptomatic and who felt that they understood their condition were more likely to attend the cardiac rehabilitation programme (see Table 1.2 for succinct summary).

While the findings from meta-analysis by French et al. (2006) are important and add substantially to the knowledge about the relationship between illness perception and attendance at cardiac rehabilitation programmes, there are other substantial limitations of this review. Firstly, changes in illness perceptions are likely to be associated with changes to other psychological and social outcomes, such as anxiety and depression (Cooper et al., 1999; O'Rourke &

Hampson, 1999; Platt et al., 2014; Stafford et al., 2009). Yet, French et al.(2006) did not consider a relationship between other psychosocial outcomes and illness perceptions. Secondly, French et al. (2006) focused their meta-analysis on a single group of participants with CHD (those who suffered MI and underwent CABG). It would have been helpful, however, to explore whether the association between changes in illness perceptions and attendance at cardiac rehabilitation programmes is also relevant to other groups of participants with CHD. Thirdly, French et al. (2006) did not investigate whether different types of psychological interventions are linked with changes in illness perception. Cardiac rehabilitation programmes vary across settings (Peterson & Kim, 2011). While some programmes are strongly embedded in psychological theories of maladaptive illness perceptions, others do not explicitly address psychological issues. This diversity, however, was not addressed by French et al. (2006). It also would have been helpful to find out whether psychologically based programmes lead to a larger reduction of maladaptive illness perceptions. Such information might have contributed to learning more about the mechanisms of change in illness perceptions. Finally, the sample of 906 participants is relatively small by meta-analysis standards (Higgins & Green, 2011). It is possible that including a wider category of participants with CHDs might have contributed to an increase in the effect size or alter the findings altogether. Finally, French et al. (2006) did not address the potential impact of socio-demographic variables on their findings.

Despite these shortcomings, the findings of the meta-analysis by French et al. (2006) are important because they provide initial evidence that positive

changes to illness perceptions can improve participants' appropriate utilization of health care. These initial encouraging indications of a direct link between illness perceptions and attendance at cardiac rehabilitation suggest that more high quality meta-analysis needs to be conducted. Findings from the future meta-analyses could strengthen empirical and theoretical links between illness perceptions and psychosocial outcomes, such as depression and anxiety.

Recently, Taylor et al. (2011), however, have attempted to address some of the above-mentioned limitations. Taylor et al. (2011) conducted a systemic review into the socio-demographic and psychological factors that influence attendance at cardiac rehabilitation. By narratively assimilating findings from 18 studies, the authors demonstrated that socio-demographic factors, such as age and gender, play particularly important role in determining attendance at cardiac rehabilitation. Younger participants were thought to be at particular risk of non-attendance. For older participants attendance at cardiac rehabilitation was more complex and related to specific domains of illness perceptions. For example, older individuals who perceived having more control over their illness were more likely to attend cardiac rehabilitation programmes. In general, participants (regardless of gender or age) who perceived their illness as more serious in consequences and appraised their symptoms as more debilitating were more likely to attend cardiac rehabilitation programmes; whereas depression predicted poor adherence to cardiac rehabilitation programmes. While these findings further emphasise the importance of psychological and socio-demographic factors in improving outcomes for individuals with CHD, a review by Taylor et al. (2011) has a

number of important limitations. Firstly, similarly to Goulding et al. (2010), it is only a narrative integration of findings. It is, therefore, subject to similar shortcoming, such as subjectivity in assimilation of findings. Secondly, the review included studies with non-random and random designs which further hampered the generalizability of findings (Borenstein et al., 2009). Thirdly, the review focused only on one outcome (i.e. attendance at cardiac rehabilitation). It would have been helpful, however, to understand how cardiac rehabilitation programmes impact on other psychological outcomes. Finally, the review largely ignored the content of cardiac rehabilitation interventions. This is despite widely recognised variability in the content among cardiac rehabilitation programmes (e.g. Cooper et al., 1999). Nonetheless, the content of intervention is likely to be important in determining recovery outcomes.

In an attempt to understand better the factors that might improve psychological outcomes for people with CHD, Dickens et al. (2013) conducted a systematic review and meta-regression (i.e. assessment of the relationship between one or more study-level moderators) of 64 empirical studies (Borenstein et al., 2009). In particular Dickens et al. (2013) wanted to explore which components of psychological interventions were the most beneficial in improving symptoms of depression in individuals with CHD. Their meta-regression, however, did not look at illness perceptions. This might be considered as a limitation of the study because the reader is restricted in making conclusions about any potential mechanisms of change. The methodological strength of the meta-regression by Dickens et al. (2010) is that it included a fairly large number

of studies (compared to other reviews in this field). The overall sample size (N=17,397), however, was moderate in terms of meta-analysis standards.

Dickens et al. (2013) isolated 11 different components of interventions, including problem solving, exercise, skills training, general discussion, relaxation, relapse prevention, behavioural therapy or cognitive-behavioural therapy. Statistically comparing the effect sizes from different studies enabled Dickens et al. (2013) to conclude that interventions that were cognitive behavioural or included problems solving and relaxation components led to improvements in depression (with a small effect size). These findings, however, need to be interpreted with caution because of a large heterogeneity among studies (e.g. differences in severity of depression, gender/age difference).

In addition, the meta-regression by Dickens et al. (2013) was also narrowly focused on only one outcome (depression) which limits the extent to which the findings could be utilised by clinicians.

Dickens et al. (2013) did well, however, in setting up detailed inclusion and exclusion criteria for studies, particularly in relation to pre-determining different components of interventions. This allowed the authors to separate psychological components from non-psychological ones. Overall, although the meta-regression conducted by Dickens et al. (2013) did not take into consideration interventions designed to change illness perceptions, the study provided relatively encouraging evidence that cardiac treatments with psychological components may improve psychosocial outcomes (specifically low mood) in people with CHD over interventions that do not contain psychological elements. It seems reasonable,

therefore, to explore whether different components of interventions could play different role in changing illness perceptions.

1.8. Overall Conclusions

The above summary and critical evaluation of individual studies, systemic reviews and meta-analyses demonstrates that there have been some considerable efforts made to systematically gather and evaluate the studies investigating the relationship between illness representations and psychosocial outcomes. The findings from these individual studies, however, provide a mixed, inconsistent and sometimes a contradictory picture of how patients' illness representations link with psychosocial outcomes, such as anxiety or depression. On the whole, individual studies point towards an importance of all dimensions of illness representations in emergence and maintenance of symptoms of anxiety and depression. For instance, single studies showed that participants who feel more in control of the symptoms associated with CHD and who appraise these symptoms as less negative and less severe in consequences are less likely to report feelings of anxiety and depression. Previous individual studies also demonstrated that illness perceptions are not rigid and can change over time and/or course of an illness. This dynamic nature of illness representations creates a possibility that they can be changed through interventions. Single studies conducted to date, however, have so far provided a complex and confusing picture of how the interventions might work in changing maladaptive illness representations. Previous systemic reviews and meta-analysis of psychological interventions

designed to target maladaptive illness representations have a number of methodological and theoretical limitations. Some reviews restricted their focus to one outcome (e.g. depression or attendance at cardiac rehabilitation programmes) and other reviews were limited to a narrative summary of the findings. There is, however, a sufficient number of data on a wider range of outcomes (e.g. illness representations, depression and anxiety) that could be systematically accumulated and analysed using quantitative methodology.

It is also important to highlight that no previous meta-analysis could be identified that examined whether interventions with psychological components could be more effective in changing illness representations. There is also a clear lack of meta-analyses that systematically investigated whether changes to illness perceptions are linked to psychosocial outcomes, such as depression and anxiety. Finally, there have been very limited attempts in systematically exploring the factors that may affect the efficacy of psychological interventions targeted at changing illness perceptions.

Taking into consideration the methodological and theoretical limitations identified in the above-mentioned literature review, the aim of the present investigation is to examine whether interventions with psychological components are more effective in changing illness perceptions than interventions that do not have psychological components. Another aim is to explore how illness perceptions relate to changes in psychosocial outcomes, such as depression and anxiety. The findings have a potential to broaden scholars' and clinicians' understanding of the interplay between representations of illness and psychosocial

outcomes. Improved understanding of this complex relationship can facilitate development of more targeted and holistic interventions which would consider medical and psychological needs of individuals with CHD.

1.9. Research Questions

1.9.1. Research question one.

Are interventions containing clearly identifiable psychological components more efficacious in changing illness perceptions than standard cardiac interventions without clearly identifiable psychological components?

1.9.2. Research question two.

Do interventions targeting illness perceptions contribute to positive changes in symptoms of depression and anxiety?

1.9.3. Research question three.

Are type of illness (chronic vs acute) and age linked with the efficacy of interventions designed to change illness perceptions?

2. Method

2.1. Chapter Overview

This chapter starts with the description of a general approach to data collection, management and analysis and a brief outline of guidelines that were applied in designing and execution of the present meta-analysis. Next, the details of studies' inclusion and exclusion criteria are described. This is followed by providing information about the process of searching and selection of relevant literature. The consecutive sections provide information about how the data has been extracted and coded to fit with the goals of the current meta-analysis. The strategies used in calculating effect sizes and analysing data are described next. Finally, the details of an approach used to assess heterogeneity and quality pertinent to any meta-analysis are described.

2.2. General Methodological Approach

The present meta-analysis was guided by the procedures outlined by Borenstein et al. (2009), the Cochrane Collaboration and the PRISMA guidelines (Higgins & Green, 2011; Moher et al., 2009).

The Cochrane Collaboration criteria outline a protocol for preparing and conducting meta-analyses of trials of healthcare interventions (Higgins &Green, 2011). This protocol sets out strategies on how to identify, select and summarise reviewed studies and how to analyse quantitative information. These stringent criteria help the researchers to make scientifically robust conclusions about the efficacy of specific treatment(s) (Higgins & Green, 2011).

2.3. Studies' Inclusion/Exclusion Criteria

In the present meta-analysis studies were assessed as relevant according to pre-determined criteria specified by the Cochrane Collaboration Criteria (Higgins & Green, 2011).

These criteria included:

1) Type of participants

The present meta-analysis included studies conducted on adults of any age and gender who were diagnosed with at least one of the following CHD conditions: stable and unstable angina, myocardial infarction (MI), Acute Coronary Syndrome (ACS), Coronary Artery Disease (CAD) and conditions that require revascularisation procedures: Coronary Artery Bypasses Graft (CABG) and Percutaneous Coronary Interventions (PCI).

2) Type of interventions and comparisons

Studies were included in the final analysis if they tested interventions designed to change maladaptive illness perceptions in coronary heart diseases (CHD). Although the author of the present meta-analysis frequently refers to maladaptive illness perceptions, other closely related terms were also considered. These terms included: illness attributions, beliefs and/or misconceptions.

Cardiac interventions are multifaceted, consisting of psychological and non-psychological components, such as informal discussions, cardiology medical reviews, psycho-education, techniques based on specific therapeutic approaches, and/or telephone follow-ups. It was, therefore, important that the present meta-analysis reflected the multidimensional nature of cardiac interventions.

Interventions under review in the present meta-analysis consisted of the following components: a) psycho-education only, b) psycho-education combined with counselling techniques, c) psycho-education combined with cognitive-behavioural and/or motivational interviewing techniques, d) interaction or non-psycho-educational contact with medical health professional (e.g. cardiologist or cardiac CNS) and/or e) telephone follow up.

Only studies with randomised control design were included because randomised controlled trials (RCTs) constrained the methodological heterogeneity among studies. Combining studies with different types of designs (e.g. quasi-experimental and RCTs) would further increase heterogeneity among studies. This in turn would have an adverse impact on a quality of conclusions and generalizability of findings (Borenstein et al., 2009).

In terms of other specific study design characteristics, the study was included if it met the following criteria: 1) trials with or without comparison (medical and non-medical) treatments, and 2) cross-sectional and longitudinal designs. All included studies had at least one follow-up time point which ranged from 1 to 12 months. In order to reduce heterogeneity amongst studies, the data closest to the most frequent time point was selected (Higgins & Green, 2011). In the present meta-analysis this time point was at 3 months.

Studies were not excluded based on their mode and frequency of delivery, treatment intensity and a type of the professional delivering the intervention. These methodological characteristics, however, were reported and discussed in the context of obtained effect sizes.

3) Types of outcomes

In the present meta-analysis any study was included that used a standardised or semi-standardized assessment measure of a primary outcome and at least one of the secondary outcomes. The primary outcome was a measure of illness perception/beliefs/attributions. The secondary outcomes included measures of severity of anxiety and depressive symptoms.

2.4. Data Sources

Data were extracted from the electronic databases, including PsycInfo, Medline, Web of Science, Scopus, Cumulative Index to Nursing and Allied Health Literature (CINAHL), Cochrane's library and EMBASE. The searches covered the period of time from 1970 to the fourth week of August 2014. An additional search was conducted from September 2014 to November 2015. This timeframe was guided by previous systematic reviews and meta-analysis in the field of interventions designed to change illness perceptions in people with CHD. Additional hand-search was conducted of references lists of articles meeting inclusion criteria and previous relevant reviews (Lipsey &Wilson, 2001). The hand search included reference lists of previous systemic reviews and meta-analysis as well as key previous empirical studies (e.g. Dusseldorp et al., 1996; French et al., 2006; Goulding et al., 2010). Relevant journals were also searched. These journals included: Journal of Advanced Nursing, Journal of Psychosomatic Research, and Psychosomatic Medicine. Finally, when necessary and appropriate

(e.g. when additional information was needed) contact was made with key authors in the field. The details of contacts can be found in Appendix C (see Table A2.1). Due to limited resources only studies written in English and Polish (as this is the mother tongue of the author of the current meta-analysis) were included in the final analysis.

A breakdown of selected and excluded studies as well as reasons for exclusions can be found in a PRISMA flowchart in the Result section (see Figure 3.1. in the Result section).

2.5. Search, Screening and Selecting of Relevant Literature

Systematic search strategy was guided by core terms (see Table 2.1). These core pre-established terms helped in identifying appropriate studies pertinent to the research questions and kept the meta-analysis focused. These search terms were chosen through initial checks of previous studies and reviews (e.g. Foxwell et al., 2013). Final results of literature search were stored in bibliographical software, the Mendeley Reference Manager version 1.12.2 (2014).

Electronic data sources were searched independently using OvidSP interface. The first step of search involved inputting pre-established core search terms with or without Boolean operators ('AND' and 'OR') and 'wildcard' symbol (*). The search terms were combined to maximise retrieval of records and to optimise unique search outcomes. A searching strategy was consulted with a university librarian specialising in systematic literature search.

The next step included screening and selection of relevant studies guided by inclusion and exclusion criteria (Boland, Cherry & Dickson, 2014). The screening and selection of studies involved:

1) Identification and deletion of any duplicate references within and across databases
2) Screening all titles and abstracts applying inclusion and exclusion criteria
3) Obtaining full text of selected research papers

Table 2.1.

Search strategy used to search PsychInfo, Medline, Web of Science, Scopus, Cumulative Index to Nursing and Allied Health Literature (CINAHL), Cochrane's library and EMBASE.

Line	Search term	Search criteria
1	Coronary heart disease OR CHD OR Coronary artery disease or CAD OR Angina OR Myocardial infraction or M OR Heart attack OR Angioplasty OR Percutaneous coronary intervention OR PCI OR Coronary artery bypass graft OR CABG OR Acute coronary syndrome OR ACS (AND)	Title, Abstract
2	Illness percept* OR Illness misconception OR Illness cognit* OR Illness expect* OR Maladaptive thinking (AND)	Abstract
3	Psych* intervention OR Psychotherapy OR Treatment OR Cardiac rehabilitation OR Rehabilitation OR Therapy OR Cognitive behavioural therapy OR CBT OR Motivational Interviewing OR MI OR Behavioural therapy OR Cardiac programme OR Cardiac management (AND)	Title, Abstract
4	Depress* OR Anxi* OR Mood OR (health related) Quality of life OR QoL OR Emotional well-being OR Behav* (AND)	Abstract
5	Randomised controlled trial OR RCT OR Controlled trail OR Random allocation OR Clinical trial OR Double blind method (AND)	Title, Abstract
6	1 OR 4	Title, Abstract
7	1 AND 2	Title, Abstract
8	2 AND 4	Title, Abstract
9	1 AND 3 AND 4	Title, Abstract
10	1 AND 2 AND 3	Title, Abstract
11	10 AND 5	Title, Abstract
12	9 AND 5	Title, Abstract

2.6. Data Extraction

All relevant information from each study were extracted and recorded in the SPSS data file. Each study was assigned a numerical ID. The information extracted included: study number, authors, design details, main characteristics of participants, CHD classification, intervention components, illness perception measure, secondary outcomes measures, key findings and quality ratings. A succinct summary of key characteristics of each study was also collated in a narrative table, which can be found in the result section (see Table 3.1 in Results section).

2.7. Data Coding

All extracted studies' characteristics were coded to ensure that necessary information was captured. Information was coded using a coding manual and coding form (see Appendix D and E). The coding form and manual were designed specifically for the purpose of the present meta-analysis and were based on the guidelines provided by Lipsey and Wilson (2001). Additionally, a design of the coding form and manual was informed by the CONSORT 2010 checklist of information to include when reporting randomised trials, and the Cochrane Collaboration guidelines (Higgins & Green, 2011; Schulz, Altman, & Moher, 2010). Coded data was transferred into the SPSS data file. The study characteristics were coded according to three categories: substantive, methodological and extrinsic variables (Sanchez-Meca & Marin–Martinez, 2010).

The coding form and manual contained also additional items aiding quality assessment.

The substantive variables relate to main research questions. In the present meta-analysis substantive variables included: 1) authors, 2) year of publication, 3) type of publication (e.g. journal article, book chapter thesis or doctoral dissertation) 4) variable name, variable measures and scores for primary and secondary outcomes (e.g. illness perceptions, anxiety, depression), 5) the types of interventions (e.g. psycho-education only, psycho-education combined with counselling techniques, psycho-education combined with the CBT-based and/or motivational interviewing techniques, interactive/non-psycho-educational contact with medical professional, and/or telephone contact), 6) participants' characteristics (e.g. sample size, age, gender, and ethnicity), and 7) the type of illness (chronic vs acute).

The methodological variables are linked to a study design. The methodological variables included in the present meta-analysis were: 1) study design (e.g. longitudinal/RCT) and 2) a nature of comparison group (e.g. no treatment, delayed treatment and alternative treatment). In addition, raw data for study findings and each outcome variable was recorded (e.g. means, standard deviations, confidence intervals, *t* value and/or *p* value). When not provided in a paper a total mean for variable (i.e. age) was calculated using the following formula:

$$M = \frac{(N1 \times M1) + (N2 \times M2)}{N1 + N2}$$

Where N1 sample size group 1;
N2 Sample size group 2;

The extrinsic variables are those that influence the results but they do not directly relate to either research questions or methodological aspects of the studies. Extrinsic variables in the present meta-analysis included: 1) mode of delivery (e.g. face to face, one to one, telephone), 2) duration of intervention (e.g. single consultation, multiple discreet sessions, and/or continuous programme), 3) type of health professional delivering the intervention (e.g. medical and/or mental health professional), 4) theoretical framework of the intervention, and 5) setting of the intervention (e.g. inpatient/outpatient).

The quality assessment items included: presentation of participant flowchart, information about randomisation procedure, blinding and allocation concealment, validated outcome measures, sample size calculation/consideration.

2.8. Calculating and Interpreting Effect Size

The effect size is a statistic denoting a magnitude and a direction of a difference between two groups or variables (Borenstein et al., 2009). Effect size is obtained by subtracting two group means than dividing it by standard deviation or pooled standard deviation. The following formula represents how effect size is calculated:

$$\overline{ES} = \frac{\overline{M}_e - \overline{M}_c}{SD_{pooled}}.$$

Effect size can be expressed in a variety of ways, such as difference between means (e.g. raw or standardized mean difference), correlation coefficient or as a percentage (Card & Casper, 2013). In the present meta-analysis conventional rules

of thumb were applied to interpret the magnitude of the effect size (small $d \leq .2$; medium $d = .50$; large $d \geq .80$) (Lipsey &Wilson, 2001).

In meta-analysis the effect sizes from different studies (and different samples) are pooled together to estimate the overall effect size for a specific population (Borenstein et al., 2009). In order to assimilate effect sizes from different studies that use different measures and samples, effect sizes need to be comparable (Card & Casper, 2013). In the present meta-analysis the effect sizes from different studies were pooled together using standardized mean difference (SMD) (i.e. *Hedges's g*). SMD is particularly common statistical metric to express effect size in meta-analysis of controlled trails (Durlak, 209). *Hedges's g* was chosen over an alternative (*Cohen's d*), because *Hedges' g* is considered to be more accurate reflection of the relationship between variables. This is because it adjusts for potential positive bias, such as small sample size (particularly when N < 20) (Card & Casper, 2013). *Hedges's g* is calculated based on difference between the means of two groups (e.g. intervention and control) (Card & Casper, 2013). A positive value of *Hedges's g* indicates that the intervention group obtained higher mean and the negative value depicts a higher score for the control group.

Formula for adjusted *Hedges'g* is:

$$g_{adjusted} = 1 - \left(\frac{3}{4df-1}\right).$$

In the studies that did not report the effect sizes, the commonly reported statistics were used to calculate *Hedges's g* (Card & Casper, 2013). An approach

described by Card and Casper (2013) was used to calculate these effect sizes. When means and standard deviations were not reported, the effect sizes were calculated from *t* and *F* statistics. Where necessary the standard error was converted into the standard deviation using the following formula:

$$SD = SE \times \sqrt{N}$$

2.9. Data Analysis

Data were stored and analysed using the Comprehensive Meta-Analysis Software (version 3.3.07; November 2014) and SPSS version 22 for Windows (SPSS, Chicago, IL, USA).

Effect sizes were grouped according to primary (illness perception) and secondary outcomes (depression and anxiety) that were selected to assess the efficacy of the interventions.

All studies reported one measure of each outcome. When the study reported data for subscales of the outcome (e.g. subscales scores corresponding to the dimensions of illness perceptions) a procedure suggested by Borenstein et al. (2009) was used to combine the scores. This approach was chosen because it is generally assumed that subscale scores are not independent of each other (Borenstein et al., 2009). It is, therefore, more appropriate to combine the subscales scores than to treat each subscale score as an independent entity. The derived *combined* score is treated as a unit of analysis in the meta-analysis (Borenstein et al., 2009). The *combined* effect size is derived firstly by calculating each subscales' effect size and its variance. These subscales' effect sizes are

finally used to compute a *combined* effect size (in the current meta-analysis marked as *combined* in the forest plots) (Borenstein et al., 2009).

Additionally, standard errors, 95% confidence intervals (CIs) range and p values were calculated for each effect size and the total effect sizes. Weighted effect sizes were calculated based on the inverse of the variance method (w_i) (Borenstein et al., 2009). Small studies have larger standard errors and therefore they have a lower weighting than large studies.

The effect sizes were calculated within a random effects model (Borenstein et al., 2009). The random effect model assumes that treatment effects are randomly distributed across populations. This model was chosen because it allows for greater between- and within-study variability (Borenstein et al., 2009).

The effect sizes and their CIs were reported in quantitative format and graphical representations (i.e. forest plots). Graphical distribution of effect sizes and CIs was inspected visually for the presence of outliers.

2.9.1. Heterogeneity assessment. The variability of the effect sizes was assessed employing a heterogeneity (Q) statistics (Card & Casper, 2013). The heterogeneity value allows the researcher to assess the variability across effect sizes and to determine whether the variability among studies can be explained by sampling error (Borenstein et al., 2009). A significant Q value ($p < .05$) indicates that a distribution of effect sizes is significantly heterogeneous and that the researcher can reject the null hypothesis of homogeneity (Borenstein et al., 2009).

The I^2 index was also calculated because the present meta-analysis was based on a relatively modest number of studies (and hence relatively low power) (N = 11) (Borenstein et al., 2009). I^2 index provides information about the *extent* of variability in a distribution of effect sizes that might be due to heterogeneity (Borenstein et al., 2009). The value of I^2 indicates how much of the observed dispersion between effect sizes of studies is likely to be associated with real difference in the effect sizes. I^2 ranges from 0 to 100%, with values around 25%, 50% and 75% denote small, medium and large heterogeneity, respectively (Borenstein et al., 2009). The formula for I^2 is as follows:

$$I^2 = \frac{Q-(k-1)}{Q} \times 100.$$

Finally, the *tau²* statistic was used to assess the between-studies variance of effect sizes (Borenstein et al., 2009). This metric of heterogeneity was an important estimate of heterogeneity because the data analysis in the current meta-analysis was conducted within a random effect model (Borenstein et al., 2009). *Tau²* value of .000 indicates no between-studies variance of effect sizes and values *tau²* >*.000* indicate presence of between-studies variance (Borenstein et al., 2009). The significance level for *tau²* is the same as for the *Q* statistics, with $p < .05$ indicating significant amount of between-study variance (Borenstein et al., 2009).

2.9.2. Moderator analysis. The impact of moderating variables on the effect sizes was also assessed within the random effect model. The effect sizes were grouped together into categories based on moderating variables. There were two moderating variables: the intervention strategy (studies which contained

clearly identifiable psychological component vs studies that did not contain clearly identifiable psychological component) and the type of illness (chronic vs acute) (for more information see Appendices D and E). The moderating variable of the intervention strategy and the type of illness was based on coding of studies (item: *type of intervention strategy included*) (see Appendices D and E). Studies classified as studies with clearly identifiable psychological component had to be coded as containing element of *psycho-education combined with cognitive-behavioural (CBT) and/or motivational interviewing techniques* (see Appendix D and E). For each moderating analysis three summary effects were calculated: a total effect size and effect sizes for two subgroups.

Within the moderating analysis the total heterogeneity of effect sizes was separated into the heterogeneity of the distribution among the effect size within a category of moderating variables (Q_w) and the heterogeneity between the category of moderators (Q_b) (Borenstein et al., 2009). Similarly to the heterogeneity assessment in the main analysis the I^2 and tau^2 metrics were calculated to depict the amount of heterogeneity (Borenstein et al., 2009).

2.10. Quality Assessment

Quality assessment in the current meta-analysis involved assessment of the quality of individual studies and the quality assessment across studies (within the meta-analysis).

2.10.1. Quality assessment on the individual study level. In the current meta-analysis the quality of each study was assessed using the RCT of Psychotherapy Rating Scale (RCT-PQRS) (Kocsis et al., 2010) (see Appendix F). This is a 25-item measure originally developed to assess healthcare interventions. This quality assessment tool has been chosen because it allows the researcher to rate the quality of all aspects of randomised controlled studies, focusing on internal and external validity (Kocsis et al., 2010). The RCT-PQRS has been also successfully used in previous meta-analysis (e.g. Thoma et al., 2012). Finally, the RCT-PQRS is relatively user friendly. The RCT-PQRS has very good psychometric properties, with inter-rater reliability of .79, the internal consistency (Cronbach α) of .88 and validity of .47 (Kocsis et al., 2010). The quality of each study was rated by two raters, the author and the supervisor. The ratings were done independently of each other and any disagreements were discussed. The IRR coefficient (Cronbach's alpha) was $r = .84$, indicating high levels of agreements between raters.

2.10.2. Publication bias. An integrative method of evaluating the quality of the meta-analysis is an assessment of publication bias. Publication bias expresses an idea that the final sample of the meta-analysis might have come from the biased publication and selection processes (Borenstein et al., 2009). This is because studies with large effect sizes and significant results are more likely to get published than studies with non-significant results and/or small sample sizes (Borenstein et al., 2009). If present the publication bias is likely to be carried over

to the meta-analysis. Publication bias is also affected by other factors, such as language bias (i.e. including only studies published in English), available bias (i.e. studies published in journal with easy and/or free access are more likely to be selected) and/or citation bias (i.e when studies with significant results are more likely to be cited) (Boresetin et al., 2009). The purpose of statistically assessing the potential presence and the extent of publication bias in meta-analysis is to detect whether missing studies (i.e. studies omitted from the meta-analysis) are systematically different from the included studies (Borestein et al., 2009). The probability of the bias increases with smaller sample sizes.

In the current meta-analysis, a number of steps were undertaken to assess the presence/absence of publication bias. Firstly, a funnel pot was derived and visually inspected for each outcome variable (illness perception, depression and anxiety). A funnel plot is a scatterplot of effect sizes from included studies against standard error of the effect size (Sterne & Harbord, 2004). Funnel plots are inspected for asymmetries in the distributions of the effect sizes. Any asymmetries indicate the presence of publication bias (Sterne et al., 2011). Typically, large studies can be located on the top of the plot around the mean of the effect size. Studies with small effect sizes can be found towards the bottom of the graph and are more likely to be spread broadly across (due to increase chance of larger standard error) (Borenstein et al., 2009). If the direction of the plot is towards the right (i.e. more effect sizes appear towards the right of the graph) then we could expect a gap on the bottom left, indicating that small studies with non-significant results are likely to be missing (Borestein et al., 2009).

The second step in assessing the publication bias in the current meta-analysis was to determine whether the obtained effect size is entirely an artefact of the publication bias (Borestein et al., 2009). In order to do this Rosenthal's method of calculating *fail-safe N* was used. The *fail-safe N* allows the researcher to determine how many missing studies would need to be included in the meta-analysis before the obtained effect size was statistically non-significant. The higher the number of missing studies in *fail-safe N* calculation, the lower the probability of the bias (Borenstein et al., 2009). The fail-safe N was calculated for all the effect sizes for all three outcomes in the current meta-analysis.

The third step in assessing the publication bias was to determine the impact of the bias and estimate what the effect would have been if the bias was absent. This was done using the Duval and Tweedie's *trim and fill* method. The *trim and fill* method removes the most extreme small studies from the positive side of the plot and re-calculates the effect size at each point until the plot becomes more symmetrical around the new effect size (Borenstein et al., 2009). The trim and fill was applied to affect sizes of all three outcomes in the current meta-analysis.

3. Results

3.1. Overview of the Chapter

This chapter presents the results from the analyses conducted on extracted data from studies investigating the efficacy of interventions design to change maladaptive illness perceptions in CHD. The PRISMA flowchart in Figure 3.1 shows the numbers of studies entered into the meta-analysis. This is followed by a more detailed discussion of the characteristics of the studies that met the inclusion criteria. This is supplemented by a narrative summary in Table 3.1.The descriptive section considers important features of the studies entered into the meta-analysis, such as characteristics of participants, characteristics of interventions (and control treatments) and presentation of measures of primary and secondary outcomes. Next, the statistical analysis of effect sizes for each research question is presented. Statistical analysis of effect sizes for each research question includes presentation and narrative description of effect sizes and their confidence intervals, heterogeneity statistics (Q, I^2 and tau^2 metrics) and forest plots of effect sizes for each outcome. The association between age and effect sizes was tested using correlation with non-parametric tests. The final section of the results chapter involves assessment for publication bias. This assessment includes narrative description of funnel plots and statistical test of *fail-safe N* and Duval and Tweedie's *trim and fill* (Borenstein et al., 2009).

3.2. Description of Included Studies

Figure 3.1 represents the PRISMA flowchart of the process and results from the search and selection of studies in the current meta-analysis. Studies published between 2002 and 2014 met the criteria for inclusion in the current meta-analysis. Table 3.1 shows main characteristics of all studies entered into the meta-analysis. All studies were published in English and in peer-reviewed journals. Five studies were conducted in the UK (studies 02, 03, 06, 07, 10), two studies were performed in the USA (study 08 and 09) and New Zealand, respectively (studies 01 and 11), one study was conducted in Canada (study 05) and one study came from China (study 04).

In this meta-analysis, 11 studies were included in total and the total sample size was 5, 267 participants with CHD, ranging from 65 (study 11) to 2905 (study 08).

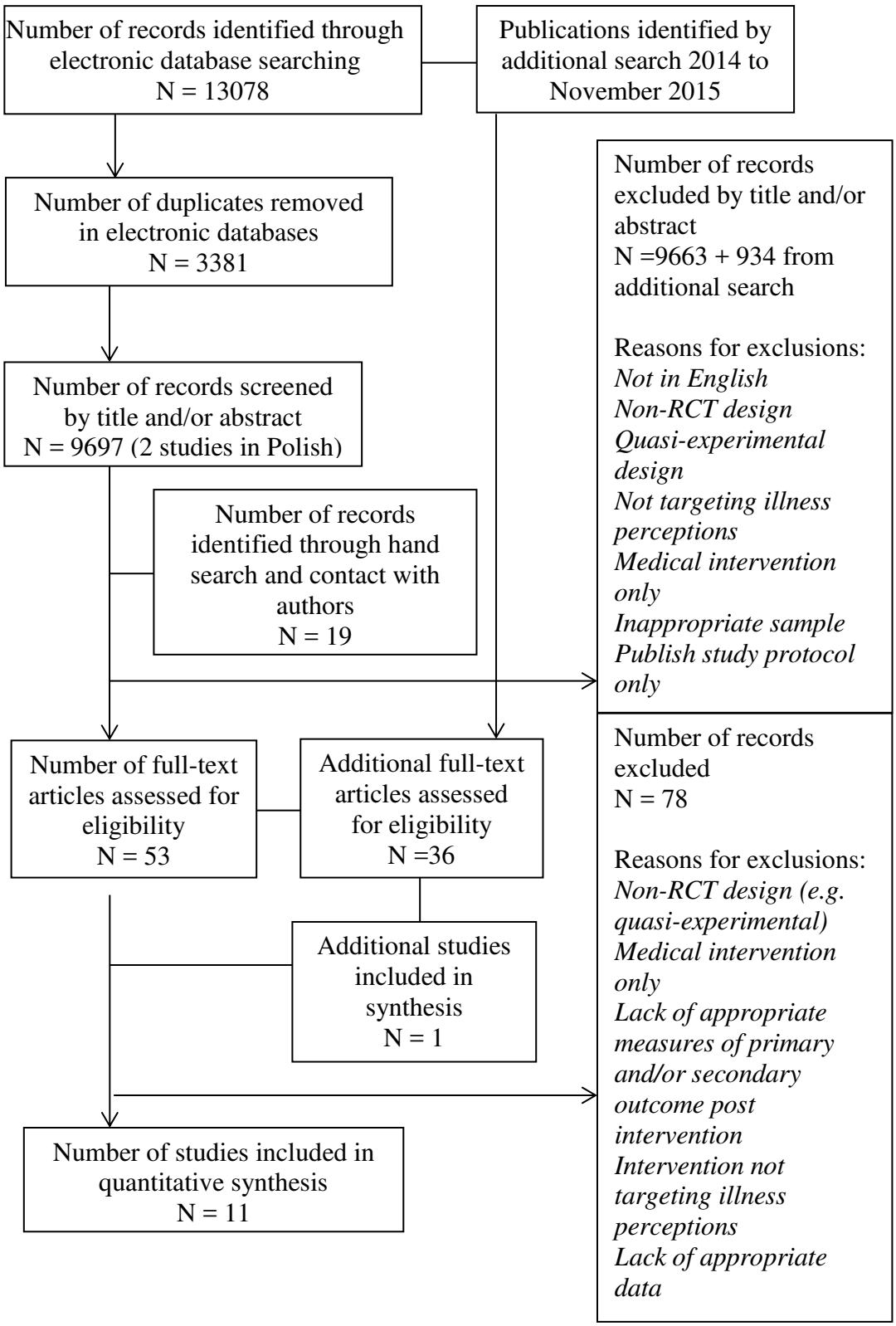

*Figure 3.1.*The PRISMA Study Selection Flowchart.
Adopted from Moher, D., Liberati, Al., Tetzalaff, J., Altman, D., & The PRISMA Group (2009).

Table 3.1.

Characteristics of RCT Studies Designed to Change Illness Perceptions in CHD Included in the Current Meta-analysis (in chronological order).

Study ID, Author (publication year), country/ Quality score*	**Sample characteristics: size (intervention/ control), mean age, gender (f/m)**	**CHD Diagnosis, Type of Illness (chronic vs acute)**	**Intervention as Described (types of strategies included in treatment); Type of Intervention Strategy**	**Control Treatment**	**Primary Outcome (measure used)**	**Secondary Outcomes (measure used)**	**Results (brief summary)**
Study 01, Pfaefflli et al. (2015), New Zealand/ 30	N = 123 (61/62) M = 59.5 23F/100M	Broad category of CHD; acute	Text4Heart: text messages and web-based intervention addressing risk factors, promoting life style changes; No psychological component	Usual care: inpatient rehabilitation, encouragement to attend cardiac rehabilitation programme	Illness perception (Brief IPQ)	Depression (HADS) Anxiety (HADS)	No changes have been reported in illness perceptions and depression post intervention. The intervention group reported more anxiety symptoms post intervention compared to control group.
Study 02, Barley et al. (2014) UK/ 36	N = 81 (41/40), M = 65, 29F/52M	Broad category of CHD; acute	Psycho-education combined with counselling techniques; Contains psychological component	Outpatient follow up by GP/Practice Nurse + signposting	Illness Perception (Brief IPQ)	Depression (HADS) Anxiety (HADS)	Illness perception: greater improvements in intervention group; Depression: both groups improved scores without group differences; Anxiety: reduced odds of depression

Study 03, O'Brien et al. (2014) UK/ 36	N = 1136 (585/551); M = 63.58; 316F/820M	ACS; acute	Psycho-education combined with cognitive behavioural therapy or motivational interviewing Contains psychological component	In patient standard education	ACS Response Index: three dimensions (knowledge, attitude & beliefs)	None	Greater changes in knowledge, attitudes and beliefs in the intervention group, the speed of change in the intervention group was also faster
Study 04, Yan et al. (2013) China/ 39	N = 124 (51/51) M = 64.27 25F/77M	MI; chronic	Psych-education combined with counselling techniques; No psychological component	Routine outpatient follow up	Illness perceptions Chinese version of IPQ	None	Patients in intervention group had significantly modified their illness perceptions about personal and treatment control, timeline acute/chronic and identity of MI
Study 05, Cossette et al. (2012) Canada 35	N = 242 (121/121) M = 59.40 35F/207M	ACS; acute	Psycho-education combined with counselling techniques; No Psychological Component	Standard Care: medical outpatient follow up	Illness perceptions (Revised IPQ)	Anxiety (State-Trait Anxiety Inventory)	Significant group difference in personal control dimension of illness perception, other dimensions non-significant; No significant group differences in anxiety or life style factors

Study 06, Furze et al. (2012) UK/ 37	N = 142 (70/72) M = 64.41 67F/75M	Angina; chronic	Angina Plan: psycho-education combined with cognitive behavioural therapy or motivational interviewing techniques; Contains Psychological Component	Standard care: discussion of risk factors, advice giving and signposting	Angina-related misconceptions (York Angina Beliefs Questionnaire)	Anxiety (HADS) Depression (HADS)	Intervention group had significance modified their beliefs about angina, reported less symptoms of anxiety and depression
Study 07, Furze et al. (2009), UK/43	N = 204 (100/104) M = 64.78 40F/164M	Patients waiting for CABG	Heart Op Programme: psycho-education combined with cognitive behavioural therapy or motivational interviewing techniques; Contains psychological component	Nurse led education and counselling intervention	Cardiac Beliefs (York Cardiac Beliefs Questionnaire)	Depression (Cardiac Depression Scale) Anxiety (State Trait Anxiety Inventory)	Cardiac misperceptions and depression were significantly reduced after the intervention. The anxiety in intervention group did not improve significantly

Study 08, McKinley et al. (2009) USA/ 32	N = 2905 (1480/1425) M = 67.20 1129F/1776M	ACS; acute	Intervention focused around three components: informational, emotional and social. Psycho-education combined with cognitive behavioural therapy or motivational interviewing; Contains psychological component	Standard Care: medical follow ups and signposting	Three domains of ACS misconceptions: knowledge, attitudes, and beliefs (the ACS Response Index)	None	Intervention group showed significant improvement in all domains of ACS misconceptions
Study 09, Tullmann et al. (2007) USA/ 21	N = 115 (58/57) M = 73.80 60F/55M	Angina, MI & angioplasty	Psycho-education combined with counselling techniques; No Psychological component	Standard Care: detailed information not provided	Response Questionnaire with 3 domains of Illness perceptions: knowledge, attitudes and beliefs	Anxiety (Brief Symptom Inventory Anxiety Subscale)	Intervention group obtained significantly higher scores for illness knowledge and beliefs but not for attitudes. There was no significant changes anxiety scores.

Study 10, Lewin et al (2002) UK/ 31	N = 130 (63/67) M = 67.20 45F/85M	Angina; acute	The Angina Plan: psycho-education combined with cognitive behavioural therapy or motivational interviewing techniques; Contains Psychological Component	Nurse-led education session with additional booklet	Angina Beliefs (York Angina Beliefs Questionnaire)	Anxiety (HADS) Depression (HADS)	Patients in intervention group demonstrated significantly better improvements in scores on anxiety, depression; Patients in intervention group also reported less maladaptive illness perceptions post intervention
Study 11, Petrie et al. (2002) New Zealand/ 28	N = 65 (31/34) M = 55.61 18F/47M	MI; acute	Psycho-education combined with cognitive behavioural and/or motivational intervention techniques; Contains psychological component	Standard Care: medical follow up and standard MI education	Five domains of illness perceptions: consequence, timeline, control/cure, identity and distress (IPQ)	None	Patients in intervention significantly modified their illness perceptions in all domains of illness perception

N-total number of participants; CHD-Coronary Herat Disease; CABG-Coronary Artery Bypass Grafting; ACS- Acute Coronary Syndrome; MI-Myocardial Infraction; HADS- Hospital Anxiety and Depression Scale; IPQ-Illness Perception Questionnaire; *quality rating based on the RCT-Psychotherapy Quality Rating Scale (higher scores indicated higher quality).

3.2.1. Participants. Five studies included patients with angina and MI (study 04, 06, 09, 10, 11). One study included patients waiting for CABG (study 07), two studies included patients with broad definition of CHD (study 01 and 02) and three studies included patients with ACS (study 03, 05, 08). Sixty six percent of the total sample were males (N = 3457). With an exception of one study (study 09) all of the studies had a higher percentage of males. The mean age of participants was 64.06 years (SD = 4.79), ranging from 55.61 to 73.80 (see Table 3.1). Five studies did not report ethnicity at all (study 03, 05, 06, 10 and 11). In the remaining studies the ethnicity was defined and categorised differently, rendering it impossible to accumulate the findings together.

3.2.2. Characteristics of interventions. All interventions were designed to change patients' maladaptive illness perceptions around CHD and were based on the Leventhal's self-regulation model of illness behaviour (Diefenbach & Leventhal, 1996). The control condition involved standard usual care (e.g. cardiac rehabilitation) in all of the studies. Interventions in six studies contained clearly identifiable psychological component, such as cognitive-behavioural therapy and/or motivational interviewing (studies 02, 03, 06, 07, 10 and 11).

The setting of intervention delivery varied across studies. Nine studies were delivered as outpatient interventions (studies 01, 02, 04, 07-11) and three studies involved both inpatient and outpatient sessions (study 03, 05 and 06). The mode of delivery was relatively consistent across studies. All studies, except study 01, included face to face and individual sessions.

Treatment duration varied across studies. Three studies included a single contact session (study 03, 08 and 09), four studies involved multiple discrete sessions (study 02, 04, 05 and 11), and four studies were structured as continuous programmes (study 01, 06, 07, and 10). Intervention was delivered by non-mental health professional (e.g. nurse or cardiologist) in 10 studies. Only one study explicitly recognised involvement of mental health professional (health psychologist) in the treatment delivery (study 11). For more information about the distribution of effect sizes across different characteristics of the interventions please see Table 3.2.

Table 3.2.

Effect sizes for Illness Perceptions Grouped by Intervention Characteristics.

Characteristics of interventions		**Study ID**										
		01	02	03	04	05	06	07	08	09	10	11
Setting of delivery	outpatient	-.117	-.187		.484			.466	.219	.651	.243	.249
	Inpatient and outpatient			.192		.011	.437					
Mode of delivery	Face to face and individual		-.187	.192	.484	.011	.437	.466	.219	.651	.243	.249
	Text message, web-based	-.117										
Treatment duration	Single session			.192					.219	.651		
	Multiple discrete session		-.187		.484	.011						.249
	Continuous programme	-.117					.437	.466			.243	

*Effect Size expressed as *Hedges's g*.
01 - Pfaeffli et al.(2015)
02 - Barley et al.(2014)
03 - O'Brien et al. (2014)
04 - Yan et al. (2013)
05 - Cossette et al. (2012)
06 - Furze et al. (2012)
07 - Furze et al (2009)
08 - McKinley et al. (2009)
09 - Tullmann et al. (2007)
10 - Lewin et al. (2002)
11 - Petrie et al. (2002)

3.2.3. Primary Outcome

All studies tested primary outcome of illness perceptions using measures with satisfactory psychometric properties. The measures were also well matched to the type of CHD and sample characteristics, enhancing internal validity of studies. Five studies used different versions of the Illness Perception Questionnaire, such as Brief IPQ (Study 01and 02), Chinese version of IPQ (study 04), Revised-IPQ (study 05) and a full version of IPQ (study 11) (Broadbent, Petrie, Main & Weinman, 2006; Moss-Morris, et al., 2002; Weinman, Petrie, Moss-Morris, & Horne, 1996; Yan et al., 2013). Two studies used ACS Response Index (study 03 and 08) (Riegel et al., 2007). Two studies used York Angina Beliefs Questionnaire (study 06 and 10) and one study applied York Cardiac Beliefs Questionnaire (study 07) (Furze et al., 2003). One study used Response Questionnaire (study 09) (Goff et al., 1998) (see Table 3.1).

The reported statistics also varied across studies. Five studies reported pre- and post- intervention means and standard deviations for intervention and control groups (01, 04, 05, 08 and 11). Three studies reported difference in means and standard deviation between pre- and post- conditions for each group (study 03, 09 and 10). Three studies presented difference in means and p values post interventions (study 02, 06 and 07).

3.2.4. Secondary outcomes

Depression. Five studies measured symptoms of depression (study 01, 02, 06, 07 and 10). All of these studies used self-report measures with satisfactory psychometric properties. Four studies used Hospital Anxiety and Depression

Scale (HADS) (study 01, 02, 06 and 10), and one study used Cardiac Depression Scale (study 07) (see Table 3.1) (Hare & Davis, 1996; Zigmond and Snaith, 1983)

The effect size for two studies was calculated based on pre- and post-treatment means and standard deviations for each group (study 01 and 02). Two studies presented difference in means and p values post intervention (study 06 and 07). One study reported change in means for each group (study 10).

Anxiety. Symptoms of anxiety were assessed in seven studies (study 01, 02, 05, 06, 07, 09 and 10). Four studies used HADS as a measure of severity of symptoms of anxiety (study 01, 02, 06 and 10) (Zigmond & Snaith, 1983). Two studies used State Trait Anxiety Inventory (study 05 and 07) and one study used Anxiety Subscale from Brief Symptom Inventory (study 09) (see Table 3.1) (Derogatis & Malisaratos, 1983; Spielberg, Gorsuch, Lushene, Vagg, & Jacobs, 1983).

Pre- and post- treatment means and standard deviations for each group were reported by three studies (study 01, 02 and 05). Two studies reported difference in means and standard deviations for each group (study 09 and 10). One study presented difference in means and p value post intervention (study 06). Finally, the effect size for one study was calculated using mean difference between pre- and post- and confidence intervals (study 07).

3.3. Research Questions

All effect sizes were expressed as *Hedges's g* metric. This effect size metric was chosen because all of the included studies reported data from measures on the continuous scales (Borenstein et al., 2009).

3.3.1. Research Question One: Are Interventions with Psychological Components More Effective in Changing Illness Perceptions than Standard Cardiac Interventions without Clearly Identified Psychological Component?

In order to test the above research question a meta-analysis was conducted with illness perceptions as an outcome variable, the individual studies as units of analysis and the intervention strategy (psychological component vs no psychological component) as a moderator variable. Six studies were identified which contained psychological component (Barley et al., 2014; O'Brien et al., 2014; Furze et al., 2012; Furze et al., 2009; Lewin et al., 2002; Petrie et al., 2002). The remaining studies were classified as not having clearly identifiable psychological component (Pfaeffli et al., 2015; Yan et al., 2013; Cossette et al., 2012; McKinley et al., 2009; Tullmann et al., 2007).

The effect sizes and associated statistics are displayed in Table 3.3. Three separate effect sizes were calculated. The overall effect size was small (*Hedges's g* = .239) with relatively narrow confidence intervals (95% CI = .114 to .365). The effect size for subgroup of studies of interventions without psychological component was also small (*Hedges's g* = .224) with relatively wide confidence intervals (95% CI = .016 to .432). The effect size for the subgroup of studies of interventions with psychological component had a moderately small effect size

(*Hedges's g* = .248) and relatively wide confidence intervals (95% CI = .090 to .406) (see Figure 3.2).

There was a non-significant amount of heterogeneity in the distribution of the total effect size (Q(10) = 20.955, p = .067) and in the distribution of studies of interventions with psychological component (Q(5) = 7.998, p = .156). The I^2 was moderate for the distribution of total effect sizes and small for the distribution of effect size for the subgroup of studies of interventions with psychological component (I^2 = 52.279 % and I^2 = 37.487 %, respectively). The dispersion between studies was relatively small with tau^2 = .014 for the overall effect size and for the subgroup of studies of interventions without psychological component, indicating small dispersion between studies. The amount of heterogeneity among studies of interventions without psychological component was statistically significant (Q(4) = 12.900, p = .012), indicating that there is substantial heterogeneity among effect sizes in this subgroup of studies. The I^2 = 68.992 %, indicating moderate amount of dispersion among effect sizes in this subgroup.

The moderator analysis revealed a non-significant amount of heterogeneity within the overall effect size ($Q_T(10)$ = 20.955, p =.067), indicating that the effect sizes were homogeneous. Similarly, a dispersion of the effect sizes between groups was statistically homogeneous ($Q_B(1)$ = .057, p = .811). This means that the subgroups' effect sizes were not statistically different. There was evidence of significant dispersion across effect sizes within each of the subgroups ($Q_W(9)$ = 20.898 , p = .013).

Summary of findings for the research question one. The moderator analysis has shown all effect sizes to be positive and small, indicating that interventions designed to change maladaptive illness perceptions lead to positive changes. Further, there was no statistically significant difference in subgroups' effect sizes, indicating that interventions with psychological components are not more efficacious in changing maladaptive illness perceptions than interventions without psychological components. The amount of heterogeneity for the total effect size and the effect size for the subgroup of studies with no psychological component was significantly large. This significant amount of heterogeneity makes it more difficult to draw any certain conclusions about the group differences.

Table 3.3

Summary Data for Weighted Effect Sizes (Hedges's g) and related statistics for Illness Perception Grouped by Intervention Strategy.

Outcome variable	Moderator	N	Hedges's g (95%CI)	Q Statistics (df)	P value	I^2 %	tau^2
Illness perception	No Psych Component	5	.224 (.016; .432)	12.900 (4)	.012	68.992	.035
	Psych Component	6	.248 (.090; .406)	7.998 (5)	.156	37.487	.014
	Total	11	.239 (.114; .365)	Q_T = 20.955 (10)	.067	52.279	.014
				Q_W =20.898 (9)	.013		
				Q_B = .057 (1)	.811		

All studies tested within a random-effect model
N = total number of studies; df- degrees of freedom; Q_T = Total heterogeneity, $Q < .05$; Q_B = Heterogeneity between studies; Q_W = Heterogeneity within studies

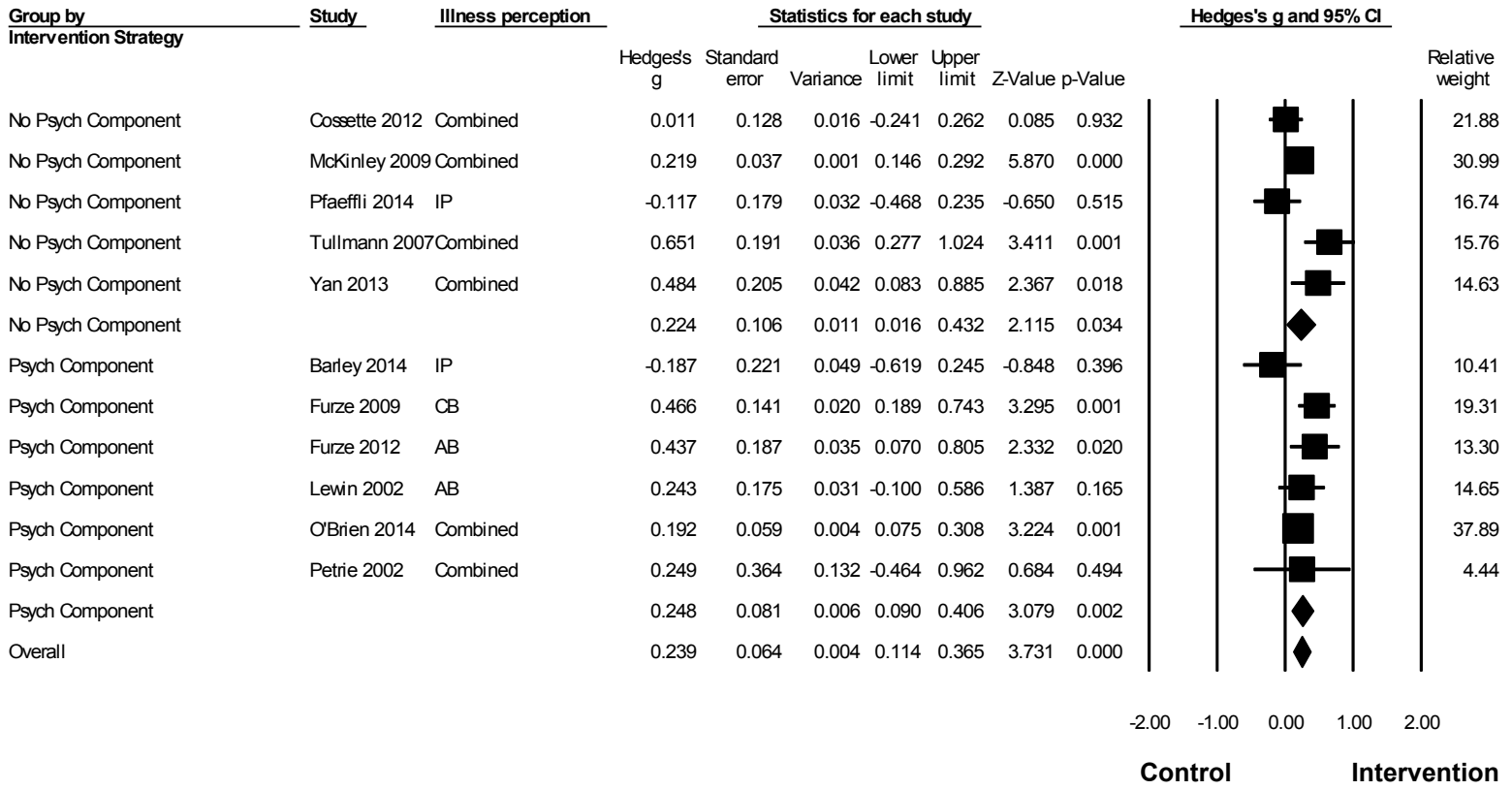

Figure 3.2. Effect sizes (Hedges's g) for Illness Perceptions Grouped by Intervention Strategy Derived from RCT Studies Designed to Change Illness Perceptions in CHD.
Note. Combined refers to the multiple outcomes (subscales) combined within study. IP-Illness Perception, CB-Cardiac Beliefs, AB-Angina Beliefs.

3.3.2. Research Question Two: Do Interventions Targeting Illness Perceptions Contribute to Positive Changes in Symptoms of Depression and Anxiety?

Two separate meta-analyses were run, one for studies that reported data on depression and one for studies that reported data on anxiety. All effect sizes and associated statistics can be found in Table 3.4.

Table 3.4.

Summary Data for Weighted Effect Sizes (Hedges's g) and related statistics for Depressive and Anxiety Symptoms.

Outcome variable	N	Hedges's g (95%CI)	Q Statistics (df)	P value	I^2 %	tau^2
Depression	5	-.089 (-.409; .231)	16.787 (4)	.002	76.173	.100
Anxiety	7	.204 (.046;.363)	9.583 (6)	.0143	37.390	.017

All studies tested within a random-effect model
N = total number of studies; df- degrees of freedom; Q < .05

Effect sizes of depressive symptoms. The total weighted effect size was negative and small (*Hedges's g* = -.089) with relatively wide confidence intervals (95% CI = -.409 to .231), indicating that there was no support for the efficacy of interventions designed to change illness perceptions for treating depression. Three studies had negative effect sizes within small to moderate range and two studies had positive effect sizes (see Figure 3.3). The weighting of the effect size was relatively evenly spread and ranged from 17.85 to 22.14 with studies with the largest sample having the largest weighting. Studies with larger weighting were more precise in estimations of effect sizes (Borenstein et al., 2009).

There was a substantial and significant amount of heterogeneity among studies (Q(4) = 16.787, p =.002). The I^2 for the total weighted effect size indicated large amount of dispersion among the effect sizes (I^2 = 76.173%), indicating that approximately 76% of the observed variance between studies is due to real differences in the effect size. The between-studies variance was moderate with tau^2 = .100.

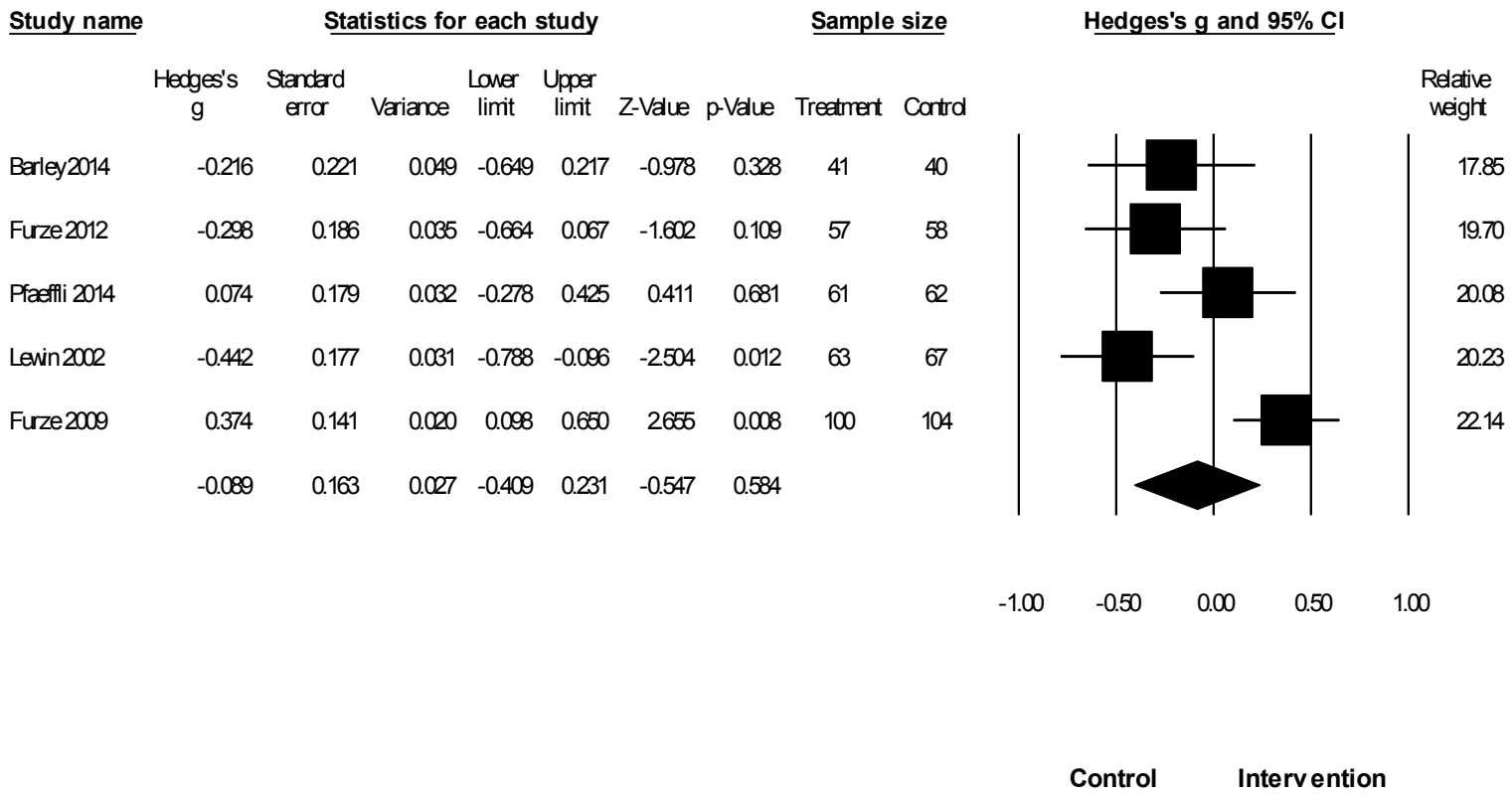

Figure 3.3. Effect Sizes (Hedges's g) for Depressive Symptoms Derived from RCT Studies Designed to Change Illness Perceptions in CHD.

Effect sizes for anxiety symptoms. The total effect size was small (*Hedges's g* = .204) with relatively wide confidence intervals (95% CI = .046 to .363). The weighting of the effect sizes was relatively evenly spread, ranging from 10.02 to 19.67.

There was a non-significant amount of heterogeneity among effect sizes (Q(6) = 9.583, p = .143). The I^2 for the total effect size indicated moderate amount of dispersion among the effect sizes (I^2 =37.390), indicating that approximately 37% of observed variance between studies is due to real differences in the effect sizes. The between-studies variance was also non-significant with tau^2 = .017, indicating non-significant dispersion between studies.

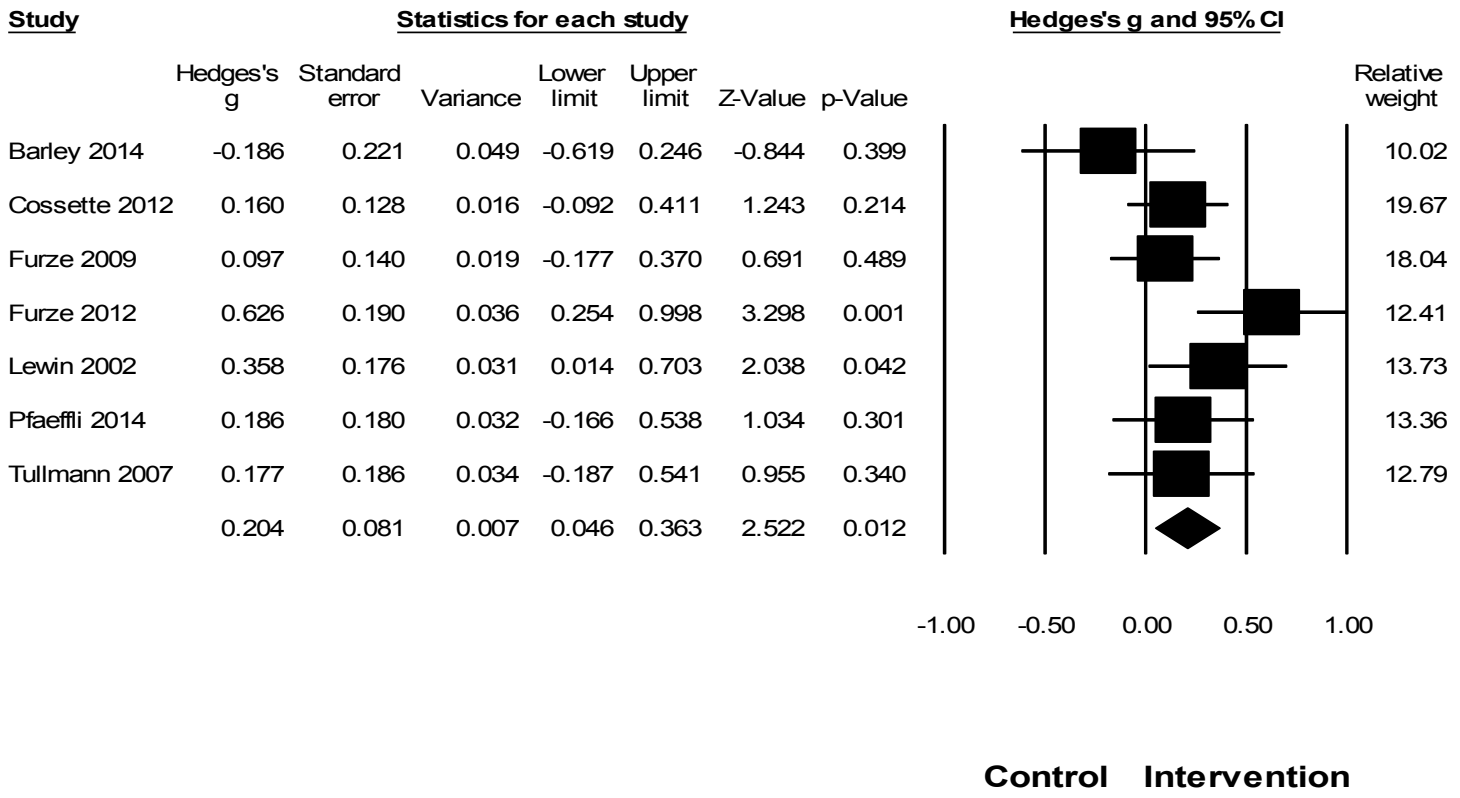

Figure 3.4. Effect Sizes (Hedges's g) for Anxiety Symptoms Derived from RCT Studies Designed to Change Illness Perceptions in CHD.

Summary of findings for research question two. The meta-analysis of studies that presented data on depressive symptoms revealed a non-significant effect size, indicating no effect for the interventions aimed at reducing depression. The Q statistics indicated significant amount of dispersion among effect sizes. Thus, it is likely that the sample size was too small to detect any meaningful changes and provide an accurate reflection of dispersion among effect sizes.

On the other hand, there was a small positive effect size for anxiety symptoms, indicating that interventions designed to change maladaptive illness perceptions contributed to small but significant positive changes in anxiety symptoms in patients with CHD. There was also a moderate but non-significant amount of dispersion among the effect sizes for symptoms of anxiety. However, caution needs to be applied when interpreting these results due to relatively small sample size.

3.3.3. Research Question Three: Are the Type of Illness and Age Linked with the Effectiveness of Interventions Designed to Change Illness Perceptions?

The above research question was tested by applying a meta-analysis, with illness perceptions as outcome variable, individual studies as a unit of analysis and a type of illness (*chronic* vs *acute*) as a moderator variable. The samples of eight studies were classified as including participants with *acute* CHD (Pfaeffli et al., 2015; Barley et al., 2014; O'Brien et al., 2014; Cossette et al., 2012; McKinley et al., 2009; Tullmann et al., 2007; Lewin et al., 2002; Petrie et al., 2002). The remaining three studies of the sample were classified as including patients with

chronic CHD (Yan et al., 2013; Furze et al., 2012; Furze et al., 2009). There were two studies with negative effect sizes within the *acute* subgroup; the remaining effect sizes were all positive (see Figure 3.5).

The effect sizes and associated statistics are displayed in Table 3.5. Three separate effect sizes were calculated. The total effect size was moderately small (*Hedges's g* = .247) with relatively narrow confidence intervals (95% CI = .147 o .347). The effect size for a subgroup of studies classified as *acute* was small (*Hedges's g* = .168) with relatively wide confidence intervals (95% CI = .051 to .285). The effect size for a subgroup of studies classified as *chronic* was moderate (*Hedges's g* = .462) with narrow confidence intervals (95% CI = .268 to .656) (see Figure 3.5).

There was a significant amount of heterogeneity in the distribution of the total effect size (Q(10) = 20.955, p = .021) and in the distribution of effect sizes for the subgroup of studies classified as *acute* (Q(7)=14.281, p = .046). The I^2 was within the moderate range, indicating a moderate amount of dispersion among the distribution of the effect sizes (I^2 = 52.279 for the total effect size and I^2 = 50.983 for the subgroup of studies classified as *acute*). This means that approximately 52% of dispersion across all of the studies and 50% of dispersion among studies in the subgroup of studies classified as *acute* is likely to be due to real differences in the effect sizes. The dispersion between studies was relatively small with tau^2 = .014 for total effect size and tau^2 = .011 for the effect sizes of the subgroup of studies classified as *acute*. The amount of heterogeneity in a distribution of the effect sizes in the subgroup of studies classified as *chronic* was small and statistically non-significant (Q(2) = .030, p = .985). The I^2 = 0% and tau^2 = 0,

indicated no substantial amount of dispersion among the distribution of the effect sizes and between studies.

The dispersion of the effect sizes between subgroups was statistically heterogeneous, indicating that the subgroups' effect sizes were different (Q_B (1) = 6.646, p = .010). This means that the differences between subgroups' effect sizes are statistically significant. There was, however, no evidence of significant dispersion across effect sizes within each of the subgroups (Q_W (9) = 14.311, p = .112).

Table 3.5.

Summary Data for Weighted Effect Sizes (Hedges's g) and related statistics for Illness Perception Grouped by Type of Illness.

Outcome variable	Moderator	N	Hedges's g (95%CI)	Q Statistics (df)	P value	I^2 %	tau^2
Illness Perception	Acute	8	.168 (.051; .285)	14.281 (7)	.046	50.983	.011
	Chronic	3	.462 (.268; .656)	.030 (2)	.985	0	.000
		11	.247 (.147; .347)	Q_T = 20.955 (10)	.021	52.279	.014
				Q_W =14.311 (9)	.112		
				Q_B = 6.645 (1)	.010		

All studies tested within a random-effect model
N = total number of studies; df- degrees of freedom;
Q_T -Total heterogeneity, Q_B - Heterogeneity between studies;
Q_W - Heterogeneity within studies; Q < .05

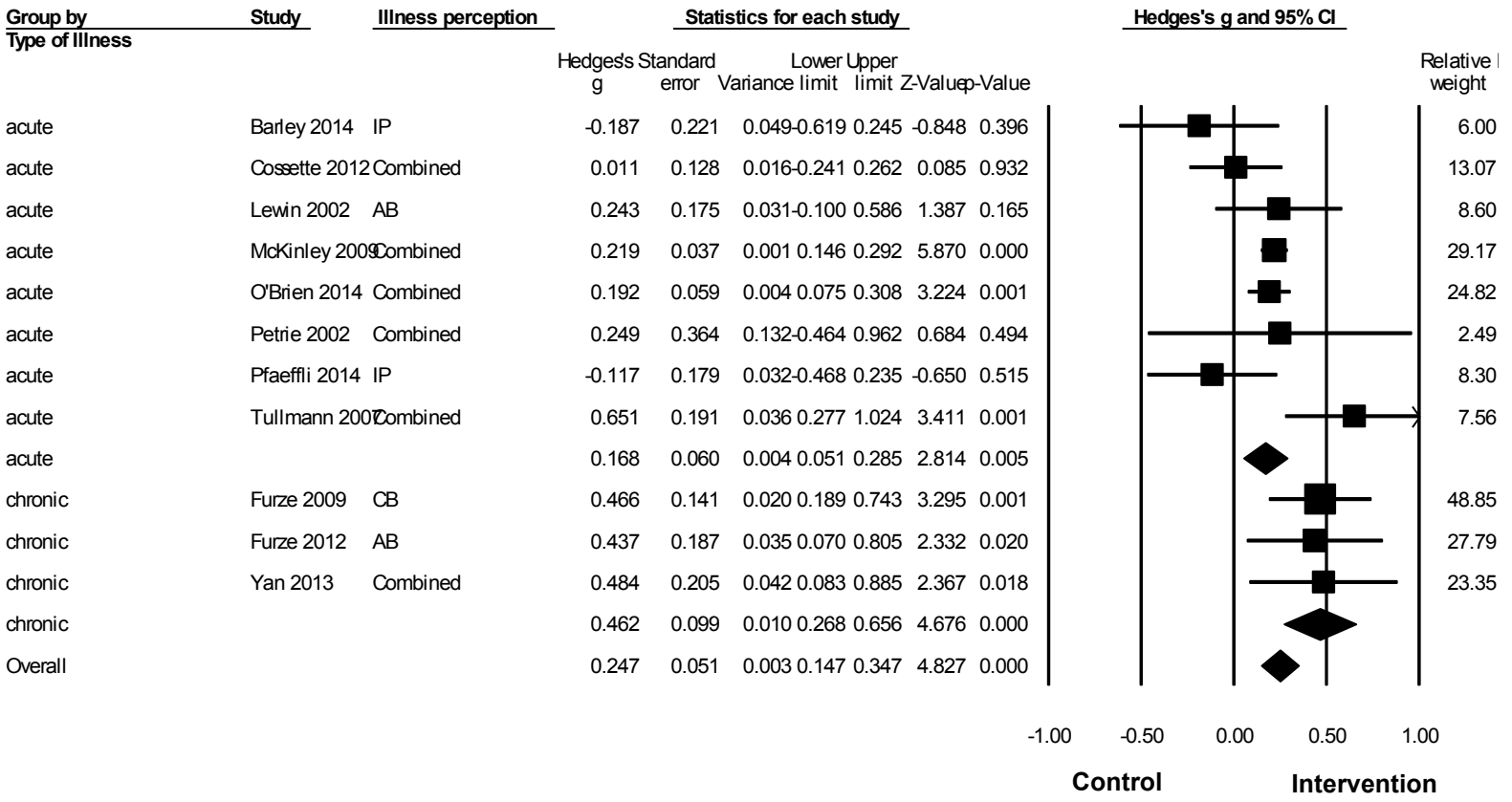

Figure 3.5. Effect sizes (Hedges's g) for Illness Perceptions Grouped by Type of Illness Derived from RCT Studies Designed to Change Illness Perceptions in CHD.
Note. Combined refers to the multiple outcomes (subscales) combined within study. IP-Illness Perception, CB-Cardiac Beliefs, AB-Angina Beliefs.

In order to test the second part of the third research question a correlation was conducted with the effect sizes for illness perception and age as outcome variables. Firstly, the data were tested for normal distribution by using the Kolmogorov-Smirnov test. Age was normally distributed (M = 64.07, SD = 4.799) however, the effect sizes were not normally distributed (M = .63, SD = 1.448). Therefore, a non-parametric test (Spearman's correlation coefficient) was used to test the hypothesis. There was a non-significant relationship between age and effect size, r = .305, p =.361 (two-tailed), N = 11.

Summary of the findings for research question three. The moderator analysis revealed that studies with a sample characterised as *chronic* had a larger effect size than studies with the sample identified as *acute*. The *acute* subgroup, however, had a moderate amount of dispersion among effect sizes. The amount of heterogeneity within a subgroup of studies classified as *chronic* was very small, but caution needs to be taken when interpreting the results from the analysis of the effect sizes from this subgroup because of the small sample size. There were no statistically significant correlations between age and the effect size for illness perception.

3.4. Publication Bias

In order to assess for the potential impact of unpublished or unidentified studies on the findings in the current meta-analysis, funnel plots were computed and visually inspected for all outcome variables (illness perceptions, depression and anxiety) (see Figures 3.6; 3.7 and 3.8). The initial visual inspection of the funnel plots for all outcome variables indicates evidence of publication bias as shown by a relative absence of small studies with positive and negative effects on both sides of the plot.

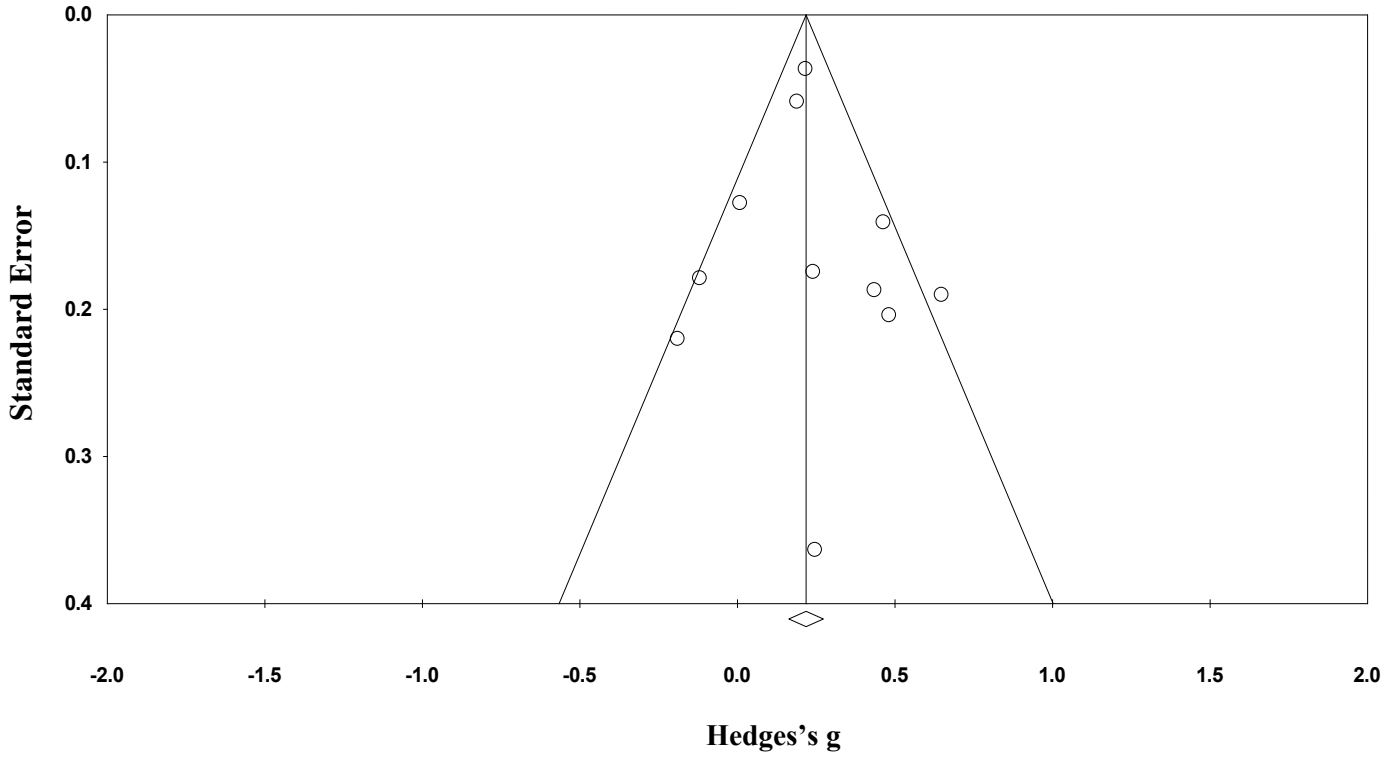

Figure 3.6. Funnel Plot of Standard Error by Hedges's g for Illness Perception.

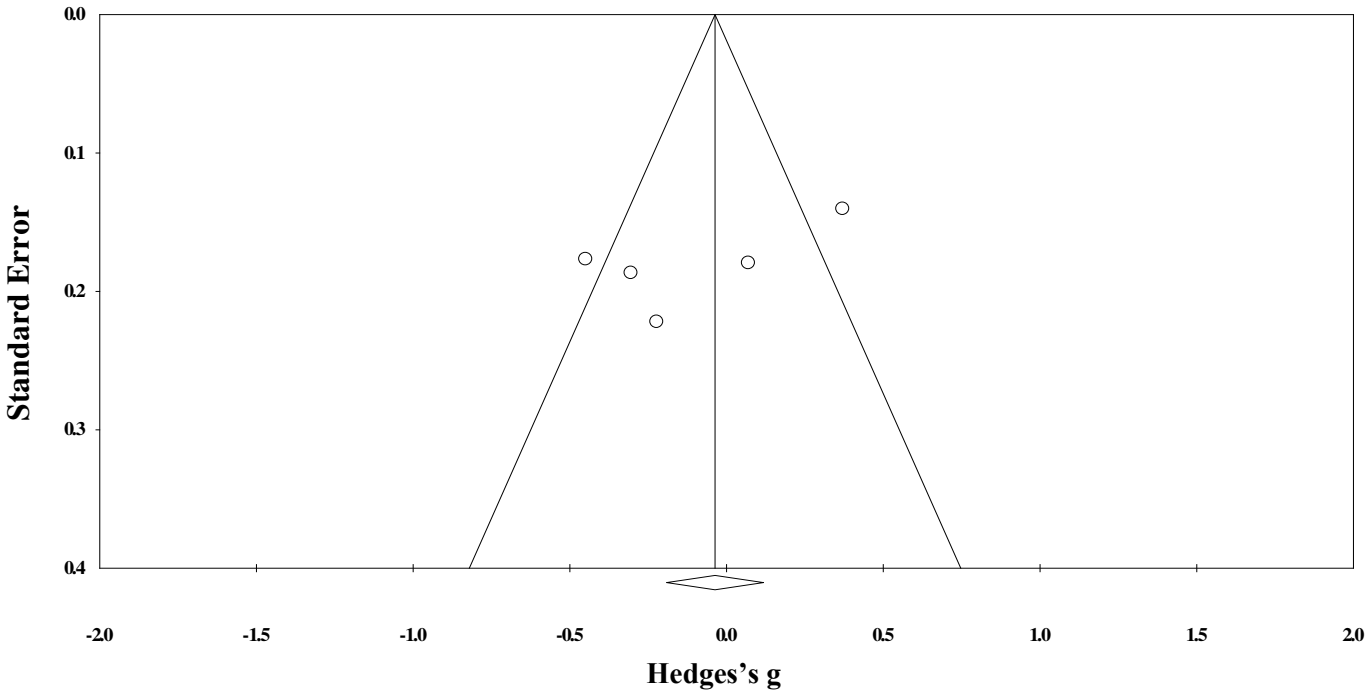

Figure 3.7. Funnel Plot of Standard Error by Hedges's g for Depressive Symptoms.

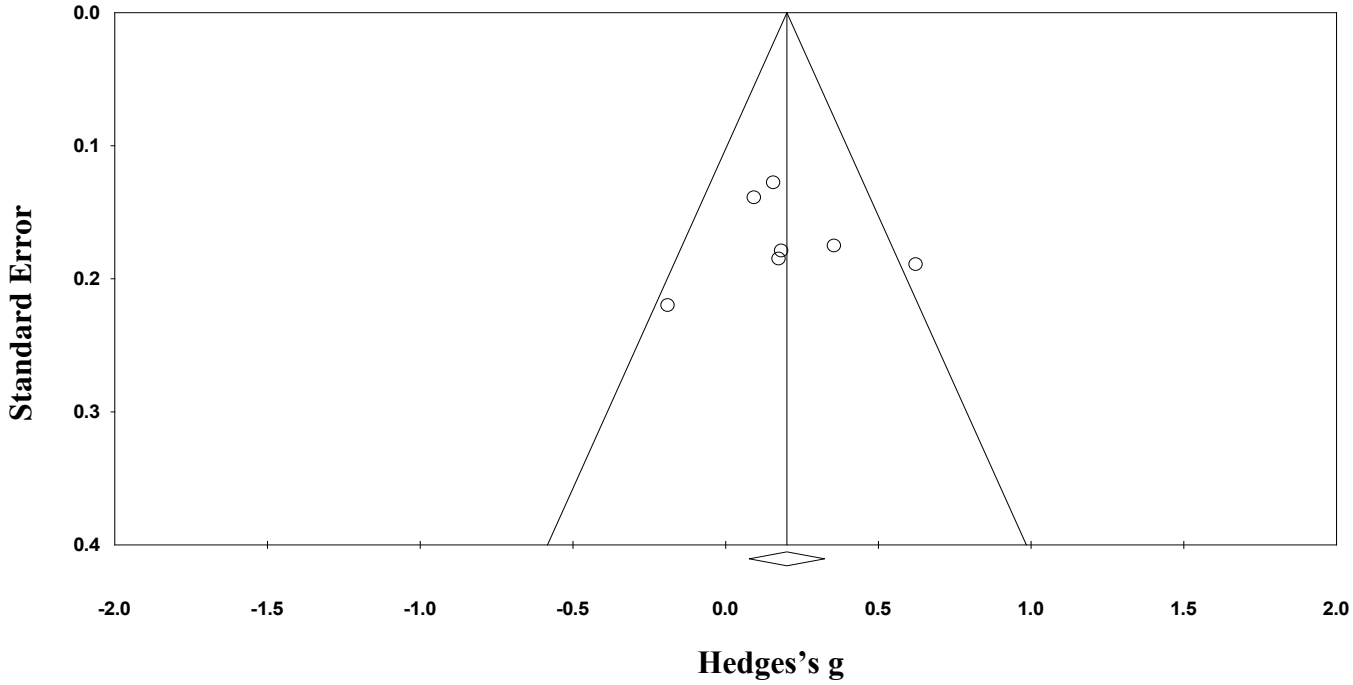

Figure 3.8. Funnel Plot of Standard Error by Hedges's g for Anxiety Symptoms.

Table 3.6.

Results of the *Fail-safe N* Analysis.

Outcome variable	N	Fail-Safe N
Illness perceptions	11	106
Anxiety	7	12

Note. *N* refers to the total number of studies included in the original analysis. The criterion effect size level used to calculate *fail-safe N* was p = .05, two tails.

The *fail-safe* values were calculated for all outcome variables. The *fail-safe N* estimates the number of studies that would need to be additionally retrieved and included into the analysis to reduce the p-value to the point when it became statistically non-significant (Borenstein et al., 2009). The *fail-safe N* ranged from 12 to 160, indicating that particularly for anxiety variable substantial number of studies with null statistical findings would need to be included within any analysis to reduce the effects to the point of non-significance (see Table 3.6).The *fail-safe N* for depression was not calculated (as there was no effect detected). In order to investigate this finding further a Duval-Tweedie's *trim and fill* method was used. The trim and fill method resulted in imputed two additional effect sizes on the right side of the plot, resulting in the correct pooled effect size of .16 for depression.

The Duval-Tweedie's *trim and fill* method was also used to calculate an estimate unbiased effect size for anxiety symptoms and illness perceptions. For anxiety symptoms the *trim and fill* analysis showed that no studies needed to be added to obtain an unbiased effect size. The Duval-Tweedie's *trim and fill* analysis for illness perception has shown that in order to obtain the unbiased effect size (*Hedges's g* = .202) only one additional study would need to be included.

Summary of findings from the assessment of publication bias. The inspection of funnel plots and associated statistical analysis revealed a presence of publication bias. Overall, small studies with positive and negative effect sizes are likely to be missing due to publication bias. It was estimated that t there are about 160 potential studies missing in order to reduce the effect size to the point of non-significance for anxiety and 12 studies missing for illness perception.

3.5. Summary of the Results

Overall, the results of the analysis in the current meta-analysis indicate that there are no statistically significant differences in effect sizes for interventions with and without psychological component in terms of maladaptive illness perceptions. The overall findings also indicated that the interventions designed to change maladaptive illness perceptions contribute to positive changes in anxiety symptoms, but not in depressive symptoms. It is likely, however, that both analyses (particularly the analysis of effect sizes from studies that presented data

on depressive symptoms) might have been affected by the small sample sizes. It is also difficult to draw certain conclusions about the impact of the type of illness on the changes in illness perceptions due to the small sample size of the subgroup of studies classified as *chronic*. Bearing in mind this small sample size, the results of the meta-analysis indicated a positive and moderate effect size for studies with samples characterised as *chronic*. However, the sample size is quite small and two out of three studies included in this meta-analysis are by the same research team thus caution is urged in interpreting these results. There were no statistically significant correlations between age and effect size. Overall results of the meta-analysis, however, ought to be interpreted with caution due to the substantial amount of heterogeneity among the effect sizes and the relatively small sample size reported here.

4. Discussion

4.1. Chapter Overview

This chapter presents the results of the current meta-analysis within the context of previous research and theory. Firstly, a detailed discussion of findings from each research question is presented. This is followed by a presentation of the methodological weaknesses of the meta-analysis. Finally, implications for clinical practice and future research are proposed.

4.2. Contextualising the current meta-analysis within the existing evidence-base

4.2.1. Research question one: Are interventions with psychological component more efficacious in changing illness perceptions than standard cardiac interventions without clearly identified psychological component?

The current meta-analysis demonstrates that psychological and non-psychological interventions designed to change illness perceptions report small effect sizes in reducing maladaptive illness perceptions (*Hedges's* g = .239). The subgroup analysis revealed that there were no statistically significant differences in effect size (*Hedges's g* = .248) for interventions with psychological components, such as motivational interviewing and/or cognitive-behavioural therapy compared to interventions without an identified psychological intervention component (*Hedges's g* = .224). As such in the current meta-analysis interventions with psychological components were not shown to be more

efficacious in changing maladaptive illness perceptions compared to the interventions without psychological components.

The results from the current meta-analysis add to the findings of previous systematic reviews and meta-analyses but raise questions about the certainty with which conclusions about treatment outcomes can be made (e.g. French et al., 2006; Goulding et al., 2010). For example, Goulding at al. (2010) were only able to reported tentative results and concluded that studies containing features of cognitive-behavioural therapy lead to significant and positive changes in illness beliefs. However, this is a preliminary finding because of the small number of studies on which this conclusion was reached (only three). As it was a textual review of the literature the 'data' is less compelling than that reported in the current meta-analysis. As one may expect, the certainty with which to judge the data from this current meta-analysis is also likely to be affected by the small sample size reported here. Nevertheless, the current meta-analysis results reported here suggest no significant subgroup difference in the efficacy of interventions with and without psychological components. The present finding might indicate that in challenging maladaptive illness perceptions in CHD, non-specific elements of both interventions (psychological and non-psychological) may play an equally important role. This also might suggests there may be a need to refine the focus and nature of psychological interventions in order to maximise efficacy.

The findings from the present investigation with regards to the efficacy of interventions designed to change dysfunctional illness perceptions extend evidence suggesting that targeted interventions can benefit individuals with CHD

(Dickens et al., 2013; Whalley et al., 2011). Some of the previous meta-analyses had a relatively weak theoretical base. This is a significant shortfall because well-specified theoretical context in meta-analysis allows for revision and/or confirmation of existing theories underlying the efficacy of particular treatments (i.e. treatments for individual affected by CHD) (Borenstein et al., 2009). Data from some of the previous meta-analyses have reported a relatively weak link with specific theories (e.g. Whalley et al., 2011). For example, Whalley et al. (2011) in their meta-analysis demonstrated psychological interventions led to small and moderate improvements in symptoms of depression. However, due to the poorly specified theoretical context of the meta-analysis by Whalley et al. (2011), it is difficult for the readers to hypothesise about the potential mechanisms behind reported effect sizes for treatment interventions.

It is possible that a reduction in maladaptive illness perceptions is achievable through use of a wide range of interventions strategies, both psychological and non-psychological in nature. This may be because interventions designed to challenge maladaptive illness perceptions are likely to impact upon a dual level (cognitive and emotional) consistent with that proposed in Leventhal's model (Diefenbach & Leventhal, 1996; Wearden & Peters, 2008). It is possible, for example, that some of the techniques can be more helpful on the cognitive level, such as information provision and/or thought challenging can help participants reappraise perception of health threat and subsequently encourage more problem-solving coping mechanisms (Meas & Karoly, 2005). If this desirable coping strategy is evaluated as helpful in managing CHD, the individual

more appropriate emotional response (e.g. reduction of anxiety) may follow. Simultaneously, other components (e.g. active listening) might work better towards addressing emotional distress around an illness (i.e. the unhelpful emotional representation of illness based on concerns) (Harvey & Lawson, 2009).

The discrepancy between findings reported in the current meta-analysis and previous results published in single studies provides a strong justification for this meta-analysis. Data from single studies can provide valuable information, but they often are believed to be insufficient in drawing more certain conclusions about the efficacy of specific interventions. Although the results from single studies can lead to statistically significant findings, individual studies by themselves can bias our understanding of what is efficacious especially where the research literature is small and developing as in this current area. Meta-analysis can often lead to contradictory findings with that understood from results of individual studies (Borenstein et al., 2009). The results of meta-analysis, however, carry more clinical value due to its cumulative and systematic nature in data collection and analysis (Borenstein et al., 2009).

Furthermore, the current findings that interventions targeting maladaptive illness perceptions can facilitate positive but small effects may be consistent with a view that in order to develop a successful intervention to change maladaptive illness perceptions in CHD it is necessary to *actively* engage with participants in interventions (Lin et al., 2012; Thomson, Bowling & Moss, 2001). It may be that studies that empower participants to be *active* agents in their treatment are likely to lead to a successful cardiac intervention (Lin et al., 2012). It is also possible

that treating people as active agents in their treatment allows them to successfully utilise on different components of cardiac interventions regardless whether they are psychological or non-psychological in nature (Diefenbach & Leventhal, 1996; McAndrew et al., 2008).

Additionally, the analysis of data from the first research question has some methodological strengths and weakness. A strength of the data is the relatively narrow confidence intervals which inform the reader about the precision of the estimated effect sizes (Higgins & Green, 2011). Narrow confidence intervals point towards more precise estimation of the effect size (Higgins & Green, 2011). The confidence intervals for the total and subgroup effect sizes for the first research question were within a narrow range, indicating the effect sizes were relatively precise estimation of the true effect sizes. This means that the reader can be relatively (95%) confident that interventions with psychological components reduce dysfunctional illness perceptions (Borenstein et al., 2009; Higgins & Green 2011).

4.2.2. Research question two: Do interventions targeting illness perceptions contribute to positive changes in symptoms of depression and anxiety?

The second research question was partially supported by the current meta-analysis. The effect size for studies that reported data on anxiety were small but significant and positive (H*edges's g* = .204). Thus indicating that interventions

designed to change illness perceptions contribute to significant and positive changes in symptoms of anxiety in individuals with CHD. On the contrary, the effect size for studies that reported symptoms of depression were non-significant (Hedges's g = -.089). This was an unanticipated finding suggesting interventions designed to change illness perceptions do not contribute to positive changes in symptoms of depression.

The findings from the current meta-analysis are partially consistent with previous evidence from single studies and reviews (e.g. Dickens et al., 2013; Juergens et al., 2010). That is, previous research has demonstrated mixed results for the efficacy of cardiac interventions in reducing symptoms of anxiety and depression (e.g. Broadbent et al., 2009; Furze et al., 2009; Lewin et al., 2002). While some studies demonstrated non-significant trends of cardiac interventions in reducing symptoms of depression and anxiety (e.g. Lewin et al., 2002), other studies demonstrated interventions can contribute to positive changes either to symptoms of depression and/or to symptoms of anxiety (e.g. Furze et al., 2009; O'Rourke & Hampson, 1999). These inconsistencies in the conclusions from previous research are likely to reflect a bias related to small sample sizes (e.g. Furze et al., 2009). This lack of sufficient power was also reflected in some of the previous meta-analysis. For example, Reid et al. (2013) in their meta-analysis of six studies reported psychological interventions significantly reduced symptoms of anxiety but not symptoms of depression in participants with CHD. Previous meta-analysis with a large number of studies, however, (i.e. N = 60) demonstrated a small effect for reduction of depressive symptoms. Specifically, a meta-

regression by Dickens et al. (2013) demonstrated that psychological interventions contributed to positive changes in depressive symptoms in participants with CHD. The meta-analyses by Dickens et al. (2013) and Reid et al. (2013), however, did not look specifically at interventions designed to change illness perceptions. The current meta-analysis is unique in that it is the only meta-analysis that could be found that quantifies the link between interventions designed to change illness perceptions and changes in psychological outcomes of individuals with CHD. The current finding that interventions designed to change illness perceptions contribute to a reduction in symptoms of anxiety but not symptoms of depression are important because they suggest that a positive change in psychological outcomes in patients with CHD is likely to reflect a complex nature of the relationship between illness representations and psychological outcomes (Harvey & Lawson, 2009; Peterson & Kim, 2011). These complexities might be related to a number of factors, such as how individuals process different dimensions of their illness, illness-related factors and/or relationship between depression and anxiety (Greco et al., 2014; Harvey & Lawson, 2009; Patel et al., 2013). It is possible, for example, that changes in depressive symptoms are more difficult to detect due to a more complex relationship with illness perceptions (Simmonds, Tyle, Walters & Rose, 2013). It is also possible that depressive symptoms in participants who also report more maladaptive illness perceptions are much more resistant to change (Furze et al., 2005). Furze et al. (2005) in their examination of a link between changes in illness perceptions and depression found that reduction in unhelpful

illness perceptions did not correspond to positive changes in symptoms of depression.

It might be easier to alleviate symptoms of anxiety by addressing maladaptive illness perceptions because there might be a more direct link between participants' health threat appraisal and emotional response to this threat (i.e. anxiety) (Maes & Karoly, 2005). Depressive symptoms, however, might make it more difficult for individual with CHD to engage in the process of change (Furze et al., 2005; Hare, Toukhsati, Johansson, & Jaarsma, 2013; Maes & Karoly, 2005). Participants who report more symptoms of depression might rely more on emotion-focus coping strategies in making sense of their illness and to help them cope with it. It is also possible that individuals whose coping with CHD is complicated by symptom of depression have less capability to self-monitor and regulate their coping behaviours (Maes & Karoly, 2005).

The current findings that interventions targeting illness perceptions contribute to a reduction in symptoms of anxiety, but not depression need to be interpreted with caution because of the substantial methodological shortcomings in the current meta-analysis but they are intriguing and may suggest the need for a component analysis of outcome studies. It is also likely that the precision of the effect size reported in the current meta-analysis was affected by the small sample size (N = 5 and N = 6 studies for depressive and anxiety symptoms respectively) (Borenstein et al., 2009). The data analysis for symptoms of depression, in particular, was likely to be affected by the small sample size and the large amount of heterogeneity in a dispersion of effect sizes. Given that the total effect size for

depression is likely to be an imprecise estimation, it seems more appropriate to consider the effect sizes for each study separately. Studies with the smallest sample size (e.g. Barley et al., 2014's sample size was N = 81) had a negative effect size with wide confidence intervals, suggesting that the effect sizes from Barley et al. (2014) study for symptoms of depression and anxiety was unlikely to be a precise estimation of true effect size (Borenstein et al., 2009). It is, therefore, possible that the analysis of depressive symptoms was the most affected by the imprecise estimation of effect sizes from single studies because of the relatively small sample size in each of these studies (Borenstein et al., 2009).

Another factor that might have affected the results for the second research question is a use of measures of depression that were not sensitive enough to detect differences. Studies that used HADS as a measure of symptoms of depression had a smaller effect sizes compare to studies which used alternative measures, such as Cardiac Depression Scale (Barley et al., 2014; Furze et al., 2009; Zigmond & Snaith, 1983). This is an interesting qualitative finding which might indicate that commonly used measures to detect symptoms of depression in health settings (e.g. HADS) may not be sensitive enough to reliably assess these symptoms in patients with CHD (Haddad et al., 2013).

4.2.3. Research question three: Are type of illness and age linked with the efficacy of interventions designed to change illness perceptions?

To the authors knowledge this is the first meta-analysis to attempt to address the impact of the type of illness on changes in maladaptive illness

perceptions. However, addressing the third research question was challenging because of a complex nature of participants' illnessand a relatively small sample size (particularly in the context of subgroup analysis).

When examining impact of presentation of illness (acute versus chronic) in changing illness perception (see table 3.5), the overall effect size is small but significant (Hedges's g = .247). The effect size for a subgroup of studies classified as *chronic* (*Hedges's g* = .462) is significant and moderate and the effect size is a subgroup of studies classified as *acute* is significant but and very small (*Hedges's g* = .168). However, comparison of subgroups' effect sizes are not appropriate in the current meta-analysis, because the estimation of the effect size for a subgroup of studies classified as *chronic* is likely to be affected by type II error due to the small sample size (N = 3). It is, therefore, more appropriate to consider the effect sizes for individual studies for a subgroup of studies classified as *chronic.* These effect sizes are significant and moderate (please refer to Figure 3.5. in the Result chapter). Taking this into consideration the aforementioned effect sizes indicate that participants identified as having *chroni*c illness are significantly more likely to benefit from interventions designed to change illness perceptions than participants with acute illness. Further, the testing of the association between changes in illness perceptions and age does not demonstrate significant relationship.

The current results demonstrating that interventions targeting illness perceptions are significantly more efficacious for participants whose illness can be classified as chronic compared to participants with acute illness is in line with

previous research. Findings from previous individual studies indicated that illness severity is associated with illness perceptions. Greco et al. (2014) and Steca et al. (2013) found that participants with more severe illness (measured by LVEF[1]) reported more unhelpful illness perceptions and more psychological distress. These findings are also confirmed in the investigation by Aalto et al. (2005), who used a proxy measure of illness severity (derived from CHD risk factors, comorbidities and use of medication). More importantly, Aalto et al. (2005) demonstrated that illness severity was associated with changes to illness perceptions over time. Participants with less severe illness reported greater reductions in unhelpful illness perceptions.

Although the findings from the current meta-analysis must be interpreted with caution, they are consistent with previous research. The difference in effect sizes obtained in the current meta-analysis by presentation of illness (acute versus chronic) suggest this may be an important factor determining possible change in illness perceptions. One explanation for that finding of a difference in outcome in illness perceptions by presentation of illness is that perhaps the high levels of perception of threat in participants who are acutely unwell make it more difficult for them to engage in interventions. It is also possible that illness perceptions in participants who suffer from acute illness are more resistant to change (Aalto et al., 2005), whereas those with a chronic presentation have lived with the illness so long that they wish to invest in coping interventions so as to improved their quality of life.

[1] Left ventricular ejection fraction; it is a proportion of blood pumped out the left ventricle at each heartbeat

The current meta-analysis reports no association between age of participants and changes in illness perceptions. This finding may be due to a small sample size in the present meta-analysis. This finding is contradictory to some previous research. Other investigations attested that older age participants were more likely to identify aging as a causal factor of CHD, indicating that age may play a role in the development and maintenance of illness perception (e.g. Grace et al., 2005; Juergens et al., 2010). Taylor et al. (2011) also found a link between age and rates of participation in cardiac rehabilitation programmes In this systematic review Taylor et al. (2011) demonstrated that participants who are younger and older are at the highest risk of dropping out from interventions.

While interpreting the results from the analysis of the third research question caution must be applied because of methodological shortcomings such as the aforementioned sample size. There is also an unusually small amount of dispersion among the effect sizes within the subgroup of studies classified as *chronic,* indicating it is not appropriate to interpret the total effect size for this subgroup. This means that dispersion among the effects sizes obtained for the subgroup of studies classified as *acut*e and the overall effect size is likely to be affected by sampling error (Bartolucci & Hillegas, 2010).

Another methodological shortcoming that could have affected the results of the third research question is the operationalisation of variable of *type of illness* in the current meta-analysis. Previous research focused on illness severity, which was assessed using physical markers, mainly LVEF (Greco et al., 2014; Pibarot & Dumesnil, 2012; Steca et al., 2013). This is in contrast to the current investigation,

in which the type of illness was categorised as either *acute* or *chronic* based on a proxy measure. This was a pragmatic decision because studies included in the current meta-analysis did not include any measure of illness type. The measure of the type of illness in the current meta-analysis was derived from a combination of factors, including frequency of symptoms, classification of severity of cardiac problems and a proportion of reported risk factors. This multifactorial categorisation of type of illness was informed by previous research and guidelines (e.g. Jopson, & Moss-Morris, 2003; The Criteria Committee for the New York Heart Association, 1994). While LVEF might be a more objective measure of illness severity, the classification in the current investigation might reflect better the complexity of CHD. This is because it takes into consideration a range of factors involved in determining the complexity of CHD (Khot et al., 2003). Further there is also a subtle difference in conceptualisation of the variable. The operationalisation in the current meta-analysis is more likely to be consistent with a theoretical appreciation of illness perceptions (Harvey & Lawson, 2008). For example, a timeline domain of illness representation directly refers to patients' perception of illness duration (acute or chronic) (Harvey & Lawson, 2008).

Overall, the results from the third research question are inconclusive and suggest methodological limitations in the literature. It is not possible to definitively state whether type of illness (acute versus chronic) is a contributing factor in changing in maladaptive illness perceptions. Likewise age does not statistically significantly impact on the efficacy of interventions in the current meta-analysis.

4.2.4. Summary

Overall, the results of the current meta-analysis indicate that psychological interventions may be effective at the level of small effect sizes when changing maladaptive illness perceptions within cardiac populations and may contribute to positive changes in symptoms of anxiety. However, interventions designed to change maladaptive illness perceptions do not contribute to significant changes in symptoms of depression. The present meta-analysis also does not demonstrate any support for the relationship between changes in illness perceptions and age. Finally, the link between changes in illness perceptions and the type of illness (*chronic vs acute*) is complex and unclear based on the data here. Taking into consideration these methodological weaknesses the results of the current meta-analysis need to be interpreted with caution.

4.3. Methodological Strengths and Weaknesses

4.3.1. Meta-analysis as a design

Meta-analysis affords the opportunity to bring clarity of understanding to a research literature composed of separate clinical trials, and allows an opportunity to examine whether there are new understandings to be derived from interventions designed to change illness perceptions in CHD. Interpreting effect sizes derived from multiple studies is theoretically and clinically more valuable than interpreting results from individual studies (Humphrey, 2011; Schmidt, 1996). Whilst a meta-analytic design is a definite strength of the current investigation

there are a few important caveats in the use of meta-analysis designs that are relevant for understanding findings from the current investigation.

Firstly, the process of selection and coding of the included studies was conducted by one person only (the main author). This approach is more prone to errors and biases, such as omission of important studies, selective approach to data extraction and/or selective reporting bias (i.e. reporting data for a selection of subtests of outcomes) (Rothstein & Bushman, 2015). Substantial efforts were made by the main author to control the extent to which these biases affected early stages of data extraction in the present meta-analysis. These efforts included having the, the primary academic supervisor for this thesis independently reviewing eligibility criteria, seeking contact with the field experts to identify any potential unpublished studies, designing the coding manual and coding form using Cochrane guidelines and with *pre-specified* eligibility criteria, and conducting bias analysis (e.g. publication bias) (Higgins & Green, 2011). Despite that a range of preventative strategies was adopted conducting double coding might have enhanced the quality of the extracted information. This was difficult to achieve, however, due to the limited personal resources on the current project.

A selection bias might have also affected the outcomes of the current meta-analysis at the data extraction stage. For example, two studies had to be excluded due to the insufficient or inaccurate data reporting (e.g. missing data for primary outcome). The efforts to source the unreported data directly from the authors of the studies were unsuccessful. Therefore, these studies had to be

excluded from the present meta-analysis (Broadbent, et al., 2009; Broadbent, Leggat, McLachlan, & Kerr, 2013).

The shortcomings resulting from the absence of double coding and selection bias were to a certain degree counterbalanced by a relatively well-developed searching strategy and the quality assessment conducted by two independent raters. A university librarian specialising in systematicic literature searching was also consulted to optimise searching terms, strategy and location. Choosing a range of databases and other sources (e.g. journals) relevant for social and health science helped to identify studies published across different types of publication types. This, in turned, enhanced retrieval of the optimal proportion of *relevant* studies (Humphrey, 2011).

Another indicator of a good quality meta-analysis is a presence of a measure to assess the quality of included studies. In the current meta-analysis a well-designed tool (RCT-Psychotherapy Quality Rating Scale) was used (Kocsis et al., 2010). The RCT-PQRS has been successfully used in the quality assessment in previous meta-analysis of clinical trials (e.g. Gerber et al., 2011; Thoma et al., 2012). In order to further enhance quality ratings for individual studies in the present meta-analysis an inter-rater reliability (IRR) was calculated, indicating that on average studies included in the current meta-analysis are of satisfactory quality.

4.3.2. Sampling strategy

The criteria for sample selection in the current meta-analysis warrant a more detailed consideration. Firstly, the selection criteria based on the characteristics of participants can be considered as a strength of the present meta-analysis. This is because these criteria are embedded in previous research into the illness perceptions in CHD (e.g. Foxwell et al., 2013; French et al., 2005). For example, studies that involved participants with heart failure were excluded because the content and types of illness perceptions is believed to be qualitatively different in participants with heart failure. (It is likely that these differences relate to a more sudden and acute onset and unpredictable course of the illness) (Goodman et al., 2013). However, participants with other types of CHDs (e.g. angina, MI or patients post-revascularisation procedures) are thought to develop closely-related illness perceptions (Goulding et al., 2010). It is, therefore, more appropriate to include patients with these types of CHDs under one umbrella of CHD. This was also successfully done in a previous meta-analysis (e.g. Whalley et al., 2014). Finally, the sampling strategy in the current meta-analysis based on characteristics of participants also allowed for the maintenance of the right balance between the sensitivity and specificity of the current meta-analysis (Humphrey et al., 2011).

On the other side, sampling of studies based of the types of outcomes might be considered to be a weakness and a source of marked amount of heterogeneity in the current meta-analysis. Firstly, different measures of assessing primary outcome were gathered together in the present meta-analysis.

While the majority of studies used different versions of the same measure (Illness Perception Questionnaire), other studies used different measures, e.g. York Angina Beliefs Questionnaire, (Furze et al., 2003). Accumulating findings in the meta-analysis based on different measures is considered to be controversial by some scholars who argue that it makes the comparisons across the studies more prone to errors (Lipsey & Wilson, 2001). On the other hand, excluding studies because of use of different measures of the same outcome can contribute to the omission of important studies and consequently incorrect conclusions (Borenstein et al., 2009). In the current meta-analysis a decision to gather data from different measures of the same outcome was balanced by the fact that measures were relatively well validated and matched to the characteristics of the participants. For example, the study by Lewin et al. (2002) used the York Angina Beliefs Questionnaire to test illness perceptions in participants with angina. In addition, in order to further counterbalance the use of different assessment methods in the same outcome within the present meta-analysis, effect sizes were presented using the same statistical metric i.e. Hedges's g, (Borenstein et al., 2009).

Furthermore, the individual studies in the current meta-analysis presented different types of data for primary outcome. For instance, while the study by Pfaeffli et al. (2015) presented a *total* score for the Brief- IPQ, a study by Petrie et al. (2002) provided scores for *each of the subscales* of IPQ separately. This is a source of heterogeneity in the current meta-analysis. In order to overcome these differences in data presentation a statistical procedure suggested by Borenstein et al. (2009) was adopted. In this procedure effect sizes are *combined* across

outcomes. While this procedure allowed for the studies with multiple outcomes to be included in the meta-analysis, it is possible that deriving *combined* effect sizes statistically, increased the amount of heterogeneity in the present meta-analysis.

The above-mentioned weaknesses in selecting studies based on the characteristics of outcomes might have been counterbalanced to a certain degree by better criteria in sampling based on the characteristics of interventions. Including studies with the RCT design improved the specificity of the current meta-analysis by restricting a number of irrelevant studies being identified. This strategy has an advantage of controlling for the potential sources of heterogeneity among studies (Humphrey, 2011). On the other hand, excluding studies with alternative designs (e.g. qualitative and quasi-experimental) could have potentially contributed to omitting some important studies. Given, however, the variability within the assessment measures of outcomes it seemed reasonable to limit other potential sources of the heterogeneity and focus on one type of design.

The sampling issues discussed above are common dilemmas for researchers conducting meta-analysis (Borenstein et al., 2009; Higgins & Green, 2011). The judgment decisions required to be made about the design of meta-analysis reflect the nature of meta-analytic design. Despite shortcomings in the design of the current meta-analysis, this investigation is unique as it is the first meta-analysis (to the author's knowledge) to attempt to qualitatively and systematically gather data from studies attesting to the efficacy of interventions deigned to change illness perceptions.

4.3.3. Sample size

Sample size is a relative weakness of the current meta-analysis, particularly in the context of the analysis being conducted within a random-effect model. The relationship between sample size and power in meta-analysis is influenced by multiple factors (Borestein et al., 2009). On the whole, larger samples are more desirable when conducting analysis within a random-effect model (Borenstein et al., 2009). This is because the random effect model estimates two sources of errors (within studies and between studies) (Borenstein et al. 2009).

In addition, the sample in the current meta-analysis is likely to be too small to detect meaningful difference for the subgroup analysis (Borenstein et al., 2009). Although the total sample of the current meta-analysis of 11 studies is relatively small in meta-analysis terms, it is still, nevertheless, one of the largest meta-analyses conducted examining interventions targeting maladaptive illness perceptions in CHD.

In the current meta-analysis, a total number of participants is also relatively small (N = 5,267) in meta-analysis terms. The minimal number of participants required to conduct meta-analysis is still disputed by scholars (Borenstein et al., 2009). While some argue that only meta-analysis with exceptionally large samples can yield meaningful results, others suggest that even with small samples meta-analysis can provide important insight into the particular phenomena (Borenstein et al., 2009; Eggar & Smith, 1997).

4.3.4. Statistical approach

Statistically combining the effect sizes for the subscales of illness perceptions questionnaire might be considered to be a weakness in the current meta-analysis. Whilst there is an ongoing debate whether combining across multiple outcomes is appropriate in the meta-analysis, Borenstein et al. (2009) argue that treating each of the outcomes separately is also problematic. Firstly, the subscales of the illness perception questionnaires are not independent of each other as they form part of the whole domain of illness perceptions. Therefore, it would have been inappropriate to separate the subscales. Secondly, treating each subscale as the separate outcome would have been inappropriate as studies with more outcomes (i.e. more subscale scores) would have had more weighting. This, in turn, would have led to an incorrect estimation of total effect size and its precision. In view of these arguments, it seemed more suitable to statistically *combine* effect sizes across subscales and to conduct the meta-analysis on *combined* effect sizes.

Choosing a random-effect model for statistical data analysis might be considered a strength as well as a weakness of the current meta-analysis. On one hand, the random effect model is more likely to address naturally occurring variations across studies (Borenstein et al., 2009). On the other hand, the application of the random effect model is sometimes considered to be controversial in meta-analysis with small number of studies (Borenstein et al., 2009). Despite a relatively small sample size in the current meta-analysis, a random-effect model seems appropriate because it provides more accurate

weighting of the effect sizes from individual studies. This is because there is a wide spread in the size of samples across the included studies (Borenstein et al., 2009).

4.3.5. Summary

Overall, the present meta-analysis has a number of methodological strengths. These strengths include, well-thought through process of literature search, pre-established criteria for data selection and extraction, and use of measures to control the sources of biases, such as quality assessment of individual studies, assessment of publication bias, and attempts to make contact with the experts in the field. However, there were also important weaknesses identified. The main methodological weakness that might have affected the interpretation of the findings from the current meta-analysis is the fact that the outcomes were *gathered* across different assessment measure and *combined* across outcomes. The lack of double coding of extracted data and relatively small sample size are also shortfalls in the present meta-analysis.

4.4. Implications

4.4.1. Clinical Implications

The findings of the current meta-analysis provide evidence that cardiac interventions lead to reduction of maladaptive illness perceptions. However, there is no evidence that interventions with clearly identifiable psychological

components report statistically significant improvements compared to interventions without psychological components. Considering findings from the current meta-analysis in the context of previous research and Leventhal's model of illness representations, the current findings gain more clinical meaning (Diefenbach & Leventhal, 1996). A clinical implication of the current findings are that cardiac rehabilitation programmes may benefit from being more explicitly linked with interventions designed to reduce distress. Traditionally cardiac rehabilitation focus on physical exercise, however, it is clear from the findings of the current meta-analysis that addressing patients' psychological needs is equally important and the target here may be missed. The lack of difference between psychological and non-psychological interventions raises important questions about the other potential factors that might influence treatment efficacy. Therapeutic techniques informed by motivational interviewing and/or cognitive behavioural therapy have been shown to be effective in treatment of a range of mental health difficulties related to physical health (Rollick, Miller & Butler, 2008; Taylor, 2006). It is possible that successful and skilful administration of such techniques might play an important role in the efficacy of cardiac treatments. Another implication for practice might be, therefore, that nurses and non-mental health professionals delivering these interventions might need additional training and appropriate supervision when using techniques to improve outcome. It is also possible that the successful administration of psychological techniques may require programmes to consist of a larger number of sessions or more

idiosyncratic approach (i.e. focus on individual patients' unhelpful illness perceptions) within each session.

The current meta-analysis demonstrates that interventions designed to change illness perceptions may contribute to positive changes in symptoms of anxiety, but not depression. Symptoms of depression might complicate a process of change and rehabilitation. This suggests that interventions designed to change illness perceptions ought to target depression as well as illness perceptions. Clinically, this might mean that patients presenting with symptoms of depression might need more multidisciplinary approach to treatment that can help them to build a more detailed and psychologically focused understanding of their illness.

Another issue emerging from the current findings is an impact of the type of illness on the efficacy of interventions designed to change maladaptive illness perceptions. An important clinical implication from the findings of the current meta-analysis is that patients with more acute illnesses are more likely to struggle to change the perceptions of their illness. This potentially might be an important factor to bear in mind when assessing patients' suitability for cardiac interventions and consequently predicting their prognosis. Patients with more acute illness might present with higher levels of distress and find it more difficult to engage in treatment. It seems, therefore, that patients' suitability for treatment should be preceded by thorough assessment of medical *and* psychological needs. Current guidelines for secondary care after MI suggest that treatment should take into consideration patients' psychological needs and that psychological therapy should not be offered routinely (NICE, 2013).

Finally, some potentially important clinical implications emerge from the methodological shortcomings of the studies included in the current meta-analysis. For example, it is possible that commonly used measures of assessment of symptoms of anxiety and depression (i.e. HADS) are not sensitive enough to reliably detect levels of distress in patients with CHD. It is possible that more population specific measures (e.g. Cardiac Depression Scale) might be more clinically helpful (Hare & Davies, 1996). Clinicians should, therefore, exercise caution when clinically interpreting the scores obtained by patients from measures such as HADS (Zigmond & Snaith, 1983).

4.4.2. Implications for Future Research

Taking into consideration the limited number of meta-analyses within the area of interventions designed to change illness perceptions in CHD there are a number of potential avenues for future empirical research and meta-analyses. Future research could be enhanced by addressing some of the methodological shortcomings and ambiguities identified in the current meta-analysis.

First the adoption of a consensus measure for indexing illness representations and the same for when measuring anxiety and depression comorbid with CHD is urgently required.

Secondly, outcomes from the analysis of publication bias in the current meta-analysis indicate missing studies that may have important information. Thus it is suggested that negative results publishing needs to be brought forward by researchers.

Thirdly, future meta-analyses could be significantly enhanced by including more studies. This could be done in a number of ways. For example, more single studies into the efficacy of interventions designed to change illness perceptions need to be conducted. Additionally, the inclusion/exclusion criteria could also be adjusted in order to enhance sample size.

Finally, future research should focus on further clarifying which components of cardiac interventions designed to change illness perceptions are likely to contribute to the largest changes in illness perceptions. Another research question that could be asked is whether interventions with psychological components lead to more reduction in symptoms of depression and anxiety compared to interventions without clearly identifiable psychological component. In future, meta-analyses might also focus on establishing whether other factors (e.g. socio-demographic, type of illness) have an impact on the efficacy of interventions designed to change illness perceptions.

4.5. Overall Conclusions

The aim of this meta-analysis is to gather evidence from single studies in order to assess whether interventions with psychological component are more effective in changing dysfunctional illness perceptions than interventions without psychological component. The current meta-analysis also aims at establishing clearer picture of how interventions designed to change unhelpful illness perceptions contribute to changes in symptoms of anxiety and depression. The final goal of the current meta-analysis is to determine whether factors, such as age

and the type of illness play a role in the efficacy of interventions designed to change maladaptive illness perceptions. The current meta-analysis proposes that answering these research questions was important because it would shed a better light on the mechanisms of change in interventions designed to change maladaptive illness perception in CHD.

Overall, there was a mixed pattern of results. The interventions included in the meta-analysis yielded a small effect in terms of reducing maladaptive illness perceptions. However, there are no statistically significant differences in the effect sizes reported for cardiac interventions with psychological components compared to those interventions without psychological component.

Additionally, the current meta-analysis confirms that interventions designed to change illness perceptions lead to positive changes in symptoms of anxiety, but not depression. While the effect size for symptoms of anxiety was significantly positive, the effect size for symptoms of depression was negative and non-significant. This might indicate that there is a more direct link between maladaptive illness perceptions and anxiety.

Furthermore, the current meta-analysis does not demonstrate any link between age and changes in maladaptive illness perceptions. The link between changes in dysfunctional illness perceptions and the type of illness was also unclear. It is possible that participants with acute CHD have a greater perception of threat which might make the illness perceptions more resistant to change.

The current findings, however, need to be interpreted with caution because of some important methodological shortcomings. These shortcomings include

relatively small sample size (in meta-analysis terms), large amount of dispersion among the obtained effect sizes and inconsistent in reporting of data across single studies.

5. Ethical Considerations

This chapter provides a brief description of ethical issues pertinent for the current meta-analysis.

The considerations of ethical issues in the present meta-analysis relates to maintaining high levels of integrity and adequacy in reporting data. This is important because it is generally believed that the findings from the meta-analysis carry more weight and offer more accurate estimates of the importance of the particular research area (Rosenberg, 1994). The author of the current meta-analysis made every effort to select, report and analyse data in a transparent format. Additionally, the design and the execution of current meta-analysis is guided by a number of pre-existing formal recommendations on how to conduct high quality meta-analysis, such as the Cochrane guidelines and the PRISMA principles (Higgins & Green, 2011; Moher et al., 2009). These guidelines help in ensuring that high standard was maintained in collection, selection and analysis of data.

Finally, wherever possible the process of selecting data was monitored by employing additional quality assurance measures, such as employment of the RCT-PQRS for individual studies and deriving an index of inter-rater reliability (Koscis et al., 2010).

6. References

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Appendix A Strategy for Literature Search

Overall, the search strategy focused on identifying relevant literature for links between illness perceptions and psychological outcomes and interventions designed to change illness perceptions. An additional search was conducted to identify previous systemic reviews and meta-analysis pertinent to the main research questions. The main databases searched included PsycInfo, Medline, Cochrane's Library and EMBASE. The search covered the period from 1970 to July 2014. The above-mentioned databases were chosen because they broadly cover the fields of psychiatry and (clinical health) psychology, which were relevant for the current research questions.

The core search terms focused around identifying literature relevant to illness perceptions, Coronary Heart Disease, psychological interventions, psychological outcomes (e.g. anxiety and depression) and meta-analysis/systemic reviews. For example, in order to identify relevant literature for illness perceptions terms, such as illness misconceptions, illness cognitions, illness expectations and maladaptive thinking were also included. The core terms for 'psychological interventions' also included terms such as cardiac rehabilitation/programme/management, cognitive behavioural therapy, psychotherapy, behavioural therapy. Finally, in order to identify studies conducted with participants with relevant illness, terms such as angina, myocardial infraction, percutaneous coronary intervention and coronary artery bypass graft were included. If and when relevant, terms were combined using Boolean

operators (‘AND’ and ‘OR’) and wildcard symbol (*). This strategy ensured that the optimal number of relevant studies was identified. Further screening and selection of relevant literature involved screening titles and abstracts and obtaining full texts of relevant papers.

In additional, the list of references and relevant journals were also searched. These journals included Journal of Advance Nursing, Heart, and Cardiovascular Disorders.

In total, eight studies investigating the link between illness perceptions and psychological outcomes and six studies testing the efficacy of interventions designed to change illness perceptions were retrieved. Finally, five relevant meta-analysis and systemic reviews were identified.

Appendix B

Table A1.1

Studies of illness perceptions and psychosocial outcomes in CHD (in alphabetical order).

Study authors, publication year, country	**Sample characteristics (size, sex, age, CHD type)**	**Design/Length of Follow-up**	**Outcomes Assessed**	**Results**
Byrne, Walsh, & Murphy (2005), Ireland	N=1084, 1047/564 (M/F), Broad spectrum of CHD	Cross-sectional	Illness Perceptions (IPQ-R) Health related behaviours (exercise, smoking, alcohol, diet Medication adherence (Medication Adherence Report Scale 5)	Participants with more emotional representations of their illness, perception of poorer personal control over their illness and treatment were less likely to engage in physical exercise. Chronic perception of illness was related to medication adherence.
Furze, Lewin, Murberg, Bull, & Thompson (2005), UK	N=131, 81/52(M/F), MI	Cross-sectional, 1 year follow-up	Maladaptive Illness (York Angina Beliefs Questionnaire) Depression (HADS) Anxiety (HADS) Physical Functioning (Seattle Angina Questionnaire)	Participants with more maladaptive beliefs about angina were more anxious and depressed, and reported more physical disabilities. Changes to maladaptive beliefs were associated with improvements in physical functioning and fewer depressive and anxiety symptoms.

Grace et al. (2005), Canada	N=661,504/157(M/F), ACS	Cross-sectional	Illness Perception (IPQ-R) Depression (HADS) Functional Capacity (Duke Activity Status Index)	Chronic illness time course, greater consequences and perceived lower control over treatment and cure was linked with higher depressive symptoms for man. Perceived chronic, longer illness was linked with higher depressive symptoms in woman.
Greco et al. (2014), Italy	N=75, 60/15(M/F), Heart Disease	Cross-sectional, 2 month follow-up	Illness Perceptions (Brief Illness Perceptions Questionnaire) Depression (Cognitive Behavioural Assessment)	Participants with stronger perceptions of symptoms intensity reported more symptoms of depression and this was sustained over time.
Juergens, Seekatz, ,Moosdorf, Petrie & Rief, (2010) Germany	N=56, 44/12 (M/F), patients undergoing CABG	Cross-sectional, 3 month follow-up	Illness beliefs (IPQ-R) Depression (HADS) Health-Related Quality of Life (Short Form-12)	Participants who perceived their illness as more chronic and with more serious personal consequences & who were more distressed by their illness were more likely to report higher levels of physical disability quality of life) (poorer and depressive symptoms.

Stafford, Berk, & Jackson, (2009) Australia	N=193, 156/37(M/F), CAD	Cross-sectional, 9months follow-up	Illness Perceptions (IPQ-R) Depression (HADS) Health-Related Quality of Life (HRQoL) (Short Form-36)	More depressive symptoms were linked with perception of poorer personal control and greater personal consequences of the illness. Improved HRQoL was associated with fewer beliefs about negative consequences of illness and less chronic illness course.
Steca et al. (2013), Italy	N=172, 131/41 (M/F), Heart Disease	Cross-sectional	Illness Perceptions (Brief-IPQ) Depression (Cognitive Behavioural Assessment)	Participants with more negative illness perceptions reported more depressive symptoms.
Whitmarsh, Koutantji, & Sidell (2003) UK	N=93, 71/22(M/F), MI	Cross-sectional	Illness perception (IPQ) Depression(HADS) Anxiety(HADS) Coping (Coping Orientation to Problems Experienced)	Attenders obtained higher scores on identity consequence & causal attributions subscale of IP Attenders scored higher on depression & anxiety & used more emotion-focused problem solving strategies.

HADS = Hospital Anxiety and Depression Scale; IPQ = Illness Perception Questionnaire; IPQ-R = Illness Perceptions Questionnaire-Revised; Brief-IPQ = Brief Illness Perceptions Questionnaire

Appendix C

Table A2.1.

Details of contacts made with scientists/researchers requesting additional information (presented in chronological order).

Authors contacted (Study ID if appropriate)	**Date Contacted**	**Reason for contact**	**Response/ outcome**
Prof John Weinman	21/08/2014	Contacted as an expert in the area, asking if he was aware of any pertinent papers that are ongoing trials or were due to be published	No response
Prof Robert Lewin (Study 10)	21/10/2014	Request for clarification on measure of illness perception	Author replied with additional information
Leila Pfaeffli (study 01)	16/11/2015	Clarification on measure of illness perception	Author replied with additional information
Dr Elizabeth Broadbent	Contact from 06/10/2015 to 27/12/2015	Request for additional data for papers: (1) Broadbent, E., Ellis, C. J., Thomas, J., Gamble, G., & Petrie, K. J. (2009). Further development of an illness perception intervention for myocardial infarction patients: a randomized controlled trial. Journal of Psychosomatic Research, 67(1), 17–23.	No data provided, studies excluded

		(2) Broadbent E., Leggat, A., McLachlan, A., & Kerr, A. (2013). Providing carsiovascular risk management information to acute coronary sundrome patients: A Randomised Trial. British Journal of Health Psychology, 18, 83-94.	
Dr Elizabeth Barley, Prof. Anthony Mann Prof. Andre Tylee (study 02)	18/12/2015 07/01/2016	Request for additional data for illness perception measure	Authors responded; Unable to get data

Appendix D

CODING MANUAL[1]

Reference: write a complete reference in APA format.

Write study id in the format 00.

Study ID: Write study id in the format 00. Assign a unique identification number to each study. If the report contains two independent studies than add a decimal to the study ID to distinguish between studies and code each studies independently.

Type of publication: specify the type of publication

1) journal article
2) book chapter
3) thesis or doctoral dissertation
4) conference paper
5) unpublished data provided by authors
6) other (specify:_____)

Year of publication: write publication year

Study details

Study design:

1) *Experimental RCT design* randomly assigns individuals to groups and randomly assigns the groups to treatment or control
2) *Longitudinal* follows individuals over a period of time and it involves collecting outcome data at specified time points or continuously.

[1] Adopted from Lipsey and Wilson (2001). Informed by the COONSORT 2010 checklist of information to include when reporting a randomised trial and Cochrane Collaboration guidelines

3) *Cross-sectional design* involves collecting data from group(s) of individuals at a single time point.

Age range: if not provided by authors put N/A

Mean and SD age of participants: if not provided by authors put N/A; note data provided for experimental and control group and total sample.

Type of coronary heart disease of participants: specify which one of the following types of CHD was included in the report. There can be more than one category within each study. This grouping of CHD has been described by Capewell and colleagues (2008) and it describes medical conditions that involve narrowing of coronary arteries through gradual build-up of fatty material (atheroma) within their walls. If the report does not specify put not specified.

1) angina
2) myocardial infraction (MI)
3) acute coronary syndrome
4) coronary artery disease (CAD)
5) conditions that require revascularisation procedures: coronary artery bypasses graft (CABG) and percutaneous coronary interventions (PCI).
6) broad category of CHD
7) Not specified

Type of participants' illness: specify the type of participant's illness. The type of the illness is determined based on the authors reporting the following information:

a) At least four cardiovascular risk factors reported, e.g. current smoker, high cholesterol, hypertension, diabetes, high BMI, family history of CHD

b) Current medication intake, e.g. beta-blockers, statins, ACE inhibitors, nitrates
c) Hospitalization
d) Symptoms frequency, e.g. frequency of angina episodes per week
e) Formal classification of severity of cardiac problems , such as New York Heart Association Functional Classification or Canadian Cardiovascular Society Grading of Angina

If information is not provided, write N/A

1) Acute (code acute if at least two of the above categories is reported in the study)
2) Chronic

Total Sample Size, sample size in treatment group and sample size in control group: specify a sample size reported at the start of the study. At the later time you are asked to code the sample size relevant to the outcome data that are reported.

Type of intervention startegies included. There is a great variability within the content and the intervention startegies across studies of interventions targeted at changing illness perceptions in cardiac problems. I will first provide general description of each category and then give specific examples of commonly encounter intervention startegies and how they would be coded. Typically, there

will be multiple strategies used within a given study. This coding category refers to the collective use of intervention startegies and NOT each strategy separately.

1) *Psycho-education only:* This category refers to didactic/educational information giving to participants as a main or dominant intervention strategy. Common subjects covered in psycho-educational interventions in the cardiac area include: risk factors, impact of the life style, explanation of symptoms. Studies coded in this category can also include group discussions, physical exercise, and/or skill-building techniques.
2) *Psycho-education combined with counselling techniques:* This category reefers to startegies that combine information giving with basic counselling techniques, such as active listening, supportive talking and/or encouragement
3) *Psycho-education combined with cognitive-behavioural (CBT) or motivational interviewing techniques:* This category refers to strategies that combine the techniques focused on information giving (didactic component) with the skill-based interventions, such as the techniques based on the CBT therapies (e.g. (progressive muscular) relaxation, cognitive restructuring, goal orientated problem solving, self-monitoring of poor lifestyle habits. This can be delivered in the face to face, group or telephone format.
4) *Interaction or non-psycho-educational contact with medical professionals (e.g. cardiologist or cardiac CNSs).* This category refers to the

individual/group contact with non-mental health professional that involves standard medical review and is focused on the interactive approach to intervention without a clearly identifiable psych-education component.

5) *Telephone follow-up:* This refers to any (e.g. reinforcing) telephone contacts with any health professional that is done either in between the sessions or post-intervention.

Examples of common intervention startegies used and how they would typically be coded:

i. Explanation of the pathophysiology of specific condition (e.g. myocardial infraction) and/or explanation of common symptoms → coded as a *psycho-education only* category.

ii. Addressing illness perceptions using encouragement or active listening, verbal praise for appropriate behaviour,

iii. Addressing common misconceptions about different aspects symptoms/illness/medical procedures, broadening patients understanding of causes and consequences of the illness, relaxation exercise, active exercises that require patients to think about how changing life style factors could contribute to developing an illness, developing plans of minimizing future risks, or balancing prons and cons of change→ this should be coded as *psycho-education combined with cognitive-behavioural (CBT) techniques or motivational interviewing* category. However, when a specific component (e.g. physical exercises) is not based

on the CBT or motivational interviewing principles and do not involve information giving, it should be coded as one of the other options (whichever is deemed more appropriate) .

iv. Giving (medical) advice and medical consultations and taking physical measures, such as BMI or ECG, should be coded as *interaction or non-psycho-educational contact with medical professionals*

v. Reinforcing in-between sessions or post-discharge telephone calls will be coded as *telephone follow-ups*.

Confidence of judgement on the intervention startegies included. The rating is based on the level of details provided by a given study's authors and how explicitly this information is provided. If the topics covered in each session are described in details and explicitly than this should increase the confidence of the rating.

Theoretical framework: Specify the theoretical framework of the intervention. If the authors do not provide this information or it is unclear from the description of the intervention select option 3.

Nature of comparison group: This category refers to the type of comparison group employed in the research study.

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1) *No treatment control:* This category is selected when there is no evidence of any kind of control group during or afterwards of the study.
2) *Delayed control (waiting list):* This category is selected when a control group is a sub-sample placed on the waiting list for intervention or when the contact is limited to screening or application only.
3) *Alternative treatment:* This category indicates that an alternative intervention (medical and non-medical) was administered to control group. This could include provision of psycho-education materials (giving leaflets or brochures), treatment as usual and/or routine medical care. It is important that the nature of the treatment is described.

Mode of delivery: This item refers to the way that the intervention is delivered. Select one or more of the following delivery modes:

1) Individual
2) Group
3) Face to face
4) telephone

Setting of intervention: specify where the intervention took place: inpatient, outpatient or both settings. If one of the sessions, for example first one, took place before discharge and the remaining were conducted at patient's home or at the outpatient hospital clinic than code 3)

1) inpatient
2) outpatient
3) inpatient and outpatient

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Duration of intervention: This category refers to a number of sessions and the time intervals between sessions at which the intervention was presented. Select of one the following categories:

1) *Single session*: choose this category if the intervention consisted of only one session
2) *Multiple discrete sessions:* choose this category if the intervention was delivered over more than one session at unequal time intervals between sessions or duration of the session varied form session to session. For example, six sessions with weekly and fortnightly time intervals over eight weeks.
3) *Continuous programme:* choose this category if the intervention was delivered continuously for a given period of time. For example, an interventions with one hour-long session on weekly basis for eight weeks.

Number of intervention sessions: This item refers to a total number of sessions of a given intervention. Specify a number.

Follow up presented: tick one of the boxes (yes/no) to specify if the follow up was present

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Timings of the follow up: write the period of time(s) at which follow-up(s) have taken time. For example, 6 months and 12 months post intervention. Report the study length in time units (weeks/months) reported by the authors of the report

Type of professionals delivering the intervention: Select one or both of the following options:

1) *mental health professional*: refers to those professionals who have mental health training background, e.g. councillor, psychologist or mental health nurses.
2) *non-mental health professional* : refers to those professionals who do not have training in mental health, e.g. cardiac CNS or cardiologist.

Recruitment: Total length of the study: This item refers to the total length of the study including any follow-up periods. Write the study length in time units (weeks/months) reported by the authors of the study.

Randomisation: This item refers to method used to generate the random allocation sequence. Provide information about how the randomisation was done and what methods have been used.

Allocation concealment mechanism: This item refers to the mechanisms used to implement the random allocation sequence. Provide information on how the allocation concealment was managed in the study and how it was implemented

(e.g. who generated the random allocation sequence, who assigned participants to interventions)

Blinding: Tick one of the boxes to specify if the blinding was done. Provide any additional information about blind procedure (e.g. who was blinded and how)

Empirical findings of the study/effect size information

Participant flow chart presented? Tick one of the boxes (yes/no) to specify if the participant flow chart was presented for each group

Relevant information presented in the participant flow chart? Tick one of the boxes (yes/no) to specify if the flow chart contained all necessary and relevant information (i.e. the number of participants who were randomly assigned in each group, received intended treatment and were analysed for primary outcome).

Equivalent of scores between groups at baseline tested: Note whether the data analysis included tests for equivalent of scores between groups at baseline. Typically *F* or *t* statistical tests are usually used to test for equivalence. If differences were found select one of the following options and provide brief information:

Differences considered, tested and judged as statistically non-significant
Differences found, statistically significant and meaningful

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Accounting for baseline scores*:* Report whether the authors used statistical techniques that accounted for baseline difference. If the authors accounted for baseline difference selects one of the following options:

With gains scores or change scores between pre- and post-test *(the most common test employed here is t test of F test)*
By using covariate analysis (e.g. ANCOVA or MANCOVA)
By using repeated measures analysis (e.g. MANOVA)
Other (specify:_____)

Note that direct comparison of pre- and post-tests scores is not considered to account for baseline differences, but it is one of the most common types of data analysis.

Primary Outcome variable 1: Write the name of the variable

Name of Measure: write the name of the measure used

Data available: Select on or both of the following options for what data are available in the report:

1) Post-test
2) Follow up

Type of outcome data presented: Select one of the following options:

1) means and sds
2) t value or F-value
3) p value
4) chi-square
5) frequencies or proportions
6) effects size (what type): _____

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This data is the data on which the effect size is based on.

Page Number or Table where raw data are found: Write where the information can be found

Treatment group sample size and control Group sample size: Write in the number a sample size for each group

Raw data favours: Select one of the following options to specific which of the groups is favoured by raw data:

1) Treatment group
2) Control group
3) Neither (groups are equal)
4) Cannot tell or statistically insignificant report only

Provide values for raw data on mean, standard deviations (SDs), proportions or frequencies for treatment and control groups.

Significance test: Write a numerical value for the significance test used in the study. It could be one of the following options:

1) t-value ____
2) F-value (df for the numerator must be 1) _____
3) Chi-square value (df = 1) _____

Effect size calculated: Write down the numerical value of the effect size calculated and the confidence intervals for it.

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Secondary outcome variable 1, secondary outcome variable 2, secondary outcome variable 3, and secondary outcome variable 4: If the study tested more than one secondary outcome specified by this meta-analysis than repeat the process of data recording similar to the primary outcome.

Appendix E

CODING FORM[1]

Reference:

Study ID:

Type of Publication:

7) journal article
8) book chapter
9) thesis or doctoral dissertation
10) conference paper
11) unpublished data provided by authors
12) other (specify(:_____

Year of publication: ___

Study details

Study design:

1) Experimental (RCT)
2) Longitudinal
3) Cross-sectional

Age Range of Participants:

Mean and SD Age of Participants: M= ____; SD=___

Gender of Participants:

females and males ☐ males only ☐ females only ☐

[1] Adopted from Lipsey and Wilson (2001). Informed by the CONSORT 2010 checklist of information to include when reporting a randomised trial and Cochrane Collaboration guidelines

Type of coronary heart disease (CHD) of participants:

1) angina
2) myocardial infraction (MI)
3) acute coronary syndrome
4) coronary artery disease (CAD)
5) conditions that require revascularisation procedures: coronary artery bypasses graft (CABG) and percutaneous coronary interventions (PCI).
6) broad category of CHD
7) Not specified

Type of participants' illness:

1) Acute
2) Chronic

Total Sample Size: N=____
Sample Size in treatment group: N= ____
Sample Size in Control group: N=_____

Type of Interventions startegies included in treatment:

1) psycho-education only
2) psycho-education combined with counselling techniques
3) psycho-education combined with cognitive-behavioural and/or motivational interviewing techniques
4) interaction or non psycho-educational contact with medical health professionals (e.g. cardiologist or cardiac CNS)
5) telephone follow up
6) information not provided

Confidence of judgment on intervention strategies included (based on level of details provided):

1) very low
2) low
3) moderate
4) high
5) very high

Theoretical framework:

1) Leventhal's self-regulation model of illness behaviour
2) Other (specify_____)
3) Not specified

Nature of comparison group:

1) no treatment control
2) delayed control (waiting list)

3) alternative treatment (specify:_____)

Mode of delivery:

1) individual
2) group
3) face to face
4) telephone

Setting of intervention:

4) inpatient
5) outpatient
6) inpatient and outpatient

Duration of intervention:

1) single session
2) multiple discrete sessions
3) continuous programme

Number of intervention sessions (specify): _____

Follow up present: YES ☐ NO ☐
Timings of the follow up (specify): _____

Type of the professional delivering the intervention:

3) mental health professional (specify_____)
4) non-mental health professional (specify_____)

Recruitment: Total length of study (including follow up):
_____

Randomisation (specify method used for randomisation:
_____)

Allocation concealment mechanism (specify mechanism used to implement the random allocation sequence_____)
Blinding:

Yes ☐ No ☐

(specify how blinding was done and at what level_____)

Empirical findings of the study/effect size information

Was the participant flow chart presented?

Yes ☐ No ☐

Was all the relevant information presented in the participant flow chart?
Yes ☐ No ☐

Was the equivalence of scores between groups at baseline tested?

1) No
2) Yes: differences considered, tested and judged as statistically non-significant
3) Yes: differences found, statistically significant and meaningful (explain further: _____ _____ _____)

Did analysis account for baseline scores?

1) No
2) Yes with gains scores or change scores
3) Yes by using covariate analysis (e.g. ANCOVA)
4) Yes by using repeated measures analysis (e.g. MANCOVA)
5) Other (specify:_____)

Results

Appendix E

Primary Outcome variable 1: _____(variable name)

Name of Measure used: _____

Data Available for:

3) Post-test
4) Follow up

Type of Outcome Data Presented (circle all that apply):

7) means and sds
8) t value or F-value
9) p value
10) chi-square
11) frequencies or proportions
12) effects size (what type): _____

Page Number or Table where raw data are found: _____

Treatment group sample size: N =___

Control Group sample size: N =_____

Raw data favours:

5) Treatment group
6) Control group
7) Neither
8) Cannot tell or statistically insignificant report only

Post-test data (e.g. M, SDs, proportions or frequencies)	
Treatment group	Control group

Significance test:

4) t-value ____
5) F-value (df for the numerator must be 1) _____
6) Chi-square value (df = 1) _____

Appendix E

Effect size calculated:

1) Effect size (specify): _____
2) Confidence interval (Specify): _____

Secondary outcome variable 1: _____ (variable name)

Name of Measure used: _____

Data Available for:

1) Post-test
2) Follow up

Type of Outcome Data Presented (circle all that apply):

1) means and sds
2) t value or F-value
3) p value
4) chi-square
5) frequencies or proportions
6) effects size (what type): _____

Page Number or Table where raw data are found: _____

Treatment group sample size: N =___

Control Group sample size: N =_____

Raw data favours:

1) Treatment group
2) Control group
3) Neither
4) Cannot tell or statistically insignificant report only

Post-test data (e.g. M, SDs, proportions or frequencies)	
Treatment group	Control group

Significance test:

1) t-value _____
2) F-value (df for the numerator must be 1) _____
3) Chi-square value (df = 1) _____

Appendix E

Effect size calculated:

1) Effect size (specify): _____
2) Confidence interval (Specify): _____

Secondary outcome variable 2: _____ (variable name)

Name of Measure used: _____

Data Available for:

1) Post-test
2) Follow up

Type of Outcome Data Presented (circle all that apply):

1) means and sds
2) t value or F-value
3) p value
4) chi-square
5) frequencies or proportions
6) effects size (what type): _____

Page Number or Table where raw data are found: _____

Treatment group sample size: N =___

Control Group sample size: N =_____

Raw data favours:

1) Treatment group
2) Control group
3) Neither
4) Cannot tell or statistically insignificant report only

Post-test data (e.g. M, SDs, proportions or frequencies)	
Treatment group	Control group

Significance test:

1) t-value _____
2) F-value (df for the numerator must be 1) _____

3) Chi-square value (df = 1) _____

Effect size calculated:

1) Effect size (specify): _____
2) Confidence interval (Specify): _____

Secondary outcome variable 3: _____ (variable name)

Name of Measure used: _____

Data Available for:

1) Post-test
2) Follow up

Type of Outcome Data Presented (circle all that apply):

1) means and sds
2) t value or F-value
3) p value
4) chi-square
5) frequencies or proportions
6) effects size (what type): _____

Page Number or Table where raw data are found: _____

Treatment group sample size: N =___

Control Group sample size: N =____

Raw data favours:

1) Treatment group
2) Control group
3) Neither
4) Cannot tell or statistically insignificant report only

Post-test data (e.g. M, SDs, proportions or frequencies)	
Treatment group	Control group

Appendix E

Significance test:

1) t-value _____
2) F-value (df for the numerator must be 1) _____
3) Chi-square value (df = 1) _____

Effect size calculated:

1) Effect size (specify): _____
2) Confidence interval (Specify): _____

Secondary outcome variable 4: _____ (variable name)

Name of Measure used: _____

Data Available for:

1) Post-test
2) Follow up

Type of Outcome Data Presented (circle all that apply):

1) means and sds
2) t value or F-value
3) p value
4) chi-square
5) frequencies or proportions
6) effects size (what type): _____

Page Number or Table where raw data are found: _____

Treatment group sample size: N =___

Control Group sample size: N =____

Appendix E

Raw data favours:

1) Treatment group
2) Control group
3) Neither
4) Cannot tell or statistically insignificant report only

Post-test data (e.g. M, SDs, proportions or frequencies)	
Treatment group	Control group

Significance test:

1) t-value _____
2) F-value (df for the numerator must be 1) _____
3) Chi-square value (df = 1) _____

Effect size calculated:

1) Effect size (specify): _____
2) Confidence interval (Specify): _____

Appendix F

RCT of Psychotherapy Quality Rating Scale (RCT-PQRS)[1]

Description of subjects

Item 1. Diagnostic method and criteria for inclusion and exclusion
0 = poor description and inappropriate method/criteria
1 = full description or appropriate method/criteria
2 = full description and appropriate method/criteria

Item 2.Documentation or demonstration of reliability of diagnostic methodology

0 = poor or no reliability documentation
1 = brief reliability documentation (documentation in the literature is sufficient, even if it is not explicitly cited)
2 = full reliability documentation (documentation of within-study reliability necessary)

Item 3. Description of relevant comorbidities

0 = poor or no description of relevant comorbidities
1 = brief description of relevant comorbidities
2 = full description of relevant comorbidities

Item 4. Description of numbers of subjects screened, included, and excluded

0 = poor or no description of numbers screened, included, and excluded
1 = brief description of numbers screened, included, and excluded
2 = full description of numbers screened, included, and excluded

Definition and delivery of treatment

Item 5. Treatment(s) (including control/comparison groups) are sufficiently described or referenced to allow for replication

0 = poor or no treatment description or references
1 = brief treatment description or references (also if full description of one group and poor description of another)
2 = full treatment description or references (manual not required)

Item 6. Method to demonstrate that treatment being studied is treatment being delivered (only satisfied by supervision if transcripts or tapes are explicitly reviewed)

0 = poor or no adherence reporting

1 = brief adherence reporting with standardized measure or full adherence reporting with non-standardized measure (eg, non-independent rater)
2 = full adherence reporting with standardized measure (must be quantitative and completed by an independent rater)

Item 7. Therapist training and level of experience in the treatment(s) under Investigation

0 = poor description and underqualified therapists
1 = full description or well-qualified therapists
2 = full description and well-qualified therapists

Item 8. Therapist supervision while treatment is being provided

0 = poor description and inadequate therapist supervision
1 = full description or adequate therapist supervision
2 = full description and adequate therapist supervision

Item 9. Description of concurrent treatments (eg, medication) allowed and administered during course of study (if patients on medication are included, a rating of 2 requires full reporting of what medications were used; if patients on medications are excluded, this alone is sufficient for a rating of 2).

0 = poor or no description of concurrent treatments
1 = brief description of concurrent treatments
2 = full description of concurrent treatments

Outcome measures

Item 10. Validated outcome measure(s) (either established or newly standardized)

0 = poor or no validation of outcome measure(s)
1 = brief validation of outcome measure(s) (shown or cited)
2 = full validation of outcome measure(s) (shown or cited)

Item 11. Primary outcome measure(s) specified in advance (although does not need to be stated explicitly for a rating of 2)

0 = poor or no specification of primary outcome measure(s) in advance
1 = brief specification of primary outcome measure(s) in advance
2 = full specification of primary outcome measure(s) in advance

Appendix F

Item 12. Outcome assessment by raters blinded to treatment group and with established reliability

0 = poor or no blinding of raters to treatment group (eg, rating by therapist, nonblind independent rater, or patient self-report) and reliability not reported
1 = blinding of independent raters to treatment group or established reliability
2 = blinding of independent raters to treatment group and established reliability

Item 13. Discussion of safety and adverse events during study treatment(s)

0 = poor or no discussion of safety and adverse events
1 = brief discussion of safety and adverse events
2 = full discussion of safety and adverse events

Item 14. Assessment of long-term post-termination outcome (should not be penalized for failure to follow comparison group if this is a waitlist or non-treatment group that is subsequently referred for active treatment)

0 = poor or no post-termination assessment of outcome
1 = medium-term assessment of post-termination outcome (2-12 months posttermination)
2 = long-term assessment of posttermination outcome (≥12 months posttermination)

Data analysis

Item 15. Intent-to-treat method for data analysis involving primary outcome measure

0 = no description or no intent-to-treat analysis with primary outcome measure
1 = partial intent-to-treat analysis with primary outcome measure
2 = full intent-to-treat analysis with primary outcome measure

Item 16. Description of dropouts and withdrawals

0 = poor or no description of dropouts and withdrawals
1 = brief description of dropouts and withdrawals

2 = full description of dropouts and withdrawals (must be explicitly

Item 17. Appropriate statistical tests (e.g., use of Bonferroni correction, longitudinal data analysis, adjustment only for a priori identified confounders)

0 = inappropriate statistics, extensive data dredging, or no information about appropriateness of statistics
1 = moderately appropriate, though unsophisticated, statistics and/or moderate data dredging
2 = fully appropriate statistics and minimal data dredging in primary Findings

Item 18. Adequate sample size

0 = inadequate justification and inadequate sample size
1 = adequate justification or adequate sample size
2 = adequate justification and adequate sample size

Item 19. Appropriate consideration of therapist and site effects

0 = therapist and site effects not discussed or considered
1 = therapist and site effects discussed or considered statistically
2 = therapist and site effects discussed and considered statistically

Treatment assignment

Item 20. A priori relevant hypotheses that justify comparison group(s)
0 = poor or no justification of comparison group(s)
1 = brief or incomplete justification of comparison group(s)
2 = full justification of comparison group(s)

Item 21. Comparison group(s) from same population and time frame as experimental group

0 = comparison group(s) from significantly different population and/or time frame
1 = comparison group(s) from moderately different population and/or time frame
2 = comparison group(s) from same population and time frame

Item 22. Randomized assignment to treatment groups

0 = poor (eg, pseudo-randomization, sequential assignment) or no randomization
1 = adequate but poorly defined randomization procedure
2 = full and appropriate method of randomization performed after screening and baseline assessment

Overall quality of study

Item 23. Balance of allegiance to types of treatment by practitioners

0 = no information or poor balance of allegiance to treatments by study therapists (eg, therapy in experimental and control groups both administered by therapists with strong allegiance to therapy being tested in the experimental group)
1 = some balance of allegiance to treatments by study therapists
2 = full balance of allegiance to treatments (eg, therapies administered by therapists with allegiance to respective techniques)

Item 24. Conclusions of study justified by sample, measures, and data analysis, as presented (note: useful to look at conclusions as stated in study abstract)

0 = poor or no justification of conclusions from results as presented or insufficient information to evaluate (eg, sample or treatment insufficiently documented, data analysis does not support conclusions, or numbers of withdrawals or dropouts makes findings unsupportable)
1 = some conclusions of study justified or partial information presented to evaluate
2 = all conclusions of study justified and complete information presented to evaluate

Item 25. Omnibus rating: please provide an overall rating of the quality of the study, taking into account the adequacy of description, the quality of study design, data analysis, and justification of conclusions.

1 = exceptionally poor
2 = very poor
3 = moderately poor
4 = average
5 = moderately good
6 = very good
7 = exceptionally good