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Dennis JA, Khan O, Ferriter M, Huband N, Powney MJ, Duggan C

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[Intervention Review]

Psychological interventions for adults who have sexually offended or are at risk of offending

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ABSTRACT

Background

Sexual offending is a legal construct that overlaps, but is not entirely congruent with, clinical constructs of disorders of sexual preference. Sexual offending is both a social and a public health issue. Victim surveys illustrate high incidence and prevalence levels, and it is commonly accepted that there is considerable hidden sexual victimisation. There are significant levels of psychiatric morbidity in survivors of sexual offences.

Psychological interventions are generally based on behavioural or psychodynamic theories.

Behavioural interventions fall into two main groups: those based on traditional classical conditioning and/or operant learning theory and those based on cognitive behavioural approaches. Approaches may overlap. Interventions associated with traditional classical and operant learning theory are referred to as behaviour modification or behaviour therapy, and focus explicitly on changing behaviour by administering a stimulus and measuring its effect on overt behaviour. Within sex offender treatment, examples include aversion therapy, covert sensitisation or olfactory conditioning. Cognitive behavioural therapies are intended to change internal processes - thoughts, beliefs, emotions, physiological arousal - alongside changing overt behaviour, such as social skills or coping behaviours. They may involve establishing links between offenders' thoughts, feelings and actions about offending behaviour; correction of offenders' misperceptions, irrational beliefs and reasoning biases associated with their offending; teaching offenders to monitor their own thoughts, feelings and behaviours associated with offending; and promoting alternative ways of coping with deviant sexual thoughts and desires.

Psychodynamic interventions share a common root in psychoanalytic theory. This posits that sexual offending arises through an imbalance of the three components of mind: the id, the ego and the superego, with sexual offenders having temperamental imbalance of a powerful id (increased sexual impulses and libido) and a weak superego (a low level of moral probation), which are also impacted by early environment.

This updates a previous Cochrane review but is based on a new protocol.

Objectives

To assess the effects of psychological interventions on those who have sexually offended or are at risk of offending.

Search methods

In September 2010 we searched: CENTRAL, MEDLINE, Allied and Complementary Medicine (AMED), Applied Social Sciences Index and Abstracts (ASSIA), Biosis Previews, CINAHL, COPAC, Dissertation Abstracts, EMBASE, International Bibliography of the Social Sciences (IBSS), ISI Proceedings, Science Citation Index Expanded (SCI), Social Sciences Citation Index (SSCI), National Criminal Justice Reference Service Abstracts Database, PsycINFO, OpenSIGLE, Social Care Online, Sociological Abstracts, UK Clinical Research Network Portfolio Database and ZETOC. We contacted numerous experts in the field.

Selection criteria

Randomised trials comparing psychological intervention with standard care or another psychological therapy given to adults treated in institutional or community settings for sexual behaviours that have resulted in conviction or caution for sexual offences, or who are seeking treatment voluntarily for behaviours classified as illegal.

Data collection and analysis

At least two authors, working independently, selected studies, extracted data and assessed the studies' risk of bias. We contacted study authors for additional information including details of methods and outcome data.

Main results

We included ten studies involving data from 944 adults, all male.

Five trials involved primarily cognitive behavioural interventions (CBT) (n = 664). Of these, four compared CBT with no treatment or wait list control, and one compared CBT with standard care. Only one study collected data on the primary outcome. The largest study (n = 484) involved the most complex intervention versus no treatment. Long-term outcome data are reported for groups in which the mean years 'at risk' in the community are similar (8.3 years for treatment (n = 259) compared to 8.4 in the control group (n = 225)). There was no difference between these groups in terms of the risk of reoffending as measured by reconviction for sexual offences (risk ratio (RR) 1.10; 95% CI 0.78 to 1.56).

Four trials (n = 70) compared one behavioural programme with an alternative behavioural programme or with wait list control. No meta-analysis was possible for this comparison. For two studies (both cross-over, n = 29) no disaggregated data were available. The remaining two behavioural studies compared imaginal desensitisation with either covert sensitisation or as part of adjunctive drug therapy (n = 20 and 21, respectively). In these two studies, results for the primary outcome (being 'charged with anomalous behaviour') were encouraging, with only one new charge for the treated groups over one year in the former study, and in the latter study, only one new charge (in the drug-only group) over two years.

One study compared psychodynamic intervention with probation. Results for this study (n = 231) indicate a slight trend in favour of the control group (probation) over the intervention (group therapy) in terms of sexual offending as measured by rearrest (RR 1.87; 95% CI 0.78 to 4.47) at 10-year follow-up.

Data for adverse events, 'sexually anomalous urges' and for secondary outcomes thought to be 'dynamic' risk factors for reoffending, including anger and cognitive distortions, were limited.

Authors' conclusions

The inescapable conclusion of this review is the need for further randomised controlled trials. While we recognise that randomisation is considered by some to be unethical or politically unacceptable (both of which are based on the faulty premise that the experimental treatment is superior to the control - this being the point of the trial to begin with), without such evidence, the area will fail to progress. Not only could this result in the continued use of ineffective (and potentially harmful) interventions, but it also means that society is lured into a false sense of security in the belief that once the individual has been treated, their risk of reoffending is reduced. Current available evidence does not support this belief. Future trials should concentrate on minimising risk of bias, maximising quality of reporting and including follow-up for a minimum of five years 'at risk' in the community.

PLAIN LANGUAGE SUMMARY

Psychological interventions for sex offenders or those who have sexually offended or are at risk of offending

Sexual offending is both a social and a public health issue. Victim surveys show that sexual abuse is common and that much of it is never brought to the attention of criminal justice systems.

Psychological interventions are generally based on behavioural or psychodynamic theories. Interventions might be designed to change an offender's thoughts, feelings or views on relationships, with the ultimate aim of changing their behaviour.

A Cochrane review published 10 years ago considered the evidence for psychological treatments for sexual offenders and found insufficient data to reach any conclusions (Kenworthy 2003). Our current review is based on a new protocol and a literature search conducted in September 2010.

We examined the evidence for the effectiveness of psychological interventions for sexual offenders or those considered likely to offend. We excluded interventions for sex offenders with learning disability as this is the subject of a separate Cochrane review (Ashman 2008).

We identified 10 relevant studies involving data from 944 adults, all male. Few of these studies provided information about the primary outcome of this review, which was reoffending. This was usually because studies did not collect data for a sufficiently long period outside prison or the treatment setting. Many studies relied on other outcome measures (for example, anger or social skills) chosen by investigators in the hope that they were linked in some way with future offending, although it cannot be stated with certainty that such connections reliably predict reoffending.

Five of the trials we found involved 664 men and used primarily cognitive behavioural interventions (CBT). In the largest study, which had the most complex and intense 'package' of treatment both within and outside of prison, there was no difference between the group who had received CBT and those who had not in terms of the risk of reoffending as measured by reconviction for sexual offences.

One study, involving 231 men, compared psychodynamic intervention with standard care, which was probation, and suggested that probation was mildly superior in terms of reducing reoffending.

Behavioural programmes were looked at in four trials involving 70 men. For two studies, not enough data were reported to assess the effectiveness of treatment. For the remaining two, encouraging results with regards to reconvictions and self-reported urges have to be treated with caution as the studies are relatively old, meaning that many participants would not now seek or be offered treatment, as some of the targeted behaviours have been decriminalised.

Data for adverse events, 'sexually anomalous urges' and for secondary outcomes thought to be 'dynamic' risk factors for reoffending, including anger and cognitive distortions, were limited.

We concluded that further randomised controlled trials are urgently needed in this area, so that society is not lured into a false sense of security in the belief that once the individual has been treated, then their risk of reoffending is reduced. Currently, the evidence does not support this belief.

BACKGROUND

Description of the condition

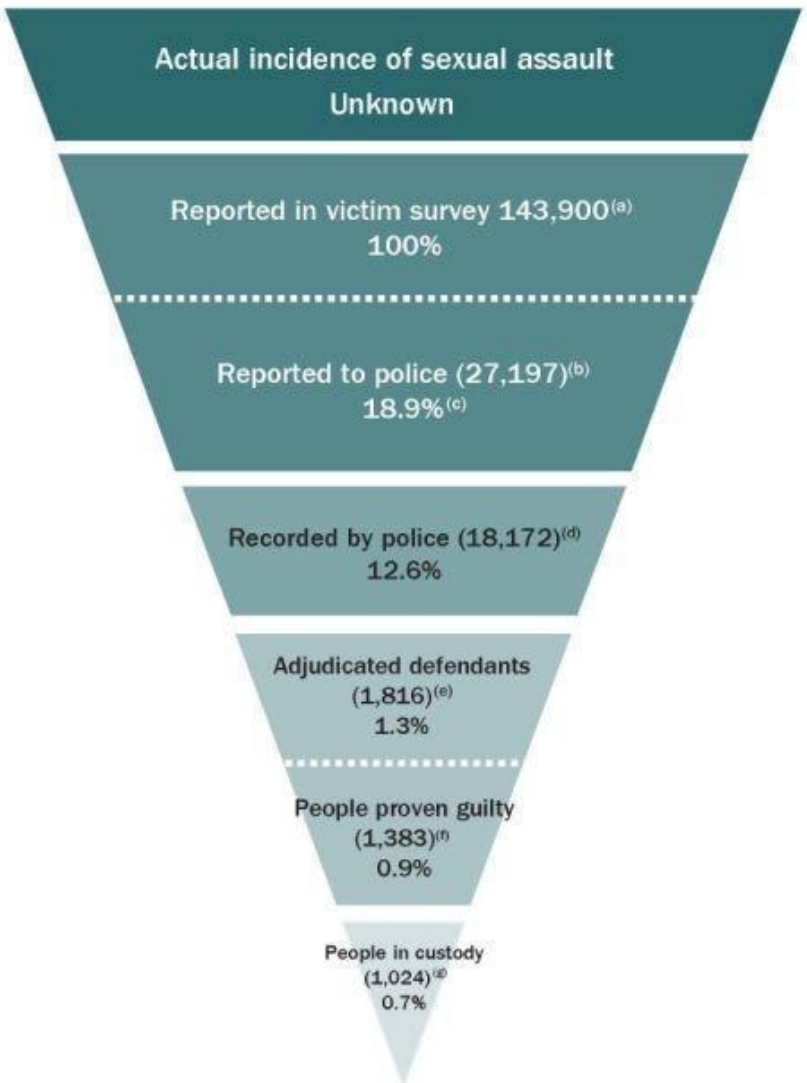
Sexual offending is a legal construct that overlaps, but is not entirely congruent with, the clinical constructs of disorders of sexual preference as described in the *ICD-10 Classification of Mental and Behavioural Disorders* (WHO 1992) or paraphilias as described in the *Diagnostic and Statistical Manual of Mental Disorders* (DSM-IV) (APA 1994). Most sexual offences do not arise as a result of disorders of sexual preference or paraphilias. Furthermore, not all disorders of sexual preference or paraphilias are sexual offences. Clinically-defined diagnoses such as paedophilia, voyeurism, frotteurism, exhibitionism, zoophilia and necrophilia (if acted upon) also meet the rubric for sexual offences but, for instance, fetishism and transvestic fetishism do not. With regard to offences against children, evidence is mixed, with some data suggesting that 50% to 60% of offenders with child victims could be identified as paedophiles (Seto 2008a). Crimes such as rape and incest with adult victims are not, of themselves, classified as disorders of sexual preference or paraphilias, although it may be that a similar proportion of offenders with adult victims show a deviant sexual preference (Lalumière 2005).

An important distinction for this review is thus between sexual offending - that is, behaviour that refers to specific, legally-defined

crimes - and clinical paraphilias, which constitute disorders of sexual preference. Mental health professionals are invariably involved in the treatment of individuals diagnosed with paraphilias. They can also be involved in treating issues relevant to sexual offenders, particularly if the offender suffers from cognitive problems or mental disorders including brain damage, learning disabilities, pervasive developmental disorder, psychosis and personality disorder, which may require treatment in their own right. However, such co-occurrences are uncommon, existing in only 10% of those convicted of sexual offending, and even when they do co-occur, there may be no causal connection between the disorder and the offence. (It should also be noted, however, that the presence of a serious mental disorder is often a reason for excluding that individual from a treatment trial). As sexual offending per se is not generally correlated with any specific mental disorder (Eastman 2012), its 'treatment' is therefore not strictly a health issue; rather, the interventions employed have the status akin to treating aggression in a population of offenders. While these interventions may include healthcare clinicians, their implementation extends beyond clinical practice and involves criminal justice professionals, including forensic psychologists.

The true prevalence of sexual offending is contentious. In the US, 9.7% of prisoners have a history of sexual offending (Greenfeld 1997) and Australian data (Gelb 2007) report a figure as high as 13.3%. These figures represent significant underestimates of the extent of the problem as many sexual offences are not reported or, if they are reported, the allegations are often subsequently withdrawn. Sexual offending accounts for only 1% of crimes recorded in England and Wales (Eastman 2012), but there is a significant discrepancy between the number of adjudicated sexual offences and the number of sexual victims according to surveys of the latter, which show a high incidence of victimisation (Hood 2002 ; Edwards 2003; Chapman 2004) (as well as high levels of psychiatric morbidity (McCauley 1997; Hill 2000; Molnar 2001; Swanston 2003; Chapman 2004; Mchichi 2004)). The true proportion of offenders in custody may be only 0.7% of those responsible for offences (Farrow 2012 [pers comm]) - see Figure 1 (Gelb 2007).

Figure 1. From Gelb 2007 (used with permission of the Sentencing Advisory Council, Melbourne, Victoria, Australia)



(a) Australian Bureau of Statistics, 2006d, *Personal Safety Australia, 2005,* Catalogue 4906.0
(b) Australian Bureau of Statistics, 2006d, *Personal Safety Australia, 2005,* Catalogue 4906.0
(c) All percentages calculated as a proportion of sexual assaults reported in the victim survey
(d) Australian Bureau of Statistics, 2006c, *Recorded Crime–Victims Australia, 2005,* Catalogue 4510.0
(e) Australian Bureau of Statistics, 2006a, *Criminal Courts Australia, 2004-05,* Catalogue 4513.0
(f) Australian Bureau of Statistics, 2006a, *Criminal Courts Australia, 2004-05,* Catalogue 4513.0
(g) Australian Bureau of Statistics, 2005, *Prisoners in Australia, 2005,* Catalogue 4517.0

Although convictions for sexual offending are relatively uncommon, legislative provisions reflect this concern that many crimes go unrecorded. For instance, in the UK, provisions in the Sexual Offences Act 2003 include substantial increases in sentence length and state control, in terms of notification requirements and supervision, for up to ten years after a sentence has been spent (Great Britain 2003). Again, in England and Wales, the Mental Health Act was amended in 2008 so that sexual deviancy was no longer excluded as a criterion for compulsory detention (DOH 2008). In Germany, penal reform in 1998 made the treatment of sexual offenders mandatory for such offenders receiving a prison sentence of more than two years (Lösel 2005). Similarly, the treatment of mentally disordered offenders in the US includes state-specific sexual predator laws that allow for the preventative detention of those believed to be at high risk of sexually offending in the future. Finally, also in the US, there is Megan's Law, which allows information on registered sex offenders against children to be made available to the community (About Megan's Law 2007). While this concern is understandable, other commentators have argued that exaggerating the danger that sexual offenders pose is problematic as it may increase public fear, and stigmatise and hinder rehabilitation of offenders who have changed their lifestyles, while wasting valuable resources on unnecessary surveillance (Soothill 2000).

The observation that the pattern of sexual reoffending is relatively infrequent but very long-term (Prenky 1997) impacts on treatment trials in two ways, both of which may lead to data that underestimate the true extent of the problem. First, its low frequency means that the difference between the two trial conditions has to be substantial in order to be demonstrated. Second, since reoffending can occur many years after release, follow-up needs to be long-term in order to capture data from all of those who reoffend. The literature recognises that those convicted of a sexual offence are not homogeneous: the most common distinction is drawn between rapists and child molesters, as these are deemed to have different aetiologies, presenting characteristics and likelihood of reoffending (Firestone 2000). Another important confounder in the interpretation of trial data is the degree of psychopathy possessed by the sexual offender, as those with high levels are less likely to benefit from treatment and more likely to reoffend (Seto 1999; Hildebrand 2004).

Description of the intervention

This review focuses solely on psychological interventions for those who have sexually offended or are at risk of offending and does not encompass anti-libidinal interventions ('chemical' castration and psychotropic medications that have anti-libidinal side effects), which are the subject of a separate Cochrane review planned for 2013 (Khan 2009). Nor does it cover effects of surgical interventions, including orchidectomy and stereotaxic neurosurgery. The crucial difference between psychological and anti-libidinal interventions for sex offenders is that the goal of the latter is the significant decrease or elimination of sexual desire and performance. Psychological interventions leave performance intact but seek to redirect the offender's sexual expression to socially and legally acceptable outlets.

Psychosocial interventions are broadly divided into two categories: behavioural (including cognitive behavioural) and psychodynamic.

Behavioural interventions

Behavioural interventions fall into two main groups: those based on traditional classical conditioning and/or operant learning theory and those based on cognitive behavioural approaches. In reality, the differences between these two approaches may be blurred, with traditional approaches admitting some cognitive components and cognitive behavioural programmes admitting some classical and/or operant components. Bearing in mind the permeability of the boundaries between these two approaches in practice, they may be described as follows: styles of intervention associated with traditional classical and operant learning theory are generally referred to as behaviour modification or behaviour therapy. The hallmark of these interventions is an explicit focus on changes in behaviour, by administering a stimulus or stimuli and measuring effects upon overt behaviour. Within sex offender treatment, examples include aversion therapy (exposure to deviant material followed by an aversive stimulus); covert sensitisation (imagining deviant sexual experience until arousal and then imagining powerful negative experience); olfactory conditioning (an unpleasant odour is paired with images or descriptions of sexual offences); and masturbation satiation/orgasmic reconditioning. In the latter variants of behavioural interventions, masturbation to what is considered an appropriate (as opposed to deviant) sexual fantasy has a central role in therapy. In orgasmic therapy, the client is encouraged to masturbate using his/her typical 'deviant fantasy' until orgasm is close, at which time s/he is instructed to change to a more appropriate, socially accepted fantasy, to aid reconditioning. Satiation therapy requires the client to masturbate to orgasm using socially acceptable, appropriate fantasies, after which she or he is told to masturbate again immediately, using customary deviant fantasies. The latter technique is thought to work by reducing arousal to deviant stimuli.

Cognitive behavioural interventions are intended to change internal processes - thoughts, beliefs, emotions, physiological arousal - alongside changing overt behaviour, such as social skills or coping behaviours. They involve all or most of the following: establishing links between offenders' thoughts, feelings and actions about offending behaviour; correction of offenders' misperceptions, irrational beliefs and reasoning biases associated with their offending;

teaching offenders to monitor their own thoughts, feelings and behaviours associated with offending; and promoting alternative ways of coping with deviant sexual thoughts and desires. Particular targets of this approach may be 'cognitive distortions' also known as 'thinking errors', which might in this context be defined as exaggerated or irrational thoughts an offender reports (for example, an offender may believe, or claim to believe, that his/her offending behaviour is attractive or desirable to his or her victims). Cognitive behavioural interventions are the basis of sex offender treatment in prison systems and community programmes in the United Kingdom (UK), Canada, New Zealand, Australia and the United States of America (USA).

Psychodynamic interventions

The terms 'psychoanalysis' and 'psychodynamic psychotherapy' are sometimes used interchangeably. For the purposes of this review, psychoanalysis is defined as regular individual sessions with a trained psychoanalyst, planned to last at least 30 minutes and taking place three to five times a week. In psychoanalysis, therapists working with this population seek to work at the 'infantile sexual relations level' as conceptualised within psychoanalytic theory. Psychodynamic psychotherapy is defined as regular individual therapy sessions with a trained psychotherapist, or a therapist under supervision. Therapy sessions are to be based on a psychodynamic or psychoanalytic model. Sessions can rely on a variety of strategies, including explorative insight-oriented, supportive or directive activity, applied flexibly. Psychodynamic psychotherapists can use a less strict technique than in psychoanalysis, but to be considered well-defined psychodynamic psychotherapy, therapists are required to deal with 'transference' issues (defined as the unconscious transfer of feelings to a person that do not befit that person and actually apply to another) (Greenson 1967).
Psychodynamic interventions encompass a range of psychological interventions that share a common root in psychoanalytic theory. It posits that sexual offending arises through an imbalance of the three components of mind: the id, the ego and the superego, with sexual offenders having temperamental imbalance of a powerful id (increased sexual impulses and libido) and a weak superego (a low level of moral probation) that is also impacted by the family environment, particularly by the relationship with key others (including parents) and any experiences of abnormal early sexualisation (Freud 1898).
It is claimed that sexual offending arises from a failure to maturate, so that the sexual abuser re-enacts a real or imagined trauma in their childhood using primitive psychological defence mechanisms of splitting and denial (Stoller 1976; Glasser 2001). The function of the treatment is therefore to moderate the drive of the id and replace the primitive defences with more mature adaptations, by gaining insight through the interaction with the therapist. Although psychodynamic approaches were common within the UK for this population (Grubin 2002), they have now been largely replaced by interventions based on cognitive behavioural therapy (CBT).

How the intervention might work

Behavioural interventions for sex offenders are based on learning theory, and inappropriate sexual behaviour is behaviour that has been learned. These behaviours should be capable of being extinguished by standard conditioning methods using negative or positive reinforcers. In addition, it is theorised that appropriate sexual behaviour can be learned. Cognitive behavioural interventions are based on the principle that thinking controls overt action, and that sexual offenders can learn new ways of thinking and new skills that can, in turn, control behaviour (Marshall 1999).
From the perspective of psychodynamic theory, sexual offending may be attributable to very weak superegos and very strong ids. Alternatively, according to this theory, the mother-son relationship may be a factor, with the mother sending ambiguous messages to the son; such a situation may also encourage 'covert incest' where the son adopts all the roles of the spouse apart from sexual relations. It is thought that these hypothesised casual factors might be treatable by psychodynamic interventions in their various forms (Cordess 1996).
Psychodynamic therapy may involve working on the 'transference' between client and therapist that allows for unconscious feelings to become manifest and corrected within the therapy.

Why it is important to do this review

As described above, the prevalence of sexual offending, as well as the association between victimisation and both becoming a perpetrator and having subsequent mental health problems, means that these offences make a significant contribution to mental health morbidity. There is strong social and political pressure to do something about this problem, both to help the victims and prevent sexual recidivism. Recent legislative changes both in the UK and in the US reflect this imperative. As a matter of social justice for the offender, and to provide reassurance to the community, it is essential that the treatments provided work and therefore inspire confidence that offenders who have completed treatment programmes really are at a reduced risk of sexual reoffending.
An early Cochrane review (White 1998) looked at all types of interventions for sexual offending; it was partially updated in 2003 in a review that considered only psychological interventions (Kenworthy 2003). The older review (White 1998) identified only two studies of psychological interventions (total n = 286) and reported no clear effects for either CBT-based 'relapse prevention programme' (Marques 1993) or group psychotherapy (Romero 1983).
In their introduction to the 2003 review, Kenworthy et al (Kenworthy 2003) reported that since the publication of the ear-

lier review (White 1998) "some evidence indicating positive effects of psychological interventions...[had] begun to emerge (Beech 1998) and the theoretical sophistication with which the phenomena are explained...[had] advanced (Ward 2002)." However, conclusions from Kenworthy's completed review of psychological interventions (incorporating nine studies and over 500 participants) were much less sanguine than their introduction had predicted (Kenworthy 2003). Lack of relevant data within the included studies made it impossible to draw conclusions for clinicians. The review made an urgent call for high-quality studies with long-term follow-up, reported according to CONSORT standards (Moher 2001), whilst showing an appreciation of the difficulties such research entails.

Subsequently, a systematic review by Lösel and Schmucker (Lösel 2005) considered all treatment options for sex offenders, but included studies with a larger range of designs. This review reported impressive treatment effect sizes in terms of preventing sexual recidivism through the anti-libidinal interventions of surgical castration (effect size 15.34) and 'chemical castration' (effect size 3.08). However the comparable treatment effect sizes for classic behaviour therapy and cognitive behavioural interventions, though still significant, were less impressive (2.19 and 1.45, respectively). Because of methodological limitations in the studies, these positive findings were described as no more than 'promising'.

The previous version of the Cochrane systematic review (Kenworthy 2003) included studies identified via literature searches up to and including 2002. The current authors decided to update this review based on an new protocol (Bilby 2008), in order to reassess the evidence supporting psychological intervention for this type of offender.

OBJECTIVES

To assess the effects of psychological interventions on those who have sexually offended or are at risk of offending.

METHODS

Criteria for considering studies for this review

Types of studies

We included all relevant randomised trials. We excluded quasi-randomised trials, that is, those where allocation was undertaken by a method that is not truly random, such as by surname.

Types of participants

Adults (aged 18 years old and over) treated in institutional (prison or psychiatric facility) or community settings for sexual behaviours that have resulted in conviction or caution for sexual offences or offences with a sexual element or violent behaviours with a sexual element (for example, murder), or who are seeking treatment voluntarily for behaviours classified as illegal. We included studies where there has been no criminal case reported but where the perpetrator sought treatment or admitted to illegal behaviours in self report.

Defining what constitutes a sexual offence in the context of the international literature can be problematic as definitions of criminally sexual behaviour differ between jurisdictions, cultures and over time. Rather than focus on just one jurisdiction, such as UK law, we included trials of interventions where the participants have committed a sexual offence that would be accepted as criminal in most jurisdictions. These core sexual offences can be described as penetrative or non-penetrative sexual acts carried out by adults on non-consenting adult victims, and penetrative or non-penetrative sexual acts carried out by adults on consenting or non-consenting minors. We would also include penetrative or non-penetrative sexual acts carried out by adults on other adults, where the victim was unable to give informed consent due to their physical or mental disability (or both).

We excluded studies of interventions for sex offenders where there was no clear international consensus as to whether the sexual behaviour is a crime or not. Examples include consenting same sex acts between adults, consenting sadomasochistic acts and transvestitism. We also excluded interventions for sex offenders with learning disability as this is the subject of a separate Cochrane review (Ashman 2008).

Types of interventions

All psychological interventions compared with standard care or another psychological therapy. We also planned to include studies of psychological interventions where medication was given as an adjunctive intervention.

Types of outcome measures

Primary outcomes

- Recidivism as measured by reconviction or caution
- Recidivism as measured by self report

Secondary outcomes

- Cognitive distortions*
- Sexual obsessions*/sexually anomalous urges* ('sexually anomalous' behaviours was a term coined by Dr Neil

McConaghy to cover "the broad range of sexual paraphilic behaviours including those that attracted criminal penalties..." The term was used to avoid pejorative connotations associated with terms such as 'deviant' and 'aberrant') (Blaszczynski 2011 [pers comm]). This measure aims to capture thoughts and behaviours that participants experience as negative but that do not result in a formal charge.

- Anxiety*
- Anger *
- Leaving treatment early
- Adverse events, including suicide or suicide attempts and sudden/unexpected death, from any cause

We divided outcomes into immediate (within 6 months), short term (> 6 months to 24 months) and medium term (> 24 months to 5 years) and long term (beyond 5 years) during the period at risk, for example, after release from prison or discharge from hospital facility. If the participants were receiving treatment in the community, the period at risk commenced from the end of treatment.

*Starred outcomes were added post-protocol and are justified as follows. Adverse outcomes can result from psychological as well as pharmacological interventions and so should be reported on; this 'umbrella term' incorporates the outcomes of suicide and sudden/unexpected death from any cause that we specified in the protocol. Based on data from 'dynamic risk factors' for sexual reoffending (Hanson 2000), we also decided to include data on cognitive distortions, sexual obsessions and dysphoric mood (anxiety and anger). Changes effected by interventions on such outcomes may be important for sex offenders yet to be released who have little opportunity to reoffend and so should also be reported on, but we acknowledge that this research on risk factors is still very much 'emerging'.

Search methods for identification of studies

Electronic searches

We ran searches between July and September 2008 and updated them in September/October 2010. We searched the following databases.

- Cochrane Central Register of Controlled Trials (CENTRAL), *The Cochrane Library,* Issue 3 2010
- MEDLINE (Ovid), 1950 to 10 September 2010
- EMBASE (Ovid), 1980 to 2010 week 37
- AMED - Allied and Complementary Medicine, 1985 to September 2010
- ASSIA - Applied Social Sciences Index and Abstracts (CSA), 1987 to 1 October 2010
- Biosis Previews, 1985 to September 2010
- CINAHL (EBSCOhost), 1982 to September 2010
- Copac Academic and National Library Catalogue, last searched January 2008
- IBSS - International Bibliography of the Social Sciences, 1951 to September 2010
- ISI Proceedings, 1990 to September 2010
- ISI-SCI - Science Citation Index Expanded, 1970 to September 2010
- ISI-SSCI - Social Sciences Citation Index, 1970 to September 2010
- National Criminal Justice Reference Service Abstracts Database, last searched September 2010
- OpenSIGLE, last searched July 2008
- Proquest Dissertations and Theses, last searched 11 October 2010
- PsycINFO (Ovid), 1806 to September Week 1 2010
- Social Care Online, last searched September 2010
- Sociological Abstracts, 1952 to September 2010
- ZETOC, last searched September 2010

The OpenSIGLE website was being redesigned at the time of searching in 2010 and the export function was not available. As our search yielded a large number of hits we decided it was impractical to record these individually, but intend to incorporate the results of an OpenSIGLE (now OpenGrey) search in future updates. Randomised control trials filters were used as appropriate. No language or date restrictions were applied. See Appendix 1 for the full list of databases and search terms used.

The electronic searches were constructed taking into account changing terminology and perception of sex offences. We recognise that several of these terms would now be regarded as unacceptable or misleading, or both, as terms signifying sexual offending.

Searching other resources

Handsearching

Reference lists of included studies were searched for additional relevant trials along with the reference lists of reviews.

Requests for additional data

Where possible, surviving authors of each included study and known experts in the field were contacted for information regarding unpublished data and ongoing studies (Marshall 2011b [pers comm]; McAnaney 2011 [pers comm]; Anderson-Varney 2011 [pers comm]; Blaszczynski 2011 [pers comm]; Marques 2011 [pers comm]; Seto 2011 [pers comm]; Williams 2011 [pers comm]; Federoff 2012 [pers comm]).

Data collection and analysis

See Appendix 2 for methods to be used in future updates of the review.

Selection of studies

Titles and abstracts of studies identified through searches were independently assessed by authors working in pairs including MF, NH, JD and MP, as well as Hannah Jones (a former staff member at Nottinghamshire Healthcare NHS Trust). NH and JD independently assessed full copies of those studies that appeared to meet the inclusion criteria. Uncertainties concerning the appropriateness of studies for inclusion in the review were resolved through consultation with a third review author (CD). Authors were not blinded to the name(s) of the study author(s), their institution(s) or publication sources at any stage of the review.

Data extraction and management

Two authors (NH and JD) extracted data independently using a piloted data extraction form. The authors extracted information on study design and implementation, setting, sample characteristics, intervention characteristics and outcomes from all included studies.

Data were entered into Review Manager 5 (RevMan) (Review Manager 2011). Where data were not available in the published trial reports, we contacted the authors and asked them to supply the missing information. All contacts are documented below.

Assessment of risk of bias in included studies

For each included study, two authors (NH and JD) independently completed the Cochrane Collaboration's tool for assessing risk of bias (Higgins 2011b).
This assessed the degree to which:

- the allocation sequence was adequately generated ('sequence generation');
- the allocation was adequately concealed ('allocation concealment');
- knowledge of the allocated interventions was adequately prevented during the study ('blinding');
- incomplete outcome data were adequately addressed;
- reports of the study were free of suggestion of selective outcome reporting;
- the study was apparently free of other problems that could put it at high risk of bias.

Measures of treatment effect

Comparisons were planned for specific follow-up periods:

- within the first 6 months;
- between 6 and 24 months;
- between 24 months and 5 years;
- beyond 5 years.

All primary outcomes and (originally) all secondary outcomes were dichotomous and we planned to use the risk ratio (RR) with a 95% confidence interval (CI) to summarise results within each study. Some secondary outcomes introduced post-protocol have been reported as continuous outcomes. These are reported, where possible, as mean differences. Where different scales measured the same outcome, we calculated standardised mean differences (SMDs).

Unit of analysis issues

Cluster-randomised controlled trials

At protocol stage (Bilby 2008), we anticipated that cluster-randomised trials were unlikely in this area and cross-over trials less likely still, given that the review included only psychological interventions where 'wash out' is not possible.
Nonetheless, we identified one cluster-randomised controlled trial (Ryan 1997), and data for it were handled using an intraclass correlation coefficient helpfully provided by authors of the previous review (Kenworthy 2003).
See Appendix 2 for other methods to be used in updates of the review.

Dealing with missing data

We assessed missing data and dropouts for each included study and reported the number of participants who were included in the final analysis as a proportion of all randomised participants in each study. We provided reasons, where known, for missing data in the narrative summary, as well as details of investigators' use of intention-to-treat analysis (where applicable).

Assessment of heterogeneity

We assessed the extent of between-trial differences and the consistency of results of any meta-analysis in three ways:

- by visual inspection of the forest plots;
- by performing the Chi^2 test of heterogeneity (where a significance level less than 0.10 was interpreted as evidence of heterogeneity);
- by examining the I^2 statistic (Higgins 2011a; section 9.5.2).

The I^2 statistic describes approximately the proportion of variation in point estimates due to heterogeneity rather than sampling error. We considered I^2 values less than 30% as indicating low heterogeneity, and values greater than 70% as indicating high heterogeneity. Because we anticipated that heterogeneity might well prove to be a significant problem we planned to carried out each

analysis twice using fixed-effect and random-effects models; however, there were insufficient studies within our analyses to necessitate this.

Assessment of reporting biases

Insufficient data existed for funnel plots to be drawn. We present plans for same for updates of this review in Appendix 2.

Data synthesis

We performed meta-analysis where studies were considered to have sufficiently similar participants, interventions, comparators and outcome measures. In carrying out meta-analysis, the weight given to each study was the inverse of the variance so that the more precise estimates (from larger studies with more events) were given more weight. We analysed outcome data separately for the three categories of intervention: behavioural, cognitive behavioural and psychodynamic.

Where possible, we planned to synthesised data using both a fixed-effect and a random-effects model and found this necessary for only one outcome.

Subgroup analysis and investigation of heterogeneity

Insufficient studies were identified to permit planned subgroup analyses. See Appendix 2.

Sensitivity analysis

Insufficient studies were identified to permit planned sensitivity analyses. See Appendix 2.

RESULTS

Description of studies

See: Characteristics of included studies; Characteristics of excluded studies; Characteristics of studies awaiting classification.

Results of the search

Search results for the original version of this review (Kenworthy 2003) were unclear. Relevant text reads: "Thousands of electronic reports are identified but reporting on the great majority of those in this section would, we feel, do no service to the reader. Very few of the studies identified by the searches were selected for closer inspection as abstracts and titles made it quite clear that the work was not relevant for inclusion. Fifty-nine studies were closely inspected for inclusion but finally were excluded."

For the current update, we performed electronic searches over two consecutive time periods to minimise the difficulty of managing large numbers of citations. Searches were not restricted to psychological interventions only, but included terms relevant to the companion review on pharmacological interventions (Khan 2009) for the same population. Searches to 2008 produced results totalling 26,197 records (of which, 343 were examined in full text). Searches from September 2008 to October 2010 produced 10,507 records, of which, 53 were examined in full text.

Data included this update but not 2003 review

One study (Brown 1996) excluded by a previous version of this review (Kenworthy 2003) was added to the present version. Although data were limited, the study does meet inclusion criteria. Marques 1999 and Marques 2005 are new articles in this update, providing additional data for Marques 1994. Marques 2005 is of particular importance as it is the final report from this large and long running study.

Personal contact with investigators from nine of the included studies has provided data (chiefly on methods but also on numbers and characteristics of participants) not available in the previous version of this review.

Studies awaiting classification

One study awaits classification (Abel 1988). Due to age, it is unlikely that further data will become available.

Ongoing studies

No ongoing studies relevant to this review were identified through comprehensive searches or via numerous contacts with experts in the field. On the contrary, we have identified a robust debate (particularly following what may be seen as the 'failure' of the SOTEP study (Marques 2005)) concerning the paucity of such research and the reasons for it; and the ethics, feasibility and utility of continuing research on many of the types of psychological relapse prevention interventions used within this review either at all, or in this format (Marshall 2007; Seto 2008b; Marshall 2008; Rice 2012).

Included studies

We included ten studies involving data from 944 men.

Design

The majority of studies included within this review were described as randomised controlled trials of parallel design, with the exception of Rooth 1974, which can be viewed either as a simple crossover across three treatment conditions or as a randomised comparison of three sequenced interventions investigating hypothesised

'priming' or even synchronistic effects of different combinations of interventions. All trials used the individual participant as the unit of allocation with the exception of one, Ryan 1997, which involved a clustered design.

Seven studies included two 'arms' or comparison groups (McAnaney 1981; Romero 1983; McConaghy 1985; Anderson-Varney 1991; Hopkins 1991; Brown 1996; Ryan 1997). Three studies included three arms each. McConaghy 1988 involved a prospectively planned three-way comparison and Marques 1994 developed a third arm control condition to accommodate some of the participants who had qualified for the project but chose not to participate (data for the latter group are not used within this review). Investigators involved in Rooth 1974 planned a study involving two active treatments (aversion therapy and self-regulation) with one attentional control arm (muscular relaxation). All were delivered consecutively in a randomised order using a Latin square design. Investigators appear to have been testing in this way to understand if there were synergistic benefits to be gained by delivering the active treatments in a particular order more than to test one active treatment against another alone (Rooth 1974).

Investigators in eight of the studies did not seek to match participants on prespecified criteria. In Hopkins 1991, matching was done on the basis of age and index offence (Marshall 2011a [pers comm]). In Marques 1994, such efforts seem to have been made only within the group of offenders who chose not to participate in the trial originally, who were 'matched' following random selection from the whole sample (Marques 1994).

Sample sizes

The total number of participants randomised in the 10 included studies is 1008. The number of those randomised to eligible arms within the studies is 944. The difference between the two figures can be accounted for by the subtraction of participants from one (ineligible) active treatment arm of a three-armed trial, McConaghy 1988, and from participants initially randomised to the oldest study in this review (Romero 1983) who were subsequently withdrawn from analysis due to changes in US law, or DSM criteria regarding homosexual acts between consenting adults, or both (see Characteristics of included studies).

Overall sample sizes varied from 12 (Rooth 1974) to 434 (Marques 1994) (see Characteristics of included studies) and the mean number of participants within the included studies is 101. Few investigators reported having used power calculations and most concluded their reports with recommendations that future research involve larger participant samples.

Setting

Five studies included within this review were conducted in the USA (McAnaney 1981; Romero 1983; Anderson-Varney 1991; Marques 1994; Ryan 1997), two in the UK (Rooth 1974; Hopkins 1991), two in Australia (McConaghy 1985; McConaghy 1988) and one in Canada (Brown 1996).

Three of the included studies proceed from investigations undertaken by postgraduate students in pursuit of PhDs (McAnaney 1981; Anderson-Varney 1991; Ryan 1997); the rest appear to have been undertaken with use of public monies by a mixture of academic researchers and state-employed practitioners involved in the delivery of services to offenders.

Studies were conducted in a variety of settings including prisons (Anderson-Varney 1991; Hopkins 1991), a forensic psychiatric hospital (McAnaney 1981) or hospitals where participants attended as outpatients (Romero 1983; McConaghy 1988; Brown 1996; Ryan 1997) or inpatients for the duration of the trial only (Rooth 1974; McConaghy 1985). In one study, Marques 1994, the group randomised to intervention were treated in hospital whilst those randomised to no treatment remained in prison: follow-up for both groups took place in the community.

In terms of age, publication date does not always correlate well with study history. The oldest data in the review come from Romero 1983, a report of a study where participants were first randomised as far back as 1966. The newest study data derive from Marques 1994, a study for which long-term follow-up data were collected as recently as 2001 and reported in 2005 (Marques 2005).

Participants

Gender, age and ethnicity

As described above, the total number of participants randomised in the 10 studies included in this review is 1008. All of these participants were male and ranged from 16 to 72 years of age (the former figure is technically below our inclusion criteria for age, but it emerges from a trial (McAnaney 1981) in which the mean age was 25.5. Underage participants were in a small minority and we do not consider them to bias our results). Information on age was missing entirely from two studies (Hopkins 1991; Ryan 1997), and whilst a range (18 to 60 years) is given for the original sample (including a group of men who later refused to enter the trial) in the largest included study (Marques 1994), ranges or means cannot be discerned for the treatment and control (see also Characteristics of included studies).

The majority of studies (Rooth 1974; McConaghy 1985; McConaghy 1988; Hopkins 1991; Brown 1996; Ryan 1997) did not provide information concerning ethnicity of participants. Data on ethnicity for the other studies were as follows: Romero 1983 reported that 33% of participants were white and 67% black; McAnaney 1981 that 88% were white and 12% black; Anderson-Varney 1991 that 82% were white and 18% nonwhite. For the study by Marques 1994, data are again given for the whole sample (n = 704) and not broken down by groups such that participants for whom data are available/eligible for this review (n =

484) can be disaggregated. Such data as exist for the overall sample indicate that 70% of participants were white; 15% African-American and 13% Latino (Marques 1994).

Baseline characteristics of participants

There was considerable heterogeneity as regards the demographic and offending characteristics of the participants in the 10 included studies in this review, and in the detail provided by study investigators concerning same. Data not related to offending behaviour (for example, marital status) are provided in the Characteristics of included studies; data directly related to offences or psychiatric diagnoses, where given, are summarised below.

The author of one study was unable to provide any data concerning participants except that they were on parole or probation following conviction for sexual offences (Ryan 1997). Another small study reported only that participants involved roughly equal numbers of incarcerated paedophiles and rapists (Hopkins 1991); two (Anderson-Varney 1991, n = 60, and Brown 1996, n = 17) exclusively involved incarcerated offenders against children.

Another study (Rooth 1974) involved only those with an established history of exhibitionism (mean duration 16 years). The majority of these participants remained in the community, with only a third having been referred from prison. Populations in both studies conducted by McConaghy et al (McConaghy 1985; McConaghy 1988) were also community-based and very 'mixed', comprising a self-selected group of men defined only as having 'anomalous urges' experienced as beyond the men's control. Many of the participants in these latter two studies had been diagnosed with paraphilias according to DSM-II criteria, but not all had been convicted with a sexual offence (roughly 60% in each study had previous convictions).

The investigator of a study concentrating on heterosocial skills selected only participants who had been convicted of sexual offences (25 rapists and 15 paedophiles) and been diagnosed with a psychosexual disorder, who were in addition single and did not define themselves as exclusively homosexual in orientation (McAnaney 1981).

Detail on offending profiles of participants in the two largest studies (Romero 1983; Marques 1994) was considerable. However, analysis in the former study is based on a much earlier, unpublished trial conducted by Roether and Peters (Roether 1972). It is clear that a number of participants in the original study were consenting homosexuals whose data were excluded from later analyses conducted by Romero and Williams (Romero 1983; Romero 1985). As was confirmed by personal communication, this change to analysis was due to changes in the law and DSM criteria for homosexuality (Williams 2011 [pers comm]). For the final analysis, 21% of participants were paedophiles, 17% exhibitionists and the majority (62%) 'assaulters' (Romero 1983). Sixty-nine per cent of the original participants (whole sample) in this study were also described as having met criteria for personality disorders by contemporary DSM criteria (Roether 1972), although these criteria have since changed substantially as well. Personal contact with a researcher affiliated with this study also established that contemporary thinking allowed offenders to be classed as assaulters and not paedophiles if the offender was within 10 years in age of the victim: this meant men of 21 or 22 who had assaulted children of 11 or 12 years of age were not classed as paedophiles (Williams 2011 [pers comm]) and this would have had an effect on the treatment groups which these offenders entered.

In the largest and most recent study included in this review, investigators reported that for the initial (whole) sample the majority of participants were offenders against children; 50% of participants were child molesters with female victims, 20% were molesters with male victims, 8% were molesters with both male and female victims, and 22% were rapists with adult victims (Marques 1994).

Exclusion criteria

For reasons to do with their particular research question, investigators involved in the McAnaney 1981 study excluded both married offenders and those who identified themselves as exclusively homosexual in orientation. Participants who had offended against individuals older than 13 years of age were excluded from Anderson-Varney 1991. Investigators in three studies (Rooth 1974; McConaghy 1985; McConaghy 1988) proscribed on mental health criteria, the first excluding participants with 'another dominant psychiatric illness' and the latter two, those with 'active' or 'overt' psychosis. Information on exclusion criteria for the Marques 1994 study was given in detail and included any current psychotic/organic mental condition; conviction for gang rape or incest; more than two previous felony convictions; felony warrant or pending immigration hold; severe medical disability or the presentation of severe management problems in prison, or both. No exclusion criteria were reported in the published papers related to Romero 1983 but contact with an investigator (Williams 2011 [pers comm]) provided information that an unpublished dissertation (Roether 1972) contained relevant information. Here exclusion criteria included age (participants could not be under age 18 or over age 50) and IQ score (participants had to score more than 70). In addition, consenting participants were excluded if, at baseline assessment, it was found that they were engaged in other treatment, were not fluent in English, could be diagnosed with either psychosis or alcoholism, had work schedules that interfered with attendance or were non-ambulatory. Offenders convicted of statutory rape who were within five years of age of their victims were excluded. Unique amongst studies in this review, participants in this trial were excluded if they were 'homosexual pedophiles' or 'homosexual exhibitionists' (defined as men offending against boys under the age of eleven) (Roether 1972); the reasons for this remain unclear. In the small cross-over study involving an intervention of masturbation prohibition (Brown 1996), participants were excluded if they expressed a firm belief that not masturbating

would increase their risk of relapse.
Two studies included in this review did not describe any exclusion criteria for participants (Hopkins 1991; Ryan 1997).

Characteristics of interventions

Interventions within studies included within this review were heterogeneous, but can be largely classified (as prespecified by the protocol (Bilby 2008)) as behavioural (including cognitive behavioural) or psychodynamic/psychotherapeutic in origin. Nine of the 10 were behavioural and we classified four of these as purely behavioural and five as cognitive-behavioural. In one study (Romero 1983), based on the work of psychoanalyst Dr Joseph J. Peters, investigators attempted to apply explicitly Freudian principles to the treatment of sex offenders. The latter did not meet strict criteria set out in the protocol for this review for psychoanalysis (see Background) in that the intervention did not last for a year, but appears nonetheless to have met the less strict criteria we described for 'psychodynamic' interventions. The majority of studies developed treatment programmes from a range of theoretical sources, which are cited where relevant below, and components of treatment sessions are also described in the Characteristics of included studies.

Behavioural interventions

Imaginal desensitisation

Imaginal desensitisation and aversion techniques of different types underlay treatments undertaken by investigators in Rooth 1974, McConaghy 1985 and McConaghy 1988. The latter also included an adjunctive pharmacological component (150mg of medroxyprogesterone by injection).
Rooth et al (Rooth 1974) used a treatment regime in which self-regulation techniques modelled on earlier work (Bergin 1969) were taught to participants, who then rehearsed 'elucidating' internal and external triggers of the 'exposure impulse-response chain'. Participants were then coached to substitute alternative behaviours to assist in interrupting the sequence. Rooth's treatment involved either a week of self-regulation therapy followed by a week of aversion therapy (shocks administered to forearm by battery-powered 'shockbox') or the reverse, with a week of standard muscle relaxation (Jacobson 1938; Schultz 1959) inserted between, or following after, as an attentional control condition. Investigators seemed primarily interested in whether or not the order in which self regulation (a form of desensitisation) and aversion therapy affected treatment outcome.
In the earlier of the two included studies conducted by McConaghey and colleagues (McConaghy 1985), investigators compared imaginal desensitisation (ID) with covert sensitisation. The former condition, ID, is based on the hypothesis that 'anomalous' (or deviant) behaviours are driven by a sense of tension and excitement that becomes aversive if behaviour not completed. Thus, each participant was initially trained in relaxation. The participant then provided descriptions of four scenes in which he was stimulated to carry out the compulsive sexual activity for which he had initially sought treatment. He was then asked to visualise the four scenarios in turn, remaining relaxed as he visualised not completing the compulsive act. Training was short but intensive (14 sessions over five days of inpatient therapy in a psychiatric unit). Training in 'covert sensitisation' was administered following the procedure described by Cautela (Cautela 1966) and differed only in that similarly provocative visualisations concluded not with relaxation, but with a request by the therapist that the participant visualise a deeply aversive incident occurring which inhibited the compulsive act (such as being caught by a police officer or interrupted by family members, or being violently sick).
McConaghey's later study (McConaghy 1988) compared a small group receiving injections of medroxyprogesterone alone (eight in total; four injections at intervals of two weeks followed by four injections at monthly intervals) with another group receiving imaginal desensitisation (ID) as defined and delivered above, with a third group receiving both medroxyprogesterone and ID in combination.

Masturbation prohibition

Investigators wrote that "in spite of the high prevalence of masturbation in males (and its potential importance in the treatment of sex offenders) no studies investigating the effect of therapist prohibition of masturbation on sexual behavior/interests have been conducted" (Brown 1996, p 398). They sought with this study to address that gap, to assess whether "therapist prohibition of masturbation for sex offenders has any effect on sexual urges (normal and paraphilic), and (ii) whether sex offenders are able and willing to alter their frequency of masturbation while in treatment. If, as predicted by some self-help groups, masturbation maintains or "fuels" sexual interests, abstinence from masturbation should be accompanied by a reduction in sexual interest. Conversely, if masturbation to orgasm reduces sexual desire, a decrease in sexual urges would be expected when masturbation is allowed" (Brown 1996, pp 398-9).

Cognitive behavioural interventions

Primary focus on social skills

The focus of the small study conducted by McAnaney and colleagues (McAnaney 1981) compared heterosocial skills training for participants diagnosed with a psychosexual disorder training with a no treatment control group. The training was based on

a social skills module derived from a number of sources (Zunin 1973; Twentyman 1975; Arkowitz 1977, Meichenbaum 1977; Zimbardo 1977) and addressed participants' conversational skills, self-esteem, body image, negative self-talk and anxiety. Treatment involved role play and video feedback activities and female co-trainers and female volunteers simulated social conversations with the participants.

In another small study conducted in a maximum security prison, Hopkins and a psychiatric colleague also delivered social skills and communication training to a group of convicted offenders (Hopkins 1991). Following a videotaped interview, participants received structured education sessions detailing goals of the programme, communication skills (defined as 'listening, verbal and non-verbal communication'); perspective taking/relationships (including discussion of participants' attitude to women); relationships (continued): including victim awareness; and finally, emotional control (looking at anxiety and aggression management). Whilst no set training manual appears to have been used, relevant work is cited (Crawford 1979; Marshall 1983).

The programme developed by Anderson-Varney (Anderson-Varney 1991) and delivered by her to a group of offenders against children drew from many sources including Abel and colleagues (Abel 1984a), and involved social skills training, sexual education and a cognitive restructuring module further modified by based on work by Lange et al (Lange 1986), which itself drew on work by Albert Ellis' Rational-Emotive Therapy (RET).

Transtheoretical counselling

Addiction theory and Christian doctrine underlay Ryan's hypotheses concerning offending behaviour and likely effective treatments for offenders as described in Ryan 1997. In this study, Ryan enlisted the assistance of therapists normally used to delivering state-mandated manualised CBT relapse prevention training, to instead deliver a relatively novel treatment for this population, transtheoretical counselling, to a group of convicted sex offenders on probation. Transtheoretical counselling, whilst also involving cognitive behavioural principles, prioritises identifying "the awareness of or desire to change the problem for which a person has entered counselling" and is based on work by Prochaska and DiClemente (Prochaska 1984). The intervention may alter throughout the treatment duration as periodic 'readiness to change' assessments are made of the participant as s/he 'spirals' upward to a goal of self efficacy (the confidence that the given problem is now under the control of the participant). Whilst attempts were made to contact the investigator, it remains unclear how much or what kind of training therapists had before delivering treatment in this study (Ryan 2011 [pers comm]).

SOTEP (Sex Offender Treatment and Evaluation Project) and SOAP (Sex Offender Aftercare Program)

The SOTEP package of 'Relapse Prevention' was the most complex and extensive of those included within this review (Marques 1994). Participants were paid a small sum and in addition, were allowed (if they accepted treatment) to be treated in a hospital rather than a prison setting. The intensive programme emphasised work on accepting responsibility, modifying cognitive distortions and learning 'what to do to avoid reoffending'. The programme also featured classes in sex education, human sexuality, relaxation training, stress, anger management and social skills. Behaviour therapy was offered to individuals in the treatment group who showed evidence of deviant sexual arousal in their phallometric assessments. 'Specialty groups' addressing other needs of participants (for example, addressing the challenges of alcohol or drug abuse) were available if required. In addition, individual therapy (one hour per week) by a participant's primary therapist, plus two hours per week with nursing staff, were provided. Finally, a pre-release class was included in the package as well as a year of aftercare known as the Sex Offender Aftercare Program (SOAP) provided by community clinicians who had been trained by SOTEP clinical staff. Failure to attend SOAP could result in parole violation and a return to prison. SOAP was an extended version of the relapse prevention programme tailored to suit the individual needs of the paroled offender.

Psychodynamic interventions

Group psychodynamic psychotherapy

The oldest study included in this review, Romero 1983, focused on a comparison between group psychotherapy and standard care (probation). The intervention condition was defined as psychoanalytically-orientated group therapy, delivered in the main by psychiatrists to groups with between 10 and 15 participants. It was built on the perceived success of an innovative program designed by psychoanalyst JJ Peters in 1955, and previously tested in a controlled (but not randomised) evaluation, details of which we have not found explicitly described as yet. However, 'group processes' evolved by Peters and others in that earlier work were felt to be crucial, and it had been noted that participants generally began treatment in denial and hostility to authority and tended to be supportive of other participants' denial and hostility, moving on to a 'middle phase' wherein (through peer identification) hostility and denial decreased and some members began to challenge new members' denials and begin to 'assert leadership', ultimately acting as co-therapists by the end of treatment. Ideally (it was hoped), "therapeutic attitudes are promulgated through [membership]...rather than on the authority of the therapist" and "personal passivity diminishes"; participants would learn that they "can ex-

press hostility, experience anxiety and tolerate tension without resorting to antisocial behavior" (Brecher 1978, pp 61-2).
The principal investigator of this trial described its theoretical framework as based on one developed by JJ Peters with Michaels et al's model of psychopathic impulsivity, aggression and lack of self control (Michaels 1968; Steg 1972). Michaels et al's model marked a departure from previous models, which class criminal behaviour as 'neurotic'. Five treatment groups were available in the first year of the programme, stratified by type of offence as follows: "homosexuals, pedophiles, exhibitionisms and sexual assault cases" (Roether 1972, p. 31) plus a further "mixed group". It was hoped further that group meetings, stratified as they were, would allow for participants to be less inhibited in discussing their offences. The creation of the fifth mixed group was to test this hypothesis. These participants also reported to probation officers and received monthly home visits from them, although less frequently than did the probation-only control group. Participants involved in group psychotherapy met one evening a week for 90 minute sessions over 40 weeks. Investigators believed attendance at 20 sessions was the therapeutic 'minimum' for change to be effected.

Duration and follow-up

The shortest duration of intervention for any study included within this review was one week of intensive imaginal desensitisation (with or without additional medroxyprogesterone) or covert sensitization, which was delivered in two studies (McConaghy 1985; McConaghy 1988). Follow-up data were collected at one month and one year post-treatment.
The intervention period in the cross-over/order effects study of aversive therapy and self regulation (Rooth 1974) lasted one week (or three weeks, if all treatment options are considered together as a package, which seems to be closer to what the investigators intended). Follow-up was conducted formally at six months, and further (less systematically collected) follow-up data reported between 14 and 18 months after the conclusion of the study. Another behavioural programme (masturbation prohibition) was tested in a cross-over design with both periods lasting four weeks (eight weeks in total) with no subsequent follow-up (Brown 1996).
Two studies (McAnaney 1981; Hopkins 1991) of social skills training each involved six weeks of treatment including eight sessions overall. No follow-up data were reported in the former trial for reasons of resources (the study was part of a PhD); the investigator (a clinician) in the latter study was able to report follow-up data at six months post-treatment. A complex package of social skills and other behavioural and cognitive behavioural strategies was delivered over eight weeks (approximately 15 sessions) in Anderson-Varney 1991. Transtheoretical counselling was delivered to offenders over a six month period in another study (Ryan 1997). Neither of the latter two studies (both conducted as part of PhD studies) included post-treatment follow up.
The two largest studies in this review were also the longest in duration and follow-up. Psychotherapeutic group therapy was delivered over a forty week period to participants in Romero 1983 and follow-up data were collected for ten years. In Marques 1994, a package of treatment and services was delivered for two years plus one year's 'aftercare' (SOAP) and follow-up data were collected and reported over a period of up to 14 years.

Treatment fidelity

Of the10 studies included within this review, investigators reported attempts to maintain or at least assess treatment fidelity in two studies only (McAnaney 1981 and Marques 1994). In the former study, which was of short duration, the investigator met with trainers between treatment sessions to help clarify issues and keep treatment 'on track' (McAnaney 1981, p. 80); in the latter, long-term study, investigators reported that "in order to maintain consistency and fidelity in the program, all treatment services (with the exception of individual psychotherapy) are provided according to highly structured treatment manuals... the primary evaluation tasks during the treatment phase involve the measurement of treatment fidelity and in-treatment changes" (Marques 1993, p. 199). In the trial with the second longest duration (Romero 1983), treatment fidelity to the principals of psychodynamic theory was not assessed, but it was noted that the same psychiatrists present at the commencement of the study in 1966 were almost entirely those still running therapy groups at the conclusion of data collection in 1969 (Roether 1972).
We could not establish conclusively evidence of manualisation of any other interventions described in this review, although substantial, potentially replicable data concerning the treatment programme developed by Theresa Anderson-Varney (Anderson-Varney 1991) appear within the appendices to her PhD thesis.

Outcomes

Primary outcomes

Five of the 10 studies included within this review include data related to the two primary outcomes identified in the protocol for this review (Bilby 2008): recidivism as measured by reconviction or caution or recidivism as measured by self report (Rooth 1974; Romero 1983; McConaghy 1985; McConaghy 1988; Marques 1994).
Unfortunately, of these, data for the two desensitisation studies conducted in Australia (McConaghy 1985; McConaghy 1988) are difficult to interpret and those data presented by Rooth and Marks (Rooth 1974) are impossible to disaggregate by treatment group. However, the two largest studies in the review (Romero 1983 and Marques 1994) provide robust long-term data for this outcome, based on rearrest rates and conviction rates respectively. We are conscious that inclusion of rearrest data is a deviation both from the original review we are updating (Kenworthy 2003 - the authors

of which mistakenly identified these data as identical to reconviction data) and from the protocol for this review (Bilby 2008). Therefore, we report these data conscious that arrest does not necessarily demonstrate either guilt or conviction. Investigators involved in this study (Romero 1983) themselves cite rearrest as a difficult outcome, given that police knowledge of the whereabouts of ex-prisoners might result in more routine surveillance and over-arrests when new crimes are reported. However, a second bias of the study (that rearrest data were only available from Philadelphia records) equally indicates a potential bias in the other direction, in that rearrests or even convictions in other jurisdictions would not have been identified in the analysis.

In contrast, Marques 1994 measured recidivism by 'rap-sheets' (automated records) and parole records. Participants were deemed to have reoffended if either data source recorded a new sex crime or non-sex violent crime (investigators in Romero 1983 also collected data on general offending which they believed, in accordance with the theoretical underpinnings of the intervention employed, should decrease in line with sex offending as participants learned to avoid conflicts with authority).

The remaining studies included within the review fail to provide data for recidivism either because follow-up was not allowed within the constraints of the projects (McAnaney 1981; Anderson-Varney 1991; Ryan 1997; Brown 1996) and/or the fact that the control group was a wait list (Hopkins 1991), which meant that ultimately all participants would have received treatment and long-term follow-up would have been unable to detect a treatment effect by group.

Secondary outcomes

All studies reported data on at least one of the secondary outcomes. We present details of the properties of scales used to measure these outcomes in Appendix 3.

Cognitive distortions

Outcome data on cognitive distortions were provided by two studies (Anderson-Varney 1991; Ryan 1997) using three different measures: the Abel & Becker Cognition Scale (ABCS) (Abel 1984b), the Justifications subscale of the Multiphasic Sex Inventory (MSI) (Nichols 1984) and the Cognitive distortions/Immaturity subscale of the MSI (Nichols 1984).

Sexual obsessions

Outcome data on sexual obsessions were provided by one study (Ryan 1997) using the Sexual Obsessions subscale of the Multiphasic Sex Inventory (MSI) (Nichols 1984).

Anxiety

Six studies measured anxiety (Rooth 1974; McAnaney 1981; McConaghy 1985; McConaghy 1988; Anderson-Varney 1991; Hopkins 1991) using five different instruments, plus (in McConaghy 1985) in the form of self-reported percentage of "general tension". Two studies specifically measured social anxiety (Anderson-Varney 1991; Hopkins 1991) using the Social Avoidance and Distress Scale (SAD) (Watson 1969); one of those studies (Anderson-Varney 1991) also specifically measured sexual anxiety on a subscale of the MSI (Nichols 1984). Both studies by the McConaghey team used the Stait-Trait Anxiety Inventory (Spielberger 1983); another study (McAnaney 1981) used the S-R Inventory of Anxiousness (SRIA; Endler 1962) whilst Rooth 1974 employed a self-designed composite measure of mood including scales related to irritability, tension, anxiety, depression and persecutory feelings.

Anomalous urges

Data on 'anomalous urges', 'behaviours' and 'desires' were collected from a mixed group of men involved in two studies conducted in Australia, as well as (arguably) by participants in Brown 1996, via a checklist developed for the study (the Sexual Behaviour Checklist).

Anger

No study reported data for this outcome.

Leaving treatment early

All trials provided data on whether participants completed the intervention and in some cases (notably Romero 1983) further data were provided on those considered to have attended enough sessions to meet prespecified criteria for a 'therapeutic dose' of treatment (Roether 1972).

Adverse events (including suicide or suicide attempts or sudden and unexpected death by other causes)

Adverse events were reported in two studies only: Rooth 1974 and Brown 1996. Both studies compared behavioural methods of treatment and the former involved actual physical pain (electrical shocks to the skin). Studies of general 'talking therapies' did not appear to expect or seek reports of adverse events. No study indicated that any participant died within the studies in which they participated. Whilst this does not necessarily mean this never occurred, our assessment of each study for incomplete outcome data mean we think it unlikely that lack of data on this point was a result of selective outcome reporting.

Outcomes not reported within this review

Outcomes not reported in this review include data from three different scales measuring self-esteem (measured in three studies - McAnaney 1981, Anderson-Varney 1991, and Hopkins 1991);

sexual knowledge and beliefs (two studies - Anderson-Varney 1991; Ryan 1997); fear of disapproval by others (Hopkins 1991); social/sexual desirability (Ryan 1997). Social skills were understandably measured in the two studies in which they were the focus of intervention (McAnaney 1981; Hopkins 1991). We judged (see above) that the evidence base for links between these outcomes and recidivism was lacking.

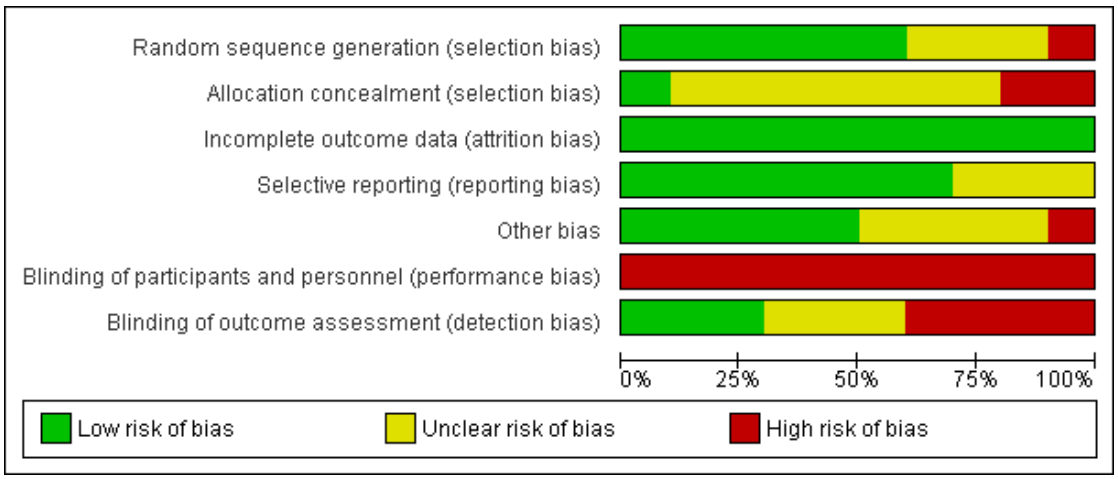
Excluded studies

Authors of the previous version of this review excluded 57 studies in October 2002 (Kenworthy 2003). We added 26 further excluded studies to the present version. Chief reasons for exclusion include design (many papers proved to report only case series or accounts of retrospectively examined outcomes for nonequivalent groups (that is, those who had chosen or refused treatment)). In some cases, we excluded randomised controlled trials because the populations were ineligible (that is, primary prevention studies of date rape amongst university students with no history of offending). See Characteristics of excluded studies.

Risk of bias in included studies

See also Figure 2.

Figure 2. Risk of bias graph: review authors' judgements about each risk of bias item presented as percentages across all included studies



Allocation

Sequence generation

Information on sequence generation was rarely present in the study reports available to the review authors, but personal contacts with investigators clarified methods in many cases.

In eight studies, the risk of bias for this criterion is assessed as 'low' with study authors reporting the following methods: coin tossing (Anderson-Varney 1991; Marques 1994; Brown 1996), drawing lots or picking names out of a hat (Hopkins 1991; Ryan 1997) or a random numbers table (McAnaney 1981; McConaghy 1985; McConaghy 1988).

In one study, the risk of bias for this criteria remains 'unclear'. Information on the method of sequence generation reported by Rooth and Marks (Rooth 1974) was missing from the published paper and the investigators can no longer be contacted.

Concerning the study reported in Romero 1983, the original investigator describes a complex procedure involving a 'ratio device', which appears to have something in common with the later technique of minimisation, but a researcher once close to another investigator involved in the study claimed in contrast that this study was randomised by drawing lots (Brecher 1978). The rating for the risk of bias for this criterion for this study, based on the lack of clarity in the description within Roether 1972, must remain 'high'.

Allocation concealment

No study in this review reported any attempt at concealment of allocation, and almost no information gained directly from investigators resulted in any information suggesting that any measures were taken to conceal allocation for any study (the exception being one investigator involved in Brown 1996, who opined that neither staff nor participants were aware of the order of crossover treatment at the start of the trial, but did not state a method used to achieve this (Federoff 2012 [pers comm])). Overall the assessment of risk of bias for this criterion is therefore 'unclear' at best in seven studies (Rooth 1974; Romero 1983; Hopkins 1991; Anderson-Varney 1991; Marques 1994; Brown 1996; Ryan 1997) and 'high' in three studies (McAnaney 1981; McConaghy 1985; McConaghy 1988) where investigators confirmed that allocation was unconcealed (McAnaney 2011 [pers comm]; Blaszczynski 2011 [pers comm]).

Blinding

Blinding of participants and personnel

Blinding of personnel and participants was impossible for any study included within this review. The risk of bias for these criteria are obviously therefore high in all cases, particularly as outcomes were often assessed (especially for studies of shorter duration) mostly or entirely by self report.

Blinding of outcome assessors

Blinding of outcome assessment was impossible in two studies where the primary investigator also was responsible for delivering therapy or collecting outcome data or both, and both were assessed as being at high risk of bias (Romero 1983; Anderson-Varney 1991). Blinding of outcome assessors was at least feasible in a third study McConaghy 1988 (and had been undertaken by the first author of in a previous study (McConaghy 1985) but was not conducted in the later one. The sole outcome of one study, the Sexual Behaviour Checklist (Brown 1996) was a self-report questionnaire and blinding was thus impossible.

We assessed three studies to have a low risk of bias for this criterion: McAnaney 1981; McConaghy 1985 and Marques 1994. These judgments were determined in two cases based on personal communications (McAnaney 2011 [pers comm]; Marques 2011 [pers comm]). Three studies for which a rating of 'unclear' was given were Rooth 1974, and Ryan 1997. This is because there is a mixture of assessors in Hopkins 1991 and whilst efforts were made to blind assessors of videotaped interactions sufficiently, substantial doubt exists as to whether attempts to blind prison landing staff to treatment status could have been successful (the investigator herself doubts this (Marshall 2011a [pers comm]). In the latter two studies, details of process in the reports are not clear and contact with investigators could not be made (Rooth 1974; Ryan 1997).

Incomplete outcome data

It has been reported that sexual offenders who refuse treatment or drop out of treatment are more likely to reoffend (Hanson 2002; Hanson 2005; Lösel 2005; Olver 2011), so this criterion is of particular importance to this review.

The risk of bias to this review as a result of incomplete outcome data appears to be, overall, low. Reporting for this criterion tended to be good and dropouts low.

For the largest study included within the review (Marques 1994), intention-to-treat analysis was performed according to *'treatment as assigned'* (Marques 1994). Five relatively short-term studies reporting little or no dropout were McConaghy 1985; McConaghy 1988; Anderson-Varney 1991; Hopkins 1991 and Rooth 1974. Three studies reported a loss of 15% or more of participant data but these were evenly distributed between arms of the study and the reasons given for leaving were clearly described; therefore these studies are also given a rating of 'low' risk of bias (McAnaney 1981; Brown 1996; Ryan 1997).

Initial concerns over the participant flow into and out of Romero 1983 were resolved by contact with one of the researchers concerned with analysing 10-year follow-up data (Williams 2011 [pers comm]) and also with scrutiny of the unpublished data underlying some of that analysis (Roether 1972). The original investigators of this study (Hermann Roether and Joseph Peters) excluded from their analysis participants who took part in a group introduced halfway through their study in which groups were 'self directed' and not led by a therapist. This led to the exclusion of data from 29 participants from the initial randomised total of 264. Romero and Williams, working years after the initial study, claimed 100% follow-up but also described the exclusion of data of a group of homosexuals with a high level of offences according to contemporary legal standards. Clarification has been received that these exclusions were due to changes both in the law and in DSM criteria and therefore we do not believe these missing data are a source of bias to this review (Williams 2011 [pers comm]) as participants neither had a psychosexual disorder nor were offenders.

Selective reporting

Overall, we consider that despite having no access to any protocols for any studies included within this review, investigators appear to have reported all outcome data that could reasonably be expected to emerge from the trials with which they are associated, in accordance with their (sometimes very different) participant groups, aims and outcomes. All studies therefore were assessed as being at 'low' risk of bias with the exception of McConaghy 1988, which was assessed as 'unclear', given that not even means were reported for one outcome (anxiety) when means had been reported in a trial conducted by the same author group three years previously (McConaghy 1985).

Other potential sources of bias

Some unexpected potential sources of bias came to light whilst extracting data for this review. For example, one investigator felt that, in retrospect, one measure she had chosen to assess changes in cognition were 'transparently obvious' and may have been affected by participants' 'social desirability and demand characteristics' (Anderson-Varney 1991, p. 81); it was felt that this represented an unclear risk of bias to the review. The absence of any baseline data on offending characteristics or demographic information of any kind (for example, age or type of offence) for any participant in the study conducted by Ryan (Ryan 1997) was considered to be a source of high risk of bias, given that the study was small, employed a cluster design, and that the investigator reported that outcome data were not normally distributed.

Baseline imbalance was the source most commonly identified by the investigators of studies themselves (three in total) as a potential source of bias. This was true not just in small studies where one might expect it (for example, McAnaney 1981) or in studies which were both small and employed a cluster design and lacked any baseline measures at all (Ryan 1997) but in the largest, latest and arguably best-conducted study included within this review (Marques 1994). Here, investigators noted that participants in the treatment group were significantly more likely to have previously been committed for treatment as mentally disordered sex offenders (and have significantly higher static risk scores) than participants in the control group. Investigators conclude in their final report: "despite random assignment, the mean risk score of the RP group (2.25) was significantly higher than the mean score of the [control group used in this review] (1.88) and the [secondary control group not used here] (1.88), $F_{(2,635)} = 3.71$, $P = 0.025$". Investigators also comment: "to determine if this difference resulted from a non-random source...we compared the Static-Lite scores of the various subgroups of offenders who were originally assigned to [treatment]. No differences were found among the risk scores of the 167 treatment completers, the 23 late dropouts, the 14 early dropouts, and the 55 individuals assigned to treatment who never showed up" (Marques 2005, p. 93). Thus, we determined that Marques et al appeared to have demonstrated that this baseline imbalance was unlikely to have introduced a risk of bias (Marques 2005).

Effects of interventions

The protocol for this review permitted comparisons between eligible treatments and no treatment, wait list control and other psychological treatment. Pharmacological treatment was eligible only if present in both arms of a study so that the psychological treatment in one arm would be the only difference between two treatment groups.

We divide outcomes where possible into immediate (within 6 months), short term (more than 6 months to 24 months), medium term (more than 24 months to 5 years) and long term (beyond 5 years) during the period at risk beginning at the point of release from prison or discharge from hospital. For participants receiving treatment in the community, the period 'at risk' was viewed as commencing from the end of treatment.

We present outcomes by comparison, grouped in the following order:

1. Behavioural only interventions (four studies: Rooth 1974**,** McConaghy 1985**,** McConaghy 1988**;** Brown 1996**)**

It was not possible to pool data for the four studies using primarily behavioural treatments for sex offending because interventions and controls differed greatly. Results are therefore given for each sub-comparison/study separately.

a) Cross-over/treatment order effects study comparing electric aversion therapy, muscle relaxation and self-regulation in different orders (Rooth 1974)

b) Imaginal desensitisation versus covert sensitisation (McConaghy 1985)

c) Imaginal desensitisation plus drug treatment versus drug treatment alone (McConaghy 1988)

d) Masturbation prohibition ("sexual sobriety") versus no treatment (Brown 1996)

2. Cognitive behavioural interventions (five studies: McAnaney 1981; Anderson-Varney 1991; Hopkins 1991; Marques 1994; Ryan 1997)

a) Cognitive behavioural treatment programmes versus no treatment/wait list control (four studies: McAnaney 1981; Anderson-Varney 1991; Hopkins 1991; Marques 1994)

b) Transtheoretical counselling versus cognitive behavioural treatment programme (one study: Ryan 1997)

3. Psychodynamic/psychoanalytic interventions (one study: Romero 1983)

Whilst not meeting the strict criteria for a psychoanalytic intervention (Description of the intervention), the treatment involved in this study appears to meet criteria for group psychodynamic psychotherapy. The intervention also involved limited probation services, whereas control group received more extensive probation services.

1. Behavioural only interventions (four studies: Rooth 1974, McConaghy 1985, McConaghy 1988; Brown 1996)

a) Comparison of electric aversion therapy, muscle relaxation and self-regulation (Rooth 1974)

Rooth 1974 reports a small study in which the order of treatment was varied to allow comparison of three interventions: electric aversion therapy, muscle relaxation and self-regulation. Participants were men (n = 12) with persistent exhibitionism who were randomly allocated to receive the three treatments "in one or other of the six possible combinations" (Rooth 1974, p. 231).

Data on recidivism and on the secondary outcomes of anxiety and anomalous urges are provided, but not always disaggregated by treatment group. A further complication is that the investigators intensified the three treatments halfway through the trial to elicit "a more clear-cut difference", so that the remaining six participants received double the number of treatment sessions. This constitutes a change in the intervention even though the investigators reported it made no difference to the outcome (p. 235).

The previous version of this review (Kenworthy 2003) viewed this study as a cross-over trial and only reported data available after one week of treatment. We choose here, however, to report data narratively and in line with the investigators' aims (that is, examination of the effect of the order of interventions, as well as of any potential synergistic effect between them).

Primary outcomes

Recidivism as measured by reconviction, caution or self report

Recividism data were only available in aggregated form at one week, at three weeks, and when participants left inpatient treatment (between 1.5 and 6 months after entering the three-week treatment phase). Data were not disaggregated by intervention, however, and it is difficult to agree with the investigators' conclusions that "the gains made and sustained during treatment are worthwhile. In the context of such chronic histories, a return of some exposing or even further convictions are still compatible with a useful degree of overall improvement. Our findings indicate the need for a reappraisal of society's current punitive and pessimistic attitudes toward persistent exposers" (p. 246).

Secondary outcomes

Mixed (aggregated)

Results of the six possible intervention combinations are reported separately by time period, but these data are limited as no single intervention lasted longer than a week. When all six main outcomes were combined, mean change scores per participant seemed to indicate that aversion was superior to self-regulation, and that both were superior to muscular relaxation. The investigators noted that aversion and self-regulation can produce a synergistic effect and, consistent with previous research, that "aversion tended to do best when given first, and self regulation when given third" although relaxation "was not examined because its effects were so small" (Rooth 1974, p.239). No deaths were reported, and no participants left the study early or were lost to follow-up. Adverse events were reported within the aversion phase during which electric shocks were applied ("seven patients had marked tension and three marked anger" (p. 240)), and within the self regulation phase ("patients were upset at having to expose themselves in the mirror while listening to their tape-recorded account of exposing" (p. 241)).

b) Imaginal desensitisation versus covert sensitisation (McConaghy 1985)

This small (n = 20) study compared imaginal desensitisation with covert sensitisation (the 'carrot' of relaxation versus the 'stick' of imagining dire consequences of a behaviour) (McConaghy 1985). In imaginal desensitisation, participants were asked to imagine potential offending situations, not to complete the behaviour, and to remain relaxed. In covert sensitisation, participants were required to imagine offending situations followed by the intrusion of negative stimuli, for example, the arrival of a police officer, family, or friends; or of suddenly being violently sick.

Primary outcomes

Recidivism

Recividism data are reported in McConaghy 1985 using data obtained from solicitors or third party requests to clinicians in this trial for reports on participants who had been formally charged for 'sexually anomalous behaviour'. Investigators report that "following treatment, no patient [of ten treated] was charged with carrying out sexually anomalous behaviour" (McConaghy 1985, p 182). This effect appeared to endure for a year, although one participant in the imaginal desensitisation group requested further treatment compared with two of those allocated to covert sensitisation; the meaning of this information out of context is difficult to interpret.

Secondary outcomes

Cognitive distortions

No data were reported for this outcome.

Sexual obsessions/anomalous urges

i) Self-report of urges/desire

Investigators reported data on 'anomalous urges' in the form of participant reports of percentage reductions in urges. Percentages are given without standard deviations and are therefore reported narratively here. For the imaginal desensitisation group (n = 10) post-treatment there was an average decrease in self-reported anomalous urges of 87%, which was stable at one month follow-up (again, 87%) and 89% at one year for the nine participants remaining. Decrease in self-reported anomalous urges for the covert sensitisation group (n = 10) was less strong, but still positive with an average decrease in self-reported anomalous urges of 58%, which improved at one month follow-up (65%) but appeared to have reduced to 55% at one year for the seven participants remaining.

ii) Anomalous desire (assessed via interview by blinded assessors)

After combining 'possibly reduced' and 'reduced' categories, the result of comparing dichotomous data for this outcome between the covert sensitisation group and the imaginal desensitisation group was not statistically significant at one month (risk ratio (RR) 0.57;

95% CI 0.24 to 1.35; P = 0.20; Analysis 1.1) or at one year (RR 0.60; 95% CI 0.19 to 1.86; P = 0.38; Analysis 1.1).

Anxiety

Investigators reported data for this outcome using the State-Trait Anxiety Inventory (Spielberger 1983) (both measures). Means were provided without either standard deviations or precise P values, and personal contact with investigators revealed such data have not been preserved (Blaszczynski 2011 [pers comm]), so results can only be reported narratively as per the investigators' findings. There "...was a significant reduction in the mean level of state anxiety at 1 month [48.7 to 34.1] and 1 year follow up [34.4] in patients who received imaginal desensitisation, but only at 1 year follow up [46.3 at pretreatment, 44.2 at 1 month, 36.7 at 1 year] for those who received covert sensitization" (p. 184).

Investigators report only combined data for both groups for the outcome of trait anxiety, which appeared to be significantly reduced at one year (from a mean of 42.1 to 35.4 in the imaginal group and from 50.0 to 40.0 in the covert group). Percentages of "general tension" were also reported as having been reduced in both groups.

Anger

No data were reported for this outcome.

Leaving treatment early and loss to follow-up

No participant left the study early. At one year follow-up, one participant had been lost to treatment from the imaginal desensitisation group and three from the covert sensitisation group (RR 0.33; 95% CI 0.04 to 2.69; P = 0.30; Analysis 1.2).

Adverse events

Investigators report that, in the view of some independent assessors, anomalous behaviours were not increased in some participants by treatment, at immediate post-treatment. However, of those participants remaining at a year's follow-up it was judged that one participant in the covert sensitisation group had demonstrated 'increased' anomalous behaviour. No deaths or parasuicidal acts were reported.

c) Imaginal desensitisation plus drug treatment versus drug treatment alone (McConaghy 1988)

Primary outcomes

Recidivism as measured by reconviction or caution or self report

Recividism data are reported in this three-armed study (n = 31 overall, of which 21 are relevant to this review) using data obtained from solicitors or third party requests to clinicians on participants who had been formally charged for 'sexually anomalous behaviour'. At two years' follow-up, investigators found that no participant who had received both imaginal desensitisation (ID) and medroxyprogesterone had received a charge for sexually anomalous behaviour, whereas one participant who had received the drug treatment alone had been charged.

Secondary outcomes

Cognitive distortions

No data were reported for this outcome.

Sexual obsessions/anomalous urges

i) Self-report of urges/desire

As in the previous study, investigators reported data on 'anomalous urges' in the form of participant reports' of percentage reductions in such urges. Percentages are given without standard deviations and are here reported narratively. At one month, for the combined treatment group (medication plus ID, n = 10), there was an average decrease in self-reported anomalous urges of 70%, which improved at one year follow-up (75%). Decrease in self-reported anomalous urges for the medication only group (n = 10) was initially less strong at 67%, but surpassed that reported by the combined group at one year, with the medication-only group reporting an 86% reduction in urges.

ii) Anomalous desire (assessed via interview by blinded assessors)

After combining 'possibly reduced' and 'reduced' categories, the result of comparing dichotomous data for this outcome was not statistically significant at one month (RR 1.67; 95% CI 0.54 to 5.17) nor at one year (RR 0.60; 95% CI 0.46 to 2.17) (Analysis 2.1).

Anxiety

'General tension' was reported as a self-report measure. All participants within the trial, but particularly those with medication, reported a decrease in tension (between 52% and 69%).

Anger

Whilst data appear to have been collected for this outcome on the Stait-Trait inventory (Spielberger 1983), no means or standard deviations were reported in the published paper and personal contact with a surviving investigator has not been successful (Blaszczynski 2011 [pers comm]).

Leaving treatment early

One participant allocated to combined treatment was "concerned about his low level of normal sexual interest decided not to have [drug] treatment when informed his plasma testosterone was low. He was treated with ID alone and excluded from the study" (p. 200).

Lost to follow-up

No participants were lost to follow-up except the one participant mentioned above.

Adverse events

No adverse events were reported in the imaginal desensitisation alone group (not included within this review). Adverse events of the drug treatment (medroxyprogesterone) were reported in both of the other treatment arms. The result of comparing combined data for any adverse events (including reduced heterosexual desire;

reduced frequency of heterosexual intercourse, painful urination and headaches) between conditions was not statistically significant (RR 0.57; 95% CI 0.24 to 1.35; P = 0.20; Analysis 2.2). No deaths or parasuicidal acts were reported.

d) Masturbation prohibition ('sexual sobriety') versus no treatment (Brown 1996)

Primary outcomes

Recidivism as measured by reconviction or caution or self report

No data were reported for this outcome.

Secondary outcomes

Cognitive distortions

No data were reported for this outcome.

Sexual obsessions/anomalous urges

Self-report of urges/desire (self-report using a nine-item Sexual Behaviour Checklist (SBC)).

Only three of the 17 participants reported being 'able to not masturbate during the 30-day MNA condition'. Data were not available for the first period (prior to cross-over). Investigators described their main finding as being that 'advising sex offenders (in this case pedophiles) to not masturbate does not change the frequency of self reported sexual behaviours, nor does it alter the intensity of normal or deviant sexual urges' (Brown 1996, p. 402).

Anxiety

No data were reported for this outcome.

Anger

No data were reported for this outcome.

Leaving treatment early

No participants left the study early; however, two participants 'failed to complete their SBC forms properly' (their data were not used) and a third reported a masturbation frequency that was '> 2 standard deviations above the mean of the MA condition' (data excluded).

Adverse events

Two participants (of 17) reported feeling that not masturbating 'increased their paraphilic interests'. No deaths or parasuicidal acts were reported.

2. Cognitive behavioural programmes (Anderson-Varney 1991; Hopkins 1991; Marques 1994; McAnaney 1981; Ryan 1997)

a) Cognitive behavioural treatment programmes versus no treatment/wait list control (four studies: Anderson-Varney 1991; Hopkins 1991; Marques 1994; McAnaney 1981)

Primary outcomes

Recidivism as measured by reconviction, rearrest or caution

In the study involving the most intense and complex intervention and the highest number of participants included in this review (Marques 1994) (n = 481, full Ns used as investigators employed intention-to-treat analysis), long-term outcome data are reported for groups in which the mean years 'at risk' in the community are similar (8.3 years for treatment (n = 259) compared to 8.4 in the control group (n = 225)). The result of comparing reconviction for sexual offences between conditions was not statistically significant (RR 1.10; 95% CI 0.78 to 1.56; P = 0.59); Analysis 3.1)

Recidivism as measured by self report

No data were reported for this outcome by any study within this comparison. Three out of four studies (McAnaney 1981; Anderson-Varney 1991; Hopkins 1991) did not report long-term on offenders (all of whom were incarcerated during the intervention periods); the fourth study (Marques 1994) had sufficient long-term post-release follow-up, but reported official statistics only.

Secondary outcomes

Cognitive distortions

Outcome data on cognitive distortions were provided by one study within this comparison (Anderson-Varney 1991) using the Abel & Becker Cognition Scale (ABCS) (Abel 1984b) as well as, arguably, the 'Justifications' subscale of the Multiphasic Sex Inventory (MSI) (Nichols 1984) (although the MSI also has a separate 'Cognitve distortions' scale not chosen by this investigator). Analysis 3.2 includes data from the former scale as this best matches the 'brief' of the measurement of cognitive distortions. We note however that the investigator herself reported concerns that the scale's items are "transparently obvious" and "may be affected significantly by social desirability and demand characteristics" (p. 81). The result of comparing the treated group and the no treatment control was statistically significant, favouring the former (mean difference (MD) 13.43; 95% CI 6.81 to 20.05; P < 0.0001; Analysis 3.2).

Data for results from the *'Justifications'* subscale of the Multiphasic Sex Inventory (MSI) (Nichols 1984) are skewed and reported in tabular form (Analysis 3.3). The investigator reported that comparisons between conditions were not statistically significant.

Sexual obsessions/anomalous urges

We assessed that data from the 'Sexual inadequacies' scale of the MSI (Multiphasic Sex Inventory) (Nichols 1984) as used in Anderson-Varney 1991, were applicable to this outcome. The result of comparing conditions on this outcome was not statistically significant (MD -6.20; 95% CI -13.46 to 1.06; P = 0.09; Analysis 3.4).

Anxiety

Anxiety was measured as a global outcome in three studies within this comparison (McAnaney 1981; Anderson-Varney 1991; Hopkins 1991) with McAnaney 1981 using two instruments (the SADs (Watson 1969) and the SRIA (Endler 1962)).

The result of comparing conditions on this outcome was not statistically significant (SMD -0.23; 95% CI -0.61 to 0.14; P = 0.22; Analysis 3.5).

Anger

No study within this comparison reported data for this outcome.

Leaving treatment early

No participants were reported to have left treatment early in three of the four studies in this comparison (McAnaney 1981; Anderson-Varney 1991, Hopkins 1991). These studies were similar in that they were of relatively short duration and involved incarcerated offenders. In one study (McAnaney 1981) outcome data of three participants were excluded because two participants missed several treatment sessions and one exercised his right not to participate in the post-testing but had in fact completed treatment. Data for leaving Marques 1994 are reported in tabular form in Analysis 3.6. It will be noted that no control group participants left the study early but that 55 participants dropped out before starting treatment and 37 left during treatment. Investigators commented that treatment completers (167) and dropouts (37) did not differ significantly on 'measures of static risk, treatment need, or demographic variable other than age' (treatment dropouts were significantly younger than completers).

Lost to follow-up

Post-treatment follow-up data were not collected in three studies within this comparison (McAnaney 1981; Anderson-Varney 1991, Hopkins 1991).

Adverse events

No study within this comparison appears to report prospectively sought or retrospectively identified adverse events amongst participants. No deaths or parasuicidal acts are reported.

b) Transtheoretical counselling versus cognitive behaviour treatment (one study: Ryan 1997)

The one study using this comparison (Ryan 1997, n = 65) investigated a novel treatment against CBT as standard care.

This study employed a cluster design, although the investigator's analysis does not appear to have controlled for the effect of clustering. We have therefore included data calculated by authors of the previous version of this review (Kenworthy 2003) as follows: 'Design effect calculated as 1+((m-1)*ICC). m=mean size of group - taken as 8. ICC = Interclass Correlation Co-efficient - taken as 0.1'.

Primary outcomes

Recidivism as measured by reconviction, rearrest or caution or self report

This study lasted six months and provided no data on recidivism of any kind.

Secondary outcomes

Cognitive distortions

Data for this outcome were measured on the using mean subscales of the Multiphasic Sex Inventory (MSI) (Nichols 1984) relating to cognitive distortions and immaturity and dichotomised to provide results in the form of relative risks. The result of comparing conditions on this outcome was not statistically significant (RR 1.00; 95% CI 0.76 to 1.32; P = 1.00; Analysis 4.1)

Sexual obsessions/anomalous urges

Data for this outcome were measured on the using mean subscales of the Multiphasic Sex Inventory (MSI) (Nichols 1984) relating to sexual obsessions and dichotomised to provide results in the form of relative risk. The result of comparing conditions on this outcome was not statistically significant (RR 1.50; 95% CI 0.80 to 2.81; P = 0.21; Analysis 4.2).

Anxiety

No data were reported for this outcome.

Anger

No data were reported for this outcome.

Leaving treatment early

A number of participants left this study early, but the result of comparing conditions on this outcome was not statistically significant (RR 1.68; 95% CI 0.80 to 3.49; P = 0.17; Analysis 4.3).

Lost to follow-up

No follow-up data were collected beyond the study's termination at six months.

Adverse events

No deaths or parasuicidal acts were reported.

3. Psychodynamic/psychoanalytic interventions (one study: Romero 1983)

Whilst not meeting the strict criteria for a psychoanalytic intervention, the treatment involved in this study appears to meet criteria for group psychodynamic psychotherapy, plus limited probation services. The control group received more intensive probation services, which comprised 'standard care' for the time and place where the trial was conducted (1960s, Philadelphia, USA).

This study (Romero 1983) is the oldest included within this review and the second largest (n = 231). Results were difficult to interpret and substantial correspondence with a surviving investigator was required to clarify many matters of participant flow, numbers of participants for whom data were analysed and general methodology (Williams 2011 [pers comm]). See also Characteristics of included studies.

Primary outcomes

Recidivism as measured by reconviction, rearrest or caution

Investigators make a strong argument that rearrest is a more valid measure than reconviction (as suggested by Soothill 2000 who favoured reconviction) because (at least at the time) 'the criminal

justice system is organized in favor of the sex offender' (Romero 1983, p. 67).

Results for this study (Romero 1983) comparing conditions in terms of sexual offending as measured by rearrest were not significant at 10-year follow-up (RR 1.87; 95% CI 0.78 to 4.47; P = 0.16; Analysis 5.1).

Secondary outcomes

No data were reported on any of the following secondary outcomes: cognitive distortions; sexual obsessions/anomalous urges; anxiety; anger; adverse events including suicide or suicide attempts; sudden and unexpected death by other causes.

DISCUSSION

Summary of main results

Ten studies (n = 1008) involving data from 944 eligible participants are included within this review.

Results of behavioural programmes

Four trials involving 70 men compared behavioural programmes with other behavioural programmes or with a wait list control. Meta-analysis was not possible for this comparison.

For two studies (both cross-over, n = 29) no disaggregated data were available. The remaining two studies compared imaginal desensitisation with either covert sensitisation or as part of adjunctive drug therapy (n = 20 and 21 respectively). Investigators reported encouraging results for the primary outcome (being 'charged with anomalous behaviour') for both of these latter studies, but these findings are not robust as controls had now disappeared. In one study there was only one new charge for the treated groups at one year post-treatment, and in the other only one new charge over two years. These results must be interpreted with caution, however, as the contemporary inclusion criteria for 'anomalous behaviour' meant that many participants would not now seek or be offered treatment.

Results of cognitive behavioural programmes

Five studies involving primarily cognitive behavioural interventions (CBT) (n = 664) were included. Of these, four compared CBT with no treatment/wait list control and one with standard care. Only one study (involving the most intense and complex intervention and the highest number of participants included within this review (n = 481)) collected data on the primary outcome of recidivism. Here, long-term outcome data are reported for groups in which the mean years 'at risk' in the community are similar (8.3 years for treatment (n = 259) compared to 8.4 in the control group (n = 225)). Results as measured by reconviction for sexual offences trend slightly in favour of the no treatment control group (RR 1.08; 95% CI 0.76 to 1.53).

Results of psychodynamic interventions

One study compared psychodynamic intervention with probation. Results for this study (n = 231) indicate a slight trend in favour of the control group (probation) over the intervention (group therapy) in terms of sexual offending as measured by rearrest (RR 1.87; 95% CI 0.78 to 4.47) at ten year follow-up.

Overall

The main finding of this systematic review is that there was no evidence from any of the trials in favour of the active intervention in a reduction of sexual recidivism - the primary outcome. While most studies did not even report on this outcome as they lacked sufficient follow-up, the two that did so (Romero 1983; Marques 1994) were also the largest (with a combined size of 777, well over half of the participants in this review) and had a long follow-up (minimum eight years). Yet neither showed any benefit for the intervention. Thus, neither Marques' SOTEP's CBT-type therapy nor Romero's lengthy psychodynamically-oriented treatment appeared to reduce sexual recidivism. Even in trials where investigators concluded on a positive note (for example, Rooth 1974), data at the close of the study (14 months) suggested that rearrests and convictions had not in fact changed.

In addition, few other studies provided convincing effects for the intervention on any of the other secondary outcomes considered to have the potential to be a 'dynamic risk factor' for sex offending (that is, sexual obsessions or anger). This may be because measurement of these outcomes was less common than for factors now considered to have a weaker link to recidivism (for example, sexual knowledge and social skills (Seto 2010)). Adverse events were only considered in two studies.

It was regrettable too that other kinds of offences (particularly other violent offences) were recorded as an outcome in only the SOTEP trial (Marques 1994) (where, again there was no difference in the rates between the two groups) and the Philadelphia study (Romero 1983). In the latter study, analyses were particularly detailed as regards type of sex offender (findings as expected: rapists "were found to commit almost as many nonsexual violent offences as sexual offences"; in contrast to exhibitionists, for example, whose nonsexual offences were rare). Failure to collect data is an unfortunate omission as sexual offenders are not restricted to this type of offending when they re-offend (Hanson 1996); hence this is surely a legitimate and valid outcome to measure in more studies. While there was no obvious benefit from the interventions, the failure routinely to measure harm meant that this could not be assessed in this review. This omission is not unique to psychological treatments for sexual offenders, but commentators are increasingly calling for it to be made routine when these are evaluated, as is the case with pharmacological interventions (Lilienfield 2007). This is especially important when treatments are delivered in a group format (which most of these treatments are) as 'deviancy

training' within the programme may cause unexpected iatrogenic effects (Dishion 1999).

In one follow-up of sexual offenders that linked the individual's behaviour in treatment with their likelihood of subsequent sexual offending, Seto and Barbaree (Seto 1999) made an unexpected discovery. This was that those who exhibited better behaviour in the programme (that is, on the quality of their homework, therapist's rating of motivation and treatment progress, etc.), committed more sexual offences on release. They hypothesised that this association between good in-treatment behaviour and subsequent increased rates of recidivism (which was found in high-scoring psychopaths) could be due to (a) client skillfulness in deceiving the therapist or (b) the treatment enabling the psychopath to learn additional interpersonal skills that could be put to use with victims after their release. Although a subsequent more extended follow-up did not confirm the original finding, this report suggests (a) one ought to be cautious on relying solely on in-session behaviour when judging the effectiveness of the treatment and (b) the importance of an extended follow-up as the final arbitrator, even when other proxy indicators are recorded.

Overall completeness and applicability of evidence

The evidence reviewed here has confirmed the impression of a move away from psychodynamically-informed therapies (Roether 1972) to more strictly behavioural therapies and CBT-type therapies. SOTEP (Marques 1994) can be seen as an exemplar of this approach. This trial's failure to show a positive effect - despite being well-designed, comprehensive, long term and costly to implement - has had the unfortunate consequence that no further major trials have been conducted within the past 15 years. This failure to carry out further trials because a well-designed trial did not show a positive effect ignores the gain in knowledge in identifying an intervention that may be no better than the comparator (and may indeed be worse). The meagre number of trials to consider, many of which are dated, is therefore a major limitation.

We also note some other limitations in the evidence. All studies identified were based in Western countries and all reports were in English. This cannot be representative. Understandably all the studies report data from male participants only, given the preponderance of male gender as sexual offenders; hence there are no data on the effects of interventions for female sex offenders that, despite being a minority, are now beginning to receive attention (Freeman 2008). Only two of the studies reported on our primary outcome of sexual re-offending (Romero 1983; Marques 1994).

Many studies suffer from heterogeneity in their target population, mixing types of offenders (for example, rapists and paedophiles (Hopkins 1991), and even paraphilics with as-yet no contact with law enforcement, with clothes fetishists, transvestites and paedophiles in the same trial (for example McConaghy 1985; McConaghy 1988)). The subsequent failure to disaggregate the outcome data for these different groups renders these data difficult to interpret.

Another difficulty is that what constitutes sexual offences is itself changing so that some offence categories disappear and new offending patterns emerge. The Romero and McConaghey studies are examples of the former, as data for consenting homosexuals may have been included or at least collected; yet these data are now of no interest to this review (Romero 1983; McConaghy 1985; McConaghy 1988). Equally, chemically-assisted sexual offences (for example, rapes facilitated by giving prospective victims particular drugs), which are currently topical, is not an area that this review can inform. Finally, this review does not include participants who form a growing proportion of those currently charged with sexual offences 'online' (those charged for generating or downloading images of child pornography, for example), although it is reassuring that the evidence here does not point to a progression from internet pornography to direct sexual offending (Seto 2009).

Finally, we should mention the importance of the 'What Works' literature as this is now highly influential in the development of programmes to reduce offending of whatever type. This is based on the Risk/Need/Responsivity (RNR) principle so that a reduction in recidivism may be expected when high risk offenders are targeted and their criminogenic needs addressed in a responsive manner (Andrews 1998). As there is no a priori reason why the reduction in sex offending ought to be any different in principle from reducing any other types of offending, it is likely that this approach ought to be equally influential in the design of interventions for sex offenders in the future. Indeed, there is evidence that that is already the case, as Hanson et al's recent meta-analysis reported that programmes that adhered to RNR principles yielded the largest reductions in both sexual and general recidivism (Hanson 2009); notwithstanding the fact that another meta-analysis has highlighted the fact those at higher risk are more likely to drop out of treatment (Olver 2011). The fact that these principles have not been sufficiently evaluated in rigorous trials is surely a missed opportunity.

Quality of the evidence

There were only 10 studies that we could include in this review, with the total number of participants randomised being 1008, which is far fewer than the number that would give one any confidence in the findings. Moreover, although the average number of participants is more than 100 per trial, the weight is skewed by the large SOTEP (Marques 1994) study that compensates for a multitude of smaller ones. This limits our confidence in the results. Blinding will never be possible for participants or personnel in this field, and with a large proportion of outcomes being by self-report, this inevitably places results of this type of review at a high risk of bias.

Incomplete outcome data were rare in this review, but this was at least partly due to the fact that so few studies were sufficiently well

resourced to follow participants beyond prison and into 'at risk' periods in the community. Even if they were, the fact that wait list controls were a feature of trial design (required in some cases by ethics committees) mean that long-term data would not always be informative.

Choice of outcomes by triallists was inevitably swayed by contemporary theories about sex offending and which hypothesised risk factors ought to be the focus of the intervention. In this review, interventions for sex offenders are at an advantage in that they can be informed by several meta-analyses (Hanson 1996; Hanson 2005) that identify some robust predictors of sexual recidivism including certain dynamic factors that might be candidates for further programme development and testing, such as deviant sexual preferences, an antisocial orientation, sexual preoccupation and difficulties in self-regulation. In addition, they have identified other variables (for example, victim empathy, denial of sexual crime) that have no relationship with sexual or violent recidivism. Unfortunately, the implications of these findings do not appear to have been appreciated by many practitioners, so that these remain treatment targets in many sexual offender programmes currently despite having no relationship with either sexual or violent recidivism. Similarly, Seto and Lalumiere (2010), in a recent meta-analysis of male adolescent sex offenders, suggest social incompetence might be associated with delinquency (consistent with longitudinal and other research) but that social incompetence does not help explain why a juvenile commits sexual offences rather than other kinds of offences. Thus, their need is not necessarily social skills training, rather it is a discussion about their abnormal sexual attitudes (Seto 2010). The findings from these meta-analyses - and others like them - ought to focus the content of the interventions in the future.

Potential biases in the review process

We believe we have identified all relevant RCTs in this field and are supported in this view by the opinion of all surviving contactable investigators (nine out of ten) of the included studies, none of whom were able to identify any trials we had missed. Our choice of outcomes could be considered a potential bias but we believe we have justified the exclusion of some (for example, social skills), which are no longer believed to have an 'evidence base' in terms of risk factors for recidivism.

Again, the failure to find a positive result from the SOTEP trial in the 1990s (Marques 1994) has not resulted in any additional new trials in the interim; rather, it has had the reverse effect, with not a single RCT conducted within the past 15 years. Having inadequate evidence to support any psychological intervention for sex offenders, and making no attempt to address this gap in the evidence, is clearly not sustainable. We are interested to observe a spirited debate between those who take the position adopted in this review - viz, that there is no evidence to support any intervention and that RCTs are urgently needed to address this deficit (for example Seto 2008b; Rice 2012) and others (for example Marshall 2007; Marshall 2008) who argue the reverse. While not wishing to rehearse these arguments and clearly having a bias in favour of RCTs, we believe that some of the arguments put forward by Marshall (that is, as some individuals benefit from the interventions, this indicates that the interventions are effective (for some) and that RCTs take no account of the contributions of the individual therapist - a crucial responsivity factor) are misplaced.

Agreements and disagreements with other studies or reviews

Although superficially it may seem that the conclusions from this review appear more negative than those of other reviews, a more careful analysis shows this not to be the case. While the preceding Kenworthy 2003 review, for instance, may have arrived at a more optimistic conclusion, this antedated the final publication of the SOTEP trial (Marques 1994) (which showed no effect), and arguably misinterpreted the data in the Romero 1983 trial, viz., the conclusion that "Re-arrest is not statistically significantly increased in the therapy group (14%) compared with the no group therapy control (7%), but if we speculate and add only a few more arrests to the intervention group there would be a suggestion that the therapy was less effective than doing nothing for prevention of re-arrest. This is a study that should be replicated and further follow up should be eagerly awaited." First, we believe, in contrast to those authors, that the intervention was intensive (meaning some effect might be expected) and the comparison group (probation) was far from "doing nothing" (see Characteristics of included studies). Analysts of the primary data did not see the experiment as a success for the treatment (Romero 1983. p.37) and do not share the optimistic view of Kenworthy et al that this study should be replicated, neither in their published work ("In fact, a smaller percentage of individuals in the probation only group were arrested for a subsequent sex offence" (p.41)) nor by subsequent personal communication (Williams 2011 [pers comm]).

Furthermore, an outcome of interest to the investigators and to Kenworthy 2003 (but not to authors of the present review) actually reported the effects of the intervention on the outcome of 'general offending, not of a sexual nature' as still more strongly in favour of the probation-only control. Nevertheless, investigators do not argue that such intervention as examined in this study should be terminated without "consideration of the following factors:...group psychotherapy delays the reoccurrence of or affects the rate of subsequent sex offences...data indicate that this is true, although the sample size is not large enough..." Secondly, it is argued that recidivism as a sole binary outcome is problematic; thirdly that arrest data are problematic. Finally and most importantly, "undetected crime is quite extensive among sex offenders and...official data may only reveal a small percentage of total criminal activity" (Romero 1983 p. 41).

Similarly, while the well-publicised reviews of Losel and Schmucker (Lösel 2005) and Hanson et al (Hanson 2009) appear to show positive effects, these were based on evidence that many regard as weak (Rice 2012). Faced with the absence of good-quality evidence, there is an understandable tendency to lower the bar and accept evidence from less rigorous trials. Unfortunately, this weaker evidence (and the conclusions drawn from it) often leads to a more optimistic conclusion about efficacy than is warranted, and unfortunately becomes embedded in clinicians' consciousness. This may result in a belief that current approaches are more effective than the evidence suggests. This is typified by the conclusion from another recent review by Corabian 2011 that states, "While further research is warranted, the available evidence suggests that CBT delivered within programmes adhering to the RNR model represents the most promising approach". Our concern is that the important qualifiers (that is, 'further research is warranted' to justify an approach that is 'promising') might well be lost by the decision makers to whom it is directed, so that they may conclude that this type of trial evaluation is a luxury, rather than a necessity.

While our findings do not show any benefit from the intervention, we note some recent evidence that gives some grounds for optimism. For instance, there was evidence in the Hanson 2009 meta-analysis (that included both RCT and non-RCT trials) that it was the more recently reported trials that produced the more positive effects, although the Losel and Schmucker review (Lösel 2005) found no evidence of a temporal effect. In addition, although not within the scope of this review as it deals with juvenile sex offenders, we note that Multi-systemic Therapy has been shown to be effective in a randomised controlled trial (Borduin 2009). A Cochrane review on CBT-based interventions for adolescent sex offenders is currently in process (Sneddon 2012).

AUTHORS' CONCLUSIONS

Implications for practice

While this review adopts the Cochrane principles of examining only evidence from RCTs, it does so without any apology, in the belief that other types of trial evidence are likely to inflate the positive findings for the intervention. Sackett et al (1996) express this well when they write on treatment studies as follows: "…we should avoid non-experimental approaches…since these routinely lead to false positive conclusions about efficacy…(so that)…the systematic review of several randomised trials…has become the 'gold standard' for judging whether a treatment does more good than harm" (Sackett 1996). If this is the standard against which current treatments for sex offenders are to be judged, then we fear that they come up short of what is required. Not only do we not have 'several randomised trials' to consider, those that we could include were so heterogeneous that comparative analysis was very limited. We are not alone in this conclusion, as the Hanson 2009 review of 129 sex offender treatment studies could rate none as 'strong' according to Collaborative Outcome Data Committee guidelines. While this absence of evidence is uncomfortable for any practitioner, it is especially so for those who manage sex offenders who are incarcerated. We have already pointed out that many countries have enacted legislation whereby incarcerated sex offenders have to participate in a sex offender programme to reduce the likelihood of further re-offending, if they are to be considered for release. If, however, the programme itself is of unknown efficacy, is it legitimate to detain such individuals? The results of this and similar reviews ought, at the very least, cause practitioners to pause and consider whether such practice is ethical. Practitioners should keep abreast of research and also look out for the results of reviews like Khan 2009. Current evidence from randomised controlled studies concerning the effects of pharmacological treatments also remains weak. In practice, it is likely that both pharmacological and psychological therapies will need to be used in unison in order to obtain the greatest benefit. ?

Implications for research

Having conducted this review and reviewed the literature, our inescapable conclusion is the need for further RCTs. While we recognise that randomisation is considered by some to be unethical, politically unacceptable or both (both of which are based on the faulty premise that the experimental treatment is superior to the control - this being the point of the trial to begin with); without such evidence, the area will fail to progress. Not only could this result in the continued use of ineffective (and potentially harmful) interventions for the recipients, but it also means that society is lured into a false sense of security in the mistaken belief that once the individual has been treated, then their risk of recidivism is reduced. Future trials should concentrate on minimising risk of bias, maximising quality of reporting and including follow-up for a minimum of five years 'at risk' in the community. Data on non-sexual offenses should also be included, given that it has been argued that some ostensibly nonsexually violent offences can in fact be sexually motivated (for example, an attempted rape results in a plea bargain or conviction only for assault) (Rice 2006).

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* *Indicates the major publication for the study*

CHARACTERISTICS OF STUDIES

Characteristics of included studies *[ordered by study ID]*

Anderson-Varney 1991

Methods	Design: parallel randomised controlled trial
Participants	Participants: male prisoners convicted of a sexual offence against an individual aged 13 years or younger Sex: all male Age: mean 38.7 (SD 9.90) years Unit of allocation: individual participant Number randomised: 60 (n = 30 intervention; n = 30 control) Number completing: 60 (n = 30 intervention; n = 30 control) Setting: medium security prison; single-site; Ionia, Michigan, USA Inclusion criteria: currently in prison; convicted of a sexual offence against an individual aged 13 years or younger; IQ 95 or above and with 'valid protocols' on the MMPI (Nichols 1984) Exclusion criteria: any previous sex offender treatment; IQ < 95 Ethnicity: 49/60 (82%) white Baseline characteristics: 45/60 (75%) married; 95% reported a childhood sexual experience with an adult. Investigator also assessed participants with a number of instruments designed to capture information to test her hypotheses regarding child molesters for example, arrested development and attitudes toward women
Interventions	Two conditions: • Behavioural sex offender treatment program (n = 30 randomised) • No treatment control (n = 30 randomised) Behavioural sex offender treatment program: utilises 3 of the 6 treatment components developed by Abel 1984a; comprises 15 sessions, 2 sessions per week, each 1.5 hours with 5 sessions on social skills training, 5 on sex education and 5 on cognitive restructuring (see note 1) Duration of intervention: 8 weeks Duration of trial: 8 weeks Length of follow up: participants were not followed up beyond end of trial
Outcomes	**Primary outcomes** None reported **Secondary outcomes** Cognitive distortions: mean scores on Abel & Becker Cognition Scale (ABCS) (Abel 1984b); mean scores on Justifications scale of the Multiphasic Sex Inventory (MSI) (Nichols 1984) Social skills/social anxiety: mean scores on Social Avoidance and Distress Scale (SADS) (Watson 1969) Sexual anxiety: mean scores on sexual inadequacies scale of the Multiphasic Sex Inventory (MSI) (Nichols 1984) **Other outcomes** Self-esteem: mean scores on perspective taking scale of the Interpersonal Reactivity Index (IRI) (Davis 1980)

Anderson-Varney 1991 (*Continued*)

	Sexual knowledge: mean scores on sexual knowledge scale on the Multiphasic Sex Inventory (MSI) (Nichols 1984)
Notes	1.Those assigned to the experimental condition were randomly divided into two groups to experience the treatment program. The numbers given in the paper (16 and 16) appear to be typographical errors: investigator has been contacted for clarification and confirmed numbers were 15 and 15

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	No information was reported in the dissertation on method of sequence generation. The investigator, when contacted by email, clarified that coin flipping had been used (Anderson-Varney 2011 [pers comm]).
Allocation concealment (selection bias)	Unclear risk	No information was reported in the dissertation on methods for this criterion. The investigator, when contacted by email, could not recall measures taken to conceal allocation of individuals to treatment group (Anderson-Varney 2011 [pers comm]). Insufficient information remains to allow a judgement to be made
Incomplete outcome data (attrition bias) All outcomes	Low risk	There were no missing outcome data.
Selective reporting (reporting bias)	Low risk	Study protocol was never archived (investigator has been approached and confirmed this (Anderson-Varney 2011 [pers comm]) but it seems clear that the published report included all expected outcomes, including those that were pre-specified
Other bias	Unclear risk	The investigator acknowledges that the Abel & Becker Cognition Scale items are "transparently obvious" and may be affected significantly by social desirability and demand characteristics" (p. 81)
Blinding of participants and personnel (performance bias) All outcomes	High risk	Participants were not blinded. Personnel were not blinded. All interviews and the treatment programme itself were conducted by the investigator

Anderson-Varney 1991 (*Continued*)

Blinding of outcome assessment (detection bias) All outcomes	High risk	Outcome assessment was not blinded. All interviews and the treatment programme itself were conducted by the investigator

Brown 1996

Methods	Design: cross-over RCT
Participants	Participants: outpatients (paedophiles) convicted of a sexual offence against a child Sex: all male Age: mean 38.5 (SD 8.5) years Unit of allocation: individual participant Number randomised: 17 Number completing: 17 (n = 14 used in the analysis) Setting: outpatient; single site; Toronto, Canada Inclusion criteria: male; convicted of a sexual offence against a child; > 18 years old Exclusion criteria: believing that not masturbating would increase risk of relapse Ethnicity: not reported Baseline characteristics: 8 single, 4 married, 5 separated/divorced, 10 reported experience of substance abuse problems previously
Interventions	Two conditions: • Abstinence from masturbation • No prohibition from masturbation Duration of intervention: 4 weeks Duration of trial: 8 weeks Length of follow-up: none
Outcomes	**Primary outcomes** None **Secondary outcomes** None **Other outcomes** Sexual urges: self-report; in this cross-over study, no data were reported before the first cross-over Sexual behaviour: self-report; in this cross-over study, no data were reported before the first cross-over
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	"Participants were randomly assigned to one of two groups" (no method reported) (pp. 399-400). Communication with one investigator resulted in information that coin-flipping was used (Federoff 2012 [pers comm]).

Brown 1996 (*Continued*)

Allocation concealment (selection bias)	Low risk	Not reported, but personal contact with an investigator resulted in information that neither staff nor participants could have been aware of the order before the allocation was made Federoff 2012 [pers comm].
Incomplete outcome data (attrition bias) All outcomes	Low risk	In the overall cross-over trial, 3/17 results were excluded from the analysis (two participants failed to complete their Sexual Behaviour Checklist forms properly; one reported a masturbation frequency during the MBNA (masturbation not allowed) condition that was more than 2 SDs above the mean of the MA (masturbation allowed) condition
Selective reporting (reporting bias)	Low risk	Study protocol does not appear available. Despite this it seems clear that the published report included all expected outcomes relevant to this review, including those that were pre-specified
Other bias	Low risk	Although four participants took antidepressant medication (fluoxetine 20 mg/day) during the course of the study, the investigators report that when these participants were removed from the analysis "results were unchanged" (p. 401)
Blinding of participants and personnel (performance bias) All outcomes	High risk	Neither participants nor personnel could be blinded.
Blinding of outcome assessment (detection bias) All outcomes	High risk	The only outcome measure was the Sexual Behaviour Checklist, which was filled out by (unblinded) participants

Hopkins 1991

Methods	Design: parallel randomised controlled trial (using matched pairs)
Participants	Participants: male prisoners convicted of a sexual offence and incarcerated at HMP Frankland, Brasside, UK Sex: all male Age: no information reported, investigator could not confirm Unit of allocation: individual participant Number randomised: 15 (n = 8 intervention; n = 7 control) Number completing: 15 (n = 8 intervention; n = 7 control) Setting: Category A (high security) prison; single-site; Brasside, Durham, UK Inclusion criteria: currently in prison; convicted of a sexual offence Exclusion criteria: none were applied (confirmed by investigator, Marshall 2011a [pers comm]) Ethnicity: no information reported, investigator could not confirm Baseline characteristics: Publications regarding this study report 4 paedophiles and four rapists in the intervention group but offending characteristics for controls are not reported. Investigator subsequently confirmed she had attempted to randomise matched

	pairs but could not recall if this resulted in the control group (n = 7) having 4 paedophiles and 3 rapists, or 3 paedophiles and 4 rapists
Interventions	Two conditions: • Communication and social skills groups for sex offenders ('4 rapists and 4 offenders against children' (n = 8 randomised) • Wait list control (n = 7 randomised) Communication and social skills groups: Following a videotaped interview, participants received sessions as follows (1) Introduction [Goals, Checklists]: rules, group and individual aims; (2) Communication skills: Listening, verbal and non-verbal communication; (3) Perspective taking/Relationships: including attitude to women; (4) Relationships (continued): including victim awareness; (5) Emotional control: anxiety and aggression management; (6) Decision making/review: group communications and teamwork After each session: self-report ratings - group leader ratings Over 8 weeks: weekly landing staff ratings of peer and staff relationships Duration of intervention: 6 weeks Duration of trial: 8 weeks Length of follow up: 6 months
Outcomes	**Primary outcomes** None reported **Secondary outcomes** Social skills/social anxiety: mean scores on Social Anxiety and Distress (SAD) scale (Watson 1969) **Other outcomes** *Self-rated scales:* Fear of disapproval by others: mean scores on "Fear of negative evaluation" (FNE) scale (Watson 1969) Self-esteem: mean scores on "Rosenburg's [sic] "Self-Esteem Scale" (Rosenberg 1965) *Measures by independent raters:* Nonverbal and verbal social skills Group leaders' ratings of verbal and nonverbal skills were rated following each session on a checklist (max value 150 points) Social behaviour/skills with fellow inmates and staff. Landing staff's ratings of social behaviour/skills were made throughout the study. Means provided for these, but no standard deviations Social skills with 'unknown female' Videotaped ratings of an unknown female interviewing each participant, rated by six independent raters (psychologists and psychological assistants). Ratings ran from 1 to 5 on three metrics for (a) non-verbal skills; (b) conversational ability and (c) speech Operational effectiveness of groups' was also measured by means of self-report questionnaires (ratings 1 to 5) evaluating participants' enjoyment of sessions, how much each felt he had learned, and how confident he felt about putting what he had learned into practice. Author made clear the caveat that these measures might well be independent of treatment effect
Notes	The investigator communicated that a typographical error appears on p. 57 of the relevant book chapter and that text should read "Given their low self-esteem prior to treatment, however, the paedophiles and incest offenders show a healthy *decrease* in FME"

Hopkins 1991 (*Continued*)

Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Unclear risk	Published data for the study reported " ... random sampling was employed in the selection of those inmates who entered treatment or control groups ... " (p. 80 of original paper, Hopkins 1991). The investigator has clarified that numbers for paired participants were put into a hat and drawn randomly (Marshall 2011a [pers comm].
Allocation concealment (selection bias)	Unclear risk	Insufficient information to allow a judgment to be made: ...random sampling was employed in the selection of those inmates who entered treatment or control groups ... ' (p. 80 of original paper, Hopkins 1991) . Clarification about method of allocation concealment remains unclear despite communication with the author
Incomplete outcome data (attrition bias) All outcomes	Low risk	There were no missing outcome data.
Selective reporting (reporting bias)	Low risk	Study protocol is not available but it seems clear that the published report included all expected outcomes, including those that were pre-specified
Other bias	Low risk	The study appeared free of other sources of bias.
Blinding of participants and personnel (performance bias) All outcomes	High risk	Participants were not blinded. Personnel (trial investigator and assisting psychiatrist, Dr Moira Livingston) could not be blinded
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Efforts were made where possible for example: "Landing staff rated the social behaviour of both treatment and control groups on the wing over eight weeks, with the assumption that they did not know who was and who was not receiving treatment" (p. 84); we feel (and the investigator confirmed) that these staff were in fact likely to discern group membership, on a relatively small wing However, video ratings of the interview of

Hopkins 1991 (*Continued*)

		prisoners by an 'unknown female' before and after group treatment were conducted and edited to minimise risk of independent raters of the videos discerning whether the interview was conducted before or after treatment (p. 85) and such efforts were likely to have been successful

Marques 1994

Methods	Design: parallel randomised controlled trial
Participants	Participants: male prisoners serving a sentence for child molestation or rape Sex: all male Age: range 18 to 60 years (no further details) Unit of allocation: individual participant Number randomised: 484 (n = 259 intervention; n = 225 control) Number completing: 392 (n = 167 intervention; n = 225 control) Setting: hospital, then community (control condition, prison, then community); single site; California, USA Inclusion criteria: serving a sentence for child molestation or rape; within 18 to 30 months of release; aged 18 to 60 yrs; admitting his sexual offence; estimated IQ > 80; English speaking Exclusion criteria: a current psychotic or organic mental condition; convicted of gang rape or incest; more than two previous felony convictions; a pending immigration hold or felony warrant; presenting severe management problems in prison; sufficiently medically debilitated as to require skilled nursing care Ethnicity: 70% white, 15% African American, 13% Hispanic/Latino Baseline characteristics: 50% molesters with female victims; 20% molesters with male victims; 8% molesters with male and female victims; 22% rapists with adult victims; 39.9% with previous convictions; 22.4% with prior arrests for sexual crimes; 18.4% prior convictions for sexual crimes (data for the whole sample, including those in the secondary control group, see note 3)
Interventions	Two conditions (see notes 1 & 2): • CBT relapse prevention (SOTEP) program (n = 259 randomised) • No treatment control (n = 225 randomised) CBT relapse prevention (SOTEP) program: Intensive CBT program; 'Relapse Prevention' (RP) approach. RP designed to prevent relapse among rapists and child molesters. Participants work on accepting responsibility for their offences, modifying their cognitive distortions, examining how they set up their past crimes, and learning what they had to do differently to avoid reoffending; treatment comprises two years of 3 x 90-minute sessions per week, plus all attend groups on sex education, human sexuality, relaxation training, stress and anger management and social skills; additional speciality groups (for example, substance abuse) as required; pre-release class; additional individual therapy (1 hour per week) by primary therapist, plus 2 hours per week with nursing staff. Includes 1-year aftercare program of 2 sessions per week in first year after release in the Sex Offender Aftercare Program (SOAP) provided by community clinicians trained by

	SOTEP clinical staff; failure to attend SOAP could result in parole violation and return to prison. SOAP is an extended version of RP program tailored to suit individual needs of the paroled offender Controls: Those allocated to control condition remained in prison until release; supervised by parole agents following release; received no treatment, but *'some may receive counselling while in prison or on parole'* (Marques 1994a, p.36) for example, assistance with anger management or substance abuse (Marques 2005, p. 82) Duration of intervention: 2 years Duration of trial: 16 years (1985-2001) Length of follow up: between 5 and 14 years; investigators report *'an 8-year follow-up period'* (Abstract) in which *'all but a few study participants had been at risk for at least 5* years' (p.83)
Outcomes	**Primary outcomes** Recidivism as measured by reconviction or caution: re-arrest following release **Secondary outcomes** Leaving treatment early
Notes	1. SOTEP = Sex Offender Treatment & Evaluation Project; includes 1-year aftercare in the community after release 2. Investigators also report results for a secondary control group (NVC) comprising inmates who qualified for the project but chose not to participate (that is, not seeking treatment). *'Matched offenders of the NVC group were selected later, also at random, from the pool of inmates who did not volunteer for the study'* (p.82) (Marques 2005) 3. Investigators report that treatment and control groups not significantly different except that 12.8% of the treatment group had previously been committed for treatment as mentally disordered sex offenders cf. 6.4% of control group, and the mean risk score was significantly higher for treatment group compared to control group

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Method of randomisation was not reported in any of the published papers. It was clear a change to procedure occurred midway through the trial. Initially, coin toss was used; the investigators reported however that this process changed over time because "a substantial number (over 20%) of volunteers assigned to treatment changed their minds and declined to participate before their transfer to the treatment facility. " The authors were unable to provide precise information on the nature of this procedural change but it is possible that specific pairs were created before assignment in every case. Nonetheless, they report they

		"continued to assess volunteers on the three matching criteria, and flipped coins to determine their group assignment" and that "it is my understanding that the probability of a participant being assigned to treatment or control remained at 50% throughout the study" (Marques 2011 [pers comm]) . Multiple personal communications resulted in detailed information as follows: "Our procedure of not assigning participants to groups until we had matched pairs was causing long delays; in some cases it took months for us to find a matching volunteer and then inform both individuals of their assignments. Those assigned to treatment after such delays were often the men who changed their minds and refused transfer to the forensic hospital. In order to improve this situation, we did the assignments more quickly, which meant that not everyone had a matched control at the time they entered treatment. If we had matching pairs, assignment continued as described above. If a volunteer did not have a match, the fair coin toss was used to assign the subject to an experimental group, and a "hole" was created in the list. This "hole" was later filled as new subjects were added to the list after another recruitment effort. Group assignment continued to be random, and all of the early dropouts were still retained for analysis" (Marques 2012 [pers comm]). Judgment: we are satisfied that risk of bias is low: the NIMH-sponsored advisory group attached to the trial was kept informed and assignment continued random
Allocation concealment (selection bias)	Unclear risk	Insufficient information to allow a judgement to be made. Clarification about method of allocation concealment has been requested from the trial investigators, but no further information was available at the time this review was prepared
Incomplete outcome data (attrition bias) All outcomes	Low risk	No missing outcome data reported. ITT analysis, which the investigators' term 'treatment as assigned', was conducted using re-arrest data from all randomised participants

Marques 1994 (*Continued*)

Selective reporting (reporting bias)	Low risk	Study protocol is not available but it seems clear that the published report included all expected outcomes, including those that were pre-specified
Other bias	Unclear risk	The study appeared free of other sources of bias.
Blinding of participants and personnel (performance bias) All outcomes	High risk	Neither participants nor personnel could be blinded.
Blinding of outcome assessment (detection bias) All outcomes	Low risk	No information on blinding of outcome assessors/those analysing outcome data was reported. A personal communication from the primary investigator revealed the following information: "Participants were given numbers at assignment that included their group membership; these were used by project administrators for general tracking and communication purposes. The project's evaluation director then assigned a different research number to each participant, which had no indication of his group membership. These are the numbers that were used by the research staff who analyzed the outcome data" (Marques 2011 [pers comm]).

McAnaney 1981

Methods	Design: parallel randomised controlled trial
Participants	Participants: male prisoners with a psychosexual disorder convicted of a sex offence Sex: all male Age: mean 25.5 (range 16 to 60) years Unit of allocation: individual participant Number randomised: 40 (n = 20 intervention; n = 20 control) Number completing: 37 (n = 18 intervention; n = 19 control) Setting: forensic evaluation hospital; single-site; Gainesville, Florida, USA Inclusion criteria: convicted of a sex offence; diagnosis of a psychosexual disorder; unmarried; heterosexual or bisexual orientation Exclusion criteria: Being married or being exclusively homosexual in orientation Ethnicity: 33/37 (89%) white; 4/37 (11%) black Baseline characteristics: 23/37 (62%) rapists; 14/37 (38%) paedophiles; mean 13.9 months in treatment

McAnaney 1981 (*Continued*)

Interventions	Two conditions: • Heterosocial skills training (n = 20 randomised) • No treatment control (n = 20 randomised) Heterosocial skills training: based on a social skills module derived from a number of sources (Zunin 1973; Twentyman 1975; Arkowitz 1977, Meichenbaum 1977; Zimbardo 1977). 8 sessions took place over 6 weeks, addressing conversational skills, self-esteem, body image, negative self-talk and anxiety. Treatment involved role play and video feedback activities; female co-trainers and female volunteers simulated social conversations with the participants; the investigator and author of the treatment module trained the staff delivering program Trainers were selected to be equivalent in age, training and experience and the importance of standardisation of procedures and time lines in the lessons of the training module was 'stressed' (p. 80). The two sets of male/female trainers, 'all paraprofessional staff in the sex offender unit, participated in four hours of in-service training to learn the module. These trainers had already worked with sex offenders, had B.A. college degrees and had at least some experience in conducting small training groups' (p. 80). The investigator met with the trainers between each lesson Duration of intervention: 6 weeks Duration of trial: 6 weeks Length of follow up: participants were not followed up beyond end of trial
Outcomes	**Primary outcomes** None reported **Secondary outcomes** Anxiety: presented as mean scores on S-R Inventory of Anxiousness (SRIA) (Endler 1962) Leaving treatment early Lost to follow-up **Other outcomes** Heterosocial skills: mean scores on Heterosocial Skills Behavior Checklist (HSB) (Barlow 1977) Self-esteem: mean scores on Social Self-Esteem Inventory (SSI) (modified to measure heterosocial self-esteem) (Lawson 1979)
Notes	Brief (and sole?) reference to Latino participants is not followed up elsewhere (p. 86). The subject of race and 'subculture' is dealt with in a cursory fashion (p. 86) but author shows concern that these issues might be relevant to generalising training further

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Sequence generation was by random number table (p. 58).
Allocation concealment (selection bias)	High risk	Method of allocation concealment was not reported in the dissertation and clarification was requested from the trial inves-

		tigator, who reported that no particular measures were taken to conceal allocation (McAnaney 2011 [pers comm]).
Incomplete outcome data (attrition bias) All outcomes	Low risk	At post-treatment, 2/20 (10%) missing from treatment condition (both cases missed several treatment sessions and so were excluded); 1/20 (5%) missing from control condition (exercised his right not to participate in the post-testing). Reasons for missing data differ across the groups, but the numbers are small and of similar size
Selective reporting (reporting bias)	Low risk	Study protocol was requested from author but none is available. Despite this it seems clear that the published report included all expected outcomes relevant to this review, including those that were pre-specified
Other bias	Low risk	There was evidence of significant baseline imbalance between conditions in terms of type of sex offence committed with rapists forming 44.4% of treatment group and 84.2% of control group, and paedophiles forming 55.6% of treatment group and 15.8% of control group This imbalance would be a potential source of bias if the offender's response to treatment differed significantly by type of offence. However, the trial investigator notes that previous work has not found significant differences on the HSB and SRIA measures by offence, and also that no significant treatment/offence interactions in the present study. Risk of bias was thus judged to be low
Blinding of participants and personnel (performance bias) All outcomes	High risk	Neither participants nor personnel could be blinded.
Blinding of outcome assessment (detection bias) All outcomes	Low risk	"The raters of the video segments had [no] knowledge of whether or not they were viewing a 'trained' participant in the rating of the second interaction and they had none. The raters were selected from outside the treatment program and therefore had no knowledge of which participants were experimental or control. The other mea-

McAnaney 1981 (*Continued*)

		sures were scores on written assessments and were also scored and tabulated by assistants with no knowledge of which group the participants belonged to" (McAnaney 2011 [pers comm]).

McConaghy 1985

Methods	Design: parallel randomised controlled trial
Participants	Participants: men seeking treatment for anomalous urges and behaviours they felt unable to control Sex: all male Age: mean 36.0 (range 15 to 72) years Unit of allocation: individual participant Number randomised: 20 (n = 10 imaginal desensitisation; n = 10 covert sensitisation) Number completing: 16 (n = 9 imaginal desensitisation; n = 7 covert sensitisation) Setting: inpatient (for duration of the treatment only); single site; Sydney, Australia Inclusion criteria: male; seeking treatment for anomalous urges and behaviours they felt unable to control Exclusion criteria: psychosis Ethnicity: not reported Baseline characteristics: 11 married, 8 exhibitionists, 5 compulsive homosexuality, 4 homosexual paedophiles, 1 heterosexual paedophile, 1 exhibitionist & voyeur, 1 clothes fetishist, 13 with previous conviction(s) for sexual behaviour, 2 with previous jail sentence (s)
Interventions	Two conditions: • Imaginal desensitisation (n = 10 randomised) • Covert sensitisation (n = 10 randomised) Imaginal desensitisation: each participant initially trained in relaxation; participant provided descriptions of 4 scenes in which he was stimulated to carry out the compulsive sexual activity for which he sought treatment; asked to visualise not completing the compulsive act but remaining relaxed; 14 sessions (2 on day one, 3 on the subsequent four days); sessions lasted 15-20 min and included visualisation of 4 scenes followed by aversion; treatment administered during 1 week's admission to a psychiatric unit; 14 sessions (2 on day one, 3 on the subsequent four days); treatment administered during 1 week's admission to a psychiatric unit Covert sensitisation: a form of aversive therapy; administered following the procedure described by Cautela (Cautela 1966); each participant initially trained in relaxation; participant provided descriptions of 4 scenes in which he was stimulated to carry out the compulsive sexual activity for which he sought treatment; asked to visualise performing activity in one of the scenes whilst relaxed; before completion of the sexual activity asked to visualise an aversive situation described by therapist; next scene given after 1 minute; 14 sessions (2 on day one, 3 on the subsequent four days); sessions lasted 15-20 min and included visualisation of 4 scenes followed by aversion; treatment administered during 1 week's admission to a psychiatric unit; only difference from imaginal desensitisation was the addition of aversion situation prior to completion of the sexual activity

	Duration of intervention: one week Duration of trial: 12 months Length of follow up: one month and one year following treatment
Outcomes	**Primary outcomes** Recidivism as measured by reconviction or caution: reports of being charged for sexually anomalous behaviour **Secondary outcomes** Lost to follow up Adverse events: increased anomalous behaviour following treatment Anomalous urge: participants' report of % reduction in their anomalous urge in response to treatment Anxiety State: mean scores on State-Trait Anxiety Inventory (STAXI) (Spielberger 1983) Anomalous desire: independent assessment of change in anomalous desire following treatment Anomalous behaviour: Independent assessment of change in anomalous behaviour following treatment **Other outcomes** General tension: participants' report of % reduction in their general tension in response to treatment
Notes	No standard deviations were reported for any continuous data in this paper. Standard deviations have been requested from one trial investigator, who reported that to his knowledge, no data from the study now survived (the first author, who held all data, is deceased [Blaszczynski 2011 [pers comm]])

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Insufficient information appeared in the published paper to allow a judgement to be made. Clarification about method of sequence generation was requested from one trial investigator, who reported that a random numbers table was used (Blaszczynski 2011 [pers comm]).
Allocation concealment (selection bias)	High risk	Insufficient information appeared in the published papers to allow a judgement to be made. Clarification about method of allocation concealment was requested from one trial investigator, who reported that the first author made no attempts to conceal allocation (Blaszczynski 2011 [pers comm]).
Incomplete outcome data (attrition bias) All outcomes	Low risk	At one month, 0/10 missing from ID condition; 0/10 missing from CS condition At 12 months, 1/10 (10%) missing from

McConaghy 1985 (*Continued*)

		ID condition (lost to follow up; see below) ; 3/10 (30%) missing from CS condition (lost to follow up; see below) Reasons not given except that participants were lost to follow up due to their failure to respond to repeated communications Investigators emphasise that any offending data would be unlikely to be lost as "public or private solicitors of patients who have been charged following treatment … have, in our experience, invariably sought reports from the treating doctors, as evidence the patients had sought treatment reduced the severity of convictions" (p 182)
Selective reporting (reporting bias)	Low risk	Study protocol is not available but it seems clear that the published report included all expected outcomes, including those that were pre-specified
Other bias	Low risk	There was no evidence of any significant imbalance in demographic or other measured baseline characteristics. The study appeared to be free of other sources of bias
Blinding of participants and personnel (performance bias) All outcomes	High risk	Neither participants nor personnel could be blinded.
Blinding of outcome assessment (detection bias) All outcomes	Low risk	The outcome assessor was not informed of allocation status of participants and took no other part in the study; he reported that subjects obeyed an instruction not to reveal information to him concerning the nature of the treatment they received and that he remained unaware of this. Authors consider it unlikely that this blinding was broken

McConaghy 1988

Methods	Design: parallel randomised controlled trial
Participants	Participants: men seeking treatment for anomalous urges and behaviours they felt unable to control Sex: all male Age: mean 30 (range 16 to 50) years Unit of allocation: individual participant Number randomised: 31 (n = 10 M; n = 10 ID; n = 11 M+ID)

	Number completing: 30 (n = 10 M; n = 10 ID; n = 10 M+ID) Setting: inpatient for duration of ID treatment only; single site; Sydney, Australia Inclusion criteria: male; seeking treatment for anomalous urges and behaviours they felt unable to control Exclusion criteria: 'active' psychosis Ethnicity: not reported Baseline characteristics: At recruitment, 17 single; 9 married; 4 separated/divorced; 22 with one DSM-II paraphilia; 8 two or more DSM-II paraphilias; 12 exhibitionism; 7 heterosexual paraphilia; 6 homosexual paraphilia; 2 bisexual paraphilia; 5 fetishism; 4 transvestism; 3 voyeurism; 19 convicted for a sexual offence; 2 subnormal intelligence and illiterate
Interventions	Three conditions: • Medroxyprogesterone (M) (n = 10 randomised) • Imaginal desensitisation (ID) (n = 10 randomised) • Medroxyprogesterone-plus-desensitisation (M+ID) (n = 11 randomised, see note 1) Medroxy-progesterone (M): 150 mg by IM injection; total of 8 injections; 4 injections at 2-week intervals followed by 4 injections at monthly intervals Imaginal desensitisation (ID): based on hypothesis that anomalous behaviours are driven by a sense of tension and excitement that becomes aversive if behaviour not completed; each participant initially trained to relax; then to visualise situation where they have carried out anomalous behaviour in the past but visualise not completing the behaviour while remaining relaxed; 14 sessions: 2 sessions on day 1, 3 sessions on subsequent 4 days; delivered during a 5-day admission to a psychiatric ward Duration of intervention: ID=5 days, M=6 months Duration of trial: 2 years Length of follow up: one month and one year following treatment
Outcomes	**Primary outcomes** Recidivism as measured by reconviction or caution: reports of being charged for sexually anomalous behaviour **Secondary outcomes** Leaving treatment early Lost to follow up Anomalous urge: participants' report of % reduction in their anomalous urge in response to treatment Anxiety State: mean scores on State-Trait Anxiety Inventory (STAXI) (Spielberger 1983) General tension: participants' report of % reduction in their general tension in response to treatment Anomalous desire: independent assessment of change in anomalous desire following treatment Anomalous behaviour: Independent assessment of change in anomalous behaviour following treatment NB: Adverse events: side effects of medroxyprogesterone **Other outcomes** General tension: participants' report of % reduction in their general tension in response to treatment

Notes	1. initially, eleven participants were randomised to combined treatment. However, 'one patient allocated to the combined treatment who was concerned about his low level of normal sexual interest decided not to have M treatment when informed his plasma testosterone was low. He was treated with ID alone and excluded from the study' (p. 200, col 1) 2. No standard deviations were reported for any continuous data in this paper. Standard deviations have been requested from one trial investigator, who reported that to his knowledge, no data from the study now survived (the first author, who held all data, is deceased [Blaszczynski 2011 [pers comm]])

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	Insufficient information appeared in the published paper to allow a judgement to be made. Clarification about method of sequence generation was requested from one trial investigator, who reported that a random numbers table was used (Blaszczynski 2011 [pers comm]).
Allocation concealment (selection bias)	Unclear risk	Insufficient information appeared in the published papers to allow a judgement to be made. Clarification about method of allocation concealment was requested from one trial investigator, who reported that the first author made no attempts to conceal allocation (Blaszczynski 2011 [pers comm]).
Incomplete outcome data (attrition bias) All outcomes	Low risk	At one month, 0/10 missing from ID condition; 0/10 missing from M condition; 1/10 (10%) missing* from ID+M condition At 12 months, 0/10 missing from ID condition; 0/10 missing from M condition; 1/10 (10%) missing* from ID+M condition (*one participant changed his mind after random allocation and withdrew from M treatment) Numbers of missing data are small, and reasons for missing outcome data are likely to be related to true outcome. Investigators emphasise, as in their previous (1985) study, their certainty that solicitors acting for any of their clients would have informed them of any post-treatment arrests or convictions (p. 200)

McConaghy 1988 (*Continued*)

Selective reporting (reporting bias)	Unclear risk	Study protocol is not available but it seems clear that the published report included all expected outcomes, including those that were pre-specified. However, neither means nor standard deviations were reported for anxiety (only correlations between participants' and assessors' views). These data have been requested from one trial investigator but as explained above, do not appear to have survived (Blaszczynski 2011 [pers comm]).
Other bias	Low risk	There was no evidence of any significant imbalance in demographic or other measured baseline characteristics. The study appeared to be free of other sources of bias
Blinding of participants and personnel (performance bias) All outcomes	High risk	Neither participants nor personnel could be blinded.
Blinding of outcome assessment (detection bias) All outcomes	High risk	Outcome assessor was not blinded and a there is thus a risk of detection bias. This is despite the investigator's observation: 'The significant correlations in this study between the assessor's and patients' measures of response and those between both these measures and the patients' STAI and tension scores further support the validity of the measures of response... this suggests that the fact that the assessor in the present study was not blind to the different treatments did not significantly bias his assessments' (p. 205, col 1)

Romero 1983

Methods	Design: parallel randomised controlled trial
Participants	Participants: convicted sex offenders currently on probation Sex: all male Age: 51% under 25 years; 64% of assaulters were under 25 years Unit of allocation: individual participant Number randomised: Unclear in published papers; clearer after communication with secondary analyst (Professor Linda Williams) and after inspection of unpublished thesis by Hermann Roether. 293 were 'assigned to the project' (Roether 1972, p 65) (that is, randomised); data for 264 were assessed in analysis (Roether 1972, p 73), data for the self directed group) (n = 29) were excluded. Subsequently, analysts Romero and Williams

	analysed 231 (n = 148 intervention; n = 83 control) (see note 1) (Romero 1983). Number completing (see note 1): 231 (n = 148 intervention; n = 83 control) Setting: outpatient; multi-site (number of sites unspecified; one researcher clarified that "in regard to study sites it was ONLY Philadelphia and mention of multiple sites refers to the fact that some groups met at the hospital and some near either city hall or the probation office..."; Philadelphia, USA Inclusion criteria: convicted sex offender; currently on probation. Age criteria (not in published papers, see below) include age cutoffs (participants had to be between 18 and 50 years of age), participants had to live within the area so that probation was 'active' and not by correspondence; participants had to score an IQ of above 70 and be fluent in English Exclusion criteria: none reported in the published papers. Contact with an investigator (Williams 2011 [pers comm]) provided information that an unpublished dissertation (Roether 1972) contained relevant information. Here exclusion criteria appeared as follows: participants could not be under age 18 or over age 50, have an IQ < 70, be engaged in other treatment or not be fluent in English; could not be (re)incarcerated or absconded, could not be psychotic or alcoholic, subject to 'work interference' (that is, unable to attend therapy or probation regularly due to incompatible shifts); could not be a 'homosexual pedophile' (defined as offending against a boy aged < 11) or be a 'homosexual exhibitionist'; could not be 'non-ambulatory due to physical handicap'. Finally, participants whose offence was statutory rape were excluded if they were < 5 years younger than the victim of a statutory rape Ethnicity: 33% white, 67% non-white Baseline characteristics: 48/231 (21%) paedophiles; 39/231 (17%) exhibitionists; 144/231 (62%) assaulters 33.6% had no more than 9 years of education; 33% never married; 38% married; 29% separated; 69% had a diagnosis of personality disorder (66% passive-aggressive PD); 2% neurotic disorder. 27% had no psychiatric diagnosis
Interventions	Two conditions: • Group psychotherapy with probation (n = 148 randomised) • Probation-only control (n = 83 randomised) Group psychotherapy with probation: psychoanalytically orientated group therapy; therapy groups with 12-15 members weekly for 1 hour over 40 weeks; participants reported to their probation officers once a month, plus a monthly home visit by a probation officer Probation-only control: Intensive probation supervision only; reported to their probation officers once a month, plus a monthly home visit by probation officer Duration of intervention: 40 weeks Duration of trial: 10 years Length of follow up: 10 years
Outcomes	**Primary outcomes** Recidivism as measured by reconviction or caution: arrest for a sexual offence during the follow-up period (see note 2) **Secondary outcomes** Arrests for nonsexual offences (see note 3)
Notes	1. Data from a group of homosexuals with a high rate of offences were not analysed in papers published in 1983 and 1985. An investigator communicated that this was related

	to first to changes in the law and then to changes in DSM criteria (Williams 2011 [pers comm]) 2. Investigators comment they were unable to obtain national arrest data on the offenders in the sample - so arrests may be underreported. On the other hand, arrests in the Philadelphia area may have 'false positives' as (once a sex offender is known) he is more likely to be picked up if a sex offence is committed in his area 3. Nonsexual offence data appear to have been collected by the original primary investigators in this study (Hermann Roether and JJ Peters [Roether 1972]) who both believed that it was "well-established" that convicted sexual offenders rarely reoffended, and that pedophiles reoffended least of all (pp 69 - 70). Roether seemed to believe that the study program should impact as much on nonsexual data as on sexual offence data and "study failure" was therefore defined as reoffending in any way

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	High risk	Information on this aspect of trial design is very unclear. A 'third-hand' account of the study, written after the death of one of its designers and without apparent discussion with the other, claimed that sequence generation was by drawing of lots ("...assigned by lot either to a therapy group or to a probation group" (Brecher 1978, p. 63, col 1) . However, the difference between the numbers in the intervention group (148) and in the control (83) are certainly large, and the process by which they were arrived at is somewhat unclear in the PhD thesis that describes them in most detail (Roether 1972, pp 38 to 44). Here, 'lots' are not mentioned, rather, it would appear that a "ratio device" was used by the main investigator, perhaps an 'ancestor' of the later technique of minimisation. This 'device' was employed from October 1966 to try to see that the demands of a randomised research design did not conflict with the needs of therapists to maintain a flow of participants such that therapy groups did not fall beneath 10 to 12 members, nor with participants being grouped by four homogenous subpopulations(pedophiles, exhibitionists, etc.) followed by fifth 'mixed group'. In September 1967, a self directed treatment option was added to the latter group so par-

		ticipants from this point on were allocated (”serially“ as before) to one of six possible treatment groups or to one control (probation). The use of the word ”serially“ is of concern in the absence of a computer program as might have been used subsequently; investigators are however adamant that the trial was randomised and personal communication with a surviving researcher suggests the model was a ”flexible one ensuring randomisation but adequate distribution amongst groups“ sufficient to ensure that an overabundance of participants in the ”assaulters“ group did not overbalance some control groups (Williams 2011 [pers comm]). The age of the study makes a final judgment impossible.
Allocation concealment (selection bias)	Unclear risk	Insufficient information to allow a judgement to be made. Clarification about method of allocation concealment has been requested from the Professor Linda Williams, involved in the analysis of data at long-term follow-up, but she was unaware of methods taken. The original PhD thesis described no methods. The primary author (Hermann Roether) is unavailable and his supervisor, deceased
Incomplete outcome data (attrition bias) All outcomes	Low risk	The original investigators (Roether and Peters) excluded from their analysis participants who took part in a group introduced halfway through their study in which groups were 'self directed' and not led by a therapist. This led to the exclusion of data from 29 participants (see above). Researchers working years after the initial study claim 100% follow up but also describe the exclusion of data of a group of homosexuals with a high level of offences. Clarification has been received from a researcher involved in the trial analysis and it is now clear this was due to changes in DSM criteria and therefore, we do not believe these missing data are a source of bias to this review (Williams 2011 [pers comm]) .

Romero 1983 (*Continued*)

Selective reporting (reporting bias)	Low risk	Study protocol is not available but it seems clear that the published report included all expected outcomes, including those that were pre-specified
Other bias	Unclear risk	Authors comment they were unable to obtain national arrest data on the offenders in the sample - so arrests may be underreported. On the other hand, arrests in the Philadelphia area may have 'false positives' as (once a sex offender is known) he is more likely to be picked up if a sex offence is committed in his area Exclusion criteria excluded men who offended against young children (< age 11) if victims were boys. The reasons for this are unclear and affect generalisability of results
Blinding of participants and personnel (performance bias) All outcomes	High risk	Neither participants nor personnel could be blinded.
Blinding of outcome assessment (detection bias) All outcomes	High risk	No information on blinding status of outcome assessors appeared in published papers describing this study. The original PhD thesis reporting it described no attempts at blinding assessors to treatment status of participants, and data appear to have been collected only by therapists and/or by the primary investigator (Hermann Roether) or his supervisor, all of whom would have been aware of treatment status

Rooth 1974

Methods	Design: cross-over randomised controlled trial (Latin square design)
Participants	Participants: male outpatients with history of exhibitionism Sex: all male Age: mean 32.0 (range 18 to 53) years Unit of allocation: individual participant Number randomised: 12 (cross-over design; all get each intervention, in random order Number completing: 12 Setting: community & hospital (see note 1); single-site; Bristol, UK Inclusion criteria: exposure urges or exposure acts several times a week; actively exposing at least once a month; history of exposing for at least 2 years Exclusion criteria: another dominant psychiatric illness; mild presentations who were likely to do well with any treatment

	Ethnicity: not reported Baseline characteristics: 12/12 had heterosexual experience; 7/12 married or with current sexual partners; 4/12 referred from prison; 6/12 history of some paedophilic activity. Intelligence was 'normal' but socially the participants were assessed as 'underachievers with problems at work' and 'poor self control.'
Interventions	Three conditions (see note 1): • Aversion • Self-regulation • Control (muscle relaxation) Aversion: electrical shock administered to underside of forearm when participant reported obtaining an image of exposing or rehearsal (approximately 15 trials per session); for therapist sessions, patient/therapist separated in soundproof room, communicating via loudspeaker system; half-way through trial, patients asked to complete two (rather than one) daily treatment sessions on their own; one of these was in hospital, the other involving excursion into potentially tempting situations; eight sessions; each 30-45 minutes Self-regulation: approach defined by Bergin 1969; triggers of exposure-impulse-response chain identified; alternative interrupting behaviours identified; patient trained to increased awareness and to deploy alternative behaviour; for therapist sessions, patient/therapist separated in soundproof room, communicating via loudspeaker system; half-way through trial, patients asked to complete two (rather than one) daily treatment sessions on their own; one of these was in hospital, the other involving excursion into potentially tempting situations; eight sessions; each 30-45 minutes Control (muscle relaxation): standard Jacobson's progressive muscle relaxation with training in 'deeper relaxation' as well (Jacobson 1938; Schultz 1959); for therapist sessions, patient/therapist separated in soundproof room, communicating via loudspeaker system; half-way through trial, patients asked to complete two (rather than one) daily treatment sessions on their own; one of these was in hospital, the other involving excursion into potentially tempting situations; eight sessions; each 30 to 45 minutes Duration of intervention: one week (three weeks to receive all treatments) Duration of trial: 18 months Length of follow up: 14 months
Outcomes	**Primary outcomes** Arrest and conviction/reconviction (but data not separated by treatment condition/order) **Secondary outcomes** Anomalous urges as measured by the individual's own 'target problem'. Measured by daily records of a) the strongest impulse to expose, and b) the amount of time spent thinking about exposure. Anxiety Linear scales rated 'mood' including depression, irritability, tension, anxiety and persecutory feelings\ **Other outcomes** Feelings about exposure Modified version of Semantic Differential Scales (score 1 to 7) (Marks and Sartorius 1968). General evaluative (GE) and Sex evaluative (SE) factors measured (score 1-7) applied to the concept 'How I feel about exposing' Subjective excitement/risk of relapse Linear rating scales in which participants were asked to rate pleasure/excitement and his own assessment of 'risk of relapse' on two scales

	Likely temptations Situations Scores in which participants described ten exposure situations and rated by attractiveness (0-4) NB: Some adverse events (marked tension and anger) noted during aversion therapy
Notes	Study too old to obtain contact with investigators

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Unclear risk	Insufficient information to allow a judgement to be made. Due to age of study, unable to contact trial investigators for more information
Allocation concealment (selection bias)	Unclear risk	Insufficient information to allow a judgement to be made. Due to age of study, unable to contact trial investigators for more information
Incomplete outcome data (attrition bias) All outcomes	Low risk	No missing data reported.
Selective reporting (reporting bias)	Unclear risk	Study protocol is not available but it seems clear that the published report included all expected outcomes, including those that were pre-specified
Other bias	Unclear risk	Some might argue against the suitability of a short-term cross-over design as a way of evaluating treatment for a condition such as persistent exhibitionism Investigators expressed concern about potential therapist's 'expectation' bias, that using one therapist only could have been a source of bias, and that cross-over design precluded assessment of long term effects (but did allow them to view the order of treatment as a 'synergistic' factor)
Blinding of participants and personnel (performance bias) All outcomes	High risk	Neither participants nor personnel could be blinded.
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	No information on blinding status of outcome assessors, but majority of outcome data were by self report. Rearrest/conviction data were objective

Ryan 1997

Methods	Design: cluster-randomised controlled trial
Participants	Participants: male sex offenders on parole or probation Sex: all male Age: not reported Unit of allocation: therapy group Number randomised: 8 groups, 65 participants (n = 33 intervention; n = 32 group therapy) Number completing: 8 groups, 44 participants (n = 25 intervention; n = 19 group therapy) Setting: outpatient; sites in two counties of Texas (Dallas and Tarrant), USA Inclusion criteria: convicted adult sex offender; aged at least 18 yrs; parole or probationary status such that violation of parole/probation stipulations would result in returning to incarceration; mandated to treatment Exclusion criteria: none reported Ethnicity: not reported Baseline characteristics: not reported
Interventions	Two conditions: • Transtheoretical Approach counselling (4 groups, 33 participants randomised) • Group therapy for sex offenders (4 groups, 32 participants randomised) Transtheoretical Approach counselling: after Prochaska and DiClemente (Prochaska 1984); a method of counselling which identifies the awareness of or desire to change the problem for which a person has entered counselling, and what processes will be applied to assist the individual through various stages of change to the point of minimizing or resolving his/her problem; 24 sessions, 90 minutes each; 6 months duration; group size 7 to 9 participants. The intervention may alter throughout the treatment duration as 'change' assessments are made of the participant Group therapy for sex offenders: combination of relapse prevention and CBT as provided by RSOTP to sex offenders in a group setting; 24 sessions, 90 minutes each; 6 months duration; group size 7 to 9 participants Duration of intervention: 6 months Duration of trial: 6 months Length of follow up: participants were not followed up beyond end of trial
Outcomes	**Primary outcomes** None reported **Secondary outcomes** Cognitive distortion/immaturity: mean subscale scores on Multiphasic Sex Inventory (MSI) (Nichols 1984) Sexual obsessions: mean subscale scores on Multiphasic Sex Inventory (MSI) (Nichols 1984) Leaving treatment early **Other outcomes** Lies: mean subscale scores on Multiphasic Sex Inventory (MSI) (Nichols 1984) Justifications: mean subscale scores on Multiphasic Sex Inventory (MSI) (Nichols 1984) Treatment attitudes: mean subscale scores on Multiphasic Sex Inventory (MSI) (Nichols 1984) Sexual knowledge and beliefs: mean subscale scores on Multiphasic Sex Inventory (MSI)

	(Nichols 1984) Social/sexual desirability: mean subscale scores on Multiphasic Sex Inventory (MSI) (Nichols 1984)
Notes	Individual participant data provided

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Sequence allocation was by drawing lots: 'Four groups were randomly assigned to receive the Transtheoretical approach through the process of drawing from the pool of eight groups. The other four groups served as the control' (p. 23)
Allocation concealment (selection bias)	Unclear risk	Insufficient information to allow a judgement to be made. Clarification about method of allocation concealment has been requested from the trial investigator who responded positively, but no further information was available at the time this review was submitted for publication (Ryan 2011 [pers comm].
Incomplete outcome data (attrition bias) All outcomes	Low risk	At endpoint, 8/33 (24%) missing from treatment condition (all 8 returned to prison following rule-breaking); 13/32 (41%) missing from control condition (2 eliminated following contamination with treatment group, 11 returned to prison following rule-breaking) Reasons for missing data do not differ substantially between conditions
Selective reporting (reporting bias)	Unclear risk	Study protocol is not available but it seems clear that the published report included all expected outcomes, including those that were pre-specified
Other bias	High risk	Data were described as not normally distributed. No demographics at baseline were collected. No assessments of number or nature of offences were collected. Therefore, no measurements were taken to assess potential baseline imbalance, which is of concern particularly in view of the small sample and the clustering employed

		in the study. The investigator acknowledged a larger sample would be needed in future research In addition, the dissertation contains no reference to the nature, level or duration of training received by the RSTOP therapist in the 'Transtheoretical Approach'. Clarifications have been requested from the trial investigator who responded positively, but no further information was available at the time this review was submitted for publication
Blinding of participants and personnel (performance bias) All outcomes	High risk	Neither participants nor personnel could be blinded.
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	No information on blinding status of outcome assessors, although *'Inventories were scored and interpreted by the researcher while supervised by a licensed psychologist'* (p.24). Insufficient information to allow a judgement to be made

Characteristics of excluded studies *[ordered by study ID]*

Study	Reason for exclusion
Abel 1970	Allocation: unclear - 1 person "randomly" selected for control condition. Participants: 3 cases of exhibitionism, 2 of transvestism and 1 of masochism. Interventions: contingent electric shocks to tape recordings of deviant sexual behaviour vs non-contingent shock control. Outcomes: MMPI, PPG, self report of deviant and non deviant sexual behaviour. Up to 18 week follow-up
Appelt 1974	Allocation: not randomised, case series
Aytes 2001	Allocation: not randomised, two group retrospective study (archival data)
Bancroft 1974	Allocation: quasi-randomisation, "Williams Square crossover design". Participants: 12 male sexual offenders Interventions: ethinyl oestradiol vs cyproterone acetate vs no treatment, not psychological interventions
Barlow 1969	Allocation: not randomised - two single case studies
Baron 1977	Allocation: not randomised, case series

(*Continued*)

Berlin 1981	Allocation: not randomised, case series
Berner 1983	Allocation: not randomised, case series
Bingham 1995	Allocation: not randomised (single group)
Bouffard 2002	Allocation: randomised. Not an intervention study. Participants were drawn from a 'normal' population. Investigators sought to measure the influence of emotion on 'hypothetical offending decisions'
Boyer 2007	Allocation: not randomised, 'quasi-experimental mixed methods format'
Bradford 1987	Allocation: not randomised, single case study.
Bradford 1993	Allocation: randomised Participants: men with paraphilias Interventions: cyproterone acetate vs placebo, not psychological interventions
Buresova 1990	Allocation: not randomised, double blind case control study
Clelland 1998	Allocation: not randomised, longitudinal
Cooney 1999	Allocation: not randomised, single case study
Cooper 1972	Allocation: not randomised, case series
Cooper 1978	Allocation: not randomised, case series
Cooper 1981	Allocation: randomised Participants: men with "deviant hypersexuality" Interventions: cyproterone acetate vs placebo, not psychological treatments
Cooper 1986	Allocation: not randomised
Cooper 1992a	Allocation: double blind. Participants: paedophiles. Interventions: medroxyprogesterone acetate vs cyproterone acetate vs placebo, not psychological interventions
Cooper 1992b	Allocation: not randomised, literature review
Cooper 1994	Allocation: not randomised, case series
Craft 1980	Allocation: not randomised, case series
Craissati 2007	Allocation: not randomised - three groups including individual treatment, group treatment and a 'miscellaneous' group of men deemed inappropriate for the program or unable to attend
Davies 1975	Allocation: not randomised, case series.

(*Continued*)

Douglass 2002	Allocation: randomised. Population ineligible ('normal' university students with no history of offending). Program sought to encourage young men to obtain 'unambiguous consent'
Drapeau 2005	Allocation: not randomised, summary of a series of small pilot studies
Eher 1997	Allocation: not randomised, single group intervention study (phrase 'both groups' in abstract refers to the spouses and parents of sex offenders, not to two intervention groups)
Fahndrich 1974	Allocation: not randomised, case series
Fedoroff 1992	Allocation: not randomised, retrospective case review
Furby 1989	Allocation: not randomised, literature review
Glander 1981	Allocation: not randomised, case series
Gottesman 1993	Allocation: not randomised, case series
Grunfeld 1986	Allocation: not randomised, case series
Haines 1986	Allocation: not randomised - allocation off waiting list
Hall 1995	Allocation: not randomised, case series
Hallam 1972a	Allocation: not randomised, case series
Hallam 1972b	Allocation: not randomised, case series
Hanson 1992	Allocation: not randomised, long-term follow-up study of treated and non treated groups of child molesters. Data from an intervention group were compared with one 'control' group formed of archival data from before the treatment plan was offered; another 'control' dataset was collected from files on contemporaneous untreated offenders (or rather those data which survived a '90%' deletion of files)
Hucker 1988	Allocation: double blind Participants: paedophiles Interventions: sex drive reducing medication, not psychological interventions
Ilescas 2008	Allocation: not randomised (retrospective nonequivalent control group design)
Jost 1975	Allocation: not randomised, case series
Kafka 1992	Allocation: not randomised
Kiersch 1990	Allocation: not randomised
Kilmann 1982	Allocation: not randomised, literature review
Kockott 1983	Allocation: not randomised

(*Continued*)

Kravitz 1995	Allocation: not randomised
Kruesi 1992	Allocation: not stated Participants: paraphilias Interventions: clomipramine vs desipramine, not psychological interventions
Krusen 1995	Allocation: not randomised, single case study
Lab 1993	Allocation: not randomised, retrospective case series
Langevin 1979	Allocation: double-blind Participants: exhibitionists Interventions: female hormones vs placebo, not psychological interventions
Laschet 1975	Allocation: not randomised, case series and review - often cited as the "original" work on the use of antiandrogens
Laws 1980	Allocation: not randomised, single case study
Laws 1985	Allocation: not randomised, four case studies
Leonard 1983	Allocation: not randomised, case series
Maletsky 1991	Allocation: not randomised, retrospective case series with controls
Marshall 1991	Allocation: not randomised, case control study
Marshall 2008a	Allocation: not randomised, description of treatment programme
McConaghy 1989	Allocation: not randomised, literature review
McGrath 2003	Allocation: not randomised, non equivalent two group design. Treated group was compared with data from a group of incarcerated offenders who denied having committed offences or refused services
McGuire 2000	Allocation: not randomised. Study compared those who had completed an offender programme and those who had refused to take part or not completed
Meyer 1992	Allocation: not randomised, case control study in which controls were treatment refusers
Money 1975	Allocation: not randomised, case series
Mothes 1973	Allocation: not randomised, case series made available by Schering Pharmaceuticals
Neuman 1991	Allocation: not randomised, literature review
O'Reilly 2010	Not allocated at random
Olver 2009	Not allocated at random

(*Continued*)

Quinsey 1980	Allocation: two studies, neither randomised, case series
Rice 1991	Allocation: not randomised, case series
Richer 1993	Allocation: not randomised
Schewe 1993a	Allocation: randomised, primary prevention of sexual aggression trial. Participant group ineligible as had no history of sexual offending; nor were any data taken on this after the study was complete
Schewe 1993b	Allocation: randomised, primary prevention of sexual aggression trial. Participant group ineligible as had no history of sexual offending; nor were any data taken on this after the study was complete
Servais 1968	Allocation: not randomised
Shaw 1995	Allocation: not randomised, retrospective view of incarcerated and outpatient sex offenders
Tancredi 1986	Allocation: not randomised, literature review
Thibaut 1996	Allocation: not randomised, case series
van Moffaert 1976	Allocation: not randomised
Walker 2000	Allocation: not randomised (retrospective outcome comparison of sex offenders treated with and without family involvement)
Wastell 2009	Allocation: not randomised
Wincze 1986	Allocation: not randomised
Wollert 1988	Allocation: randomised (with wait list control) Participants: 9 family members (three couples and three single members from families in which sexual abuse had taken place. This appears to have included non-offending partners of couples within families). All abusers within the study were male; non offending partners, female. It is unclear how many were randomised to treatment and how many to wait list control Interventions: communications training vs standard care (agency-mandated 'Parents United' self-help groups) Outcomes: satisfaction, communication
Zohar 1994	Allocation: not randomised

Characteristics of studies awaiting assessment *[ordered by study ID]*

Abel 1988

Methods	Allocation: unclear, "divided into groups", cross-over
Participants	Participants: non-incarcerated paedophiles (192)
Interventions	Interventions: weekly group sessions (total 30), 10 sessions each of i. Decreasing deviant arousal; ii. Sex education/ sex dysfunction and cognitive restructuring; iii. Social and assertiveness training
Outcomes	Outcomes: leaving the study early, recidivism (12 months), data not reported by group, no data suitable for meta-analysis
Notes	Attempt has been made to contact first author

DATA AND ANALYSES

Comparison 1. Behavioural: imaginal desensitisation versus covert sensitisation

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Anomalous desire (target behaviour) assessed by interview	1		Risk Ratio (M-H, Fixed, 95% CI)	Subtotals only
1.1 Reduced at one month	1	20	Risk Ratio (M-H, Fixed, 95% CI)	0.57 [0.24, 1.35]
1.2 Reduced at one year	1	20	Risk Ratio (M-H, Fixed, 95% CI)	0.6 [0.19, 1.86]
2 Lost to follow-up	1		Risk Ratio (M-H, Random, 95% CI)	Subtotals only

Comparison 2. Behavioural: medication plus imaginal desensitisation vs medication alone

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Anomalous desire (target behaviour) assessed by interview	1		Risk Ratio (M-H, Fixed, 95% CI)	Subtotals only
1.1 Reduced by 1 month	1	20	Risk Ratio (M-H, Fixed, 95% CI)	1.67 [0.54, 5.17]
1.2 Reduced by 1 year	1	20	Risk Ratio (M-H, Fixed, 95% CI)	0.6 [0.19, 1.86]
2 Adverse events	1	20	Risk Ratio (M-H, Random, 95% CI)	0.57 [0.24, 1.35]

Comparison 3. CBT group therapy versus no treatment

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Recidivism at 1 year + at risk	1		Risk Ratio (M-H, Fixed, 95% CI)	Subtotals only
1.1 Involving only sex offences	1	484	Risk Ratio (M-H, Fixed, 95% CI)	1.10 [0.78, 1.56]
2 Cognitive distortions: 1. Average endpoint score at 2 months (ABCS, high = good)	1	60	Mean Difference (IV, Fixed, 95% CI)	13.43 [6.81, 20.05]
3 Cognitive distortions: 2. Average endpoint score at 2 months (MSI, high = good, data skewed)			Other data	No numeric data
4 Sexual obsessions	1		Mean Difference (IV, Random, 95% CI)	Subtotals only
4.1 Sexual inadequacies scale of the MSI	1	60	Mean Difference (IV, Random, 95% CI)	-6.20 [-13.46, 1.06]
5 Anxiety (generic social avoidance or distress)	3	112	Std. Mean Difference (IV, Random, 95% CI)	-0.23 [-0.61, 0.14]

6 Leaving treatment early	Other data	No numeric data

Comparison 4. CBT group therapy versus transtheoretical counselling (cluster)

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Cognitive distortions/immaturity - at 24 weeks	1	38	Risk Ratio (M-H, Random, 95% CI)	1.0 [0.76, 1.32]
2 Sexual obsessions: high levels of obsession - at 24 weeks	1	38	Risk Ratio (M-H, Random, 95% CI)	1.5 [0.80, 2.81]
3 Leaving treatment early	1	65	Risk Ratio (M-H, Random, 95% CI)	1.68 [0.80, 3.49]

Comparison 5. Group psychotherapy versus probation

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Rearrest within 10 years	1		Risk Ratio (M-H, Fixed, 95% CI)	Subtotals only
1.1 For sex offence	1	231	Risk Ratio (M-H, Fixed, 95% CI)	1.87 [0.78, 4.47]

Analysis 1.1. Comparison 1 Behavioural: imaginal desensitisation versus covert sensitisation, Outcome 1 Anomalous desire (target behaviour) assessed by interview.

Review: Psychological interventions for adults who have sexually offended or are at risk of offending

Comparison: 1 Behavioural: imaginal desensitisation versus covert sensitisation

Outcome: 1 Anomalous desire (target behaviour) assessed by interview

Study or subgroup	Imaginal de'sensit'n n/N	Covert de'sensit'n n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
1 Reduced at one month					
McConaghy 1985	4/10	7/10		100.0 %	0.57 [0.24, 1.35]
Subtotal (95% CI)	**10**	**10**		**100.0 %**	**0.57 [0.24, 1.35]**
Total events: 4 (Imaginal de'sensit'n), 7 (Covert de'sensit'n)					
Heterogeneity: not applicable					
Test for overall effect: Z = 1.27 (P = 0.20)					
2 Reduced at one year					
McConaghy 1985	3/10	5/10		100.0 %	0.60 [0.19, 1.86]
Subtotal (95% CI)	**10**	**10**		**100.0 %**	**0.60 [0.19, 1.86]**
Total events: 3 (Imaginal de'sensit'n), 5 (Covert de'sensit'n)					
Heterogeneity: not applicable					
Test for overall effect: Z = 0.88 (P = 0.38)					

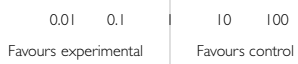
Analysis 1.2. Comparison 1 Behavioural: imaginal desensitisation versus covert sensitisation, Outcome 2 Lost to follow-up.

Review: Psychological interventions for adults who have sexually offended or are at risk of offending

Comparison: 1 Behavioural: imaginal desensitisation versus covert sensitisation

Outcome: 2 Lost to follow-up

Study or subgroup	Favours experimental n/N	Covert sensitization n/N	Risk Ratio M-H,Random,95% CI	Risk Ratio M-H,Random,95% CI
McConaghy 1985	1/10	3/10		0.33 [0.04, 2.69]
Subtotal (95% CI)	**0**	**0**		**0.0 [0.0, 0.0]**
Total events: 1 (Favours experimental), 3 (Covert sensitization)				
Heterogeneity: not applicable				
Test for overall effect: Z = 0.0 (P < 0.00001)				

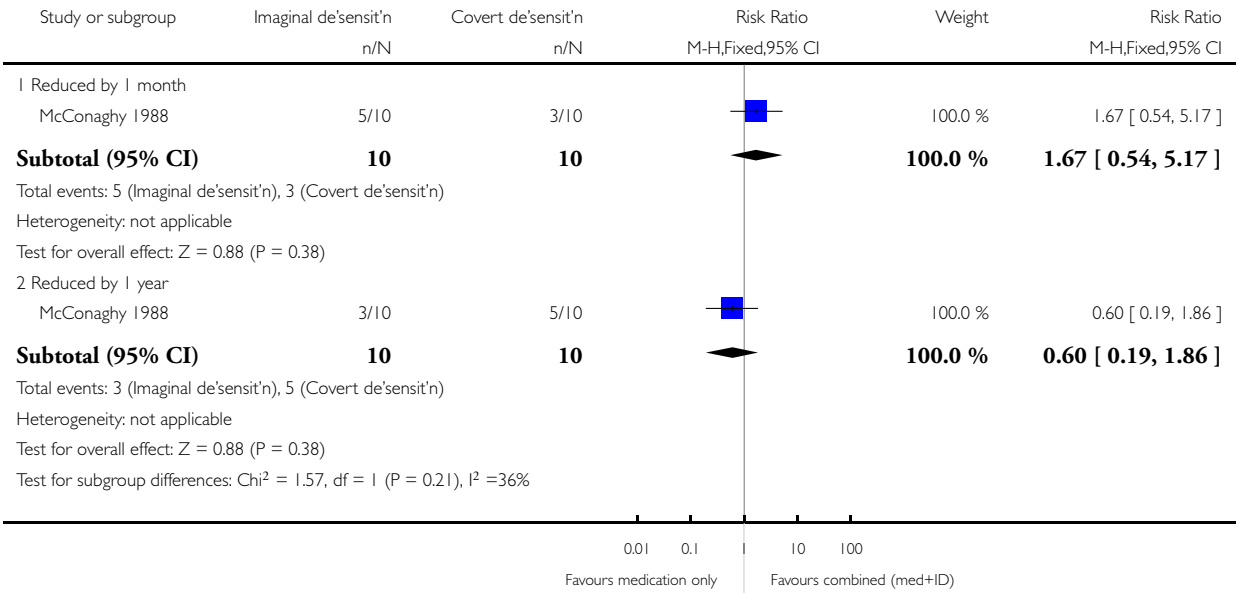


Analysis 2.1. Comparison 2 Behavioural: medication plus imaginal desensitisation vs medication alone, Outcome 1 Anomalous desire (target behaviour) assessed by interview.

Review: Psychological interventions for adults who have sexually offended or are at risk of offending

Comparison: 2 Behavioural: medication plus imaginal desensitisation vs medication alone

Outcome: 1 Anomalous desire (target behaviour) assessed by interview

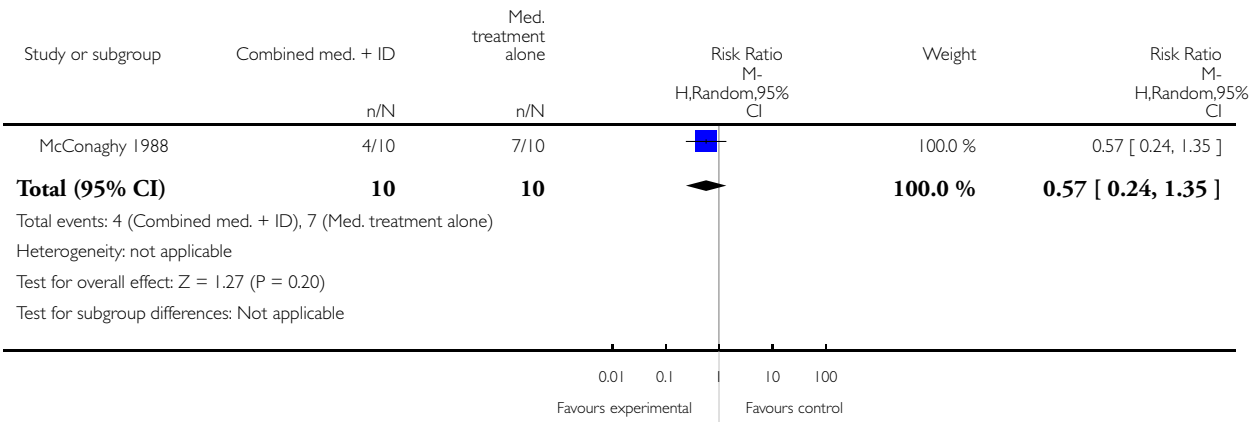


Analysis 2.2. Comparison 2 Behavioural: medication plus imaginal desensitisation vs medication alone, Outcome 2 Adverse events.

Review: Psychological interventions for adults who have sexually offended or are at risk of offending

Comparison: 2 Behavioural: medication plus imaginal desensitisation vs medication alone

Outcome: 2 Adverse events

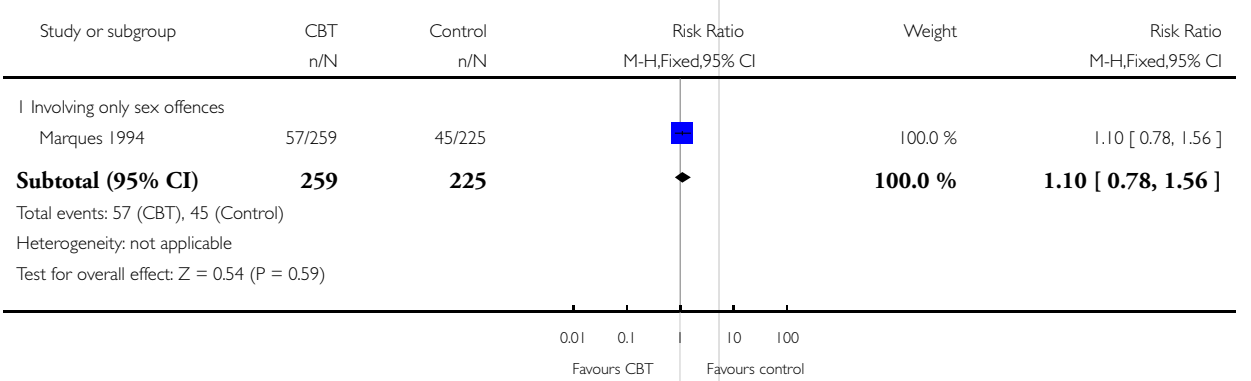


Analysis 3.1. Comparison 3 CBT group therapy versus no treatment, Outcome 1 Recidivism at 1 year + at risk.

Review: Psychological interventions for adults who have sexually offended or are at risk of offending

Comparison: 3 CBT group therapy versus no treatment

Outcome: 1 Recidivism at 1 year + at risk

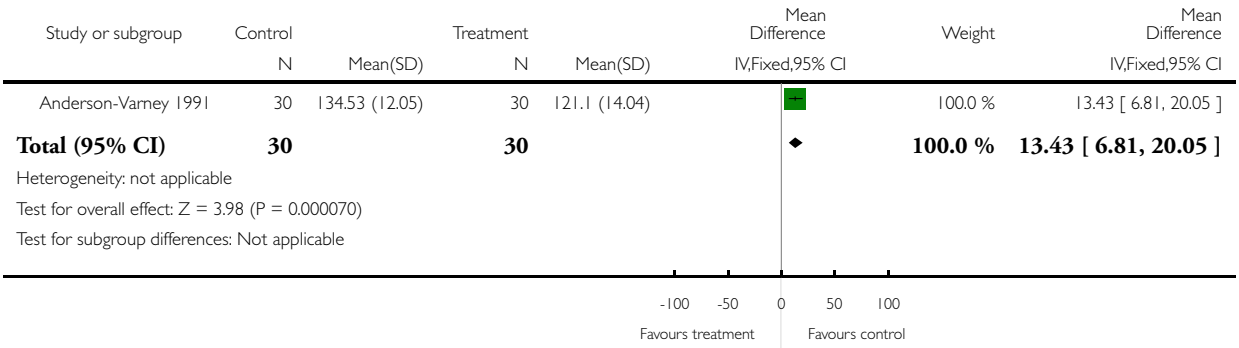


Analysis 3.2. Comparison 3 CBT group therapy versus no treatment, Outcome 2 Cognitive distortions: 1. Average endpoint score at 2 months (ABCS, high = good).

Review: Psychological interventions for adults who have sexually offended or are at risk of offending

Comparison: 3 CBT group therapy versus no treatment

Outcome: 2 Cognitive distortions: 1. Average endpoint score at 2 months (ABCS, high = good)



Analysis 3.3. Comparison 3 CBT group therapy versus no treatment, Outcome 3 Cognitive distortions: 2. Average endpoint score at 2 months (MSI, high = good, data skewed).

Cognitive distortions: 2. Average endpoint score at 2 months (MSI, high = good, data skewed)

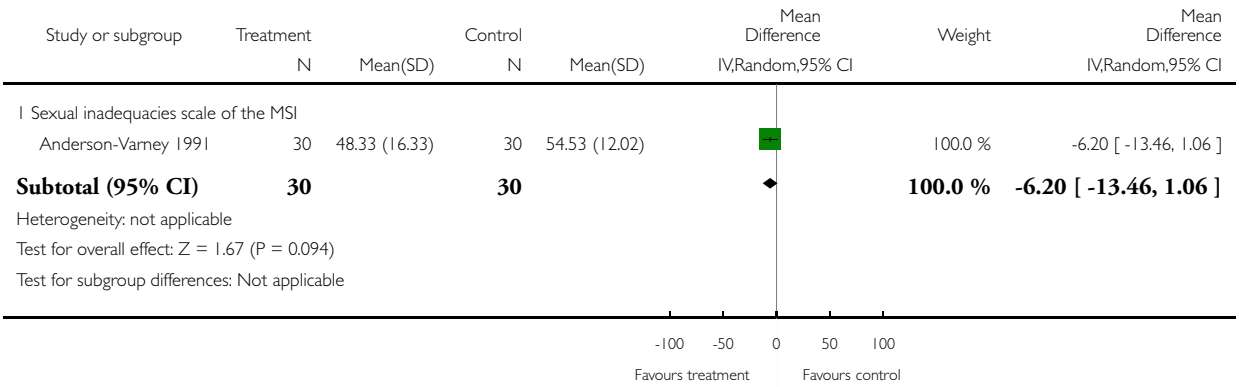
Study	Intervention	Mean	SD	N	Notes
Anderson-Varney 1991	Cognitive, social educational programme	12.73	10.25	30	[F (1,58)=0.637]
Anderson-Varney 1991	Standard care	13.53	10.82	30	

Analysis 3.4. Comparison 3 CBT group therapy versus no treatment, Outcome 4 Sexual obsessions.

Review: Psychological interventions for adults who have sexually offended or are at risk of offending

Comparison: 3 CBT group therapy versus no treatment

Outcome: 4 Sexual obsessions

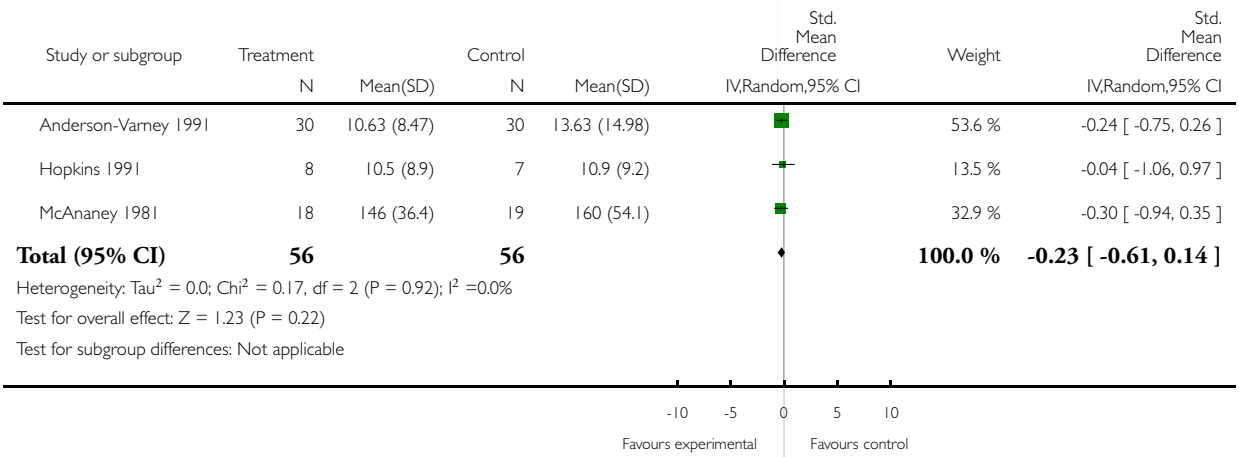


Analysis 3.5. Comparison 3 CBT group therapy versus no treatment, Outcome 5 Anxiety (generic social avoidance or distress).

Review: Psychological interventions for adults who have sexually offended or are at risk of offending

Comparison: 3 CBT group therapy versus no treatment

Outcome: 5 Anxiety (generic social avoidance or distress)



Analysis 3.6. Comparison 3 CBT group therapy versus no treatment, Outcome 6 Leaving treatment early.

Leaving treatment early

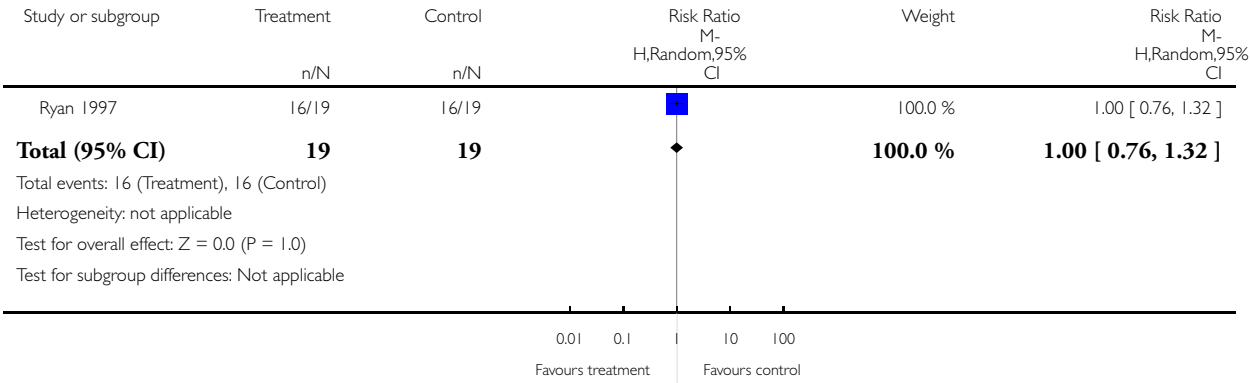
Study	**Outcome**	**Treatment (SOTEP) (n = 259)**	**Control (no treatment) (n = 225)**
Marques 1994	Dropped out before starting treatment	55/259	0/225
Marques 1994	Left treatment early (*voluntarily*)	27/259	0/225
Marques 1994	Left treatment early (*compulsory return to prison due to management problems)*	10/259	0/255

Analysis 4.1. Comparison 4 CBT group therapy versus transtheoretical counselling (cluster), Outcome 1 Cognitive distortions/immaturity - at 24 weeks.

Review: Psychological interventions for adults who have sexually offended or are at risk of offending

Comparison: 4 CBT group therapy versus transtheoretical counselling (cluster)

Outcome: 1 Cognitive distortions/immaturity - at 24 weeks

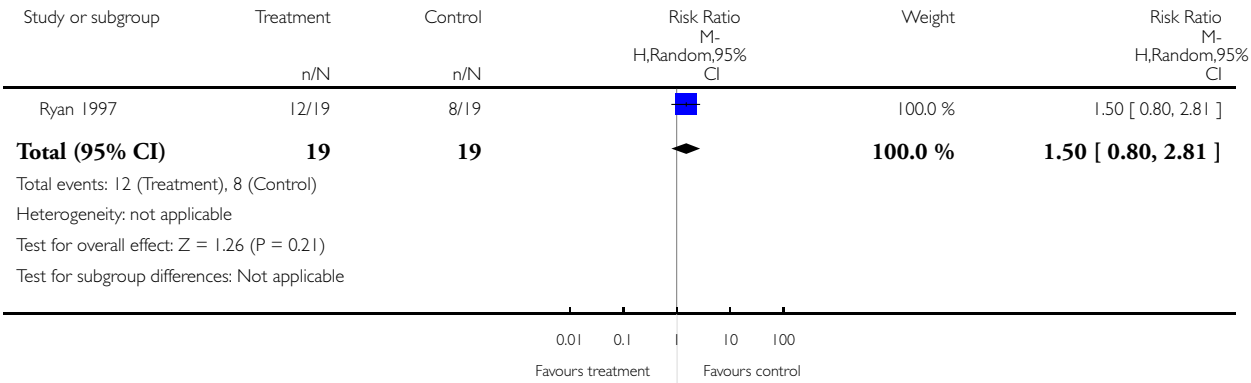


Analysis 4.2. Comparison 4 CBT group therapy versus transtheoretical counselling (cluster), Outcome 2 Sexual obsessions: high levels of obsession - at 24 weeks.

Review: Psychological interventions for adults who have sexually offended or are at risk of offending

Comparison: 4 CBT group therapy versus transtheoretical counselling (cluster)

Outcome: 2 Sexual obsessions: high levels of obsession - at 24 weeks

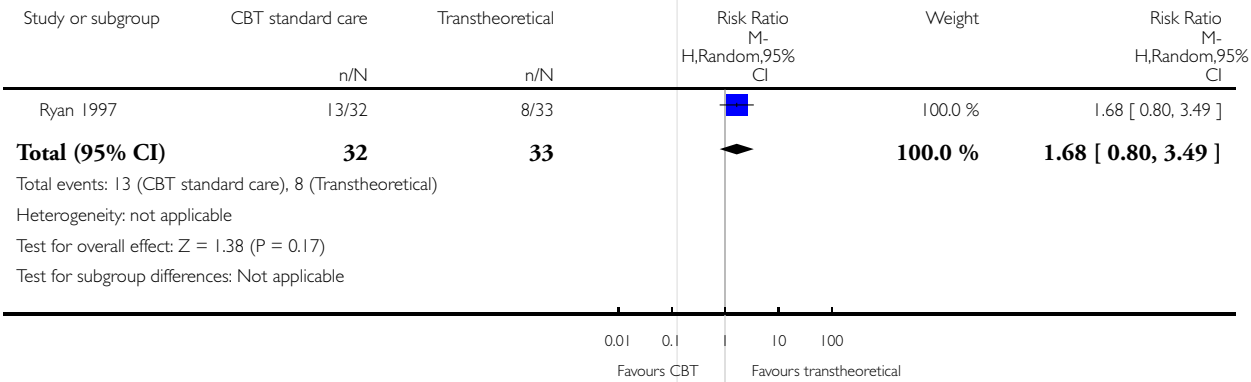


Analysis 4.3. Comparison 4 CBT group therapy versus transtheoretical counselling (cluster), Outcome 3 Leaving treatment early.

Review: Psychological interventions for adults who have sexually offended or are at risk of offending

Comparison: 4 CBT group therapy versus transtheoretical counselling (cluster)

Outcome: 3 Leaving treatment early

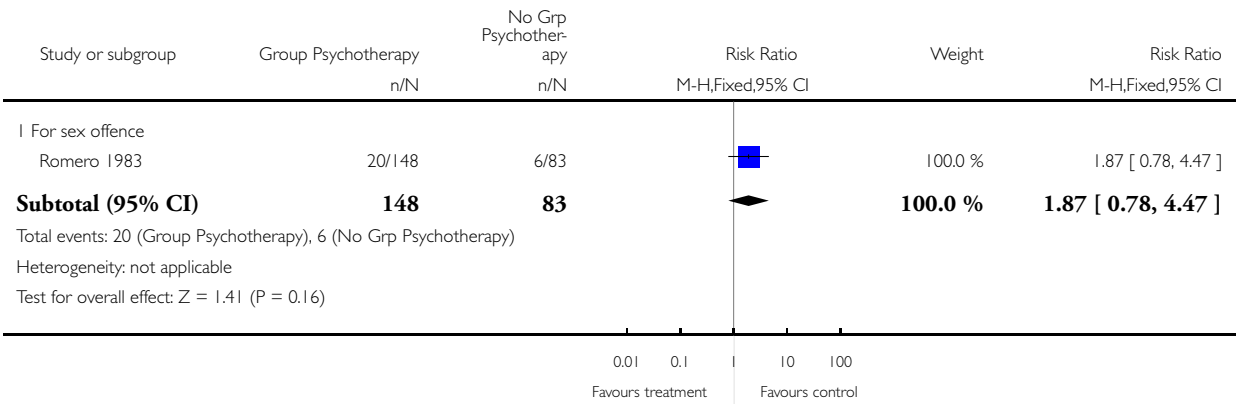


Analysis 5.1. Comparison 5 Group psychotherapy versus probation, Outcome 1 Rearrest within 10 years.

Review: Psychological interventions for adults who have sexually offended or are at risk of offending

Comparison: 5 Group psychotherapy versus probation

Outcome: 1 Rearrest within 10 years



APPENDICES

Appendix 1. Search strategies used in October 2010

AMED (Allied and Complementary Medicine)

1 (sex$ adj2 devia$).tw.)
2 (public adj2 masturbat$).tw.
3 (child$ adj2 molest$).tw.
4 (child$ adj2 (sex$ or abuse$)).tw.
5 (sex$ adj2 (murder$ or tortur$ or abus$ or fondl$)).tw.
6 (indecen$ adj2 behav$).tw.
7 (child$ adj2 porn$).tw.
8 (lewd$ adj2 (behav$ or act)).tw.
9 bondag$.tw.
10 frotteur$.tw.
11 necrophi$.tw.
12 bugger$.tw.
13 molest$.tw.
14 pederast$.tw.
15 paedoph$.tw.
16 pedoph$.tw.
17 scatologia.tw.
18 necrophilia.tw.
19 zoophilia.tw.
20 coprophilia.tw.

21 urophilia.tw.
22 partialism.tw.
23 klismaphilia.tw.
24 bestiality.tw.
25 sodom$.tw.
26 molest$.tw.
27 paraphil$.tw.
28 voyeur$.tw.
29 exp Sex offenses/
30 exp Paraphilias/
31 exp Sex behavior/
32 exp Child abuse sexual/
33 or/1-32
34 random$.ab,ti.
35 (double adj2 blind).ab,ti.
36 (triple adj2 blind).ab,ti.
37 (single adj2 blind).ab,ti.
38 (treble adj2 blind).ab,ti.
39 (random adj2 (allocat$ or assign$)).ab,ti.
40 exp clinical trials/
41 exp Double blind method/
42 exp Random allocation/
43 or/34-42
44 33 and 43

2. ASSIA

Query: ((DE=(”rape“ or ”acquaintance rape“ or ”date rape“ or ”drug rape“ or ”gang rape“ or ”male rape“ or ”marital rape“ or ”serial rape“)) or (DE=(”incest“ or ”father daughter incest“ or ”mother son incest“)) or (DE=”exhibitionism“) or (DE=”sex offending“) or (DE=(”sexual deviance“ or ”erotomania“ or ”frotteurism“ or ”gender dysphoria“ or ”necrophilia“ or ”paedophilia“ or ”paedophiliacs“ or ”paraphilia“ or ”fetishism“ or ”klismaphilia“ or ”paraphiliacs“ or ”sadism“ or ”sadomasochism“ or ”masochism“ or ”sodomy“ or ”transsexuals“ or ”transvestism“ or ”transvestites“ or ”zoophilia“ or ”zoophiliacs“)) or (sex* within 2 devia*) or (public* within 2 masturbat*) or (child* within 2 (sex* or abuse*)) or (child* within 2 porn*) or (child* within 2 molest*) or (lewd* within 2 (behav* or act)) or (indecen* within 2 behav*) or (sex* within 2 (murder* or tortur* or abus* or fondl*)) or (partialism or klismaphilia or bestiality* or sodom* or molest* or paraphil* or voyeur*) or (bondag* or frotteur* or necrophi* or bugger* or molest* or pederast* or paedoph* or pedoph* or scatalogia or necrophilia or zoophilia or coprophilia or urophilia)) and ((DE=(double blind studies) or (single* near blind*)) or (TI=(double* blind*) or AB=(double* blind*)) or (DE=(randomi?ed controlled trials)) or (TI=random* or AB=random*))

3. BIOSIS

12 #11 AND #10
11 #7 OR #6 OR #5 OR #4 OR #3 OR #2 OR #1
10 #9 OR #8
9 TS=((singl* OR doubl* OR tripl* OR trebl*) SAME (mask* OR blind*))
8 TS=(random* OR crossover)
7 TS=(urophilia OR partialism OR klismaphilia OR bestiality OR sodom*)
6 TS=(molest* OR paraphil* OR voyeur* OR bondag* OR frotteur* OR necrophi* OR bugger* OR pederast* OR paedoph* OR pedoph* OR scatalogia)
5 TS=(zoophilia OR coprophilia OR fetishi* OR exhibitionism OR voyeuris* OR sadis* OR masochis* OR incest* OR rapist* OR rape*)
4 TS=(sex* SAME (murder* OR tortur* OR abus* OR fondl*))
3 TS=(sex* SAME devia*) OR TS=(public SAME masturbat*) OR TS=(child* SAME molest*) OR TS=(child* SAME sex*)
2 TS=(indecen* SAME behav*) OR TS=(child* SAME porn*) OR TS=(lewd* SAME behav*) OR TS=(lewd* SAME act*)
1 Topic=((sex* SAME offen*))

4. CENTRAL

#1 MeSH descriptor Child Abuse, Sexual explode all trees
#2 MeSH descriptor Sexual and Gender Disorders explode all trees

#3 MeSH descriptor Sexual Dysfunctions, Psychological explode all trees
#4 MeSH descriptor Sex Offenses, this term only
#5 MeSH descriptor Incest explode all trees
#6 MeSH descriptor Rape explode all trees
#7 sex* near/2 devia*
#8 public* near/2 masturbat*
#9 child* near/2 sex*
#10 child* near/2 abuse*
#11 child* near/2 porn*
#12 child* near/2 molest*
#13 sex* near/2 murder*
#14 sex* near/2 tortur*
#15 sex* near/2 abus*
#16 sex* near/2 fondl*
#17 indecen* near/2 behav*
#18 lewd* near/2 behav*
#19 lewd* near/2 act
#20 bondag*
#21 frotteur*
#22 necrophi*
#23 bugger*
#24 molest*
#25 pederast*
#26 paedoph*
#27 pedoph*
#28 scatologia
#29 zoophilia
#30 coprophilia
#31 urophilia
#32 partialism
#33 klismaphilia
#34 beastiality
#35 sodom*
#36 paraphil*
#37 voyeur*
#38 (#1 OR #2 OR #3 OR #4 OR #5 OR #6 OR #7 OR #8 OR #9 OR #10 OR #11 OR #12 OR #13 OR #14 OR #15 OR #16 OR #17 OR #18 OR #19 OR #20 OR #21 OR #22 OR #23 OR #24 OR #25 OR #26 OR #27 OR #28 OR #29 OR #30)
#39 (#31 OR #32 OR #33 OR #34 OR #35 OR #36 OR #37)
#40 (#38 OR #39)

5. CINAHL

1 (sex$ adj2 devia$).tw.
2 (public adj2 masturbat$).tw.
3 (child$ adj2 molest$).tw.
4 (child$ adj2 (sex$ or abuse$)).tw.
5 (sex$ adj2 (murder$ or tortur$ or abus$ or fondl$)).tw.
6 (indecen$ adj2 behav$).tw.
7 (child$ adj2 porn$).tw.
8 (lewd$ adj2 (behav$ or act)).tw.
9 bondag$.tw.
10 frotteur$.tw.
11 necrophi$.tw.
12 bugger$.tw.
13 molest$.tw.

14 pederast$.tw.
15 paedoph$.tw.
16 pedoph$.tw.
17 scatologia.tw.
18 necrophilia.tw.
19 zoophilia.tw.
20 coprophilia.tw.
21 urophilia.tw.
22 partialism.tw.
23 klismaphilia.tw.
24 bestiality.tw.
25 sodom$.tw.
26 molest$.tw.
27 paraphil$.tw.
28 voyeur$.tw.
29 exp PARAPHILIAS/
30 exp Sexuality/
31 exp Child Abuse, Sexual/
32 exp INCEST/
33 exp RAPE/
34 or/1-33
35 randomi$.mp.
36 clin$.mp.
37 trial$.mp.
38 (clin$ adj3 trial$).mp.
39 singl$.mp.
40 doubl$.mp.
41 tripl$.mp.
42 trebl$.mp.
43 mask$.mp.
44 blind$.mp.
45 (39 or 40 or 41 or 42) and (43 or 44)
46 crossover.mp.
47 random$.mp.
48 allocate$.mp.
49 assign$.mp.
50 (random$ adj3 (allocate$ or assign$)).mp.
51 Random Assignment/
52 exp Clinical Trials/
53 exp Meta Analysis/
54 50 or 46 or 45 or 38 or 35 or 51 or 52 or 53
55 34 and 54

6. Dissertation Abstracts International (July 2008 searched by Jo Abbott, October 2010 by Nick Huband)

((randomi* w/8 control*) or (triple w/10 blind*) or (double w/10 blind*) or (treble w/10 blind*) or (single w/10 blind*) or (treble w/10 blind*) or ab(double blind*) or ab(randomi*) or (controlled clinical trial)) and (rape or (date and rape) or (male and rape) or (sexual and fetishism) or (child* and sexual and abuse) or (sexual and violence) or (sex and identity) or (sex and offender) or (sex and offence) or (sex* and devia*) or (public and masturbate*) or (child and molest*) or (sex* and (murder or tortur* or abus* or fondl*)) or (indecent* and behav*) or (child* and porn*) or (lewd and (behave* or act*)) or (gender and dysphoria) or bestiality or bondag* or bugger* or coprophilia or erotomania or exhibitionism or fetishism or frotteur* or incest or klismaphilia or masochism or molest* or necrophi* or necrophilia or paedoph* or pedoph* or paedophilia or paedophiliacs or paraphil* or paraphilia or paraphiliacs or partialism or pederast* or pedoph* or rape or sadism or sadomasochism or scatalogia or sodom* or sodomy or transsexuals or transvestism or transvestites or urophilia or voyeur* or zoophilia or zoophilia or zoophiliac)

7. EMBASE

1 exp Sex Offenses/
2 exp Paraphilias/
3 exp Sexual Behavior/
4 exp Child Abuse, Sexual/
5 exp "Fetishism (Psychiatric)"/
6 exp Exhibitionism/
7 exp Voyeurism/
8 exp Pedophilia/
9 exp Sadism/
10 exp Masochism/
11 exp Incest/
12 exp Rape/
13 (sex$ adj2 devia$).tw.
14 (public adj2 masturbat$).tw.
15 (child$ adj2 molest$).tw.
16 (child$ adj2 (sex$ or abuse$)).tw.
17 (sex$ adj2 (murder$ or tortur$ or abus$ or fondl$)).tw.
18 (indecen$ adj2 behav$).tw.
19 (child$ adj2 porn$).tw.
20 (lewd$ adj2 (behav$ or act)).tw.
21 bondag$.tw.
22 frotteur$.tw.
23 necrophi$.tw.
24 bugger$.tw.
25 molest$.tw.
26 pederast$.tw.
27 paedoph$.tw.
28 pedoph$.tw.
29 scatologia.tw.
30 necrophilia.tw.
31 zoophilia.tw.
32 coprophilia.tw.
33 urophilia.tw.
34 partialism.tw.
35 klismaphilia.tw.
36 bestiality.tw.
37 sodom$.tw.
38 molest$.tw.
39 paraphil$.tw.
40 voyeur$.tw.
41 or/1-40
42 clin$.tw.
43 trial$.tw.
44 (clin$ adj3 trial$).tw.
45 singl$.tw.
46 doubl$.tw.
47 trebl$.tw.
48 tripl$.tw.
49 blind$.tw.
50 mask$.tw.
51 ((singl$ or doubl$ or trebl$ or tripl$) adj3 (blind$ or mask$)).tw.
52 randomi$.tw.
53 random$.tw.

54 allocat$.tw.
55 assign$.tw.
56 (random$ adj3 (allocat$ or assign$)).tw.
57 crossover.tw.
58 57 or 56 or 52 or 51 or 44
59 exp Randomized Controlled Trial/
60 exp Double Blind Procedure/
61 exp Crossover Procedure/
62 exp Single Blind Procedure/
63 exp RANDOMIZATION/
64 59 or 60 or 61 or 62 or 63 or 58
65 41 and 64

8. International Bibliography of Social Sciences

Query: (((sex* within 2 devia*) or (public* within 2 masturbat*) or (child* within 2 (sex* or abuse*)) or (child* within 2 porn*) or (child*within 2 molest*) or (lewd* within 2 (behav* or act)) or (indecen* within 2 behav*) or (sex* within 2 (murder* or tortur* or abus* or fondl*)) or (partialism or klismaphilia or bestiality* or sodom* or molest* or paraphil* or voyeur*) or (bondag* or frotteur* or necrophi* or bugger* or molest* or pederast* or paedoph* or pedoph* or scatalogia or necrophilia or zoophilia or coprophilia or urophilia)) or(DE=("incest" or "rape")))and(((single within 3 blind*) or (triple within 3 blind*) or (treble within 3 blind*)) or(TI=random* or AB=random*) or(TI=trial* or AB=trial*))

9. ISI Proceedings

12 #11 AND #10
11 #7 OR #6 OR #5 OR #4 OR #3 OR #2 OR #1
10 #9 OR #8
9 TS=((singl* OR doubl* OR tripl* OR trebl*) SAME (mask* OR blind*))
8 TS=(random* OR crossover)
7 TS=(urophilia OR partialism OR klismaphilia OR bestiality OR sodom*)
6 TS=(molest* OR paraphil* OR voyeur* OR bondag* OR frotteur* OR necrophi* OR bugger* OR pederast* OR paedoph* OR pedoph* OR scatalogia)
5 TS=(zoophilia OR coprophilia OR fetishi* OR exhibitionism OR voyeuris* OR sadis* OR masochis* OR incest* OR rapist* OR rape*)
4 TS=(sex* SAME (murder* OR tortur* OR abus* OR fondl*))
3 TS=(sex* SAME devia*) OR TS=(public SAME masturbat*) OR TS=(child* SAME molest*) OR TS=(child* SAME sex*)
2 TS=(indecen* SAME behav*) OR TS=(child* SAME porn*) OR TS=(lewd* SAME behav*) OR TS=(lewd* SAME act*)
1 Topic=((sex* SAME offen*))

10. Science Citation Index

12 #11 AND #10
11 #7 OR #6 OR #5 OR #4 OR #3 OR #2 OR #1
10 #9 OR #8
9 TS=((singl* OR doubl* OR tripl* OR trebl*) SAME (mask* OR blind*))
8 TS=(random* OR crossover)
7 TS=(urophilia OR partialism OR klismaphilia OR bestiality OR sodom*)
6 TS=(molest* OR paraphil* OR voyeur* OR bondag* OR frotteur* OR necrophi* OR bugger* OR pederast* OR paedoph* OR pedoph* OR scatalogia)
5 TS=(zoophilia OR coprophilia OR fetishi* OR exhibitionism OR voyeuris* OR sadis* OR masochis* OR incest* OR rapist* OR rape*)
4 TS=(sex* SAME (murder* OR tortur* OR abus* OR fondl*))
3 TS=(sex* SAME devia*) OR TS=(public SAME masturbat*) OR TS=(child* SAME molest*) OR TS=(child* SAME sex*)
2 TS=(indecen* SAME behav*) OR TS=(child* SAME porn*) OR TS=(lewd* SAME behav*) OR TS=(lewd* SAME act*)
1 Topic=((sex* SAME offen*))

11. Social Science Citation Index

12 #11 AND #10
11 #7 OR #6 OR #5 OR #4 OR #3 OR #2 OR #1
10 #9 OR #8

9 TS=((singl* OR doubl* OR tripl* OR trebl*) SAME (mask* OR blind*))
8 TS=(random* OR crossover)
7 TS=(urophilia OR partialism OR klismaphilia OR bestiality OR sodom*)
6 TS=(molest* OR paraphil* OR voyeur* OR bondag* OR frotteur* OR necrophi* OR bugger* OR pederast* OR paedoph* OR pedoph* OR scatalogia)
5 TS=(zoophilia OR coprophilia OR fetishi* OR exhibitionism OR voyeuris* OR sadis* OR masochis* OR incest* OR rapist* OR rape*)
4 TS=(sex* SAME (murder* OR tortur* OR abus* OR fondl*))
3 TS=(sex* SAME devia*) OR TS=(public SAME masturbat*) OR TS=(child* SAME molest*) OR TS=(child* SAME sex*)
2 TS=(indecen* SAME behav*) OR TS=(child* SAME porn*) OR TS=(lewd* SAME behav*) OR TS=(lewd* SAME act*)
1 Topic=((sex* SAME offen*))

12. MEDLINE

1 exp Sex Offenses/
2 exp Paraphilias/
3 exp Sexual Behavior/
4 exp Child Abuse, Sexual/
5 exp "Fetishism (Psychiatric)"/
6 exp Exhibitionism/
7 exp Voyeurism/
8 exp Pedophilia/ (570)
9 exp Sadism/ (461)
10 exp Masochism/
11 exp Incest/
12 exp Rape/
13 (sex$ adj2 devia$).tw.
14 (public adj2 masturbat$).tw.
15 (child$ adj2 molest$).tw.
16 (child$ adj2 (sex$ or abuse$)).tw.
17 (sex$ adj2 (murder$ or tortur$ or abus$ or fondl$)).tw.
18 (indecen$ adj2 behav$).tw.
19 (child$ adj2 porn$).tw.
20 (lewd$ adj2 (behav$ or act)).tw.
21 bondag$.tw.
22 frotteur$.tw.
23 necrophi$.tw.
24 bugger$.tw.
25 molest$.tw.
26 pederast$.tw.
27 paedoph$.tw.
28 pedoph$.tw.
29 scatologia.tw.
30 necrophilia.tw.
31 zoophilia.tw.
32 coprophilia.tw.
33 urophilia.tw.
34 partialism.tw.
35 klismaphilia.tw.
36 bestiality.tw.
37 sodom$.tw.
38 molest$.tw.
39 paraphil$.tw.
40 voyeur$.tw.
41 or/1-40

42 randomized controlled trial.pt.
43 controlled clinical trial.pt.
44 randomized controlled trials.sh.
45 random allocation.sh.
46 double blind method.sh.
47 single blind method.sh.
48 or/42-47
49 (animals not humans).sh.
50 48 not 49
51 clinical trial.pt.
52 exp Clinical Trials/
53 (clin$ adj25 trial$).ti,ab.
54 ((singl$ or doubl$ or trebl$ or tripl$) adj25 (blind$ or mask$)).ti,ab.
55 placebos.sh.
56 placebo$.ti,ab.
57 random$.ti,ab.
58 research design.sh.
59 or/51-58
60 59 not 49
61 60 not 50
62 comparative study.sh.
63 exp Evaluation Studies/
64 follow up studies.sh.
65 prospective studies.sh.
66 (control$ or prospectiv$ or volunteer$).ti,ab.
67 or/62-66
68 67 not 49
69 68 not (50 or 61)
70 50 or 61 or 69
71 41 and 70

13. NCJRS

sex* offend*
sex* offend* random*
sex* offend* control*
sex* offend* blind*
sex* abus
Sex* violence

14. openSIGLE (last searched July 2008; NOT searched for 2011 - unreliable access to interface/ records not downloadable)

random* OR "double blind" AND bestiality
random* OR "double blind" AND bondag*
random* OR "double blind" AND bugger*
random* OR "double blind" AND coprophilia*
random* OR "double blind" AND erotomania*
random* OR "double blind" AND exhibition*
random* OR "double blind" AND fetishism
random* OR "double blind" AND frotteur*
random* OR "double blind" AND incest
random* OR "double blind" AND klismaphilia*
random* OR "double blind" AND masochism
random* OR "double blind" AND molest*
random* OR "double blind" AND necrophi*
random* OR "double blind" AND paedoph*
random* OR "double blind" AND paraphil*

random* OR "double blind" AND partialism
random* OR "double blind" AND pederast*
random* OR "double blind" AND pedoph*
random* OR "double blind" AND rape
random* OR "double blind" AND sadism
random* OR "double blind" AND sadomasochism
random* OR "double blind" AND scatalogia
random* OR "double blind" AND sodom*
random* OR "double blind" AND urophilia*
random* OR "double blind" AND voyeur*
random* OR "double blind" AND zoophilia*
random* OR "double blind" AND "sex offen*"
random* OR "double blind" AND "sexual deviance"
random* OR "double blind" AND "public masturbation"
random* OR "double blind" AND "child molestation"
random* OR "double blind" AND "child sexual abuse"
random* OR "double blind" AND "child abuse"
random* OR "double blind" AND murder*
random* OR "double blind" AND tortur*
random* OR "double blind" AND "sexual abuse"
random* OR "double blind" AND fondl*
random* OR "double blind" AND "child porn*"
random* OR "double blind" AND "lewd behav*"
random* OR "double blind" AND "lewd act"

15. PsycINFO

1 exp Sex Offenses/
2 exp PARAPHILIAS/
3 exp Psychosexual Behavior/
4 Child Abuse/
5 exp SADISM/
6 MASOCHISM/
7 Acquaintance Rape/
8 exp Sexual Orientation/
9 Sexual Attitudes/
10 Sexual Risk Taking/
11 Sexual Development/
12 1 or 2 or 3 or 4 or 5 or 6 or 7 or 8 or 9 or 10 or 11
13 ((sex* adj2 devia*) or public masturbat* or child* molest*).mp.
14 (child sex or child abuse or (sex* adj2 murder*)).mp.
15 ((sex* adj2 tortut*) or (sex* adj2 abus*) or (sex* adj2 fondl*)).mp.
16 (indecen* behav* or child* porn* or (lewd* behav* or lewd* act*)).mp.
17 (sex* offen* or (bondag* or frotteur* or necrophi* or bugger* or molest* or pederast*)).mp.
18 (paedoph* or pedoph* or scatalogia or necrophilia or zoophilia or coprophilia or urophilia or partialism or klismaphilia or bestiality or sodom* or molest* or paraphil* or voyeur*).mp.
19 randomi$.tw.
20 singl$.tw.
21 doubl$.tw.
22 trebl$.tw.
23 tripl$.tw.
24 blind$.tw.
25 mask$.tw.
26 (or/20-23) adj3 (or/24-25)
27 clin$.tw.

28 trial$.tw.
29 (clin$ adj3 trial$).tw.
30 placebo$.tw.
31 exp PLACEBO/
32 crossover.tw.
33 exp Treatment Effectiveness Evaluation/
34 exp Mental Health Program Evaluation/
35 random$.tw.
36 assign$.tw.
37 allocate$.tw.
38 (random$ adj3 (assign$ or allocate$)).tw.
39 38 or 34 or 33 or 32 or 31 or 30 or 29 or 26 or 19
40 12 or 13 or 14 or 15 or 16 or 17 or 18
41 39 and 40

16. Social Care Online

Searched using the term "SEXUAL OFFENCES"

17. Sociological Abstracts

Query: ((DE="incest") or (DE="rape") or (DE=("sexual deviation" or "sodomy")) or ((sex* within 2 devia*) or (public* within 2 masturbat*) or (child* within 2 (sex* or abuse*)) or (child* within 2 porn*) or (child* within 2 molest*) or (lewd* within 2 (behav* or act)) or (indecen* within 2 behav*) or (sex* within 2 (murder* or tortur* or abus* or fondl*)) or (partialism or klismaphilia or bestiality* or sodom* or molest* or paraphil* or voyeur*) or (bondag* or frotteur* or necrophi* or bugger* or molest* or pederast* or paedoph* or pedoph* or scatalogia or necrophilia or zoophilia or coprophilia or urophilia))) and (((single within 3 blind*) or (triple within 3 blind*) or (treble within 3 blind*)) or (AB=random* or TI=random* or DE=(randomi?ed controlled trial*)))

18. ZETOC

45 general: zoophil
44 general: voyeur*
43 general: transvestite
42 general: transvestism
41 general: torture
40 general: sodom*
39 general: sadism
38 general: rapist
37 general: rape
36 general: pederast*
35 general: paedoph*
34 general: pedoph*
33 general: partialism
32 general: paraphil*
31 general: necrophi*
30 general: murder*
29 general: molest*
28 general: masochism
27 general: klismaphilia
26 general: frotteur*
25 general: frotter*
24 general: fetish
23 general: exhibitionism
22 general: eroticism

21 general: erotisicm
20 general: erotica
19 general: bugger*
18 general: bondage
17 general: bestiality
16 general: sexual* and devia*
15 general: sex* and violence
14 general: sex* and offending
13 general: sex* and offender*
12 general: sex* and offence*
11 general: child and porn*
10 general: child and porn*
9 general: public and masturb*
8 general: gender and dysphoria
7 general: lewd and behav*
6 general: child and molest
5 general: child and sex and abuse
4 general: indecent and behavior
3 lewd and act
2 general: indecent and behaviour
1 general: sex and abuse

Appendix 2. Methods archived for updates of review

Cross-over trials

As stated, at protocol stage we did not anticipate finding cross-over trials of psychological interventions. Options included using only first period data (which were unavailable from study investigators), or treating the studies as their investigators apparently intended, and looking at them as a comparison of the potential synergistic value of delivering different active behavioural treatments including no treatment phases, in a particular order. We present the investigators' conclusions narratively.

Multiple observations

In the protocol we identified multiple observations as a possible issue, hence the decision to define specific follow-up periods.

Multiple treatment arms

In the protocol (Bilby 2008) we neglected to state our plans for handling studies in which more than one eligible treatment was compared with control. Whilst this does not affect current results (as the only included study for which such methods were required for this review (Rooth 1974) reported no relevant numerical data for separate conditions), for future updates we plan to treat such studies as follows.

In the event that two or more intervention arms test treatments which would (if employed against similar controls in separate trials) be amenable to combine in meta-analysis within our review, then we will pool data from both eligible arms against control. An example of such a study might be one in which different dosages of CBT were compared against nothing (as for example, one hour's therapy per week versus two hours' therapy per week versus a no treatment control).

If, in contrast, trials compare two eligible interventions with an eligible control but the former would not be amenable to pooling had they been reported in separate trials, we will treat the study as two studiesand compare them individiually against control. An example of such a study might be one in which (as in Rooth 1974) aversive 'electrical shock' therapy were compared with CBT, or CBT were compared with psychodynamic psychotherapy, against a no treatment control. All decisions made in regard to such studies will be transparently documented.

Assessment of reporting biases

Funnel plots (effect size versus standard error) will be drawn if sufficient studies are found. We recognise that symmetry of the plots may indicate publication bias, although they may also represent a true relationship between trial size and effect size. If such a relationship is identified, we will examine the clinical diversity of the studies as a possible explanation (Egger 1997).

Subgroup analysis and investigation of heterogeneity

If sufficient studies are found, we will undertake subgroup analysis by severity of offending and type of intervention (for example, classic behavioural-only interventions such as aversion therapy).

Sensitivity analysis

We will use sensitivity analysis to assess the impact of study quality if there are sufficient data, particularly to consider the potential impact of outcome assessors being blinded, or not.

We will also undertake a sensitivity analysis to investigate the robustness of the overall findings where there has been uncertainty or disagreement regarding, for example, the inclusion of studies, data extraction or missing data, or in the event of one or more large studies dominating the results.

Appendix 3. Information on measures used by investigators of studies included within this review

(Based on work done in Kenworthy 2003 *review)*

Outcomes

Reoffending

Marques 1994 measured reoffending by automated records (rap-sheets) and reports from parole system. Participants were deemed to have reoffended if either data source recorded a new sex crime or non-sex violent crime.

Romero 1983 measured recidivism by rearrest. This form of measurement is subject to bias in two directions (see also 'Characteristics of included studies').

Behaviour

Both McConaghy 1985 and McConaghy 1988 recorded the simple but explicit outcome of 'no change in target behaviour'.

McAnaney 1981 used a scale to record 'heterosocial' skills and social avoidance, the Heterosocial Skills Behavior Checklist (HSB) (Barlow 1977)

Desire

Both McConaghy 1985 and McConaghy 1988 recorded simple binary outcomes relevant to desire for target behaviour.

Attitude to treatment and leaving the study early

Rooth 1974, McConaghy 1988 and McAnaney 1981 all seemed to offer the possibility of leaving the study early. Other studies did not clearly have this as an option. McConaghy 1985 specifically recorded attitudes to treatment.

Self esteem

McAnaney 1981, Anderson-Varney 1991, and Hopkins 1991 all used different rating scales to try to measure self esteem.

Cognitive distortions

Anderson-Varney 1991 used a scale to measure cognitive distortions relating to paedophilic behaviour (Abel 1984b). This consists of 29 statements that measure cognitive distortions relating to paedophilic behaviour. Higher scores suggest fewer cognitive distortions.

Ryan 1997 also rated cognitive distortions and immaturity.

Abel and Becker Cognition Scale (ABCS - Abel 1984a)

Sexual knowledge and beliefs

Anderson-Varney 1991 rated knowledge and beliefs using a continuous scale. Ryan 1997, however, reported a binary outcome of 'poor knowledge of sex'.

Sexual obsessions

Ryan 1997 was the only study to rate sexual obsessions and again reported these in binary form.

Anxiety

McAnaney 1981, Anderson-Varney 1991, and Hopkins 1991 all used different rating scales to try to measure anxiety

Anxiety: presented as mean scores on S-R Inventory of Anxiousness (SRIA) (Endler 1962) in McAnaney 1981.

Social sexual desirability

Ryan 1997 reported on participants' level of interest in sexual activity.

Scales

Atitudes Towards Women Scale (ATWS - Spence 1978)

This 15 item scale measures attitudes toward a number of aspects of the female role, including vocational, educational, and intellectual aspects as well as interpersonal relationships. Anderson-Varney 1991 used this scale.

Fear of Negative Evaluation Scale (FNE - Watson 1969)

This has 30 items to be answered 'yes' or 'no' which provide a measure of the extent to which a person is worried about other people making negative comments or evaluations about him. Hopkins 1991 was the only study to use this scale.

Heterosocial Skills Behavior Checklist (HSB Barlow 1977)

Trained raters fill out a checklist whilst watching videotapes of role play between the men and a female confederate. It is designed to measure behaviours associated with initial heterosocial contact. McAnaney 1981 used this scale.

Interpersonal Reactivity Index (IRI - Davis 1980)

This 28 item index measures personal distress, self esteem, fantasy and empathetic concern. Anderson-Varney 1991 used this scale.

Multiphasic Sex Inventory (MSI - Nichols 1984)

This scale consists of 300 true/false items. These produce 20 clinical scales and a sexual history. Subscales such as the Child Molest Scale, the Exhibitionism Scale and the Rape Scale are valid in themselves. Anderson-Varney 1991and Ryan 1997 used this scale.

Rosenberg's Self-Esteem Scale (Rosenberg 1965)

This scale rate ten items from 'strongly agree' to 'strongly disagree' relating to self-confidence and a positive or negative self-image. Hopkins 1991 used this scale.

Social Anxiety and Distress Scale (SADS) (Watson 1969)

This has 28 items to be answered 'yes' or 'no' which provide a measure of the extent to which a person is worried about particular social situations. This scale was employed by Anderson-Varney 1991 and Hopkins 1991.

Social Self-Esteem Inventory (SSM) (Lawson 1979)

Despite being extensively modified within McAnaney 1981 to make them specifically heterosexual in nature, we have reported data from this scale. The original 30 item scale measures self-esteem and was only used by McAnaney 1981.

S-R Inventory of Anxiousness (SRIA) (Endler 1962)

This was employed by McAnaney 1981 and asks participants to self report levels of anxiety in specific social situations. High scores indicate higher levels of anxiety.

Appendix 4. Differences between Kenworthy 2003 review and this review

Authors of the previous version of this review (Kenworthy 2003) conceived the comparison groups in Hopkins 1991, McAnaney 1981 and Anderson-Varney 1991 as 'standard care' whereas the present authors termed them 'no treatment'. Authors of the previous review also reported on *all* outcomes collected by triallists.

WHAT'S NEW

Last assessed as up-to-date: 27 March 2011.

Date	Event	Description
17 October 2012	New search has been performed	A Cochrane review on this topic was published in 2003 (Kenworthy 2003). It was withdrawn in 2008 and a new protocol was written and published as the basis for this review (Bilby 2008)

HISTORY

Protocol first published: Issue 4, 2008

Review first published: Issue 12, 2012

CONTRIBUTIONS OF AUTHORS

MF drafted the protocol with input from CD, NH and former authors Nadja Smailagic and Charlotte Bilby. The search strategy was originally written by Mark Fenton and was updated and rerun at the editorial base of the Cochrane Developmental, Psychosocial and Learning Problems Review Group (CDPLPG). NH and JD led the search for grey literature/ongoing studies/contact of experts in the field. MF, NH, MP and JD screened titles and abstracts with assistance from Nadja Smailagic and Hannah Jones. The editorial base of the CDPLPG assisted in retrieving potentially eligible new studies as well as copies of studies from the previous version of this review. All authors worked in pairs to make eligibility decisions. NH and JD independently extracted data and entered it into RevMan 5. All authors contributed to the final write-up of the review, with CD taking special responsibility for the Discussion.

DECLARATIONS OF INTEREST

Omer Khan - none known.

Michael Ferriter - The Institute of Mental Health at the University of Nottingham (who were at the time 0.5 FTE employer) received a grant from the National Institute for Health Research for the completion of this review (amongst others).

Nick Huband - holds a full time post as Clinical Research Fellow with Nottinghamshire Healthcare NHS Trust and the Institute of Mental Health, Nottingham, and was employed in that role during the production of this review.

Jane Dennis - received payment from the Institute of Mental Health at the University of Nottingham, which had received a grant from the National Institute for Health Research for the completion of this review (amongst others).

Conor Duggan - in October 2011, I began a part-time appointment as Head of Research and Development for Partnerships in Care, an independent secure hospital provider.

Melanie Powney - none known. At the time of the review, I was affiliated with the Forensic Division of Nottinghamshire Healthcare NHS Trust.

SOURCES OF SUPPORT

Internal sources

- Nottinghamshire Healthcare Trust, UK.

External sources

- NHS Cochrane Collaboration Programme Grant Scheme, UK.

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

- Author byline has changed. Nadja Smalagic and Charlotte Bilby are no longer authors; Jane Dennis and Melanie Powney are on the author byline now.

- Under 'Types of interventions' we added the sentence "We will include studies of psychological interventions where medication is given as an adjunctive intervention." It was omitted from the protocol in error.

- The review includes four additional 'proxy' outcomes not specified in the original protocol. These were: cognitive distortions (measured in two trials, Anderson-Varney 1991 and Ryan 1997); sexual obsessions (measured in Ryan 1997); anxiety (measured in McAnaney 1981; McConaghy 1985; McConaghy 1988; Anderson-Varney 1991; Hopkins 1991 and Rooth 1974), and finally anomalous urges (measured in McConaghy 1985; McConaghy 1988 and Brown 1996). This was done because it was decided these admittedly proxy outcomes best assessed inappropriate arousal and stress and had a sound evidence base in terms of being 'dynamic risk factors' for recidivism.

- More authors contributed to the scanning and selection of studies than were initially described in the protocol (Bilby 2008).

- Methods for dealing with 'Unit of Analysis' and 'Multiple treatment arms', absent from the protocol (Bilby 2008), were added.

- Inclusion of rearrest data as a measure of the primary outcome.

NOTES

This review updates the out-of-date review with the same title withdrawn at Issue 4, 2008 (Kenworthy 2003). It is based on the updated protocol published in the same issue (Bilby 2008).

INDEX TERMS

Medical Subject Headings (MeSH)

Behavior Therapy [methods]; Criminals [*psychology]; Psychoanalytic Therapy [methods]; Psychotherapy [*methods]; Randomized Controlled Trials as Topic; Risk; Sex Offenses [*psychology]; Watchful Waiting

MeSH check words

Adolescent; Adult; Aged; Humans; Male; Middle Aged; Young Adult