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Review

Does specialized psychological treatment for offending reduce recidivism? A meta-analysis examining staff and program variables as predictors of treatment effectiveness

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HIGHLIGHTS

- This meta-analysis examined psychological offense treatment and recidivism.
- Overall, 70 studies were identified; including over 55,000 individuals.
- Treatment was associated with offense-specific and general recidivism reductions.
- Programs with consistent input from a qualified psychologist had best results.

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ABSTRACT

A meta-analysis was conducted to examine whether specialized psychological offense treatments were associated with reductions in offense specific and non-offense specific recidivism. Staff and treatment program moderators were also explored. The review examined 70 studies and 55,604 individuals who had offended. Three specialized treatments were examined: sexual offense, domestic violence, and general violence programs. Across all programs, offense specific recidivism was 13.4% for treated individuals and 19.4% for untreated comparisons over an average follow up of 66.1 months. Relative reductions in offense specific recidivism were 32.6% for sexual offense programs, 36.0% for domestic violence programs, and 24.3% for general violence programs. All programs were also associated with significant reductions in non-offense specific recidivism. Overall, treatment effectiveness appeared improved when programs received consistent hands-on input from a qualified registered psychologist and facilitating staff were provided with clinical supervision. Numerous program variables appeared important for optimizing the effectiveness of specialized psychological offense programs (e.g., arousal reconditioning for sexual offense programs, treatment approach for domestic violence programs). The findings show that such treatments are associated with robust reductions in offense specific and non-offense specific recidivism. We urge treatment providers to pay particular attention to staffing and program implementation variables for optimal recidivism reductions.

1. Introduction

The overarching aim of offense specific (i.e., specialized) psychological treatments for individuals who have offended is to reduce recidivism. Knowing whether such treatments result in meaningful recidivism reduction is crucial for informing future rehabilitative policy. Sexual offense and domestic violence programs comprise the lion's share of specialized psychological programs offered in correctional and community settings, although some programs have emerged targeting general non-familial violence (Cortoni, Nunes, & Latendresse, 2006; Polaschek, 2006). To date, meta-analyses and reviews have been conducted separately to examine sexual offense and domestic violence programs. Evaluations of general violence programs have tended to either group these in with sexual and domestic violence programs (Dowden & Andrews, 2000) or focus broadly on violent offenders but not violence specific programs per se (Jolliffe & Farrington, 2007). As such, no review has yet synthesized all specialized treatments across these three violent offending groups.

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2. Sexual offending

Meta-analyses examining sexual offense programs appear to indicate some level of treatment effectiveness (Alexander, 1999; Gallagher, Wilson, Hirschfield, Coggeshall, & MacKenzie, 1999; Hall, 1995; Hanson et al., 2002; Lösel & Schmucker, 2005; Schmucker & Lösel, 2015). The three most comprehensive meta-analyses to date are the best illustrations. Hanson et al. (2002) examined 43 evaluations of specialized and non-specialized[1] psychological treatment for adults and adolescents who had sexually offended ($N = 9454$) and found significant unweighted average reductions for sexual recidivism (12.3% treated vs. 16.8% untreated) and any general recidivism (27.9% treated vs. 39.2% untreated). Although few program variables were examined, Hanson et al. found that specialized treatments produced the best effects. Significant treatment effects were comparable across institutions and community settings.

Lösel and Schmucker (2005) examined 69 treatment evaluations for individuals who had sexually offended ($N = 22,181$)—incorporating biological (e.g., castration) and psychological treatments as well as adult and adolescent clients—and found significant *n*-weighted relative reductions for sexual (11.1% treated vs. 17.5% untreated), violent (6.6% treated vs. 11.8% untreated), and any general recidivism (22.4% treated vs. 32.5% untreated). Biological treatments (vs. psychological) produced the strongest treatment effects, as did treatments specifically targeting sexual offenses. Of the psychological treatments, only CBT and behavioral approaches were effective. Quality of evaluation design did not moderate the results, although studies with smaller samples produced stronger overall effects. Schmucker and Lösel (2015) later updated this meta-analysis, restricting the inclusion criteria to only the highest quality research designs (i.e., studies of at least quasi-experimental design with between-group equality; 27 studies, $N = 10,387$). This time, biological treatments did not meet inclusion criteria, and *n*-weighted treatment effects for recidivism, although significant, were notably smaller (sexual recidivism, 10.1% treated vs. 13.7% untreated; general recidivism, 32.6% treated vs. 41.2% untreated). In addition, only community programs (but not prison programs) significantly reduced sexual recidivism. Specialized psychological treatment targeting sexual offenses and treatment for adolescents also produced stronger effects, as did treatment that was individualized (rather than purely group based). Schmucker and Lösel's study represents the latest authoritative meta-analysis on psychological treatment for individuals who have sexually offended.

One large scale single study evaluation ($N = 15,781$) published by Mews, Di Bella, and Purver (2017) for the UK Ministry of Justice examined the "Core" sexual offense treatment program delivered to men across prisons in England and Wales from 2000 to 2012. Mews et al. (2017) propensity matched 87 variables to promote equivalence between the treated ($N = 2562$) and untreated groups ($N = 13,219$) and found that sexual recidivism for treated individuals *increased* by an absolute value of 2% and a relative value of 25% (10% treated vs. 8% untreated) over a mean 8.2-year follow-up. The sheer scale and apparent rigor of this individual study has cast significant international doubt on whether individuals who have sexually offended can be rehabilitated using specialized psychological programs (see Forde, 2017). This is despite the fact that Mews et al.'s findings have not yet been incorporated into a meta-analysis.

3. Domestic violence

Several reviews and meta-analyses have been published that focus on treatment for domestic violence, each generating largely equivocal findings (Babcock, Green, & Robie, 2004; Davis & Taylor, 1999; Eckhardt et al., 2013; Feder & Wilson, 2005; Sartin, Hansen, & Huss, 2006; Smedslund, Dalsbø, Steiro, Winsvold, & Clench-Aas, 2007). In the first meta-analysis, Babcock et al. (2004) reported a "small" treatment effect ($d = 0.18$) for studies using police reports as the recidivism outcome. However, they did not publish comparative weighted or unweighted reoffending rates and their study was not limited to specialized psychological treatment. A limited number of moderators were examined showing that, although results did not vary according to treatment approach (i.e., Duluth vs. CBT), experimental designs were associated with a slight reduction in treatment effects. This meta-analysis was relatively large ($k = 22$) but many comparison groups included treatment dropouts who hold unique risk characteristics that impact recidivism (Hanson et al., 2002; Olver, Stockdale, & Wormith, 2011). Two later published meta-analyses have been unable to establish treatment effectiveness for specialized domestic violence programs (Feder & Wilson, 2005; Smedslund et al., 2007). Feder and Wilson (2005) limited their meta-analysis to court-mandated treatment programs in North America ($k = 10$) and found a significant reduction in domestic violence recidivism for studies using some type of randomization, but no effects for those conducted without randomization. Smedslund et al. (2007) focused their meta-analysis solely on treatments using CBT elements and randomized controlled designs. In this small meta-analysis of North American studies ($k = 4$), Smedslund et al. concluded that findings were "inconsistent and heterogeneous" (p. 12). Given the difficulty researchers have had examining domestic violence program effectiveness, it is unsurprising that potential program and staffing moderators have not yet received attention. Further, no meta-analysis has examined how specialized domestic violence programs might impact recidivism more generally.

4. Staff and program variables

Researchers have typically focused on research design as a key factor hindering knowledge proliferation regarding treatment effectiveness (Beech, Freemantle, Power, & Fisher, 2015; Dennis et al., 2012; Hanson, Bourgon, Helmus, & Hodgson, 2009; Seto et al., 2008; Walton & Chou, 2015). However, variables relating to the program and its implementation are also important (Dowden & Andrews, 2004; Hoberman, 2016).

Correctional policy makers experience huge pressures to provide effective specialized offense treatments on a large scale at low cost. This has resulted in a growing reliance on paraprofessionals—rather than qualified psychologists—to implement treatment (Forde, 2017; Gannon & Ward, 2014). Gannon and Ward (2014) hypothesized that programs facilitated by qualified psychologists should produce optimal outcomes. Their predictions centered on the premise that fully trained psychologists hold the level of expertise and associated clinical competencies necessary to expertly detect and respond to complex client need. Problems with treatment delivery may well have underpinned the disappointing results from the British Ministry of Justice sexual offense program evaluation (Bullock, Bunce, & Dodds, 2017; Mews et al., 2017), since fully qualified psychologists were rarely involved in hands-on treatment. Yet, to our knowledge, this variable remains untested. Other staff variables such as the provision of facilitator clinical supervision (Bullock et al., 2017) may also impact upon treatment effectiveness and, as a corollary to Gannon and Ward's predictions, whether or not supervising staff hold psychological expertise. However, again, these variables have not yet been formally tested.

Regarding program variables, meta-analyses show that adherence to the Risk, Need, and Responsivity (RNR) principles of correctional treatment (Andrews & Bonta, 2006, 2010a) reduce many types of recidivism (Andrews & Bonta, 2010b; Babcock et al., 2004; Dowden & Andrews, 2000; Hanson et al., 2009). For psychological approaches, CBT appears to generate optimal recidivism reductions (Hanson et al., 2002; Lösel & Schmucker, 2005; Schmucker & Lösel, 2008) with the seeming exception of domestic violence programs (Babcock et al., 2004;

[1] These are non-offense specific psychological treatments such as cognitive skills programs.

Smedslund et al., 2007). Other program variables—except for a small selection investigated in sexual offending (Lösel & Schmucker, 2005; Schmucker & Lösel, 2015)—have received less attention.

5. Meta-analysis need and open science framework preplanned hypotheses

Previous meta-analyses examining offense programs have focused on one single offense type and have often examined a mixture of specialized and non-specialized treatments. No previous work has synthesized specialized psychological offense treatments to examine their impact on both offense specific and non-offense specific recidivism. Our predefined hypotheses are publicly available via the Open Science Framework repository (https://osf.io/euv7t/). We predict that individuals treated with a specialized psychological offense program (vs. comparison untreated individuals) will show reduced offense specific and non-offense specific recidivism. Based on the extant literature, we expect the largest recidivism effects to be associated with sexual offense (vs. domestic violence) programs. Previous meta-analyses have not examined the impact of staff variables—in particular qualified psychological input—as a moderator of recidivism outcomes. We examine this and predict that specialized psychological offense treatment facilitated by psychologists (vs. non-psychologists) will be associated with greater reductions in both offense specific and non-offense specific recidivism. In addition to these key hypotheses, we explore the effects of demographic variables, data source variables, treatment staff, and treatment program variables on both offense specific and non-offense specific recidivism.

6. Method

We report our method in line with the Meta-Analysis Reporting Standards (MARS), PRISMA (Moher, Liberati, Tetzlaff, & Altman, 2009), and with our publicly available Open Science Framework study plan.

6.1. Study selection

We did not time limit publication or study completion dates when undertaking searches. However, we did limit searches to articles published in English. We electronically searched PsychINFO®, Web of Science™, ProQuest®, MEDLINE, Dissertation Abstracts International, the Cochrane Controlled Trials Register, the National Criminal Justice Reference Service, the UK Ministry of Justice, UK Home Office, Canada Correctional Services, New Zealand Correctional Services, the UK National Archives, and the National Police Library (UK). All keyword combinations used in our searches are available in our Open Science Framework study plan. We searched publication reference lists and sent requests to three international Listservs (Association for the Treatment of Sexual Abusers, Gender-based Research Network, National Organisation for the Treatment of Abusers) and one national Listserv (Division of Forensic Psychology Trainees, UK). We also sent individual e-mails to key researchers identified in our search strategy asking them to identify unpublished data. We concluded the search process on 1 February 2018; approximately 12 months following our first computerized search.

For inclusion, studies needed to (1) evaluate an offense specific (i.e., specialized) psychological treatment provided to adjudicated offenders, (2) examine recidivism as an outcome variable, (3) include a comparison group of adjudicated offenders who did not receive the specialized treatment in question (or comparable treatment)—and for whom recidivism was also examined, and (4) provide descriptive or inferential statistics adequate for effect size calculation. We excluded studies focusing on clients under 18 years since these clients have been associated with strongest treatment effects (Schmucker & Lösel, 2015), clients with learning disability or other cognitive impairment, or those committed to a mental health facility due to a significant mental disorder.[2] We also excluded drink driving treatment evaluations since these programs are less usual within clinical-forensic settings. Where multiple studies described the same treatment outcome data or programme, the manuscript outlining the highest quality data and typically the largest and most representative sample was used for analysis.

6.2. Variables

We coded 27 predictor and outcome variables using over 80 categories. Variables were informed by previous offending behavior meta-analyses and research literature gaps. Key variable descriptions are provided below. For each variable, an *unknown* category was used to incorporate information that could not be classified using preexisting categories.

6.3. Predictors

6.3.1. Demographic variables

Age (closest available to time of institutional release); race; gender; offense type; and sample size *N* (treatment, comparison).

6.3.2. Data source variables

Year of publication or study completion; country of publication origin; type of publication (i.e., journal, government report, book chapter, thesis, presentation, unpublished).

6.3.3. Treatment program variables

Facility setting (prison, community, special facility); therapeutic community (yes, no); primary treatment method used (CBT, Duluth, psychoeducation, behavioral, mixed); type of offense targeted in treatment (sexual, general violence, family violence); mode of treatment provision (group, individual, mixed); treatment format (closed, rolling); treatment length (hours); treatment site roll out (single site, multiple sites); polygraph usage (yes, no); treatment quality (Most promising [uses RNR or evidence based practice], Promising [uses some RNR or evidence based practice], Weaker [does not use RNR or evidence based practice]). For programs targeting sexual offending we also examined whether behavioral conditioning procedures had been used in an attempt to recondition inappropriate sexual arousal (yes, no).

6.3.4. Treatment staff variables[3]

Presence of registered autonomous postgraduate psychologist in hands-on program provision (consistently present [i.e., always], inconsistently present [i.e., usually/sometimes present], or never present); facilitator supervision (yes, no); profession of individual(s) providing facilitator supervision (registered autonomous postgraduate psychologist, non-psychologist, or mixed).

6.4. Outcomes

6.4.1. Recidivism variables

Recidivism source (conviction, arrests or charges, institutional records, unofficial reports, self-report[4]); recidivism type (sexual, domestic violence, and any violence or any general recidivism); recidivism follow

[2] We did not exclude individuals relocated or committed to mental health facilities specifically to receive treatment for their offending or offense relevant disorder (e.g., pedophilia).

[3] As outlined in our preregistration document, we also attempted to collect information on other key program and staff variables (e.g., number and type of staff facilitating treatment). However, we were unable to populate these variables sufficiently for analysis and so we do not describe them.

[4] If a paper reported multiple recidivism types then we always took the conviction data since this offered us the highest level of assurance that a new offense had been committed.

up time (months); and recidivism/non-recidivism sample size *ns* (treatment, comparison).

6.4.2. Study quality variables

Matching of the control and treatment participants (yes, no); study design (randomized or not); and recidivism quality score[5] (1 = very low quality [poor data source such as self-report *and* inadequate follow up time of one or less years], 2 = low quality [uses either a poor data source such as self-report *or* inadequate follow up time of one or less years but not both], 3 = moderate quality [uses either a moderate data source such as arrests or charges *or* adequate follow up time of more than 1 year], 4 = high quality [uses a moderate data source such as arrests or charges *and* adequate follow up time of more than 1 year], 5 = very high quality [uses a high quality data source such as national conviction data *and* three or more years follow up]).

6.5. Study coding protocol and procedure

A coding protocol incorporating all variables described above was used to code each individual study. Studies were independently double coded and cross-checked by Theresa A. Gannon and Jaimee S. Mallion. Discrepancies stemmed from minor coding oversights and were resolved easily through discussion. When information was missing for key predictor and outcome categories, Theresa A. Gannon used electronic mail to make contact with either the corresponding manuscript author or, if that contact was unsuccessful, another co-author. At least two reminder emails were sent and when contact was unsuccessful, a follow up phone call was made. We attempted to contact the study author of all but three articles[6] and obtained a response rate of 79% (n = 53). Responding authors were not always able to provide all information requested due to job changes or significant time lapses. Categories were purposefully merged with other categories when they were underused prior to hypothesis testing. The final coding protocol is available, upon request, from the first author.

6.6. Effect size calculations

Odds Ratios (ORs) were computed for the treatment and comparison groups, comparing the ratio of recidivists to non-recidivists for each offense specific and non-offense specific recidivism type (i.e., sexual recidivism, domestic violence recidivism, general violence [combined sexual and nonsexual], or any general recidivism [all recidivism, violent and nonviolent, as a single outcome variable]). ORs were computed so that values below 1.0 indicated lower rates of recidivism for treatment, above 1.0 indicated higher rates of recidivism for treatment, and 1.0 indicated zero effect. We did not include studies that contained treatment drop-outs in the comparison group due to the higher recidivism rates associated with this group (see Lösel & Schmucker, 2005; Olver et al., 2011). Instead, we included all participants originally assigned to receive the offense specific treatment in the treatment group wherever possible (i.e., intent to treat analysis). This is likely to represent a more conservative test of the effects of specialized psychological offense treatment. All effect size calculations were electronically calculated by Mark E. Olver and seven studies (10%) were randomly selected and hand recalculated by Mark James. Overall, there was 100% agreement across the 13 effect sizes.

6.7. Effect size aggregation and analyses

ORs were aggregated to generate overall effect sizes with 95% confidence intervals with both fixed and random effects models using Comprehensive Meta-Analysis 3.0. A minimum of k = 3 studies was required to compute a meaningful effect size. Effect size heterogeneity across studies was examined using the Q test with associated p value (Cochran, 1954) and I^2 statistic (Higgins, Thompson, Deeks, & Altman, 2003). Analyses were conducted including outliers (defined as extreme values that contribute approximately 50% or more of the variability in effect size heterogeneity) and with outliers removed. Moderator variables were examined through aggregating effect sizes at different levels within moderators and examining the difference in effect size magnitude for a given moderator to ascertain the effects of these variables on recidivism outcomes. Publication bias was examined for each moderator variable that met the criteria for asymmetry testing proposed by Ioannidis and Trikalinos (2007). Three sets of asymmetry testing were conducted: funnel plots of precision, trim and fill (Duval & Tweedie, 2000), and fail-safe N (Rosenthal, 1979).

7. Results

As Fig. 1 shows, our searches initially identified 6633 articles of which 68 articles describing 70 studies met the full inclusion criteria. These studies described the recidivism of 55,604 offenders (22,321 treated, 33,283 comparison) from 70 independent samples. Studies originated from 39 peer reviewed journal articles, 6 theses/dissertations, 2 poster/presentations, 19 government reports, 1 book chapter, and 3 unpublished materials. Most studies had been published since 2000 (k = 43), with some published in the 1990s (k = 22) and 1980s (k = 5). Overall, studies were judged to be of reasonable quality with 77.1% (k = 54) holding a recidivism quality score of high or very high. Only six studies used a randomized design (five examining domestic violence), and of the remaining studies just under one third (k = 20) used an appropriately matched treatment and comparison group (13 examining sexual offense programs, 4 domestic violence programs, and 3 general violence programs). Key variables are shown in Table 1. Open access data is available from http://dx.doi.org/10.17632/mvdw7xd9rb.2

7.1. Offense specific recidivism

Across all program types (i.e., sexual, domestic violence, or general violence; k = 62[7]), using an average follow up of 66.1 months, offense specific recidivism was significantly lower for individuals who received specialized treatment relative to those who had not (13.4% [SD = 10.6] vs. 19.4% [SD = 14.4] respectively; unweighted means) in both the random (OR = 0.65, 95% CI = 0.57, 0.76) and fixed effect models (OR = 0.72, 95% CI = 0.68, 0.76). This represents an absolute decrease in recidivism of 6% and a relative decrease of 30.9%.

7.2. Sexual offending

Table 2 shows meta-analysis results for sexual recidivism. Readers should note that Mews et al. (2017) was identified as an outlier for the bulk of analyses, featuring an extremely large sample size. For this reason, we report all findings with this study removed and included. Readers should also note that random effects models are less influenced by outliers than fixed effects models which weight effect sizes strictly by sample size; as such, random effects models were less impacted by inclusion of Mews et al.

Sexual offense programs (k = 44) generated a stable and significant treatment effect regardless of whether random (OR = 0.64, 95% CI = 0.53, 0.76) or fixed effects (OR = 0.65, 95% CI = 0.59, 0.72) models were used. Similar to previous meta-analyses, significant

[5] Adapted from Hanson and Bussière (1998).

[6] In these cases, it was clear that contact would not produce the information required (e.g., the author specified in the article that this information was unobtainable).

[7] Overall k does not equal 70 because not all studies examined offense specific recidivism.

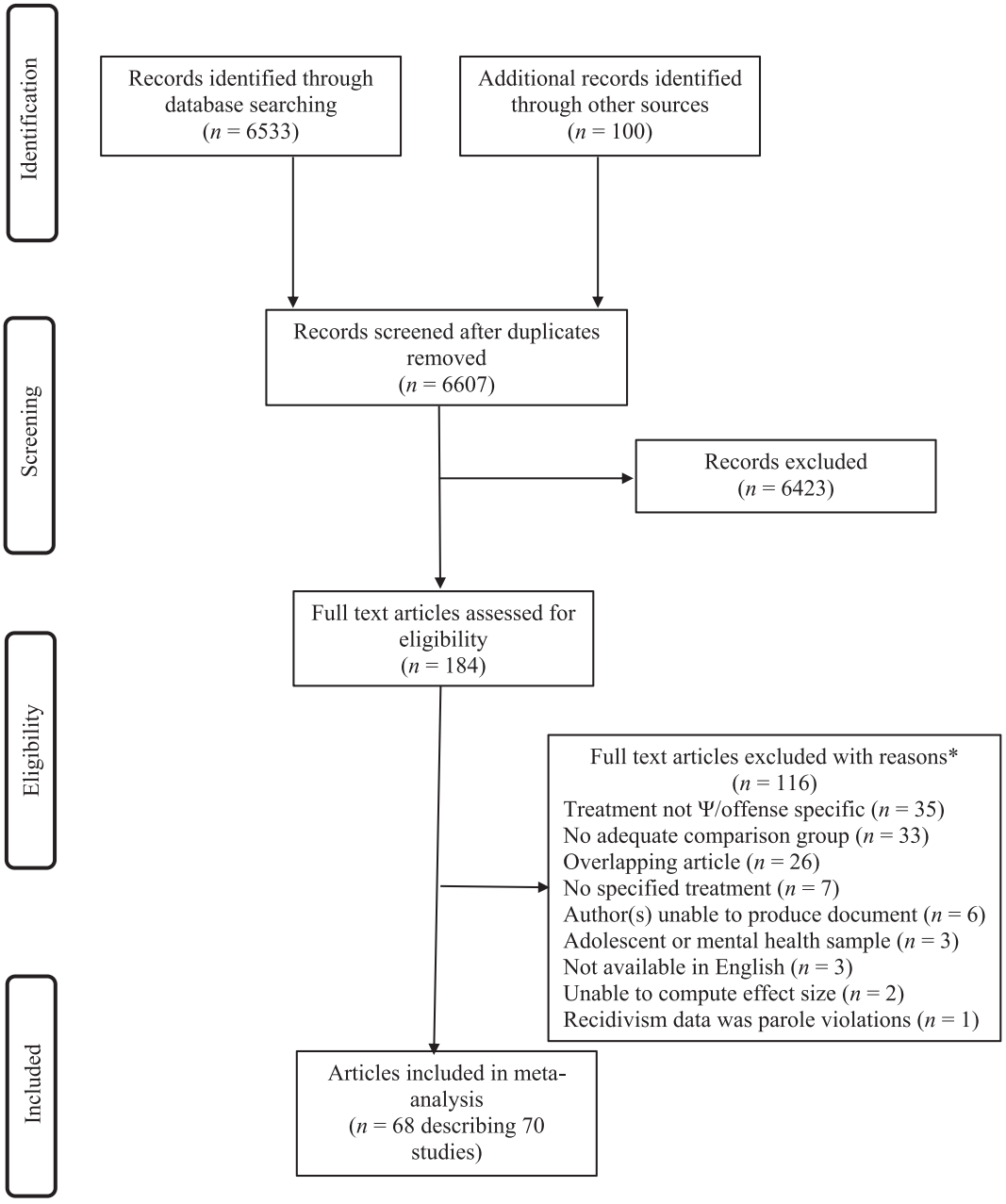

Fig. 1. PRISMA Group (2009) flow diagram of article selection. Ψ = psychological. *Exclusions of full text articles occurred for multiple reasons and so only the primary reason is documented here.

heterogeneity was present across studies ($Qs = 118.75$, $p < .001$). Over an average follow up time of 76.2 months (SD = 34.2), sexual recidivism was 9.5% for treated and 14.1% for untreated individuals (unweighted means). This represents an absolute decrease in recidivism of 4.6% and a relative decrease of 32.6%. While the Mews et al. (2017) evaluation had a limited effect on the random effects model, it impacted the fixed effect model, which maintained significant, but smaller, associations with decreased sexual recidivism. We limit our moderator commentary below to key findings.

7.2.1. Staff moderators

Treatment was most effective in reducing sexual recidivism when a qualified licensed psychologist was consistently present in treatment (vs. inconsistently present, unknown, or not present at all). This effect remained when Mews et al. (2017) was included. Receiving supervision from other staff when facilitating treatment also led to better reductions in sexual redivism relative to supervision not being provided or its provision being unknown. This effect remained when Mews et al. was included in the random effects model but reduced in the fixed effects model. Supervision provided by psychologists held the best associations with reduced sexual recidivism. A *k* of 1 for non-psychologist provision made it impossible to draw adequate comparisons. However, provision by both psychologists and non psychologists appeared less effective (random effects model) or not effective (fixed effects model).

7.2.2. Treatment program moderators

All sexual offense treatment was CBT. There were larger reductions in sexual recidivism when treatment service quality was rated as promising or most promising relative to weaker services. The fixed effect for most promising programs (OR = 1.09) was driven by the single large sample study of Mews et al. (2017). The association between program intensity and outcome was not uniform, with treatment effects

Table 1
Summary table of demographic and descriptive variables for treatment outcome studies.

Variable	*k*	*n* or *M* (SD)
Age (years)	51	35.1 (4.7)
Racial ancestry	40	
White		10,950
Black		2863
Indigenous		2323
Hispanic		707
Asian		92
Other		1600
Unknown		111
Program focus		
Sexual offense	47	41,476
Domestic violence	19	12,900
Violent offending	4	1228
Setting		
Prison	27	
Special facility (e.g., hospital)	7	
Community	36	
Treatment approach		
CBT	50	
Duluth	6	
Psychoeducational	5	
Behavioral	2	
Unknown	7	
Modality		
Group	39	
Mixed	21	
Individual	1	
Unknown	9	
Program length (hours)	51	170.2 (171.5)
Treatment service quality		
Weaker	11	
Promising	23	
Most promising	14	
Unknown	22	
Psychologist present		
Inconsistent	28	
Consistent	12	
None/Unknown	30	
Supervision provided		
No	2	
Yes	36	
Unknown	32	
Supervision provider		
Psychologist	22	
Non-psychologist	3	
Psychologists and non-psychologists	8	
Unknown	37	
Staff delivery		
Individually facilitated	11	
Co-facilitated	28	
Mixed	1	
Unknown	30	
Matched control group		
Yes	21	
No	49	
Randomized design	6	
Recidivism quality score		
Very high quality	21	
High quality	33	
Moderate quality	12	
Low quality	2	
Very low quality	1	
Unknown	1	
Publication source		
Journal article	39	
Government report	19	
Theses/dissertation	6	
Unpublished materials	3	
Poster/presentation	2	
Book chapter	1	
Country		
USA	32	
Canada	17	
UK	8	
New Zealand	6	
Australia	4	
Israel	1	
Netherlands	1	
Taiwan	1	
Follow-up time (months)	30	67.6 (36.0)

generally observed across programs of various lengths, although 100–200 h programs (with and without Mews et al.) generated smaller effects. Treatment across institutions and the community produced comparable sexual recidivism reductions. When Mews et al. was included within institutional settings, however, community programs generated comparably larger effects.

Group-based treatment, rather than mixed group and individual treament, produced the greatest reductions in sexual recidivism except, again, when Mews et al. (2017) was adjusted for in the fixed effects model. Relatively larger treatment effects were observed for programs that incorporated some form of arousal reconditioning (vs. none or unknown). Programs that incorporated polygraph use (vs. those that did not or its use was unknown) produced less convincing recidivism reductions; the fixed effects model for polygraph absent programs (OR = 0.96) was driven by Mews et al. Finally, programs provided in New Zealand or Australia and Canada produced substantial reductions in sexual recidivism relative to other countries. One in four of these programs was characterized by consistent psychologist input.

7.2.3. Study quality moderators

With the exception of studies rated fair-moderate ($k < 3$) studies rated as high or very high on recidivism quality were associated with robust recidivism reductions (OR range 0.61–0.66). The fixed effects model with Mews et al. (2017) included was the only exception. Studies that employed matching criteria produced less superior, yet significant, reductions in sexual recidivism. Again, the addition of Mews et al. in the fixed effects model was the only exception.

7.3. Domestic violence

Domestic violence programs ($k = 14$) generated a significant treatment effect regardless of whether random (OR = 0.65, 95% CI = 0.44, 0.97) or fixed effects (OR = 0.61, 95% CI = 0.56, 0.68) models were used, with significant heterogeneity across studies ($Qs = 72.84$, $p < .001$). Over an average 62-month follow-up, domestic violence recidivism was 15.5% (SD = 8.4) for individuals who received treatment and 24.2% (SD = 16.0) for untreated comparisons (unweighted means). This represents an absolute decrease in recidivism of 8.7% and a relative decrease of 36.0%.

7.3.1. Staff moderators

As shown in Table 3, *k*s were < 3 for many staff variables. Similar to sexual offense programs, however, domestic violence treatment appeared most effective when a qualified psychologist was consistently present (vs. inconsistently present, unknown, or not present at all). The exception was the fixed effects model for consistant psychologist presence driven by a single large sample study (Dutton, Bodnarchuk, Kropp, Hart, & Ogloff, 1997). Receiving supervision from other staff when facilitating treatment for domestic violence perpetrators also appeared important in reducing domestic violence recidivism (vs. supervision not being provided or its provision unknown). The relative effects of various professions providing supervision was unclear, however, due to the large number of studies for which supervisor profession remained unknown.

Table 2
Sexual offense specific programs: associations with reductions in sexual recidivism.

Moderator	Random		Fixed		Q	I^2	n	k
	OR	95% CI	OR	95% CI				
Overall	0.64	0.53, 0.76	0.65	0.59, 0.72	118.75***	64.63	25,521	43
With outlier	0.66	0.54, 0.80	0.84	0.77, 0.91	203.74***	78.90	41,291	44
Psychologist present								
Inconsistent	0.71	0.55, 0.90	0.72	0.64, 0.82	62.56***	69.63	12,996	20
With outlier	0.74	0.57, 0.97	0.97	0.88, 1.06	117.78***	83.02	28,766	21
Consistent	0.43	0.23, 0.81	0.42	0.32, 0.55	26.53***	77.38	2875	7
None or unknown	0.64	0.52, 0.78	0.63	0.52, 0.77	16.14	7.03	9650	16
Supervision provided								
Yes	0.56	0.43, 0.73	0.61	0.54, 0.68	82.38***	74.51	14,011	22
With outlier	0.59	0.44, 0.79	0.87	0.79, 0.95	168.22***	86.92	29,781	23
None or unknown	0.74	0.59, 0.93	0.74	0.63, 0.87	32.72*	38.88	11,510	21
Supervision provider								
Psychologist	0.54	0.40, 0.73	0.52	0.44, 0.60	55.35***	71.09	10,486	17
Non-psychologist	0.28	0.07, 1.07	0.28	0.07, 1.07	0.00	0.00	173	1
Psychologist and non-psychologist	0.80	0.46, 1.42	1.17	1.04, 1.21	40.41***	90.10	18,989	5
Unknown	0.81	0.65, 1.02	0.83	0.71, 0.96	29.13*	45.08	10,800	17
Staff delivery								
Individually facilitated	0.56	0.35, 0.91	0.58	0.48, 0.71	42.91***	81.36	4554	9
Co-facilitated	0.54	0.37, 0.77	0.63	0.51, 0.77	30.88***	64.38	6022	12
With outlier	0.59	0.38, 0.89	1.10	0.98, 1.23	77.75***	84.57	21,792	13
Unknown	0.73	0.59, 0.91	0.70	0.61, 0.80	42.42**	50.49	14,945	22
Service quality								
Weaker	0.76	0.56, 1.04	0.73	0.60, 0.89	5.91	32.26	5612	5
Promising	0.56	0.40, 0.79	0.64	0.53, 0.77	39.37***	64.44	5935	15
Most promising	0.57	0.35, 0.93	0.54	0.43, 0.67	31.62***	77.86	10,501	8
With outlier	0.66	0.38, 1.14	1.09	0.96, 1.23	88.81***	90.99	26,271	9
Unknown	0.72	0.53, 0.99	0.69	0.58, 0.83	37.12***	62.28	10,025	15
Program intensity								
100 h	0.45	0.22, 0.93	0.68	0.49, 0.94	15.57**	67.88	1471	6
100–200 h	0.75	0.48, 1.19	0.80	0.66, 0.98	38.17***	79.04	6348	9
With outlier	0.82	0.54, 1.24	1.19	1.06, 1.34	62.38***	85.57	22,118	10
200–300 h	0.41	0.24, 0.71	0.37	0.26, 0.54	5.78	48.09	1158	4
300+ hours	0.54	0.35, 0.83	0.57	0.48, 0.68	23.00***	73.91	4954	7
Therapeutic community								
No	0.69	0.54, 0.89	0.71	0.62, 0.82	52.32***	57.95	11,254	23
With outlier	0.73	0.55, 0.96	1.03	0.93, 1.14	100.93***	77.21	27,024	24
Yes	0.57	0.33, 0.98	0.67	0.56, 0.80	25.84***	84.52	4322	5
Unknown	0.54	0.41, 0.71	0.52	0.43, 0.63	25.34*	48.70	9679	14
Setting								
Institution	0.67	0.52, 0.85	0.65	0.58, 0.73	87.44***	72.55	14,224	25
With outlier	0.70	0.54, 0.92	0.89	0.82, 0.98	163.55***	84.71	29,995	26
Community	0.61	0.47, 0.79	0.66	0.56, 0.79	31.26*	45.61	11,296	18
Modality								
Group	0.47	0.34, 0.66	0.47	0.40, 0.56	47.03***	70.23	8826	15
With outlier	0.51	0.33, 0.79	0.93	0.83, 1.04	143.96***	89.58	24,596	16
Mixed	0.79	0.62, 1.02	0.78	0.69, 0.89	44.11***	61.46	8602	18
Unknown	0.66	0.52, 0.83	0.66	0.52, 0.83	6.98	0.00	7961	9
Program format								
Rolling group	0.54	0.35, 0.86	0.66	0.56, 0.78	33.47***	79.08	4711	8
Closed group	0.59	0.40, 0.85	0.59	0.48, 0.72	36.40***	69.78	7257	12
With outlier	0.64	0.42, 0.99	1.07	0.95, 1.20	91.32***	86.86	23,027	13
Unknown	0.67	0.54, 0.85	0.66	0.57, 0.77	39.47**	49.33	12,953	21
Program roll out								
Single site	0.60	0.45, 0.81	0.58	0.50, 0.67	85.32***	73.04	8787	24
Multiple site	0.66	0.50, 0.87	0.67	0.56, 0.81	5.86	31.75	6209	5
With outlier	0.77	0.48, 1.23	1.10	0.98, 1.24	48.32***	89.65	21,979	6
Unknown	0.73	0.57, 0.93	0.76	0.64, 0.90	22.18	41.40	10,525	14
Arousal conditioning								
No	0.73	0.37, 1.46	0.82	0.55, 1.23	7.14	57.97	3063	4
With outlier	0.92	0.53, 1.59	1.39	1.21, 1.59	14.42**	72.27	18,833	5
Yes	0.57	0.44, 0.74	0.62	0.55, 0.69	89.42***	75.39	11,753	23
Unknown	0.73	0.59, 0.91	0.73	0.60, 0.88	18.76*	20.05	10,705	16
Polygraph								
No	0.61	0.46, 0.81	0.66	0.57, 0.75	82.11***	73.21	11,666	23
With outlier	0.64	0.47, 0.87	0.96	0.87, 1.06	145.86***	84.23	27,436	24
Yes	0.89	0.62, 1.29	0.77	0.64, 0.94	10.12	50.61	4200	6
Unknown	0.56	0.44, 0.72	0.55	0.46, 0.67	20.43	36.36	9655	14
Country of program								
United Kingdom	0.62	0.37, 1.04	0.68	0.45, 1.02	5.07	21.17	3304	5
With outlier	0.75	0.42, 1.35	1.36	1.19, 1.56	17.38**	71.23	19,074	6
United States	0.79	0.65, 0.96	0.78	0.69, 0.88	35.02*	42.89	15,173	21
Canada	0.50	0.33, 0.76	0.50	0.41, 0.60	36.62***	75.42	4359	10

(*continued on next page*)

Table 2 (*continued*)

Moderator	Random		Fixed		*Q*	I^2	*n*	*k*
	OR	95% CI	OR	95% CI				
New Zealand/Australia	0.39	0.27, 0.55	0.38	0.28, 0.51	6.52	23.34	2419	6
Other international	1.75	0.88, 3.46	1.75	0.88, 3.46	0.00	0.00	266	1
Recidivism quality								
Fair-moderate	1.54	0.71, 3.36	1.54	0.71, 3.36	0.20	0.00	293	2
High	0.61	0.48, 0.78	0.66	0.58, 0.76	66.77***	65.56	15,712	24
Very high	0.61	0.47, 0.80	0.62	0.54, 0.71	44.79***	66.51	9230	16
With outlier	0.66	0.47, 0.92	0.94	0.85, 1.04	116.85***	86.31	25,000	17
Matching employed								
No	0.59	0.48, 0.74	0.58	0.52, 0.66	82.33***	63.56	17,041	31
Yes	0.76	0.57, 1.02	0.78	0.67, 0.91	28.11**	60.87	8480	12
With outlier	0.82	0.59, 1.13	1.09	0.98, 1.21	63.80***	81.19	24,250	13
Year of study								
1980s	0.69	0.24, 2.03	0.60	0.32, 1.12	5.19	61.48	386	3
1990s	0.64	0.49, 0.83	0.64	0.52, 0.79	15.51	22.65	5532	13
2000s	0.62	0.47, 0.80	0.65	0.58, 0.74	64.31***	73.57	15,075	18
2010s	0.68	0.42, 1.10	0.68	0.54, 0.85	33.49***	76.12	4528	9
With outlier	0.75	0.47, 1.21	1.18	1.04, 1.33	65.80***	86.32	20,298	10

Note. CIs that do not include zero are statistically significant ($p < .05$). All programs were CBT. Effect sizes $n < 3$ should be interpreted cautiously.
*** p < .001, ** p < .01, and * p < .05.

7.3.2. Treatment program moderators

All domestic violence programs were provided in groups, mostly closed in format ($k = 9$), almost exclusively community based ($k = 13$), and of short duration (i.e., < 100 h; $k = 13$). In addition, none involved therapeutic communities; likely because treatment was largely community based. Interestingly, the association between program quality and domestic violence recidivism ran counter to that for sexual offense programs. The fixed effect for promising programs was driven by a single large sample study with a positive treatment effect (Bloomfield & Dixon, 2015). However, the random effects reduced the impact of this study on the overall effect. The so-called "weaker" programs, which tended to feature education based groups, generated strong treatment effects, accounting for large reductions in domestic violence recidivism (ORs 0.23 ^{random}, 0.28 ^{fixed}). CBT treatment methods did not produce convincing reductions in domestic violence recidivism. However, the Duluth model—which itself is a pro-feminist yet also CBT-based program—and psychoeducational models both produced robust reductions in domestic violence recidivism. Programs provided in one location, as opposed to multiple locations, were most effective in reducing domestic violence recidivism.

7.3.3. Study quality moderators

Variations on recidivism quality score were difficult to interpret due to small *k* in the poor and very high categories. However, studies rated moderate and high were associated with comparably robust reductions in domestic violence. The random effects OR for the high category was driven by Dutton et al. (1997). Only one study employed matching criteria making interpretation of this variable difficult. Since four studies employed a randomized design, however, we were able to examine ORs for studies with and without this feature. Both studies that employed randomization and studies that did not employ randomization were associated with robust reductions in domestic violence although randomization was associated with weaker ORs.

7.4. Violence

Programs targetting general violence comprised only a small subcategory of studies ($k = 4$) and so we could not examine staff or treatment program moderators. However, a stable and significant treatment effect was found regardless of whether random (OR = 0.60, 95% CI = 0.46, 0.79) or fixed effects (OR = 0.60, 95% CI = 0.46, 0.79) models were used with almost negligible study effect size heterogeneity ($Q = 1.74$, *ns*). Over an average follow up of 25.0 months (SD = 15.1), general violence recidivism was 29.0% for treated and 38.3% for untreated individuals (unweighted means; absolute decrease 9.3%; relative decrease 24.3%).

7.5. Non-offense specific recidivism

7.5.1. Any violent recidivism

We examined the overall ability of all specialized programs (i.e., sexual, domestic violence, or general violence) to reduce *any* form of violent recidivism, operationalized as a single outcome variable that included both sexual and nonsexual violence, where this information was available ($k = 33$; see Table 4). Programs produced a significant reduction in violence in the random (OR = 0.56, 95% CI = 0.46, 0.68) and fixed effects (OR = 0.75, 95% CI = 0.70, 0.79) models with significant heterogeneity ($Q = 186.95$, $p < .001$). Across programs, over an average follow up time of 65.4 months (SD = 35.3), general violence recidivism was 14.4% for treated and 21.6% for untreated individuals (unweighted means), corresponding to an absolute decrease in recidivism of 7.2% and relative decrease of 33.3%. When effects were disaggregated across each of the three program types, similar OR magnitudes were observed, with a little more variation observed for sexual offense programs.

7.5.1.1. Staff, treatment, and study quality moderators. Consistent with findings for offense specific recidivism, facilitator input from a qualified psychologist produced superior reductions in violence relative to inconsistent psychological facilitator input. It is unclear what produced the superior ORs noted for the none or unknown category. Reductions in general violence across programs did not appear to be substantively impacted by whether staff supervision was provided. However, when psychologists and non-psychologists provided supervision on the same program, treatment effectiveness diminished substantially. Treatment effects were found across the various levels of service quality although programs classified as most promising were associated with the best violence reductions, except when Mews et al. (2017) was entered in the fixed effects model. Treatment effects were also found across the various levels of treatment intensity although programs of lower intensity (< 100 h) appeared slightly less effective than higher intensity programs. Treatment that was group-based, rather than a mixture of group and individual modalities, produced the greatest reductions in violent recidivism, except when Mews et al. was entered into the fixed effects model. Programs administered at one treatment site also appeared slightly more effective than treatments

Table 3
Domestic violence programs: associations with reductions in domestic violence recidivism.

Moderator	Random		Fixed		Q	I^2	n	k
	OR	95% CI	OR	95% CI				
Overall	0.65	0.44, 0.97	0.61	0.56, 0.68	72.84***	82.15	9845	14
Psychologist present								
Inconsistent	0.58	0.52, 0.65	0.58	0.52, 0.65	0.49	0.00	6771	2
Consistent	0.27	0.02, 4.07	0.74	0.46, 1.21	11.31***	91.16	546	2
None or unknown	0.75	0.41, 1.39	0.75	0.60, 0.95	56.45***	84.06	2528	10
Supervision provided								
Yes	0.57	0.33, 0.997	0.58	0.53, 0.65	39.67***	87.39	8088	6
None or unknown	0.73	0.39, 1.37	0.94	0.70, 1.28	24.64***	71.60	1757	8
Supervision provider								
Non-psychologist	0.85	0.36, 1.99	0.60	0.53, 0.67	6.51*	84.63	6877	2
Psychologist and non-psychologist	0.39	0.13, 1.20	0.39	0.13, 1.20	0.00	0.00	76	1
Unknown	0.62	0.34, 1.12	0.70	0.56, 0.88	64.03***	84.38	2892	11
Staff delivery								
Individually facilitated	0.25	0.06, 1.07	0.25	0.06, 1.07	0.00	0.00	56	1
Co-facilitated	0.69	0.45, 1.07	0.62	0.55, 0.68	27.84**	78.45	8295	7
Mixed	0.13	0.07, 0.24	0.13	0.07, 0.24	0.00	0.00	339	1
Unknown	1.15	0.64, 2.07	1.23	0.82, 1.84	7.59	47.28	1155	5
Service quality								
Weaker	0.23	0.10, 0.52	0.28	0.20, 0.40	16.70**	76.04	947	5
Unspecified	1.13	0.80, 1.61	1.13	0.80, 1.61	4.01	0.29	1382	5
Promising	0.87	0.50, 1.50	0.61	0.55, 0.68	10.03**	80.05	7323	3
With outlier	1.13	0.59, 2.16	0.62	0.56, 0.69	21.39***	85.97	7516	4
Treatment model								
CBT	0.89	0.39, 2.04	1.09	0.77, 1.54	18.92***	78.86	1239	5
Duluth	0.52	0.28, 0.96	0.57	0.51, 0.63	30.80***	87.01	7833	5
Psychoeducational	0.58	0.25, 1.35	0.83	0.54, 1.28	8.71	65.54	773	4
Program intensity								
100 h	0.59	0.40, 0.87	0.60	0.55, 0.67	61.28***	80.42	9652	13
100–200 h	2.96	1.19, 7.35	2.96	1.19, 7.35	0.00	0.00	193	1
Setting								
Institution	1.40	0.72, 2.73	1.40	0.72, 2.73	0.00	0.00	182	1
Community	0.61	0.41, 0.93	0.60	0.54, 0.67	66.81***	82.04	9663	13
Program format								
Rolling group	0.58	0.52, 0.65	0.58	0.52, 0.65	0.00	0.00	6695	1
Closed group	0.52	0.27, 0.97	0.62	0.49, 0.79	51.91***	84.59	2282	9
Both	2.96	1.19, 7.35	2.96	1.19, 7.35	0.00	0.00	193	1
Unknown	0.81	0.33, 2.01	1.03	0.62, 1.71	4.59	56.42	775	3
Program roll out								
Single site	0.38	0.16, 0.90	0.52	0.39, 0.68	42.59***	88.26	1499	6
Multiple sites	0.84	0.44, 1.62	0.60	0.54, 0.67	7.87*	74.59	7314	3
Unknown	0.97	0.48, 1.96	1.13	0.77, 1.66	11.14*	64.10	1032	5
Country of program								
United Kingdom	0.58	0.52, 0.65	0.58	0.52, 0.65	0.20	0.00	6817	2
United States	0.71	0.35, 1.45	0.68	0.53, 0.87	51.13***	86.31	2125	8
Canada	0.28	0.05, 1.54	0.67	0.42, 1.06	13.24***	84.90	602	3
Other international	1.38	0.75, 2.56	1.38	0.75, 2.56	0.00	0.00	301	1
Recidivism quality								
Poor	1.38	0.75, 2.56	1.38	0.75, 2.56	0.00	0.00	301	1
Moderate	0.50	0.24, 1.05	0.57	0.44, 0.75	48.60	85.60	1933	8
High	0.72	0.31, 1.69	0.59	0.53, 0.66	13.55	77.86	7165	4
Very high	0.99	0.59, 1.66	0.99	0.59, 1.66	0.00	0.00	446	1
Matching employed								
No	0.64	0.38, 1.09	0.73	0.60, 0.90	69.03***	82.62	3150	13
Yes	0.58	0.52, 0.65	0.58	0.52, 0.65	0.00	0.00	6695	1
Randomized design								
No	0.63	0.37, 1.05	0.60	0.54, 0.67	62.28***	85.55	8675	10
Yes	0.73	0.37, 1.42	0.78	0.55, 1.10	8.55*	64.91	1170	4
Year of study								
1980s	0.19	0.03, 1.36	0.25	0.11, 0.59	4.66*	78.52	321	2
1990s	0.64	0.27, 1.55	0.66	0.49, 0.89	47.19***	87.29	1414	7
2000s	0.98	0.57, 1.67	0.95	0.70, 1.29	7.99*	62.46	1415	4
2010s	0.58	0.52, 0.65	0.58	0.52, 0.65	0.00	0.00	6695	1

Note. CIs that do not include zero are statistically significant ($p < .05$). All were group programs. Effect sizes $n < 3$ should be interpreted cautiously.
*** $p < .001$, ** $p < .01$, and * $p < .05$.

administered across multiple sites. For recidivism quality ratings, all categories were associated with robust recidivism reductions; however, ratings of very high quality, which included Mews et al. (2017), produced slightly weaker associations with violent recidivism. Similarly, whilst both matched and non-matched designs produced notable reductions in violence recidivism, the weakest associations were found for matched designs.

Table 4
All programs: associations with reductions in violent recidivism.

Moderator	Random		Fixed		Q	I^2	n	k
	OR	95% CI	OR	95% CI				
Overall	0.56	0.46, 0.68	0.75	0.70, 0.79	192.10***	83.33	42,134	33
Treatment type								
Sexual offense	0.52	0.40, 0.67	0.79	0.74, 0.85	178.00***	86.52	33,346	25
Domestic violence	0.69	0.53, 0.89	0.66	0.59, 0.75	4.23	29.02	7560	4
Violent offense	0.60	0.46, 0.79	0.60	0.46, 0.79	1.74	0.00	1228	4
Psychologist present								
Inconsistent	0.66	0.53, 0.82	0.80	0.75, 0.85	106.71***	85.94	32,301	16
Consistent	0.52	0.31, 0.90	0.57	0.46, 0.71	28.22***	78.74	2952	7
None or unknown	0.38	0.25, 0.56	0.38	0.30, 0.48	17.33*	48.07	6881	10
Supervision provided								
Yes	0.57	0.46, 0.72	0.76	0.71, 0.81	136.60***	85.36	34,145	21
None or unknown	0.51	0.33, 0.79	0.64	0.54, 0.77	52.31***	78.97	7989	12
Supervision provider								
Psychologist	0.47	0.33, 0.65	0.45	0.39, 0.53	46.56***	76.38	7318	12
Non-psychologist	0.40	0.11, 1.44	0.64	0.57, 0.73	3.45	71.02	6859	2
Psychologist and non-psychologist	0.87	0.71, 1.06	0.94	0.86, 1.02	9.73	48.61	19,264	6
Unknown	0.57	0.37, 0.88	0.72	0.61, 0.85	55.70***	82.05	8217	11
Staff delivery								
Individually facilitated	0.48	0.16, 1.37	0.64	0.51, 0.80	52.78***	94.32	2555	4
Co-facilitated	0.62	0.52, 0.74	0.64	0.58, 0.71	28.27*	43.41	13,817	17
With outlier	0.64	0.53, 0.79	0.79	0.74, 0.85	63.00***	73.02	29,587	18
Unknown	0.47	0.30, 0.75	0.62	0.54, 0.72	65.21***	84.66	9992	11
Service quality								
Weaker	0.62	0.38, 1.02	0.78	0.66, 0.92	7.14*	71.98	4856	3
Promising	0.68	0.55, 0.85	0.68	0.61, 0.76	24.13*	46.12	11,543	14
Most promising	0.46	0.28, 0.74	0.44	0.36, 0.54	34.27***	79.58	3287	8
With outlier	0.51	0.31, 0.83	0.84	0.77, 0.92	81.58***	90.19	19,057	9
Unknown	0.44	0.22, 0.91	0.54	0.44, 0.66	58.86***	89.81	6678	7
Program intensity								
100 h	0.75	0.51, 1.09	0.71	0.63, 0.80	17.27**	71.05	8375	6
100–200 h	0.57	0.38, 0.85	0.84	0.77, 0.92	89.05***	89.89	22,528	10
200–300 h	0.48	0.32, 0.71	0.48	0.32, 0.71	0.88	0.00	883	4
300+ hours	0.57	0.33, 0.997[a]	0.71	0.61, 0.83	28.23***	85.83	3461	5
Therapeutic community								
No	0.56	0.45, 0.71	0.77	0.72, 0.82	131.36***	83.25	33,019	23
Yes	0.82	0.59, 1.15	0.86	0.74, 1.01	6.87	56.31	3192	4
Unknown	0.34	0.23, 0.52	0.33	0.26, 0.42	8.97*	55.38	5657	5
Setting								
Institution	0.56	0.43, 0.72	0.80	0.74, 0.86	128.95***	86.04	27,123	19
Community	0.56	0.41, 0.76	0.66	0.59, 0.73	53.98***	75.92	15,011	14
Modality								
Group	0.47	0.34, 0.64	0.48	0.42, 0.56	59.68***	74.87	8422	16
With outlier	0.49	0.36, 0.69	0.78	0.72, 0.85	122.44***	86.93	24,192	17
Mixed	0.80	0.65, 0.99	0.76	0.69, 0.83	28.58***	65.00	14,470	11
Unknown	0.27	0.15, 0.50	0.29	0.21, 0.40	7.15	44.04	5472	5
Program format								
Rolling group	0.49	0.31, 0.76	0.67	0.61, 0.74	61.15***	88.55	10,845	8
Closed group	0.69	0.58, 0.82	0.69	0.59, 0.81	13.51	11.15	7307	13
With outlier	0.73	0.60, 0.88	0.89	0.82, 0.97	27.37*	52.51	23,077	14
Both	0.92	0.53, 1.59	0.92	0.53, 1.59	0.00	0.00	266	1
Unknown	0.36	0.21, 0.62	0.47	0.40, 0.57	57.78***	84.42	7946	10
Program roll out								
Single site	0.54	0.35, 0.82	0.54	0.46, 0.64	74.03***	81.09	4793	15
Multiple sites	0.65	0.52, 0.81	0.80	0.75, 0.86	45.47***	82.41	29,016	9
Unknown	0.47	0.27, 0.80	0.66	0.55, 0.79	51.80***	84.56	8325	9
Country of program								
United Kingdom	0.57	0.41, 0.80	0.63	0.56, 0.72	3.97	49.61	9416	3
With outlier	0.67	0.47, 0.96	0.83	0.76, 0.89	33.25***	90.98	25,186	4
United States	0.54	0.35, 0.81	0.72	0.63, 0.83	53.62***	81.35	10,225	11
Canada	0.50	0.29, 0.84	0.54	0.46, 0.64	72.87***	89.02	4543	10
New Zealand/Australia	0.60	0.45, 0.79	0.60	0.45, 0.79	4.74	0.00	1624	6
Other international	0.68	0.35, 1.33	0.72	0.46, 1.12	2.11	52.51	556	2
Recidivism quality								
Fair-moderate	0.48	0.28, 0.84	0.48	0.28, 0.84	0.37	0.00	413	2
High	0.49	0.38, 0.64	0.61	0.55, 0.67	85.01***	77.65	19,362	20
Very high	0.68	0.50, 0.92	0.87	0.80, 0.94	72.13***	86.14	22,359	11
Matching employed								
No	0.42	0.29, 0.60	0.48	0.42, 0.55	100.87***	81.16	10,932	20
Yes	0.74	0.62, 0.88	0.72	0.66, 0.79	22.48*	51.07	15,432	12
With outlier	0.77	0.64, 0.92	0.83	0.77, 0.89	42.68***	71.88	31,202	13
Year of study								
1990s	0.49	0.24, 1.04	0.63	0.45, 0.88	20.33**	70.48	2092	7

(*continued on next page*)

Table 4 (*continued*)

Moderator	Random		Fixed		*Q*	I^2	*n*	*k*
	OR	95% CI	OR	95% CI				
2000s	0.54	0.39, 0.75	0.68	0.61, 0.77	72.84***	83.53	12,467	13
2010s	0.54	0.39, 0.74	0.60	0.54, 0.67	50.57***	78.25	11,805	12
With outlier	0.58	0.43, 0.77	0.78	0.72, 0.84	94.20***	87.26	27,575	13

Note. CIs that do not include zero are statistically significant ($p < .05$). Effect sizes $n < 3$ should be interpreted cautiously.
*** $p < .001$, ** $p < .01$, and * $p < .05$.
[a] Indicates a figure rounded to three decimal places to show that this CI does not overlap with 1.0.

7.6. General (any) recidivism

Thirty-six specialized programs examined general, that is any and all, recidivism operationalized as a single outcome variable (see Table 5). These programs significantly reduced general recidivism in both the random (OR = 0.66, 95% CI = 0.58, 0.76) and fixed effects (OR = 0.64, 95% CI = 0.61, 0.68) models with significant heterogeneity ($Q = 132.16$, $p < .001$). Across all program types, over an average 62.4 month (SD = 35.1) follow-up, any general recidivism was 30.0% for treated individuals and 37.7% for untreated comparisons (unweighted means), corresponding to absolute and relative recidivism decreases of 7.7% and 20.4% respectively. Similar OR magnitudes were observed across the three program types.

7.6.1. Staff, treatment, and study quality moderators

Here, findings did not always mirror those already reported since treatment effects did not vary according to the presence of a qualified psychologist. However, treatment effects lessened when supervision was provided for the same treatment program by both psychologists and non-psychologists (vs. supervision provided only by psychologists or non-psychologists). Co-facilitation of programs appeared beneficial relative to individually facilitated programs. The promising and most promising programs produced the strongest associations with general recidivism reduction relative to programs rated as weaker. For the most part, treatments of varying intensity exerted robust treatment effects with the exception of the fixed effect for longer-term treatment (> 300 h). Programs across all countries exhibited reductions in general offending although Canada held the lowest associations. There did not appear to be a uniform relationship between recidivism quality score and reductions in general recidivism. However, matched designs held slightly lower associations with recidivism reduction.

7.7. Publication bias analyses

We used tests of asymmetry to assess publication bias associated with the file drawer problem for all moderating variables that met Ioannidis and Trikinos' (2007) criteria (see Table 6). Thirteen variables qualified for testing. When visually inspected, funnel plots showed clear symmetrical dispersal of effects sizes around the mean. Based on the funnel plots, trim and fill tests assign any missing values as required to create symmetry as well as provide an adjusted overall effect size. These analyses are based on the premise that without a publication bias, studies would show natural sampling error and a symmetrical distribution of results. The trim and fill test adds studies hypothetically missing due to publication bias to recreate what an unbiased summary is likely to look like. As shown in Table 6, very few variables required effect sizes to be imputed to obtain symmetry, with the adjusted imputed value not substantially different from the observed effect size. The fail-safe *N* figures are also impressive, showing that 6–255 of missing studies would be needed to diminish significant effect sizes to non-significance.

8. Discussion

The present meta-analysis is the first to review the impact of various specialized psychological offense treatments on recidivism. In relation to our preplanned hypothesis, we found substantially lower recidivism rates (offense specific and non-offense specific) for individuals who received specialized psychological treatment versus untreated comparisons, using a sample of > 55,000 individuals. We hypothesized that the strongest treatment effects would be found for programs targeting sexual offending rather than domestic violence; yet surprisingly we found comparable significant treatment effects across domestic violence and sexual offense programs. Indeed, our meta-analysis is the first to suggest that domestic violence programs produce reductions in more general offending and differs from previously conducted reviews since we found evidence of a reduction in domestic violence regardless of whether or not a randomized study design had been used. It is unclear why our results regarding domestic violence programs differ from the previous literature which presents largely equivocal findings. Our meta-analysis differs from those conducted previously in various ways; all of which are associated with our inclusion criteria. For example, we focused only on specialized domestic violence treatment (cf. Babcock et al., 2004 who included unspecified therapy), used intent-to-treat analyses (cf. Babcock et al., 2004 who used control groups made up of treatment drop outs), included treatments from various countries (cf. Feder & Wilson, 2005; Smedslund et al., 2007 who focused only on North American studies), and included a range of study designs and treatment approaches (cf. Smedslund et al., 2007 who focused only on randomized controlled treatments that contained elements of CBT[8]). Readers should note that our results in relation to the effects of domestic violence programs on offense-specific recidivism are associated with the findings of fourteen studies.

This meta-analysis is also the most exhaustive to date that examines the effects of specialized psychological treatments for sexual offending, including 11 new studies since Schmucker and Lösel's (2015) original searches in 2010. The sexual recidivism reductions that we found for these programs were higher than, or at the top end of, those reported in previous meta-analyses (Hanson et al., 2002; Lösel & Schmucker, 2005; Schmucker & Lösel, 2015). This is especially notable given that this meta-analysis included the large scale study of Mews et al. (2017) which has cast significant international doubt on the effectiveness of specialized psychological programs for individuals who have sexually offended (Forde, 2017). Further, in contrast to the most recent meta-analysis on sexual offending (Schmucker & Lösel, 2015), both prison and community treatments were associated with reduced recidivism (see also Hanson et al., 2002; Hanson et al., 2009). The non-offense specific recidivism reductions were broadly comparable to those reported previously (Hanson et al., 2002; Lösel & Schmucker, 2005; Schmucker & Lösel, 2015). Finally, our review also showed that general violence programs ($k = 4$) were associated with significant offense specific and non-offense specific recidivism reductions. This meta-

[8] We found little to no effects for CBT treatments when this was coded as the primary treatment method used for domestic violence.

Table 5
All programs: associations with reductions in general recidivism.

Moderator	Random		Fixed		Q	I^2	n	k
	OR	95% CI	OR	95% CI				
Overall	0.66	0.58, 0.76	0.64	0.61, 0.68	132.16***	73.52	28,848	36
Treatment type								
Sexual offense	0.66	0.55, 0.79	0.68	0.62, 0.73	107.72***	76.79	17,632	26
Domestic violence	0.69	0.56, 0.86	0.61	0.56, 0.67	18.05**	66.76	10,146	7
Violent offense	0.57	0.41, 0.79	0.57	0.44, 0.74	2.67	24.95	1070	3
Psychologist present								
Inconsistent	0.64	0.54, 0.76	0.65	0.61, 0.70	60.98***	75.40	17,961	16
Consistent	0.67	0.50, 0.90	0.67	0.51, 0.86	3.39	11.58	1326	4
None or unknown	0.70	0.54, 0.91	0.60	0.53, 0.67	66.07***	77.29	9561	16
Supervision provided								
Yes	0.68	0.58, 0.80	0.66	0.62, 0.71	55.81***	71.33	18,056	17
None or unknown	0.66	0.52, 0.83	0.60	0.54, 0.66	73.64***	75.56	10,792	19
Supervision provider								
Psychologist	0.61	0.49, 0.76	0.62	0.54, 0.72	15.84*	49.50	5779	9
Non-psychologist	0.53	0.27, 1.04	0.57	0.52, 0.64	10.54**	81.03	7050	3
Psychologist and non-psychologist	0.71	0.53, 0.94	0.76	0.67, 0.85	16.76**	76.14	5378	5
Unknown	0.67	0.52, 0.86	0.61	0.55, 0.68	63.99***	76.56	9745	16
Staff delivery								
Individually facilitated	0.83	0.65, 1.06	0.85	0.69, 1.05	4.62	13.50	1525	5
Co-facilitated	0.61	0.52, 0.72	0.60	0.56, 0.65	32.63*	63.23	14,119	13
Unknown	0.68	0.54, 0.86	0.67	0.61, 0.73	84.55***	79.89	13,024	18
Service quality								
Weaker	0.75	0.57, 0.99	0.83	0.73, 0.94	14.04*	64.38	5809	6
Unspecified	0.71	0.53, 0.96	0.63	0.56, 0.71	64.52***	81.40	9193	13
Promising	0.57	0.47, 0.69	0.57	0.52, 0.62	21.02**	52.42	10,100	11
Most promising	0.69	0.53, 0.90	0.66	0.57, 0.77	10.50	52.39	3746	6
Program intensity								
100 h	0.75	0.62, 0.92	0.64	0.59, 0.69	24.76**	67.69	10,971	9
100–200 h	0.61	0.41, 0.89	0.67	0.56, 0.81	13.11*	69.48	3657	5
200–300 h	0.59	0.39, 0.89	0.57	0.44, 0.73	9.89*	59.56	1201	5
300+ hours	0.67	0.40, 1.10	0.80	0.69, 0.94	16.85***	77.49	3249	4
Therapeutic community								
No	0.71	0.60, 0.83	0.65	0.61, 0.70	62.35***	67.92	17,126	21
Yes	0.66	0.42, 1.04	0.79	0.68, 0.91	25.32***	84.20	3347	5
Unknown	0.58	0.45, 0.74	0.51	0.45, 0.58	24.90***	63.86	8375	10
Setting								
Institution	0.68	0.56, 0.81	0.72	0.65, 0.79	54.82***	65.34	10,038	20
Community	0.65	0.53, 0.79	0.60	0.56, 0.65	69.38***	78.38	18,810	16
Modality								
Group	0.68	0.57, 0.81	0.67	0.61, 0.75	30.14*	56.87	8703	14
Mixed	0.71	0.56, 0.89	0.67	0.62, 0.73	66.69***	80.51	12,956	14
Unknown	0.57	0.42, 0.78	0.49	0.43, 0.57	19.94**	69.91	7057	7
Proram format								
Rolling group	0.60	0.41, 0.89	0.64	0.59, 0.70	36.06***	88.91	9359	5
Closed group	0.72	0.63, 0.83	0.71	0.64, 0.79	21.38	29.83	9309	16
Both	0.16	0.06, 0.41	0.16	0.06, 0.41	0.00	0.00	324	1
Unknown	0.67	0.51, 0.86	0.58	0.52, 0.65	59.57***	78.18	9856	14
Program roll out								
Single site	0.68	0.53, 0.88	0.65	0.57, 0.76	46.23***	65.39	5092	17
Multiple sites	0.65	0.54, 0.79	0.65	0.60, 0.70	27.76***	78.39	14,303	7
Unknown	0.66	0.49, 0.87	0.62	0.55, 0.70	57.75***	80.95	9453	12
Country of program								
United Kingdom	0.46	0.31, 0.69	0.58	0.53, 0.64	18.50***	78.38	9881	5
United States	0.70	0.56, 0.87	0.67	0.62, 0.74	80.75***	78.95	13,095	18
Canada	0.83	0.60, 1.13	0.82	0.68, 0.98	11.40*	56.16	2151	6
New Zealand/Australia	0.63	0.51, 0.78	0.62	0.54, 0.73	6.75	25.91	3431	6
Other international	0.41	0.24, 0.70	0.41	0.24, 0.70	0.00	0.00	290	1
Recidivism quality								
Fair-moderate	0.81	0.60, 1.10	0.82	0.67, 1.00	12.24	50.97	1823	7
High	0.56	0.48, 0.65	0.57	0.54, 0.61	62.42***	71.16	21,736	19
Very high	0.82	0.66, 1.03	0.87	0.76, 0.99	15.44	48.19	5003	9
Matching employed								
No	0.61	0.49, 0.77	0.59	0.53, 0.65	86.19***	76.80	11,942	21
Yes	0.72	0.62, 0.84	0.67	0.62, 0.72	41.32***	66.11	16,906	15
Year of study								
1980s	0.88	0.44, 1.75	0.88	0.44, 1.75	0.87	0.00	270	2
1990s	0.63	0.45, 0.90	0.60	0.51, 0.70	35.30***	74.50	4245	10
2000s	0.72	0.57, 0.89	0.72	0.66, 0.79	73.50***	79.59	13,352	16
2010s	0.60	0.52, 0.68	0.59	0.54, 0.64	10.04	30.27	10,981	8

Note. CIs that do not include zero are statistically significant ($p < .05$). Effect sizes $n < 3$ should be interpreted cautiously.
*** $p < .001$, ** $p < .01$, and * $p < .05$.

Table 6
Summary of publication bias analyses on program moderators meeting criteria of appropriateness for asymmetry tests.

Variable	*k*	Observed OR	95% CI	Failsafe *N* (z)	Trim and fill (studies added)	Adjusted OR	95% CI
Sexual recidivism (sexual offense specific programs)							
Psychologist present: none/unknown	16	0.63	0.52, 0.76	50 (3.96)	1	0.63	0.52, 0.78
Supervision provided: none/unknown	21	0.74	0.63, 0.87	33 (3.13)	1	0.74	0.63, 0.87
Supervision provider unknown	17	0.82	0.71, 0.96	6 (2.27)	0	0.82	0.71, 0.96
Therapeutic community: unknown	14	0.52	0.43, 0.63	127 (6.19)	1	0.51	0.42, 0.61
Community treatment setting	18	0.66	0.56, 0.79	113 (5.27)	4	0.72	0.61, 0.84
Arousal conditioning: unknown	16	0.73	0.60, 0.88	18 (2.82)	1	0.72	0.60, 0.87
Country of program: USA	21	0.78	0.69, 0.88	45 (3.45)	0	0.78	0.68, 0.88
Violent recidivism (all programs)							
Supervision provided: none/unknown	12	0.64	0.54, 0.77	91 (5.73)	3	0.72	0.61, 0.85
Co-facilitated services	17	0.64	0.58, 0.71	253 (7.80)	5	0.66	0.60, 0.73
Promising service	14	0.68	0.61, 0.76	112 (5.86)	3	0.69	0.63, 0.77
Closed group program format	13	0.69	0.59, 0.81	58 (4.58)	1	0.71	0.61, 0.83
General recidivism (all programs)							
Promising service	11	0.57	0.52, 0.62	255 (9.63)	0	0.57	0.52, 0.62
Closed group program format	16	0.71	0.64, 0.79	122 (5.74)	0	0.71	0.64, 0.78

analytic evidence is the first to exclusively focus on offense specific violence programs suggesting that they are exerting their intended effects (see also Jolliffe & Farrington, 2007 who examined interventions for violent offenders more generally).

8.1. Predictors of offense specific recidivism

8.1.1. Staff variables

In line with our preregistered hypothesis, sexual and domestic violence psychological programs characterized by consistent qualified psychologist facilitator input were associated with better outcomes than programs without this feature. This supports previous researcher assertions that qualified psychologists are important for the treatment success of specialized psychological offense programs (Gannon & Ward, 2014). Programs that provided clinical supervision for facilitating staff were also associated with better outcomes and variations in outcome according to supervisor profession. For example, for sexual offense programs, qualified psychologist supervisors were associated with superior sexual recidivism reductions. However, the provision of supervision by qualified psychologists *and* non-psychologists across the same program appeared to result in reduced effectiveness and—in some cases—ineffective treatment. This suggests that psychologists and non-psychologists offer guidance that conflicts in some way, resulting in confused facilitation.

8.1.2. Treatment variables

Our review found that numerous program variables impacted treatment effectiveness. The clearest results were associated with sexual offense programs. Here, predictors associated with the best sexual recidivism reductions were: treatment rated as higher quality; treatments of shorter (i.e., < 100 h) or longer (i.e., > 200 h) duration; a group-based treatment format; polygraph absence; and arousal reconditioning. The first outcome supports previous research indicating that RNR adherence (Andrews & Bonta, 2006, 2010b) reduces sexual recidivism (Hanson et al., 2009). The findings regarding treatment intensity are harder to interpret, however, since we did not code treatment participants according to risk level. The superior effects for group only programs may stem from qualified psychologist faciliators being consistently present most often in the group only programs (n = 9; 75%) relative to the other coded categories for treatment modality (n = 3; 25%). Furthermore, since facilitators knew there were no "mop up" sessions, this may have forced all critical issues to be discussed within the group; improving group cohesion which is critical for treatment effectiveness (Beech & Fordham, 1997; Burlingame, McClendon, & Alonso, 2011). Our findings on this aspect stand in direct contrast to those of Schmucker and Lösel (2015), who reported that programs with more individualized formats (e.g., mixed group and individual; k = 4) exerted best effects. Our findings may differ simply because our meta-analysis included more studies in the mixed group and individual category for comparison (k = 18).

Polygraph testing and arousal reconditioning had yet to be examinined in previous treatment meta-analyses, despite widespread use on many programs (McGrath, Cumming, Burchard, Zeoli, & Ellerby, 2010). Proponants of polygraphy hypothesize that it enables more effective treatment through ensuring clients adhere to program conditions and provide accurate sexual histories (Grubin, 2010; Wilcox, 2009). The only single-study research available suggests that combining treatment with the polygraph has little discernable effect on sexual recidivism (see McGrath, Cumming, Hoke, & Bonn-Miller, 2007). Our meta-analytic results are the first, however, to suggest that polygraph use is associated with lower treatment effect sizes. Although the mechanism of this effect is as yet unclear, we anticipate—as others have—that the therapeutic alliance may be negatively impacted when honesty is formally tested and challenged as part of the treatment process (see McGrath et al., 2010; Meijer, Verschuere, Merckelbach, & Crombez, 2008). Moreover, the use of arousal reconditioning for addressing inappropriate sexual interests appears to have lost favor in some jurisdictions (e.g., UK Ministry of Justice; Mews et al., 2017). Waning enthusiasm may stem from the lack of research examining such techniques (Laws & Marshall, 1991; Seto, 2018), as well as recent research suggesting that pedophilia represents a sexual preference with biological origins (Långström, Babchischin, Fazel, Lichtenstein, & Frisell, 2015). The present findings, however, are the first to report that programs incorporating active behavioral attempts to restructure and manage such arousal are associated with larger reductions in sexual recidivism. Given that inappropriate sexual arousal is a key predictor of re-offending sexually (Hanson & Morton-Bourgon, 2005), this finding is particularly pertinent.

Due to relatively small k for the domestic violence programs, establishing more definitive program predictors of decreased recidivism and, hence, improved treatment success was more difficult. However, a set of key predictors did emerge: treatment rated as lower quality; treatments using the Duluth approach; and treatments that were provided at a single institution (vs. multiple institutions). Initially it was unclear why treatments rated as less evidence-based exhibited more effectiveness. A close examination of program content, however, showed that they tended to be Duluth or purely psychoeducational programs. This suggests that it is the provision of educational information—that may or may not be rooted in feminism—that is important for reducing domestic violence, rather than complex psychotherapeutic manipulations designed according to "best practice" (Edleson & Syers, 1991). This may explain why Duluth and psychoeducational approaches produced superior recidivism reductions relative to CBT (cf. Babcock et al., 2004). However, readers should note these

suggestions cautiously since they are just that and are based on relatively small *k*s. Finally, the superior outcomes associated with treatments administered at a single site suggests that treatments are most effective when administration is tightly focused.

8.2. Predictors of non-offense specific recidivism

Our findings for general violent recidivism, across all programs, showed that qualified psychologist input, receiving supervision, and the absence of conflicting psychologist/non-psychologist supervision were associated with the largest violent recidivism reductions. This mirrored the staff effects found for offense specific recidivism outcomes; however, similar effects were not found for general recidivism. It may be that the effects of qualified psychological input, receiving supervision, and supervisor professions are less visible for general recidivism since the content of specialized offense programs and, by extension, supervision are most likely to focus on offense specific—and typically violent—criminogenic issues. In fact, few program variables emerged as consistent predictors of non-offense recidivism and, when they did, they largely reflected those already targeted for offense specific recidivism. The finding that treatment is associated with best results when administered at a single site suggests that treatment integrity may be a critical, yet neglected, factor associated with treatment success more broadly (see Schmucker & Lösel, 2015).

8.3. Interpretative context

Good meta-analyses should represent a complete and accurate picture of the overall study population (Bown & Sutton, 2010; Lipsey & Wilson, 2001). Limiting our searches to documents written in English may have omitted a small number of studies from our analyses. Nevertheless, we made every effort to obtain a full cohort of studies. Just under half of the documents we obtained (44%) were gathered from materials other than peer reviewed journals and asymmetry tests illustrated that publication bias was not a concern.

Previous meta-analyses examining specialized offense treatments have been critiqued regarding the quality of evaluation studies examined, with most authors arguing that stronger randomized designs are required (Babcock et al., 2004; Hanson et al., 2002; Hanson et al., 2009; Schmucker & Lösel, 2015; Walton & Chou, 2015). Our meta-analysis is no exception to such critique since few studies used a randomized design. However, we did record quality of study design through examining whether each study employed matching criteria as well as the overall quality of recidivism variables used within each study. Using these indicators we were able to show that, with the exception of domestic violence programs, study design and matching had surprisingly little impact on recidivism reductions (see also Hanson et al., 2002; Hanson et al., 2009). In fact, since higher recidivism rates are associated with drop-outs (Lösel & Schmucker, 2005), our intent-to-treat meta-analysis is likely to represent a more conservative test of the effects of specialized psychological offense treatment.

All meta-analyses, including this one, are affected by potentially confounded moderator effects (Lipsey, 2003). Where possible, we examined the individual studies generating each key moderating effect for any obvious patterns of confounding variables.

However, we recognize that numerous unidentified confounders could also be present. A further key limitation was that we did not always have enough information to populate both an “unknown” and a “not present” group for each moderating variable. Whilst this could not be avoided, it suggests that study authors could improve upon the quality of staffing and treatment program information provided in published and unpublished reports. We know, for example, that many competent professionals (e.g., social workers, psychiatrists) would not have been classified as independent registered psychologists. However, information was simply not available to conduct coding and analyses based on facilitator profession. We suggest authors clearly report each of the program and staff variables outlined in Tables 2 and 3 in all future evaluations as an absolute minimum.

8.4. Future policy and practice directions

The outcomes of this meta-analysis are the first to suggest that specialized psychological programs that target various offending behaviors are effective. Although there was significant heterogeneity across the outcomes of individual studies, our review suggests ways that policy makers and program providers might optimize program outcomes. First, the results indicate that program developers should provide qualified psychologists who are consistently present in hands-on treatment; and second, facilitators should be provided with supervision opportunities that are similar across the program. Interestingly, less than one in five programs consistently used qualified psychologists in hands-on facilitation and the majority of these (83.3%; $n = 10$) were implemented in the 1970s, 1980s, or 1990s rather than more recently. The provision of supervision was more evenly spread. We recognize the significant pressures that policy makers face providing cost effective programs to large numbers of individuals (Gannon & Ward, 2014). As an indication of this, correctional systems in a number of international jurisdictions have been moving away from the direct involvement of psychologists as treatment providers, with therapeutic activities such as running manual-based groups being delegated to correctional program officers who may have little or no formal clinical training. Ironically, it seems that this variable is correlated with optimum behavioral change and yet qualified psychologist hands-on input is lacking in programs implemented in recent years. This may explain why we did not find more modern treatments to bring about improved outcomes (see also Lösel & Schmucker, 2005). Qualified psychological staff and regular supervision come at a clear financial cost. Program providers could consider the benefits of pruning down staff facilitation numbers as a compensatory financial strategy given that individual and co-facilitated programs seem to be equally beneficial. Program providers might also want to consider methods for tightly controlling program implementation given that we found single site treatments seemed to fare better than multisite treatments.

Further offense specific practice implications are available for those involved in sexual offense and domestic violence policy. Regarding sexual offense programming, the results indicate that best practice guidelines in this area should be revised to include (1) cautionary messages regarding polygraph use within the therapeutic context, and (2) further commentary on—and expansion of—the evidence base around behavioral reconditioning as a treatment tool. Those tasked with developing and managing programs for those who have been domestically violent should seek out the best educational materials possible and consider how such materials can be skilfully woven into program facilitation to produce optimal results.

9. Conclusions

Previous researchers have noted that it is difficult to ascertain the exact variables responsible for apparent recidivism reductions when engaging in large scale meta-analytic work (Hanson et al., 2002); we agree, particularly when heterogeneity of findings is present across studies. However, the findings from this review across traditional and emerging specialized psychological offense programs presents converging evidence that such programs impact a broad range of offending behaviors in addition to impressive reductions in offense specific recidivism. Amidst these findings, however, lies an important moderating variable that has been neglected in previous meta-analyses: program staffing. If specialized psychological offense programs are to be effective, then our review suggests that researchers and clinicians must seriously consider these factors *in addition to* study design quality.

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Contributors

Authors Theresa A. Gannon, Mark E. Olver, and Jaimee S. Mallion designed the study and coding manual. Authors Theresa A. Gannon, Jaimee S. Mallion, and Mark James conducted the literature searches. Author Theresa A. Gannon contacted all authors of identified manuscripts. Authors Mark E. Olver and Mark James conducted the statistical analyses. Author Theresa A. Gannon wrote the final draft of the manuscript and all authors contributed to and have approved the final copy.

Conflict of interests

There are no known conflicts of interest that could have inappropriately influenced or be perceived to have influenced this research manuscript.

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Appendix A. Studies included in meta-analysis

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