A Meta-Analysis of Moral Reconation Therapy

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Abstract

This study reports on a meta-analysis of moral reconation therapy (MRT). Recipients of MRT included adult and juvenile offenders who were in custody or in the community, typically on parole or probation. The study considered criminal offending subsequent to treatment as the outcome variable. The overall effect size measured by the correlation across 33 studies and 30,259 offenders was significant ($r = .16$), indicating that MRT had a small but important effect on recidivism. Moderator analyses were conducted to detect the possible factors affecting the relationship between MRT and recidivism. Moderators included setting, age, gender, research design, sample size, type of recidivism, follow-up period, publisher, and year of publication. Moderator analysis demonstrated that MRT was more successful with adult than juvenile offenders in institutional settings as opposed to the community, and where researchers in the primary studies used randomization to allocate participants to either a treatment or control condition. The treatment effect size was greater when the type of recidivism used was rearrest rather than rearrest followed by conviction or reincarceration. The benefits of MRT were strongest with a relatively short follow-up period. MRT was more successful for relatively small samples and for large samples rather than medium-sized samples. The effect size was smaller for studies published by the owners of MRT than by other independent studies. The effect size was also smaller for studies published after 1999.

Keywords

moral reconation therapy, moral development, delayed moral maturity, cognitive-behavioral treatments

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Background to Moral Reconation Therapy (MRT)

MRT was developed and implemented by Kenneth Robinson, Gregory Little, and their colleagues at Correctional Counseling, Inc. (CCI; Little & Robinson, 1988). "Reconation" comes from the psychological terms *conative* and *conation*, which refer to the process of making deliberate, conscious moral decisions (Good Success Consulting, Inc., 2011). Correctional agencies in Puerto Rico, Australia, Scotland, Canada, and across the United States have partnered with CCI to implement MRT-based programs to treat offenders and curb recidivism. MRT was initially developed between 1979 and 1983 at the Federal Correctional Institute in Memphis, Tennessee. CCI was formed in 1987 as a subsidiary of a privately held behavioral health hospital. The first published study on MRT appeared in 1988. In 1990, CCI was established as a freestanding privately held company.[1] Since then, the company has delivered MRT to dozens of correctional agencies. To date, more than one million offenders have been enrolled in MRT-treatment programs (CCI, 2010). CCI claims success for a variety of offenders, offenses, problems, and settings: adults and juveniles, males and females, driving offenses, substance abuse, domestic violence, drug courts, pretrial service agencies, as well as probation and parole offices. MRT programs are used in correctional facilities in 47 states.

Research on MRT

In 2010, in its latest efforts to bolster its claims of success, CCI published "Twenty-Year Recidivism Results for MRT-Treated Offenders" (Little, Robinson, Burnette, & Swan, 2010). Results from the analysis of the postrelease criminal records of 1,052 MRT-treated offenders and 329 untreated controls revealed a similar pattern to that which was reported after 10 years (Little, Robinson, Burnette, & Swan, 1999). After the 20-year period, 93.6% of the untreated controls had at least one rearrest, compared with 81.2% of MRT-treated offenders. In addition, 81.2% of untreated controls had been reincarcerated for a new offense at least once over the 20 years of release compared with 60.8% of the MRT-treated group. Clearly, as evidenced by the recidivism rate of the control group, this application of MRT adhered to Andrew's well-established risk principle that offender treatment should be directed to moderate- and high-risk offenders (Andrews, Bonta, & Hoge, 1990). The researchers concluded as follows: the present study clearly shows that offender participation in MRT leads to significantly lower reincarceration rates, lower rearrest rates, and a higher rate of clean records following MRT. It is also clear that the cost benefits of MRT are not only substantial but also meaningful in several ways. The costs to society in processing arrests, paying for incarceration, and associated crime expenditures are greatly lessened by MRT (Little, Robinson, et al., 2010).

The article has posed and answered a rhetorical question that CCI has asked its potential customers: "Why is MRT the best choice for your prison treatment programs?" (Little, 2006, p. 3). But exactly how does MRT lower recidivism rates?

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A Theoretical Basis for MRT

MRT is grounded in the framework of cognitive behaviorism, and draws inspiration from Kohlberg's (1976) theory of moral development. Kohlberg's theory assumes that moral development progresses through three main stages. The preconventional level is common to children, although adults can also show this level of moral reasoning. Reasoners, at this level, judge the morality of an action by its direct consequences and are solely concerned with the self in an egocentric manner. The conventional level is typical of adolescents and adults, who may judge the morality of actions compared with society's views and expectations. Although an individual obeys society's rules and norms even when there are no obvious consequences for obedience or disobedience, adherence is often rigid, and a rule's appropriateness or fairness is seldom questioned. At the postconventional level, there is a growing realization that one's own perspective may, on occasion, take precedence over society's views. Therefore, an individual may disobey rules inconsistent with his or her own principles. These people live (and sometimes die) by adherence to their own abstract principles about right and wrong—principles that usually include basic human rights as life, liberty, and justice. Theorists suggest that many people never reach this level of abstract moral reasoning (Cherry, 2011).

Although Kohlberg's cognitive development approach continues to hold a special place in developmental psychology textbooks, his original stage typology has undergone a number of revisions. For example, the model proposed by Gibbs, Basinger, Grime, and Snarey (2007) rejected Kohlberg's three stages (preconventional, conventional, and postconventional) in favour of two overlapping phases, the standard and existential development phases. Moral judgment in the first phase is seen to progress with age from immature (Stages 1 and 2, each involving pronounced ego-centrism in making moral judgments) to mature (Stages 3 and 4, ideal moral reciprocity and social systems). In addition, life-span moral judgment development occurs across cultures and involves an existential development phase, which is moral judgment maturity in its fullest sense; an emerging sense of moral confusion in the face of an apparently absurd moral world sparks deep philosophical reflection on ethics and a search for meaning.[2]

The implications of Kohlberg's and Gibbs' theories of moral development are clear: Breaking the law would seem more acceptable to those at the earlier, more self-centered, stages of development. According to Little and Robinson (1988), offenders "enter treatment with low levels of moral development, strong narcissism, low ego/identity strength, poor self-concept, low self-esteem, inability to delay gratification, relatively strong defense mechanisms, and relatively strong resistance to change and treatment" (p. 135). Consequently, MRT seeks to move offenders from a lower, hedonistic level of moral reasoning (pleasure vs. pain) to a higher level where social rules and others become important. The appropriateness of such a strategy is supported by a recent meta-analysis that found a negative relationship between moral development and the recidivism of offenders. A total of 19 correlations generated an overall effect

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size with offender recidivism of $r = .11$, with moral cognition producing a stronger effect, $r = .20$, than moral emotion, $r = .11$ (Van Vugt et al., 2011). In their meta-analysis of 50 studies comparing juvenile delinquents to nondelinquents, Stams et al. (2006) found that delinquency was strongly associated with developmentally delayed moral judgment, even when controlling for issues such as age, gender, socioeconomic status, and intelligence. Moreover, analyses of moderator variables revealed that differences between the two groups were large for comparisons involving male offenders, older adolescents, delinquents with low intelligence, incarcerated delinquents (particularly for those with prison sentences of more than 18 months), and psychopathic delinquents. Similarly, Gibbs et al. (2007) found substantial cross-cultural moral judgment delay among delinquents relative to controls.

Because MRT takes a cognitive-behavioral approach, it assumes that cognitions affect behaviors, which we can monitor and alter our cognitive activity, and that changes in cognitions lead to changes in behavior. The program is designed to influence how offenders think about moral issues and make moral judgments. Moral reasoning represents how a person makes decisions about how to act in a given situation. Being a cognitive-behavioral treatment (CBT)–based program, MRT conforms, at least in principle, to the concept of general responsivity (Andrews et al., 1990). However, the extent to which its delivery is modified to accommodate the individual characteristics of its clientele, such as cognitive ability, learning style, ethnicity, and gender (i.e., specific responsivity), is unclear.

MRT is a structured program that uses a manual describing exercises and lessons directed at groups of 10 to 15 offenders. The therapy involves 12 to 16 sessions. Each session lasts 1 to 2 hr, and there are usually two sessions per week. Each participant is given a workbook that contains the exercises and lessons that constitute the program. MRT has a deficit orientation that focuses on several areas, including confrontation of beliefs, attitudes and behaviors, assessment of current relationships, reinforcement of positive behavior and habits, positive identity formation, development of frustration tolerance, and development of higher stages of moral reasoning. Following completion of treatment, society should find improvements in the recidivism rates of MRT-treated offenders.

CCI has reported that various state and federal associations, services, correctional departments, institutes, journals, commissions, and academics have applauded MRT for its ability to assist offenders with a host of social and personal ailments: chemical abusers and offenders, male and female, and adult and juvenile. CCI claims that MRT can reduce stress and quell anger, and that it works with domestic violence offenders and those who abuse alcohol. It has been suggested that it can assist offenders to find jobs and help to fill a spiritual void (Little, Robinson, et al., 2010). However, given the vested interest that CCI has in MRT, it is understandable that criminal justice personnel might be cautious about CCI's claims of success.

The Current Study

The present meta-analysis examined the relationship between MRT and rates of recidivism among adult and young offenders. Most of the effect sizes (59%) were

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derived from studies reported by CCI and, therefore, their findings may *not* have been vetted by independent peer-reviewed journals. Second, most of the studies reported by CCI are *reviews* of studies, and not in-depth studies per se. As a result, the reviews tend not to provide important information about participants, such as age, ethnicity, or level of risk. The current study represents an independent investigation of MRT by researchers who have no connections to MRT or relationship with CCI.

Method

Strategies for Searching the Literature. Multiple search methods were used to avoid biased retrieval of studies from different sources, especially the major journals which may selectively publish only positive and significant results (Rosenthal, 1995). First, a computerized search of relevant journals and databases was conducted. Examples include PsychINFO, the Psychology and Behavioral Sciences Collection, Google Scholar, and the journals *Psychological Reports, Crime and Justice, Journal of Research in Crime and Delinquency, Psychology, Crime and Law*, as well as *Crime, Law, and Social Change*. The following keywords, in varying combinations, were used for the search: *moral, moral judgment, moral reasoning, cognitive-behavioral, recidivism, crime*, and *moral reconation therapy*. This strategy yielded 13 published and otherwise posted studies about MRT. The CCI office was subsequently contacted and asked about the availability of other studies, unpublished documents, theses, and doctoral dissertations involving MRT. The research team was subsequently directed to the two main sources for material on MRT, the websites for CCI and for the *Cognitive-Behavioral Treatment Review* (*CBTR*), which is a journal owned and operated by CCI. Finally, another search strategy was to locate articles/reviews that were cited in the previously acquired articles.

Inclusion Criteria

Four criteria were established for the inclusion of studies in the analysis. First, the research must have employed an experimental or quasiexperimental design comparing the recidivism rates of MRT-treatment offenders with a nontreated comparison group. Second, studies must have been published between 1988 and 2010. Third, it must have been possible to calculate an effect size statistic (r) from data included in the article or provided from a source that was cited in the document. Finally, studies were required to specify the setting (i.e., institutional/community), type of recidivism, and length of the follow-up period for recidivism. Although 48 studies/reviews were initially identified for potential inclusion in the meta-analysis, 15 studies were rejected because they did not provide sufficient information to calculate an effect size (9 studies) or they did not specify a setting (institutional/community; 6 studies). Ultimately, 33 studies yielding 38 effect sizes were included in the meta-analysis. However, because several effect sizes were based on the same sample (some of which were derived from separate papers), it was important to consider the issue of statistical independence between samples. Typically, one or more outcome measures were

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varied in a second article, usually the length of follow-up or type of recidivism (rearrest, rearrest then conviction, reincarceration). Therefore, the overall mean effect size for the present study was calculated using only one effect size per sample. The decision rules to select the effect size included the *longest* follow-up period, and the *most general* definition of recidivism, which is rearrest. However, the 38 effect sizes were maintained for comparisons across the nine moderator variables that were analyzed. Therefore, as is customary in meta-analyses, one sample was allowed to contribute more than one effect size, but only in the investigation of the moderator variables and only if its multiple effect sizes fell into different categories along the moderator variable (e.g., rearrest and reincarceration as outcome measures).

Coding Procedures

Each of the 33 studies was coded for variables related to setting (institutional/community), gender (male/female), age (adult/young offender), publisher (*CBTR*/non-*CBTR*), research design (experimental/matching/nonequivalent), publication year (1988-2010), follow-up period in months, and type of recidivism (rearrest, rearrest followed by reconviction, reincarcerated). The overall sample size for the MRT-treated and nontreated comparison groups were recorded. The methodological quality of the study was evaluated by the research team using a hybrid of Lipsey and Wilson's (1993) simple two-group classification system and Losel and Schmucker's (2005) four groups, which were adapted from the Maryland Scale of Scientific Rigor (Sherman et al., 1997). Studies were coded as randomly assigned groups, matched groups, or unmatched (nonequivalent) groups taken from the same offender base from which the treatment participants were obtained. Each of the studies included in the current analysis reported the number and proportion of recidivists for the MRT-treated and comparison groups. Consequently, the effect size (r) reported in the present study represents the magnitude of the difference in proportion between the two samples. Treatment and comparison groups were coded such that a positive correlation indicated a positive treatment effect (i.e., lower recidivism in the treatment group), whereas a negative correlation indicated a negative treatment effect (i.e., higher recidivism in the treatment group).

When multiple outcomes were reported from a single study or sample, all possible effect sizes were calculated and maintained for use in the analysis of outcome-based moderator variables as is customary in meta-analysis. However, only one effect size per sample was chosen for the calculation of the overall effect size to maintain the statistical independence of the contributing effect sizes. In these cases, the more inclusive measure (e.g., rearrest as opposed to reincarceration and longer, rather than shorter, follow-up periods) was used.

Effect Size Coding and Meta-Analytic Strategy

Using Pearson's r correlation, a single overall effect size comparing MRT with non-MRT recidivism rates was calculated, along with the corresponding confidence interval

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at the .05 level, for each study. Where possible, the effect size was calculated from data provided in the article or review (i.e., means, sample size, proportions). The Meta-Analysis Easy to Answer [META] program (see Kenny, 2007) was used with an arcsine transformation to convert differences in proportion of recidivism between MRT-treated offenders and control groups into effect size, r. Studies were weighted by sample size.

Homogeneity of effect sizes was tested to establish whether the individual study effect sizes estimated the same population mean; that is, to detect the extent to which effect sizes are constant across studies. Although a certain amount of variation in effect size is expected, when there is more variation than would be expected by chance alone, the estimates are considered heterogeneous. In the face of heterogeneity, a single estimate may be misleading and should be avoided (Durlak & Lipsey, 1991). Consequently, in the event of heterogeneity, a random effects model was used to interpret the effect size. In the event of homogeneity, a fixed effect model was used (see Huedo-Medina, Sanchez-Meca, Marin-Martinez, & Botella, 2006).

The Comprehensive Meta-Analysis Program (Version 2.2.057; Borenstein, Hedges, Higgins, & Rothstein, 2009) was used to conduct Q-statistic tests and to test the influence of moderator variables. Nine potential moderator variables were considered by examining the Q statistic between groups of studies. When the Q statistic was significant for a specific moderator, further Q statistics were calculated for each group of studies defined by the moderator to assess heterogeneity within groups of studies. These results were then used to determine whether a fixed effect or random effects model should be used to calculate the effect size for the subgroup of studies. The nine potential moderators fell into four categories: (a) the nature of treatment participants as defined by setting (custody or community), age category (youth or adult), and gender; (b) the nature of the research as defined by the research design (random, matched, or nonequivalent), and sample size (less than 200 participants, 200 to 499 participants, 500 or more participants); (c) the nature of recidivism as defined by type (rearrest, rearrest plus conviction, reincarceration) and length of follow-up (less than 2 years, 2-5 years, more than 5 years); and (d) the nature of the report as defined by the publisher (*CBTR* or other) and the year of publication (pre-2000 or 2000 to present).

An important issue to consider is publishing bias. Specifically, the problem is that researchers *may* be more likely to submit, and editors *may* be more likely to publish, articles that report positive or statistically significant findings, whereas research that generates negative findings *may* be less likely to be submitted or published. Such an overemphasize on positive findings means that the published studies may not be truly representative of all valid studies undertaken, and this bias may distort the findings of a meta-analysis of a large number of studies.

A useful technique for detecting publication bias is the Trim and Fill method for estimating and adjusting for the number and outcomes of missing studies in a meta-analysis (Duval & Tweedie, 2000). The method is based on the formalization of the qualitative approach using the funnel plot, where publication bias in the data might be inferred when the funnel shape is asymmetrical. It is assumed that asymmetry is due to missing studies which were not published, perhaps because they showed no

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significance or hypotheses were not supported. An algorithm is used to impute the missing values that will give an estimate of their effect on the summary effect size, *r*, in the meta-analysis. After allowing for potential publication bias, the summary value can remain relatively unchanged or it can shift toward "0" (no effect) or even to a negative value indicating a negative overall effect size. If the summary effect size, *r*, and confidence interval remain substantially unchanged after allowing for publication bias, it is suggested that the observed effect size is not affected by publication bias.

Results

Summary Information About the Studies

As reported in Table 1, the analysis is based on 33 different studies reporting 38 effect sizes (*r*) and a total of 30,259 offenders. Of all, 20 (62%) studies were conducted on incarcerated offenders with the balance on community-based offenders. A total of 27 (84%) studies involved male offenders, 2 studies (6%) involved female offenders, with the remaining 3 (9%) studies having mixed participants or not specifying the gender of their participants. In all, 30 (93%) studies involved only adult offenders, with 2 studies (7%) involving youth. A total of 19 studies (59%) were published in *CBTR*. The remaining studies were published or posted by other sources. With respect to research design, 5 studies (16%) used random assignment to allocate participants to either an MRT-treated group or a control group, 3 studies (9%) employed some kind of matching process in an effort to create an equivalent comparison group, and 19 studies (59%) used an unmatched, nonequivalent comparison group, whereas 5 studies (16%) did not supply sufficient information to determine the nature of the comparison group. Publication dates range from 1989 to 2010. The mean follow-up period in months was 35.2 months ($SD = 44.0$; minimum = 6 months, maximum = 240 months). In all, 21 studies (65%) assessed recidivism using any rearrest, whereas 5 studies (16%) used rearrest followed by conviction and 6 (19%) used reincarceration.

Individual and Overall Effect Sizes

Table 1 reports the effect sizes and confidence intervals for each of the 33 studies included in the meta-analysis. Based on Cohen's criteria (Cohen, 1992), the majority of studies indicated either small or medium effect sizes. All of the effect sizes except one (Grandberry, 1998) were positive with two of them being considered large (i.e., *CBTR*, 2004). The review also revealed that 19 of the 38 effect sizes from 33 studies (50%) did not show a significant difference in the recidivism rates of the MRT-treated offenders compared with controls. However, this finding, in itself, should not be considered an indication of negative results.

The overall Q statistic was significant, $Q(33) = 254.35$, $p < .0001$, indicating a heterogeneous collection of effect sizes. Therefore, a random effects model was applied to obtain an overall effect size. The mean effect size, $r_{random} = .16$ was significant

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Table 1. Primary Study Characteristics, (Random) Effect Sizes, and Confidence Intervals for 39 MRT Studies.

Study	Setting	Gender	Adult/youth	CBTR or non-CBTR	Research design	Follow-up in months	Sample *n*	Type of recidivism	*r*	95% CI
Anderson (2002)	Comm.	Male	Adult	CBTR	Matched	36	2,374	Reincarcerated	.17	[.13, .21]
Armstrong (2000)	Comm.	Male	Youth	Non-CBTR	Non-equiv.	6	256	Rearrest	.01	[−.12, .13]
Armstrong (2003)	Inst.	Male	Youth	Non-CBTR	Random	58	212	Rearrest-reconv.	.01	[−.13, .14]
Better People (2000)	Comm.	Male	Adult	Non-CBTR	Non-equiv.	6	136	Rearrest	.77	[.15, .84]
Boston (2001)	Inst.	Male	Adult	CBTR	Unspecified	6	136	Rearrest-reconv.	.22	[−.08, .25]
Burnett (1997)	Inst.	Male	Adult	CBTR	Random	12	60	Rearrest	.28	[−.06, .43]
Burnett (1997)	Inst.	Male	Adult	CBTR	Matched	12	60	Reincarcerated	.13	[−.13, .37]
Burnette, Leonard, Robinson, and Swan (2004)	Comm.	Female	Adult	CBTR	Non-equiv.	24	1,155	Rearrest	.23	[−.02, .45]
Burnette, Prachnick, Leonard, Robinson, and Swan (2005)	Inst.	Female	Adult	CBTR	Non-equiv.	24	1,280	Rearrest	.34	[.29, .39]
CBTR (1994)	Inst.	Male	Adult	CBTR	Non-equiv.	60	133	Reincarcerated	.15	[.12, .18]
CBTR (2004)	Comm.	Male	Adult	CBTR	Non-equiv.	12	264	Rearrest	.57	[.38, 72]
CBTR (2004)	Comm.	Male	Adult	CBTR	Non-equiv.	12	264	Rearrest	.43	[.22, .60]

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Table 1. (continued)

Study	Setting	Gender	Adult/youth	CBTR or non-CBTR	Research design	Follow-up in months	Sample *n*	Type of recidivism	*r*	95% CI
Deschamps (1998)	Inst.	Unspecified	Youth	Non-CBTR	Random	12	168	Rearrest-reconv.	.10	[−.05, .25]
Godwin, Stone, and Hambrook (1995)	Inst.	Male	Adult	CBTR	Non-equiv.	12	5,217	Rearrest	.14	[.11, .16]
Grandberry (1998)	Inst.	Male	Adult	Non-CBTR	Unspecified	12	210	Rearrest	–.04	[−.18, .09]
Guerin (2001)	Comm.	Male	Youth	Non-CBTR	Matched	12	167	Rearrest	.25	[.01, .46]
Hanson (2000)	Comm.	Unspecified	Adult	Non-CBTR	Non-equiv.	12	271	Rearrest	.10	[−.01, .22]
Huffman (2005)	Comm.	Male	Adult	CBTR	Non-equiv.	6	74	Rearrest	.08	[−.15, .31]
Kirchner and Kirchner (2008)	Comm.	Male	Unspecified	CBTR	Non-equiv.	36	121	Rearrest	.04	[−.13, .22]
Krueger (1997)	Inst.	Male	Adult	CBTR	Random	60	184	Rearrest	.33	[.19, .45]
Little and Robinson (1989)	Inst.	Male	Adult	Non-CBTR	Non-equiv.	12	180	Rearrest	.08	[−.07, .22]
Little, Robinson, and Burnette (1990)	Inst.	Male	Adult	Non-CBTR	Non-equiv.	24	180	Rearrest	.26	[.12, .40]

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Table 1. (continued)

Study	Setting	Gender	Adult/youth	CBTR or non-CBTR	Research design	Follow-up in months	Sample *n*	Type of recidivism	*r*	95% CI
Little, Robinson, and Burnette (1991)	Inst.	Male	Adult	Non-CBTR	Non-equiv.	36	152	Rearrest-reconv.	.12	[−.04, .28]
Little et al. (1991)	Inst.	Male	Adult	Non-CBTR	Non-equiv.	36	180	Reincarcerated	.11	[−.03, .25]
Little, Robinson, and Burnette (1993)	Inst.	Male	Adult	Non-CBTR	Matched	60	152	Rearrest-reconv.	.21	[.06, .36]
Little et al. (1993)	Inst.	Male	Adult	Non-CBTR	Random	42	152	Reincarcerated	.22	[.06, .37]
Little, Robinson, and Burnette (1994)	Inst.	Male	Adult	CBTR	Non-equiv.	60	1,381	Reincarcerated	.16	[.10, .21]
Little, Robinson, Burnette, and Swan (1999)	Inst.	Male	Adult	CBTR	Non-equiv.	42	180	Reincarcerated	.07	[−.07, .22]
Little et al. (1999)	Comm.	Male	Adult	CBTR	Non-equiv.	120	1,481	Reincarcerated	.15	[.09, .20]
Little et al. (1999)	Inst.	Male	Adult	CBTR	Non-equiv.	120	1,481	Rearrest	.21	[.05, .35]
Little (2006)	Inst.	Male	Adult	CBTR	Non-equiv.	36	3,409	Rearrest-reconv.	.13	[.09, .16]

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Table 1. (continued)

Study	Setting	Gender	Adult/youth	CBTR or non-CBTR	Research design	Follow-up in months	Sample *n*	Type of recidivism	*r*	95% CI
Little et al. (2010)	Inst.	Male	Unspecified	CBTR	Unspecified	120	1,381	Reincarcerated	.15	[.09, .20]
Little et al. (2010)	Inst.	Male	Unspecified	CBTR	Unspecified	240	1,381	Reincarcerated	.12	[.07, .17]
Little et al. (2010)	Inst.	Male	Adult	CBTR	Unspecified	24	2,203	Rearrest	.01	[−.03, .05]
Mackenzie, Brame, Waggoner, and Robinson (1995)	Inst.	Male	Adult	Non-CBTR	Unspecified	12	6,631	Rearrest	.27	[.25, .30]
McCracken, Hearn, and Stuckey (2003)	Comm.	Male	Adult	CBTR	Non-equiv.	12	68	Rearrest	.26	[.03, .47]
Shields (2003)	Comm.	Male	Adult	Non-CBTR	Random	6	136	Rearrest	.12	[−.05, .28]
Wallace (2001)	Inst.	Mixed	Youth	CBTR	Non-equiv.	7	179	Rearrest	.26	[.04, .45]

Note: CBTR = Cognitive-Behavioral Treatment Review; CI = confidence interval; Comm. = Community; Inst. = Institution; Non-equiv. = nonequivalent research design; Rearrest-reconv. = rearrest followed by reconviction.

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($Z_{39} = 9.07$, $p = .0001$). This corresponds to (unweighted) mean recidivism rates for the treatment and comparison groups of 28% and 44%, respectively, across the 33 different studies.

Moderator Effects and Within-Group Effects

Given the heterogeneity of the effect size, moderator analyses were conducted to detect the possible factors affecting the relationship between MRT and recidivism. In interpreting the findings from the moderator analysis, four kinds of moderators were analyzed. Offender group moderators included setting (institution, community), age (adult, young offender), and gender (male, female). Research moderators included research design (random, matched, nonequivalent), and sample size (N = 60-199, N = 200-499, N = 500 or more). Outcome moderators included type of recidivism (rearrest, rearrest followed by conviction, reincarceration), and follow-up period in months (6 months to 2 years, 3-5 years, 5 years or more). Publication moderators included publisher (*CBTR*, non-*CBTR*) and year of publication (pre-2000, 2000-2010).

Table 2 reports the effect sizes, confidence intervals, and Q values for the nine moderator variables (between group studies) and for the various subgroups of studies as defined by each of the moderator variables (within-group studies). With respect to setting and client characteristics, larger effect sizes were found for treatment provided in institutions ($r_{random} = .18$) than in community settings ($r_{fixed} = .10$), to adult offenders ($r_{random} = .17$) than to juvenile offenders ($r_{random} = .07$), with the latter finding being marginally significant (95% confidence interval = [.001, .14]; $k = 5$), and to female offenders ($r_{fixed} = .34$) than to male offenders ($r_{random} = .15$), although the female effect size was based on only two studies.

Concerning the type of research design, there was a smaller effect size among those studies that used a random research design ($r_{fixed} = .11$) than for studies using a matched group or nonequivalent group design ($r_{fixed} = .18$ and $r_{random} = .15$, respectively), with the effect sizes for the random and matched group studies having nonoverlapping confidence intervals ($p < .05$). Moreover, the analysis revealed that the effect size for studies with a small sample size of 60 to 199, and a large sample size of 500 or more had a larger effect size ($r_{random} = .17$ for both sample sizes) than those studies with a sample size of 200 to 499 ($r_{fixed} = .02$). This latter effect size was the only subgroup effect size among all subgroup analyses that was not significant (95% confidence interval = [−.02, .09, with $k = 5$).

Turning to variations in the outcome variable, the treatment effect size for rearrest ($r_{random} = .18$) was higher than the effect sizes for the other two types of recidivism, rearrest followed by conviction ($r_{fixed} = .03$) and reincarceration ($r_{fixed} = .13$). There was also a difference in the magnitude of the treatment effect size by length of follow-up with studies using a 6-month to 2-year follow-up period producing a larger effect size ($r_{random} = .19$) than those using longer follow-up periods of 3 to 5 years ($r_{fixed} = .15$) and 5 years or more ($r_{fixed} = .14$).

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Table 2. Testing for Heterogeneity: *Q* Total, *Q* Between, *Q* Within, and *Z* Test Across Nine Moderator Variables (Fixed).

Moderator variables	Number of respondents (*n*)	Number of studies *k*	Effect size *r*	*Z*	*p*	95% confidence interval	*Q* value between groups	*Q* value within groups
Overall	30,259	33	.17	30.44	.0001	[.16, .18]		254.35***
Setting							5.01 *	
Institutional	24,968	25	.17	30.36	.0001	[.16, .18]		241.70 ***
Community	5,443	7	.10	3.41	.001	[.04, .16]		7.56 (*ns*)
Age							7.98 **	
Adult	28,399	31	.17	31.51	.0001	[.16, .18]		238.8***
Young	860	2	.07	2.00	.04	[.001, .14]		7.54 **
Gender							42.01***	
Male	27,206	27	.16	29.53	.0001	[.15, .17]		211.40***
Female	2,435	2	.34	12.44	.0001	[.29, .39]		.94 *ns*
Publisher							17.00***	
CBTR	21,408	21	.14	18.44	.0001	[.13, .16]		132.05***
Non-CBTR	8,851	12	.19	24.57	.0001	[.18, .21]		106.23***
Design							14.50 **	
Random	760	5	.11	2.93	.003	[.04, .18]		4.86 (*ns*)
Matched	2,693	5	.18	9.91	.0001	[.15, .22]		5.67 (*ns*)
Nonequivalent	16,066	16	.16	20.77	.0001	[.14, .17]		89.93***
Not specified	10,740	4	.19	20.25	.0001	[.17, .21]		140.06***
Recidivism							21.73**	
Rearrest	2,401	22	.18	25.78	.0001	[.18, .21]		223.36***
Rearrest then conviction	4,077	4	.03	2.66	.004	[.03, .18]		4.25 (*ns*)
Reincarceration	5,841	8	.13	17.76	.0001	[.13, .16]		4.70 (*ns*)
Publication year							14.50***	
Pre-2000	16,972	13	.19	26.51	.0001	[.17, .20]		114.90***
2000 to 2010	13,287	16	.14	15.43	.0001	[.12, .16]		127.53***
Follow-up							16.23***	
6 months to 2 years	20.632	19	.19	25.40	.0001	[.17, .20]		231.84***
3 years to 5 years	8,478	10	.15	16.63	.0001	[.13, .17]		6.94 (*ns*)
5 years+	2,862	4	.14	7.70	.0001	[.10, .17]		1.95 (*ns*)
Sample size							22.42***	
60 to 199	2,270	20	.18	12.48	.0001	[.15, .21]		12.48***
200 to 499	1,477	4	.02	.65	.51	[−.04, .08]		2.90 (*ns*)
500+	26,512	8	.17	28.16	.0001	[.16, .19]		193.53***

Note: CBTR = Cognitive-Behavioral Treatment Review.
*$p < .05$. **$p < .01$. ***$p < .0001$.

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Table 3. Testing for Publication Bias: Trim and Fill Analysis.

	Studies trimmed	Point estimate	Lower limit	Upper limit	*Q* value
Observed values		0.16	.15	.18	258.72
Adjusted values	0	0.16	.15	.18	258.72

Finally, investigating publication characteristics, studies that were published by *CBTR* produced a lower effect size (r_{random} = .15) than those published or reported elsewhere (r_{random} = .19). The moderator analysis also revealed that the effect size for articles published before 2000 (r_{random} = .18) was higher than the effect size for those articles published from 2000 to 2010 (r_{random} = .14).

Table 2 also reports the effects sizes and *z*-test results for MRT across all levels of the nine moderator variables. There was a significant within-group difference between MRT-treated offenders and controls across 14 of the 23 levels of the nine factors. The two subgroups that did not generate a significant within-group treatment effect were studies that employed medium-size samples (200-499).

Trim and Fill

The Trim and Fill method relies on an examination of the funnel plot for asymmetry due to publication bias. The method gives an estimate of the effect of publication bias on the overall effect size, *r*. If the observed summary effect size remains substantially unchanged after allowing for publication bias, then this provides evidence that the observed effect size is robust and unaffected by publication bias. As seen in Table 3, the observed summary effect size is .16. After allowing for publication bias, the adjusted value ($r = .16$) as well as its confidence interval and *Q* value are substantially unchanged. Furthermore, no studies were trimmed from the original funnel plot indicating that the observed plot was symmetrical. The findings indicate that publication bias should have no effect on the inferences drawn from the meta-analysis.[3]

Discussion

As much of the research on MRT has been conducted by researchers who may be perceived as having a vested interest in this treatment modality, the present study was undertaken as an "arm's length" review of this treatment modality for offenders. We do not mean to imply that those who have a proprietor relationship with MRT or have been involved in its delivery have acted in a suspicious or unethical manner. To the contrary, they have applied the much-lauded scientist–practitioner model to their intervention and have done so more vigorously than have most providers of offender treatment programs. To this end, they are to be applauded for their efforts. In fact, we and others (Andrews et al., 2011; Gendreau & Andrews, 1979, 2001; Lipsey, 1999; Losel & Schmucker, 2005; McGuire, 2002) have argued that findings of stronger

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effects by researchers on their own products (instruments and programs) may not be related to suspect integrity or unethical standards. Rather, they may be a direct result of the fidelity of their intervention and research standards (in a positive way), such that these studies are more likely to be true "demonstration projects" (Lipsey, 1999) or "efficacy evaluations" (McGuire, 2002) of the program delivered as planned and not examinations of "routine practice" or "effectiveness evaluations" that have been migrated to another jurisdiction and delivered to a large offender base, sometimes throughout an entire correctional agency, generally producing weaker effects (Lipsey & Cullen, 2007).

The Effect of MRT on Offender Recidivism

In this meta-analytic investigation, we analyzed 33 MRT outcome studies that provided recidivism statistics for MRT-treated and untreated offenders. Recipients of MRT included adult and juvenile offenders who were in custody or in the community, typically on parole or probation. The study considered criminal offending subsequent to treatment as the only outcome variable.

The overall effect size measured by the correlation across 33 studies and 30,259 offenders was significant, indicating that MRT had a small, but important, effect on recidivism. In practice, the treatment effect represents an average recidivism rate of the MRT-treated offenders that was approximately two-thirds the rate of the untreated offenders. The Trim and Fill analysis, as well as the fail-safe analysis, indicated that publication bias is unlikely to be responsible for the significant findings in this collection of studies. These are relevant analyses to the current meta-analysis because of the relationship (described earlier) between MRT owners, MRT service providers, and MRT researchers. Hence, we have been particularly cautious in our follow-up analyses and interpretation. In sum, it is reasonable to conclude that MRT is an appropriate, empirically supported treatment (EST) for offenders when the goal is to reduce criminal offending.

Although an effect size of $r = .16$ might *seem* small, it can have dramatic practical effects. One is reminded of the Steering Committee of the Physician's Health Study Research Group (1987), which decided to end an experiment prematurely on the effects of Aspirin on reducing heart attacks (Rosenthal, 1990). The reason for the termination was that it had become abundantly clear that aspirin prevented heart attacks. In fact, it was so clear that it would have been unethical to continue giving half of the research participants a placebo. The magnitude of the effect size was $r = .034$.

Although the magnitude of the treatment effect is not considered large, it is, nevertheless, comparable with other ESTs in the field of offender rehabilitation. Fortunately, there have been dozens of offender treatment meta-analyses, not to mention systematic reviews of these meta-analyses (Lipsey & Cullen, 2007; Lipsey & Wilson, 1993; McGuire, 2002, 2008), to make such comparisons. For example, Andrews et al. (1990) in their classic meta-analysis of 124 intervention programs found an overall treatment effect of $r = .10$, suggesting that MRT is comparable with the outcome of programs that are generally offered to offenders. Losel and Schmucker

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(2005) examined 35 cognitive-behavioral treatment investigations (they appear not to be MRT interventions) and derived an odds ratio of 1.45, indicating that the recidivism rate of the untreated comparison groups was 45% higher than the offenders treated with CBT. This corresponds closely to our findings of offenders treated with MRT recidivating at a rate that was one third less than the comparison group offenders. In their review of Andrews et al. (1990) and seven other meta-analyses of offender rehabilitation treatment programs, Lipsey and Cullen (2007) cited 19 effect sizes ranging from .07 to .19 for adult and juvenile offenders in custody and community settings, again suggesting that MRT stands up well to correctional interventions in general.

Lipsey (1992) argued that relatively small effects can lead to meaningful reductions in community-level criminal behavior when programs are implemented on a large scale. In other words, a relatively small reduction in offending behavior by a large number of offenders will represent a large number of crimes prevented, and fewer crimes mean fewer tangible and intangible costs. More than a decade ago, Cohen (1998) estimated that one chronic juvenile offender could cost victims and society between 1.3 and 1.5 million dollars over their life of crime. Consequently, relatively small treatment effects translate into substantial cost savings. A program that costs $500,000 US to treat 100 young offenders will be cost-effective even with a success rate as low as 1%. However, in reality, success rates with MRT are substantially higher. In general, by the 10th year of release, MRT-treated groups tend to show actual rearrest and reincarceration rates about 20% to 35% lower than that observed by untreated controls (Shields, 2003). Aos, Phipps, Barnoski, and Leib (2001) have shown that MRT provides a cost benefit of $5,134 per offender ($k = 8$), which is comparable with the cost benefit calculated for Reasoning and Rehabilitation, another popular CBT manualized intervention ($4,653; $k = 6$; Ross & Fabiano, 1985), but less than Multisystemic Therapy (MST; Henggeler, Melton, & Smith, 1992), Functional Family Therapy (FFT; Gordon, 1995), and Aggression Replacement Training (ART; Goldstein & Glick, 1987).

The mean recidivism rates across these studies were dramatically different from those reported by Little et al. (2010). This appears to be a direct result of the different follow-up periods. Although the average follow-up time in the 39 studies in this meta-analysis was slightly less than 3 years, the results presented by Little et al. were from a long-term follow-up of 21 years. Graphic representation of their data suggests that the recidivism rates after 3 years were quite comparable with those of the current meta-analysis (i.e., about 26% and 41% for MRT-treated and untreated offenders, respectively). It can also be observed in their long-term study that treated and untreated offenders, after appearing to desist at about 6 years, continued to reoffend. During the second decade following their release, the recidivism rate for treated and untreated offenders increased by another 10% and 15%, respectively. Consequently, the treatment effect was maintained over the long term. This is also reflected in the effect size of .14 ($k = 4$) after 5 years in the present analysis.

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Moderators of Treatment

One is reminded that MRT is based on a manualized intervention with fixed content, format, and extent of service. Although this kind of treatment format should reduce variability in findings, it cannot eliminate it, in part, because other sorts of factors affect the research outcome. The magnitude of treatment effect sizes collected for the current investigation differed significantly across studies. This is not uncommon, even for well-defined programs. In their examination of 31 meta-analyses of offender treatment, Lipsey and Cullen (2007) found significant heterogeneity in each of these investigations. Lacking details that might reveal differences in the delivery of MRT between settings, our focus was on other kinds of potential moderators, each of which is important, but for different reasons. Some may relate to differences in effectiveness, whereas others may simply be a function of evaluation methodology.

The first set of moderators concerned the client group. Adults benefited from MRT more than youth. Although based on only five studies, the smaller treatment effect on youth is particularly interesting given the nature of MRT and its underpinnings in moral development. In fact, Kolhberg's theory is fundamentally a developmental stage theory whereby the most advanced stage does not begin to emerge until young adulthood. Quite possibly, adolescents are not yet sufficiently equipped, either cognitively or emotionally, to appreciate the finer nuances of a more sophisticated form of human relations.

The examination of MRT by setting is encouraging for service providers in institutions. Interestingly, this is the reverse of what has been found elsewhere, with greater treatment effects coming from community-based interventions. Andrews et al. (1990) reported mean effect sizes, r, of .11 for community treatment and .07 for institution treatment across youth and adults, whereas Lipsey and Wilson (1993) reported correlations of .13 for community treatment and .07 for institutional treatment. One can only speculate as to why MRT has been more successful in custody settings, where the conventional wisdom is that treatment services must contend with the overwhelming antisocial milieu that permeates most prisons. As described at the outset, MRT is a very intensive program. This may reduce the proportion of time that the offender is exposed to the prison subculture to such a degree that the intervention "takes hold."

With respect to gender, it is premature to conclude that MRT is effective with women offenders for three reasons. First, although women in the present study did benefit more from MRT treatment than men, the small sample of women (only two studies) precludes drawing any general conclusions about whether MRT is effective with women offenders. Second, Kohlberg's theory has been criticized as being overly male centered. Gilligan (1982) argued that the theory does not pay proper attention to the concerns of women because Kohlberg's initial research was based on empirical research involving only male participants. With moral development theory being the basis of MRT, the possible impact of any such difference on the effectiveness of MRT is unknown. In fairness, other research has found no significant differences in the

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moral development of males and females (see Colby, Gibbs, Lieberman, & Kohlberg, 1983; Walker, 1989).

Third, the specific responsivity factors that may affect treatment effectiveness with women must be explored (Hubbard & Matthew, 2008). Schlarb (2009) suggested that if women are provided gender-responsive programs designed to address female pathways to crime, such as substance abuse, poverty, and trauma, they might be even more responsive to treatment than men. Others have suggested that female delinquents have an "ethic of care," which differentiates them from their male counterparts, regardless of intervention (Watt, Frausin, Dixon, & Nimmo, 2000). CCI is engaging in ongoing efforts to research and develop female-specific components of MRT (Schlarb, 2009). The impact of these innovations should be of interest not only to proponents of MRT but also to proponents of gender-responsive interventions more generally (e.g., Blanchette & Brown, 2006; Heilbrun et al., 2008).

The second kind of moderator concerns design features of the evaluation studies. As such, they are investigated to determine the extent to which characteristics of the various evaluation methodologies were systematically related to the collection of effect sizes. These factors do not address the possible differential effectiveness of MRT. Rather, they assess whether different research designs are related to the estimate of effect size. Studies that randomly allocated participants to treatment and control conditions produced a larger effect size than studies that made some effort to match treated and untreated participants and those that used unmatched convenience samples. This finding was unexpected and contrary to findings in other meta-analyses. Traditionally, methodological rigor is associated with lower effect sizes (e.g., Andrews et al., 1990; Lipsey, 2003). However, Lipsey and Cullen's (2007) recent review of 20 meta-analyses found no systematic bias in relation to the quality of research design. Yet, it is of interest to note that the six reports in the current review that lacked sufficient information to classify their methodology generated one of only two nonsignificant effect sizes in this meta-analysis.

The relationship between sample size and effect size is more solidly established (Lipsey, 2003). As expected, studies in the current investigation that were based on samples of less than 200 participants generated stronger effect sizes that midsized studies with up to 500 participants. However, large studies with more than 500 participants generated equally strong effect sizes as the small studies. On reviewing the 12 large studies, it was revealed that 9 of them occurred in institutions, whereas both of the studies with women offenders were also large sample studies. However, the small number of large studies precluded any further breakdown of groups or multivariate analysis to isolate the independent contribution of study size and type of setting. Regardless, it is encouraging for MRT that the largest studies, presumably ones that Lipsey (2003) would refer to as *routine practice*, showed no decrement in treatment effectiveness when compared with the smallest studies, presumably the "research demonstration" projects. By comparison, Lipsey (1999) found that effect size of juvenile intervention programs, described as routine practice, was as little as one-half that found in research demonstration projects based on the same kind of intervention.

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The third kind of moderator concerned variations in the measurement of recidivism as the outcome measure. MRT produced a stronger effect in studies when the type of recidivism used in a primary study was rearrest rather than rearrest followed by conviction or reincarceration. However, there is no reason to believe that MRT actually worked better in these studies, although it is possible. Rather, it is more likely that rearrest provided a more accurate estimate of the dependent variable, any subsequent criminal behavior. Reconviction, on the other hand, is dependent on the vagaries of the court, which includes the expedience of the process such that the conviction is determined by the follow-up date, whereas reincarceration includes simple violations of community conditions, but misses offenses resulting in community-based sanctions.

As expected, the benefits of MRT were strongest when the follow-up periods were 2 years or less. Although Little et al.'s (2010) long-term follow-up demonstrated a sustained treatment effect over 21 years, the MRT studies collectively suggest a diminution in effect over time. However, any reduction in the treatment effect appears to occur between 2 and 5 years, after which it remains stable. In their meta-analysis of sexual offender interventions, Losel and Schmucker (2005) reported absolutely no relationship between follow-up time and treatment effect across 74 effect sizes ($r = .00$).

The last kind of moderator concerned the nature of the publication in which the research was reported. Studies that were published prior to 2000 produced a larger effect size than studies published between 2000 and 2010. This finding may be considered surprising and disappointing as one would prefer and perhaps expect the efficacy of any treatment intervention to increase over time as the program is updated and fine-tuned to meet the challenges faced in earlier offerings. Such should be the goal of any well-defined kind of offender treatment. However, the decrement in effect size with more recent offerings may be a reflection of the same issues described in the discussion of other moderators, such as research design and sample size. With time, interventions are transported to other agencies and locations, are delivered by newly trained staff, and may be expanded to capture a larger offender population, all of which is likely to affect the quality of service delivery.

The effect size for studies published by *CBTR* was smaller than for studies published in other independent sources. This is encouraging for MRT and should help to ally any concerns that correctional professionals might have about the possibility of an overstated case for MRT, at least in terms of offender outcome. Given our earlier commentary about potential publication bias, this finding should give any skeptics more faith in the reliability of findings collected by CCI.

MRT in the Context of Risk, Need, and Responsivity (RNR)

How does MRT stand up to the most powerful treatment programs that have been designed for offenders? For this review, we turn to the principles of RNR (Andrews et al., 1990). The *risk* principle asserts that criminal behavior can be reliably predicted and that treatment should focus on the higher risk offenders. The *need* principle highlights

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the importance of criminogenic needs in the design and delivery of treatment. The *responsivity* principle asserts that rehabilitation intervention should be cognitive-behavioral or behavioral and tailored to the learning style, motivation, abilities, and strengths of the offender. Interventions that adhere to these principles produce stronger treatment effects. This was demonstrated in Andrews et al.'s (1990) initial meta-analysis where 54 RNR-based treatments generated a treatment effect of .30 as opposed to the negative effect (−.06) of interventions that were void of all three principles. In further meta-analytic work, Andrews and Bonta (2010) have demonstrated that positive effect of treatment on offender outcome increases with the number of principles that are practiced, with the biggest increment occurring when two of the three principles are included. Others have demonstrated the importance of these principles to offender outcome across numerous settings within one U.S. state (e.g., Lowenkamp, Latessa, & Holsinger, 2006). From this perspective, the current collection of MRT studies does not place it in the upper echelon of ESTs.

Unfortunately, none of the reviewed studies made any mention of the RNR model. Consequently, we are left with the following conjecture. First, the delivery of MRT to high-risk offenders depends on local correctional policy and practice, which is something that cannot be controlled by the program, or its developers, per se. Moreover, descriptions of the selection process for participants, and their comparison offenders, was insufficient to determine the extent to which MRT was delivered to high and moderate risk offenders, although we have tried to make some inference from the recidivism rates of comparison offenders.

Second, in principle, MRT does address criminogenic need by virtue of its focus on moral development, antisocial attitudes, and inappropriate treatment of others. An important unanswered question from most MRT studies, however, is whether the selection of offenders to participate in MRT was based on an identified need in this particular domain for each selected offender.

Third, MRT is sufficiently cognitive-behavioral in its format to meet Andrews' definition of general responsivity (Andrews & Bonta, 2010), but program descriptions do not indicate that its delivery is sufficiently tailored to individual characteristics of the offender (e.g., cognitive ability, learning style, motivation, demographic attributes) to qualify for the specific responsivity principle of effective correctional intervention. Rather, like many "manualized" correctional interventions, it may be criticized for its cookie-cutter approach (e.g., Ward, Melser, & Yates, 2007) until we learn otherwise. In sum, it appears that at least one of the principles of RNR (i.e., criminogenic need) applies to most MRT interventions, but we cannot comment on the extent to which they do, nor which applications adhere to RNR more than others.

Critique of MRT as a Viable Offender Treatment

MRT lies at the crossroads between ethics and psychology, where issues such as moral reasoning, moral responsibility, psychological egoism, and moral character are featured. Most offenders are not psychiatrically disabled, amoral, or hopeless psychopaths.

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Rather, their moral development was somehow stunted at a lower, more child-like stage of development. However, with the proper treatment, many offenders can resume their natural development. Although MRT emphasizes the passions and formal moral reasoning, the latter is preeminent. Formal moral reasoning is the avenue whereby offenders modify beliefs, identify distorted thinking, and gain control over their passions. Yet, MRT is often criticized for being culturally specific, as well as too individualistic and rationalistic (although the individualistic and rationalistic critiques of Kohlberg's stage typology are more relevant to the classic formulation than they are to more recent, research-based revisions).

MRT is founded on Kohlberg's theory of moral development. A first criticism relates to the notion of cultural relativism; that is, the validity of Kohlberg's theory is mainly applicable within the limited context of Western culture. The main point is that, like much of Western culture, the theory is highly individualistic. Cultures with an individualistic orientation (e.g., United States, Canada, Western Europe) structure social experience around autonomous persons, who are capable of standing above society and community, and are motivated to attain freedom and personal goals (Wainryb, 2004). Within collectivist cultures, social experience is structured around such collectivities as family or community. The goal here is not to maximize autonomy per se; rather, the goal is interdependence. The point is that individualistic and collectivist cultures can be described as maintaining fundamentally divergent conceptions of morality (Shweder, Mahapatra, & Miller, 1987). Consequently, MRT would not ring true to the ears of someone raised in a collectivist culture or who emphasizes the priority of the group over the priority of the individual. In reply, this criticism would not apply to the majority of offenders in North America and Western Europe.

Second, MRT's emphasis is on individual change, where the individual promotes his or her rational goals and desires while mustering the strength to oppose those harmful external influences on one's interests, whether by family, friends, or any other group or institution. MRT may be criticized by some for individualizing criminality without taking into sufficient account how an offender's journey to desistance is obstacle-strewn, a journey often marked by poverty, underresourced communities, racism, lack of family support, or easy availability of alcohol and drugs. Thus, MRT can be criticized for being too agent focused (Shapland & Bottoms, 2011). The position is that offenders do not exist in a social vacuum, and their path to desistance is not simply an internal adjustment of routines and lifestyles. Rather, the desire to improve one's life is also shaped by the assorted obstacles an offender encounters on their journey to becoming full members of society.

To the extent that MRT emphasizes traditional notions of free will and personal autonomy, it may also be criticized for placing too much emphasis on mental functions and processes, and too little on the social milieu within which an offender interacts on release. It has long been held, and still is in many circles, that the problem with many offenders has less to do with their values and aspirations and more to do with the assorted obstacles that prevent them from expressing their conformist values and that offenders want to conform to convention, but are pulled in the opposite direction by

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various social pressures such as poverty (Merton, 1957; Shapland & Bottoms, 2011). Consequently, MRT is criticized for overpromoting independence and self-reliance while underemphasizing opposing external interferences on one's interests. However, these concerns have been directed at offender treatments generally and RNR-based treatments specifically (e.g., Birgden, 2004; Ward, Melser, & Yates, 2007), often with no regard for empirical evidence.

Third, Kohlberg's theory and MRT are strongly rationalistic, which is a problem for some observers. Formal reasoning is the means by which rational beings propose and consider explanations concerning cause and effect, true and false, what is good and bad, and right and wrong. However, some psychologists question the assumption that moral action is primarily a result of formal reasoning. Social intuitionists such as Haidt (2001) de-emphasize the role of formal reasoning in reaching moral conclusions. Instead, they argue that moral judgments arise mainly from fast and automatic intuitions with reasoning playing only a marginal role. In this perspective, conscious thought processes play little causal role. Instead, they are used mostly to construct post hoc justifications for moral judgments that have already been made. In other words, moral behaviors are generated by intuition and emotion and reasoning and willpower, although important, are secondary to the more affective-intensive processes. Consequently, for some observers, MRT's emphasis on formal moral reasoning is stripped of theoretical justification. In reply, we note that MRT is not unique in its focus on moral reasoning as a key to reducing criminal behavior. For example, other investigators have demonstrated the impact of using moral dilemmas in discussion groups with behaviorally disordered youth to reduce police contacts (Arbuthnot & Gordon, 1986)[4] and a moral education component in juvenile offender treatment to reduce recidivism (Gibbs, 1995).

Limitations of the Current Meta-Analysis

The main limitations to the present meta-analysis come from the lack of detail in many of the original studies. This included research methodology and program delivery, as well as important client characteristics such as age, race or ethnicity, and level of risk. What is known from criminological research on crime and age is that age is inversely related to criminality (Farrington & Coid, 2003). Although the present study distinguished between adult and young offenders, it would have been helpful to develop finer-grained age differences to explore how the effectiveness of MRT varies with age. The same can be said about race and ethnicity. Unfortunately, the large majority of reviews used in the present analysis did not provide details about the ancestry of offenders. Current efforts to treat Aboriginal peoples with MRT provide some hint about the therapy's potential. For example, the Anchorage, Alaska Wellness Court, was established in 2001 as an alternative for misdemeanor defendants who were charged with an alcohol-related offense. The program requires participation in MRT. Although some suggested that MRT would be less effective with Native Alaskans, findings concerning rearrest rates refuted the assumption. In fact, a report about the program concluded that

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"Native Alaskans have succeeded at a higher rate than other ethnic groups in the Wellness Court Program" (*CBTR*, 2004, p. 1). Turning to risk level, most of the reviewed studies did not mention offender's level of risk. Again, future studies would benefit by exploring how the effectiveness of MRT varies with risk level and, of course, "high-risk offenders" can be expanded to include an assortment of offenders such as violent offenders, sex offenders, or substance abusing offenders. Because the present meta-analysis did not distinguish between levels of risk or type of offender, there is no way to know how MRT interacts with level of risk or type of offender.

Concerning research methodology, some studies provided insufficient information even to assign the study to a location on a simple research design classification. Second, it is important to consider the issue of treatment drop-out rates. None of the studies or reviews released by CBTR broached the issue of drop-out rates. Early drop-out from treatment is a widespread problem, limiting overall treatment effectiveness, increasing the likelihood of relapse, and therefore recidivism. However, one potential influence of a relatively high drop-out rate in a treatment group is that the apparent effectiveness of treatment can become inflated. For example, if those who are in the strongest position to benefit from treatment are also those who are most likely to complete treatment, then a lower subsequent recidivism rate for completers might give an overly optimistic view of treatment effectiveness (King & Canada, 2004). If program noncompleters are also more likely to recidivate, then overall benefits of treatment—for example, reduced rates of recidivism—may become unfairly skewed in favour of program completers (see Fiorentine, Nakashima, & Anglin, 1999; Landenberger & Lipsey, 2005; McCaul, Svikis, & Moore, 2001).

Concerning program delivery, it was also difficult to locate studies that examined the different subcomponents of MRT. The therapy attempts to strengthen cognitive deficits in several areas, including self-control, critical reasoning, social perspective-taking, and interpersonal problem solving. Without studies that examine conditions before and after treatment, it is impossible to establish exactly which components of MRT might be most effective in improving moral reasoning. In other words, few studies provided any information about intermediate gains made through MRT. Hence, one is left with a "black box" evaluation of outcome (see Wormith et al., 2007) with little hint as to what progress was made on specific targets to realize the objective of reduced antisocial and criminal behavior.

The research design of the reviewed studies and the accessibility of the current search presented some concerns about the present meta-analysis. In particular, the fact that only 15% of the reviewed studies employed a randomized control trial might raise some question about the overall veracity of our findings. The key distinguishing feature of randomized trials is that study participants are randomly assigned to receive one or other of the alternative treatments under study. The most important advantage of proper randomization is that it minimizes allocation bias, balancing known and unknown prognostic factors, in the assignment of treatments. This feature, of course,

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reduces spurious causal inferences. In the present study, although only a small proportion of studies used randomization, the results nevertheless demonstrated that those reviews/studies that used randomization to allocate participants to conditions also produced the largest effect size. Second, our fears about the relatively few random assignment studies in this review are allayed by Lipsey and Cullen's (2007) conclusion that nonexperimental designs do not routinely produce spuriously high effect sizes. We were also concerned about the "file drawer problem" (Rosenthal, 1979; Rothstein, Sutton, & Borenstein, 2005), the possibility that we had not located other unpublished MRT evaluations with minimal or nonsignificant effect sizes. However, this concern was alleviated by the Trim and Fill and fail-safe analyses. Finally, any concern about researcher bias was alleviated by the finding that 41% of the primary studies used in the meta-analysis were derived from sources independent of CCI and, as reported, their mean effect size was slightly larger than for those studies derived from CCI.

Conclusion

Despite the preceding criticisms, MRT continues to gain ground. Overall, one million individuals have been treated with the method. MRT programs are used in 47 states (CCI, 2010). CCI reports success for a variety of offenders and problems, and in a variety of settings: Males and females, adult and juvenile; driving offenders; substance abusers; and domestic violence offenders have all been helped by MRT. The approach has been used by drug courts, pretrial service agencies, as well as probation and parole offices. MRT programs are used in correctional facilities in 47 states. Despite criticism, the advocates of MRT believe that they have developed an approach that is superior to other cognitive-behavioral approaches.

Although the overall mean effect size of MRT might seem marginal to some observers, it was statistically significant with potential for substantial social significance. In fact, its success rate is high enough that MRT has been granted "Evidence-Based Practice Status" by the Substance Abuse and Mental Health Services Administration (SAMHSA), the Oregon Department of Human Services, and the Florida Department of Juvenile Justice (CCI, 2010). The current meta-analysis is consistent with studies which show that MRT is effective in reducing recidivism. In our view, it warrants serious consideration by any correctional agency that has designs to influence the antisocial and criminal attitudes, behavior, and lifestyle of its clientele. We also encourage more detailed, descriptive, and analytic research on this meritorious mode of offender treatment.

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Notes

1. One of the developers of moral reconation therapy (MRT; Kenneth Robinson) remains president of Correctional Counseling, Inc. (CCI) to this day.
2. In his book, *Moral Development and Reality: Beyond the Theories of Kohlberg and Hoffman*, Gibbs (2010) explored the nature of moral development and social behavior. Gibbs elucidates the range of moral development from superficial perception to a deeper understanding through social perspective-taking.
3. Another useful technique to test for publication bias is based on the calculation of a "fail-safe" number (Rosenthal, 1979). Although there is not a standard fail-safe number, Durlak and Lipsey (1991) reported that meta-analytic findings are robust if the fail-safe number exceeds the critical value of $5k + 10$, where k is the number of studies used in the meta-analysis. In the present meta-analysis, the fail-safe number for the random model was $n = 524$, indicating that more than 524 studies would be required to raise the p value of alpha to more than .05; that is, approximately 524 similar-sized studies, each showing no effect, would be needed to be added to the meta-analysis to render the current effect size nonsignificant. Because the fail-safe number is considerably larger than the critical number (i.e., $5 \times 39 + 10 = 205$), it may be reasonably concluded that any publication bias is insufficient to account for the significant outcome findings of the present meta-analysis (see Durlak & Lipsey, 1991).
4. Gibbs' revisionist model is associated with an alternative, dilemma-free assessment method that has been used in cross-cultural research, the Sociomoral Reflection Measure–Short Form (Gibbs, Basinger, & Fuller, 1992). The measure assesses maturity of sociomoral reflection. Participant's maturity level is measured by scoring their justification for various moral behaviors such as promise keeping, telling the truth, helping parents, saving a friend, and obeying the law.

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