

WHAT WORKS CRIME REDUCTION SYSTEMATIC REVIEW SERIES

No. 13 A SYSTEMATIC REVIEW OF THE EFFECTIVENESS OF THE ELECTRONIC MONITORING OF OFFENDERS

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1. ABSTRACT

In this paper we systematically review the evidence on the effectiveness of the electronic monitoring (EM) of offenders. Guided by the EMMIE framework (Johnson, Tilley and Bowers 2015), the review seeks to: 1) establish whether the EM of offenders is associated with reductions in reoffending; 2) investigate how, in what form(s), and under what conditions the EM of offenders is found to be effective, ineffective, and/or produce unintended effects; and 3) summarise information on the implementation and costs of electronically monitoring offenders. Following a systematic search of the published and unpublished literature, 33 articles were identified that met the inclusion criteria. Of these, 17 studies contained quantitative data on the effectiveness of EM and were included in a meta-analysis. Our findings indicate that, across 17 studies, EM of offenders does not have a statistically significant effect on reducing re-offending. However, EM is associated with statistically significant reductions in re-offending for sex offenders (in 3 studies) and also when it is compared specifically to the alternative of prison (3 studies). Situational mechanisms (such as increasing the risk of getting caught) and behavioural mechanisms (such as encouraging pro-social behaviours) offer explanations as to how EM reduces reoffending. Further, there is some evidence to suggest that EM works better when combined with other interventions such as therapy or counselling. The success of EM ultimately depends upon how it is implemented in terms of availability of resources, technology, and buy-in from key stakeholders. Finally, EM as an offender management technique is found to be a cheaper alternative to prison but is more expensive than ordinary probation or parole. The implications of our findings and explanations for the observed variation in treatment effects are discussed, as are directions for future research.

2. BACKGROUND

Electronic monitoring (EM), commonly known as offender tagging, typically refers to a device being attached to an offender's ankle or wrist so as to track his or her whereabouts and can be monitored remotely to establish whether the individual is violating a set of pre-established conditions determined by the courts (Di Tella and Schargrotsky 2013). EM is used for adults or juveniles at three stages of the criminal justice process: as a condition for bail, as part of a community sentence or suspended sentence order (curfew orders), or to allow for the early release of prisoners (home detention curfews) (Hucklesby 2008). Curfew orders vary, but generally they require offenders to be present at a curfew address for a fixed number of hours per day for a pre-

determined period. The decision to recommend monitoring usually depends on a competent authority's assessment of offender suitability for EM.

Interest in the EM of offenders originated in the penal crisis of the 1970's and 1980s, when prison overcrowding and the escalating cost of incarceration was recognized as a major problem in the US and UK (DeMichele and Payne 2009; Paterson 2007; Renzema and Mayo-Wilson 2005). EM was designed, in part, to prevent the need for offenders to remain in custody, be that in pre-trial detention or during their entire sentence length, thereby allowing governments to reduce costs by providing cheaper alternatives to custody (Garland 2002, Hucklesby and Holdsworth 2016). EM has been linked to a myriad of outcomes other than cost-cutting, however. These include reducing reoffending through increased deterrence, acting as a rehabilitative tool by providing structure to offenders' lives and the opportunity to work, and even, sometimes, as form of punishment in community sentences (Hucklesby and Holdsworth 2016). Reducing reoffending is therefore only one possible outcome of EM. However, it must be stressed, it is the outcome on which this review focuses.

EM technology has developed considerably in recent decades. Initial systems in the 1980s involved bracelets on the ankle or wrist of the offender and a monitoring device fitted in the offender's home location which sent signals, alerting the authorities if the offender strayed too far from their home (Renzema and Mayo-Wilson 2005). The late 1990s saw the development of GPS monitoring systems logging offenders' movements, which had the capacity to allow the police to know their whereabouts at any time of the day or night. Recently, police forces in the UK have begun to use GPS technology to track prolific offenders on a voluntary basis and are rolling out 'sobriety bracelets' as alcohol monitoring devices in other pilot schemes (Hucklesby and Holdsworth 2016).

EM systems may be implemented alone, or as part of a suite of interventions including intensive supervision by parole or probation officers alongside drug treatment (Renzema and Mayo-Wilson 2005). All EM schemes have some private sector involvement, ranging from development and implementation of technology to partial or full control of administration of curfew orders. For example, in England and Wales until 2014 the national contract for EM was held by G4S and Serco, which was then taken over on an interim basis by Capita (Whitehead et al. 2013, Hucklesby and Holdsworth 2016). The contract, currently under negotiation and reportedly worth £2.9 billion

for a 6-year deal (*The Guardian* 2012), has been held up due to “issues with procurement including a scandal with the previous contractors, difficulties with the initial preferred equipment provider and subsequent delays with the introduction of new monitoring equipment” (Hucklesby and Holdsworth 2016:5).

A large number of jurisdictions utilise EM, including England and Wales (Mair 2005; Paterson 2007, Hucklesby and Holdsworth 2016), Scotland (Smith 2001, McIvor and Graham 2016), New Zealand (Gibbs and King 2003), Australia (Black and Smith 2003), Sweden (Marklund and Holmberg 2009), wider Europe (Haverkamp, Mayer and Levy 2004, Beyens and Roosen 2016, Boone, van der Kooij, & Rap 2016, Dunkel, Thiele and Treig 2017), Argentina (Di Tella and Schargrodsky 2013) and the USA (Austin and Hardyman 1991; Baumer and Mendelsohn 1991; Jones and Ross 1997; Lipner 1993). The reach of EM has extended to cover those on bail, adult and juvenile offenders, terror suspects, those suspected of breaching immigration laws, alcohol abstinence maintenance requirements and potentially, it has been suggested, could even be used to monitor those refusing to pay child support (Paterson 2007) as well as to monitor prolific offenders on a voluntary basis (Hucklesby and Holdsworth 2016).

Extensive use of EM and the sizable expenditure incurred by the public exchequer is evidenced by studies indicating EM contracts in the UK incurred an estimated cost of £117 million in 2011-2, with the introduction of 105,000 new tags and an average caseload of 25,000 (Geoghegan 2011, Whitehead et al 2013). A survey conducted by a charitable trust reported that in the USA, more than 125,000 people were monitored in 2015, as compared to 53,000 in 2005. The survey further reported that the number of GPS units rose by 140% over the time period 2005-15 to 88,000, but the number of RFID units fell 25% to below 38,000 (*Pew Charitable Trust Report* 2016).

In an era of austerity, cost cutting in public spending and ensuring value for money assume greater importance. In England and Wales, the Home Office have conducted several studies assessing the cost effectiveness of EM (National Audit Office 2006; Dodgson et al. 2001; Mair 2005; Mair and Nee 1990; Mair and Mortimer 1996; Shute 2007; Sugg, Moore and Howard 1991). One study showed that a 90-day curfew period with a tagged offender is around £5,300 cheaper than a custodial sentence of the same length (National Audit Office 2006). In another study, Yeh’s (2010) cost benefit analysis concluded that in the US, EM plus home detention could avert an estimated 781,383 crimes every year, and the social value of this reduction would approximately equal to

\$481 billion annually. Thus, every dollar spent on EM combined with home detention would yield social benefits worth \$12.70 (Yeh 2010). However, the National Audit Office (2006) cautioned that the effectiveness of EM depends on the quality of the performance of the contractors responsible for installing and maintaining equipment and monitoring offenders. This in turn might be related to costs and quality of implementation – findings that have been echoed in other studies included in our analysis below.

EM has been used for offenders committing a range of crime types, and its effects, specifically, on domestic violence offenders, other violent offenders, and sex offenders have been studied (Erez et al. 2012; Finn and Muirhead-Steves 2002; Payne and DeMichele 2011). Conditions associated with EM may differ for different types of offenders and offences. For example, sex offenders with tags may have geographic restrictions in not being allowed within a certain distance of schools, playgrounds and other areas in which children commonly congregate. EM can also be implemented at different points within the criminal justice system depending on the needs of the court or victims, for example, as part of a restraining order in the case of domestic violence offenders. It could also serve to assist offender re-entry into the community and there is some evidence that drug and/ or sex offenders on EM are more likely to complete treatment than other (non-tagged) offenders, which might be related to better re-offending outcomes (DeMichele and Payne 2009; Crowe et al. 2002).

EM is not without its critics. Ethical issues have been raised about the intrusiveness of the technology, its impact upon the offender and their family, and how it may adversely affect minority ethnic groups as a result of the shame associated with EM (Payne, DeMichele and Okufo 2009). Further, it is sometimes seen as a punishment by some offenders in terms of restrictions on lifestyle and stigma associated with it (Hucklesby and Holdsworth 2016). Technological issues such as the ability of offenders to remove tags, false alarms, and inappropriate implementation have also been documented (DeMichele and Payne 2009; House of Commons Committee on Public Accounts 2006). The commercialisation of EM due to government subcontracting remains contentious (Paterson 2007). While EM has the potential to enhance supervision of offenders in the community, its effectiveness is only as good as the technology. Moreover, increased supervision demands can cause supervisory officers undue stress and lead to other negative consequences (DeMichele and Payne 2009). Attitudes towards EM are not, therefore, exclusively positive.

One recurrent criticism of EM is with regard to net-widening – increasing the use of sanctions against individuals who may not have been sanctioned otherwise. Net-widening can happen at the ‘front-end’ of the EM process (at the time of sentencing), with individuals being sentenced to EM when they would not have been imprisoned otherwise and normal probation may have sufficed. These risks are of particular concern when low risk offenders are sentenced to EM, where existing community sentences would be more appropriate and the stricter conditions associated with EM are unnecessary (Padgett, Bales and Blomberg 2006). Net-widening can also happen at the ‘back-end’ of the criminal justice process (post prison), whereby the heightened surveillance of offenders on EM can lead to an increase in the likelihood of technical violations (missing curfew deadlines by as little as a few minutes) occurring and being detected, and the offenders then being sent back to prison. These technical violations are not a crime in the traditional sense, but nevertheless may result in the incarceration of offenders who would otherwise have been in the community on parole or probation.

The authors are aware of one previous systematic review of the effectiveness of EM (Renzema and Mayo-Wilson 2005). That review found only three studies that met its inclusion criteria, concluding that overall these studies showed no impact of EM upon recidivism rates after one to three years. There are several reasons why we believe an updated and extended review of the effectiveness of EM is warranted. First, the review of Renzema and Mayo-Wilson (2005) is now over a decade old. Since then, EM technology has improved considerably (DeMichele and Payne 2009). Second, Renzema and Mayo-Wilson’s review was limited to primary studies that a) were experimental or quasi-experimental in design and b) focused on moderate to high-risk offenders. Further primary studies evaluating the effectiveness of EM have since been published (for example Shute 2007; Bales 2010; Bulman 2013). Moreover, we contend that primary studies using different methods might usefully be included in any systematic review so as to strengthen the evidence base and inform decision-making in this area. Third, the previous review provides little information about the mechanisms through which EM might prevent crime, the contexts in which EM works best, and any implementation and economic considerations. EM has been sometimes marred by technological and implementation failures, on the part of private contractors in monitoring, as well as criminal justice agencies in responding to breaches, thus reducing the immediacy of sanctions and affecting its deterrent effect on offenders (National Audit Office 2006; House of Commons Committee of Public Accounts 2006; Shute 2007, Hucklesby 2013). This review will therefore

synthesise information on mechanisms, costs, contexts and implementation issues in the hope to aid practitioners and policy makers considering the use of EM.

Types of Electronic Monitoring Technology

There are various types of technology which can be considered EM (DeMichele and Payne 2009):

- Continuous signalling house arrest device. These are placed on the offender and emit a signal every minute or more, transmitting the offender's location to another device within the home to confirm their proximity.
- Mobile monitoring devices. These can be used by law enforcement officials who can then detect the location of an offender wearing a tag within a certain proximity. Officers can drive by offenders' homes and ensure they are present.
- Location tracking devices (or GPS). These usually comprise portable tracking devices that the offender must carry with them in combination with a tag worn by the offender. They are able to constantly record the location of the offender in almost real time. More recently single piece ankle devices using GPS technology have also been developed.
- Programmed contact systems (voice verification, video verification, device verification). These are automated calling systems designed to check if an offender is in a given location at a certain time. The offender may need to answer the telephone for voice recognition, be visible for video verification, or be close to device verification software.
- Remote alcohol/drugs detection devices (periodical or continuous). These continuously or periodically test for the amount of alcohol or drugs in an offender's system as the misuse of such substances may be part of the monitoring conditions.
- Victim protection devices. These can be located in a victim's home to alert them if an offender is within a certain proximity (most often used for domestic violence cases).

In addition, various devices count as physical offender tags: Verifier wristlets, anklets and bracelets, as well as different technologies such as RFID and GPS technology are used for EM.

3. THE APPROACH OF THIS SYSTEMATIC REVIEW

Johnson et al. (2015) propose EMMIE as a five-pronged framework to assess the quality and breadth of evidence generated by systematic reviews. The first ‘E’ of EMMIE examines information on the size of the ‘effect’ of a given policy, programme, practice or intervention. The first ‘M’ refers to ‘mechanism’ or an explanation of how a policy, programme, practice or intervention brings about any desired change. The second ‘M’ refers to ‘moderator’ and describes the conditions that need to be in place if the policy, programme, practice or intervention is to operate effectively. The ‘I’ refers to ‘implementation’ and examines information that can assist with ensuring planned activities are successfully completed. Finally, the second ‘E’ refers to ‘Economics’ which details how much an activity will cost in relation to outputs, outcomes or benefits.

To speak to all elements of EMMIE, the systematic reviews by Sidebottom et al. (2017a; 2017b) on alley gating and retail tagging combined two distinct and seldom integrated approaches to systematic review: meta-analysis and realist synthesis. This review adopts the same approach for EM of offenders. More specifically, questions concerning the effectiveness of EM will be examined using meta-analytic methods whereas questions concerning the other elements of EMMIE will be explored using realist review methods, with each branch of our review adopted different inclusion criteria. This process is described in detail below.

4. METHODS

This section begins with a description of the search strategy, inclusion criteria, a brief overview of the data extraction and management processes, and the statistical procedures used in the meta-analysis.

Search strategy

Our search strategy involved:

- 1) Keyword searches of relevant electronic databases, including grey literature and dissertation databases (see Appendix A and B);
- 2) A keyword search of publications by relevant government, research and professional agencies by an information specialist (see Appendix C)

- 3) Forward and backward citation searches of all studies which met our inclusion criteria.

Criteria for considering studies for this review

This review will focus exclusively on studies of EM that include some sort of tagging device worn by an offender, and hence we exclude studies relating to monitoring which is solely conducted via CCTV, telephone, human monitoring boxes, or those focusing on goods or places rather than offenders. The review also does not include studies of EM used for alcohol monitoring or victim protection devices i.e. the focus of the review is on offender monitoring as opposed to other aspects of surveillance. Crucially, the inclusion criteria for the realist synthesis differ from those for the meta-analytical branch of this review. For both styles of synthesis the following criteria was used when selecting studies for this review:

- a) *The study must have reported an explicit goal of reducing crime (i.e. re-offending) through the use of EM.* This meant that outcome data had to refer to a measure of law-breaking (or contact with the criminal justice system regarding that law-breaking), rather than just violations of conditions associated with EM. Studies that only reported violations of parole or EM conditions were hence excluded from this review. We included studies on EM implemented by any stakeholder: law enforcement, criminal justice agencies, public sector and private entities, etc. Studies that reported the effects of EM implemented in isolation or as part of a wider package of crime reduction interventions were also considered. Importantly, the study focuses on EM of offenders as opposed to goods or places.

To be included in our meta-analysis, a study has to satisfy point a) above and:

- b) *Have reported at least one quantitative crime outcome measure.* Outcome data could comprise official measures (e.g. reconviction or arrest data) or unofficial measures (self-reported levels of offending and/or victimization; breaches of curfew conditions). Studies that only reported non-crime-related outcome measures (such as perceptions of effectiveness and technological or ethical issues) were not included.
- c) *Have reported original research findings.* The quantitative findings for any single study were only incorporated once, even if reported in multiple publications. Where this was the

case, the study reporting the most detailed information was included or, where necessary, any dependency in the data was dealt with appropriately.

- d) *Employ a research design that permitted the computation of a reliable effect size* (i.e. an experimental or quasi-experimental evaluation design with control group or a suitable single study interrupted time series design).

Items *b*, *c* and *d* did not form part of the inclusion criteria for the realist synthesis. To be included in the realist synthesis of EM, studies had to satisfy point *a*) above - report an explicit goal of reducing crime through the use of EM - and report substantive information on *at least one* of the items below:

- e) the causal mechanisms activated by EM
- f) the conditions considered necessary for EM to produce its effects
- g) the implementation of EM programmes
- h) the costs associated with EM programmes

For items e to h, the inclusion criteria was more generous to ensure that any information that might usefully contribute to the theory, design, implementation and evaluation of EM was documented. Eligibility however was contingent on studies being primary evaluations and reporting some empirical evidence even if it was not numeric.

More generally:

- i) In light of the fact that EM technology has evolved considerably over recent decades, we included only studies published during or after 1999.
- j) Studies had to be available in English.

Identifying relevant studies

All information management for this review (including screening and data extraction) was done within the EPPI reviewer 4 software¹. Searches were completed in January 2016. A two-stage screening process was employed. The first stage involved the screening of title and abstract by one of three review authors to determine eligibility based on the aforementioned inclusion criteria. The second stage involved consulting the full texts of candidate studies to confirm eligibility. Forwards and backwards searching was completed for all eligible studies. Tests of inter-rater reliability were carried out after the initial screening stage and secondary screening stage (with an agreement rate of 92%). Any disagreements were resolved by discussion.

Data extraction and management

The data extraction instrument drew heavily from the offender-oriented ‘Review Guidelines for Extracting Data and Quality Assessing Primary Studies for Home Office Offender reviews’ (EPPI-Centre, 2007). This instrument was reorganised into the EMMIE framework and, based on a preliminary reading of a sample of EM studies, these codes were supplemented with additional ones that would help to answer the research questions of interest to this review. The data extraction instrument was largely formed when coding the included studies began, but when appropriate, codes were added inductively to capture or clarify understanding about aspects of EM (for example, a code to capture active vs. passive monitoring was added when this manifested in multiple studies)².

For the studies eligible for meta-analysis, two review authors independently extracted information according to the following six categories, where possible:

- 1) PROGRAMME DETAILS: title, date, funding source, publication status, aims of the intervention/evaluation
- 2) EFFECT:
 - a. The sampling procedure: the sampling frame, the method of selection, sample sizes, drop-outs, attrition

¹ See <http://eppi.ioe.ac.uk/cms/Default.aspx?alias=eppi.ioe.ac.uk/cms/er4>

² This data extraction instrument is available from the corresponding author.

- b. What treatment the control group(s) received and how they differed from the treatment group
 - c. Research design and statistical tests: research design, appropriateness of statistical procedure, when the outcomes were measured, which treatment group participants were included in the analysis, statistical/power size calculations
 - d. Measurement: the crime-related outcome/s, the data used to measure the outcomes, reliability checks of the outcome data
 - e. Results: the overall reported effect, the standard error of this effect and whether it was statistically significant, what type of effect size was reported, what type of data were reported, any indirect/unintended positive or negative effects, the conclusions of the study authors with regard to effect.
- 3) MECHANISM: were any mechanisms mentioned, empirically tested and were any intermediate outcome measures collected (such as offender compliance with curfews) that could be considered relevant to the noted mechanisms.
 - 4) MODERATOR: the dosage and duration of EM, the stage of the CJS at which the intervention acts, the country EM was implemented, the offender type in the intervention, the EM technology used, any (geographical, temporal or other) restrictions regarding the EM, whether EM was implemented in isolation or part of a suite of measures, whether there was 24-hour monitoring or not.
 - 5) IMPLEMENTATION: who was responsible for deciding eligibility to participate in the EM programme, who was responsible for implementing it, the resources required to implement EM, the training required to implement and sustain EM, the response to a breach in EM conditions, the enablers and barriers to success.
 - 6) ECONOMICS: the cost of the intervention, the cost of tagging individuals, the cost of equipment, the cost of personnel, the direct and indirect benefits, any cost-benefit analysis undertaken.

For the studies judged eligible for the realist synthesis, two review authors independently extracted information from points 2-6 above. The coding instrument³ contained binary codes, to represent the presence or absence of information and open question codes, so that a diversity of information could be collected.

The coding for each of the points 1-6 above was double coded and discussed and disagreements were resolved by discussion and, where necessary, through the involvement of a third review author.

Evidence appraisal of the studies

To account for varying levels of methodological rigor, all studies that were eligible for meta-analysis, and hence made causal inferences about the effectiveness of EM, underwent evidence appraisal, conducted independently by two review authors⁴. In addition to the usual assessment of study design (see Maryland Scale by Sherman et al. 1998), we wished to capture a number of other facets of research *execution* that might affect the internal validity for these studies.

Five domains were assessed: 1) statistical power; 2) sampling bias; 3) attrition bias; 4) data collection and 5) study design⁵. Inspired by the ‘Weight of Evidence’ scale devised by the EPPI-Centre (for best description, see Newman et al. 2012), we weighted these five domains. This resulted in the first four domain scores collectively having the same weighting as the fifth domain (study design). The evidence appraisal score was weighted thus in recognition of the importance of the study design as a proxy for strength of internal validity. Each domain was scored along a numeric scale, where 4 related to high quality evidence, 3 to medium quality, 2 to low quality and 1 to the information on the domain being unclear or not reported. Any disagreements were resolved through discussion with the research team. We acknowledge that our assessment of any presenting

³ The full evidence appraisal and data extraction tools are available from the corresponding author.

⁴ This considered a number of biases that could compromise the probity of the study and the strength of the causal claims. Many of these emerged from the data extraction instrument (see above), and were supplemented with codes from the Cochrane quality rating framework from controlled before and after designs (used by Steinbach et al., 2016) and the Crime Solutions Scoring Instrument (Crime Solutions ©).

bias may relate more to the descriptive validity of studies rather than their internal validity (Farrington, 2003).

Two groups of study were created based on the results of our evidence appraisal; those that produced an evidence appraisal score below the mean across all five domains were considered 'lower quality' and those that produced an evidence appraisal score above the mean score for all five domains were considered 'higher quality'. Moderator analysis was performed to see if there was an appreciable difference in mean effect across these two groups.

Quantitative data analysis

Meta-analysis was used to estimate the effectiveness of EM interventions overall, and for relevant subgroups of studies. First, the data reported in the eligible studies were extracted. Some studies had multiple treatment and control groups, used different outcome measures and/or used different statistical procedures to generate the reported data. For each of these conditions the quantitative data were extracted to construct effect sizes. The effect sizes were then converted into a common metric. For the proportional data - that is, those studies that reported the proportion of the treatment and control group re-offending after EM – odds ratios (ORs) and their confidence intervals were computed for each reported effect (see Appendix D for the formulae used). To assist interpretation, the ORs were converted to the *successful* outcome of 'not-re-offending' using the formula $1/OR$. For example, an OR of 0.8 for the unsuccessful outcome of re-offending was converted into $1/0.8 = 1.25$ for the successful outcome of not-re-offending. Hence, a significant effect size of over 1 would favour the treatment. The variance, and hence the size of the confidence intervals, remained the same after these conversions.

Studies that reported time to event (survival data), for example time to re-arrest or re-offend, were analysed separately. The most common metric used for quantifying the treatment effect in studies using these data was the (log) hazard rate. This metric was selected as it can be calculated from time-to-event data with censoring and measures the size of the difference between two Kaplan-Meier curves (Tierney, Stewart, Gheri, Burdett and Sydes 2007). The hazard ratio is calculated by dividing the hazard rate under treatment by the hazard rate under control. In short, it measures the change in risk of treatment versus control over the follow-up period (see Appendix D for the

formulae used). In this case, as the measure quantifies observed re-offending, a significant hazard ratio of under 1 would favour the treatment.

Meta-analytic models

For the meta-analytic models reported here, individual metrics were weighted using inverse variance weights when combining the effect sizes into an overall mean effect (see Appendix D). This standard practice (e.g. Lipsey and Wilson, 2001) ensures that more reliable effect sizes are given more weight in the calculations. Along with the overall mean effect size, individual effect sizes are presented with their associated confidence intervals.

To quantify the degree of heterogeneity observed, we computed a Q statistic in each of the samples of studies used for meta-analysis. This statistic is used to determine if any observed variation in effect sizes is likely to be above that which would be expected on the basis of sampling error alone (see Lipsey and Wilson 2001). A statistically significant Q statistic implies that there are systematic variations in effect sizes that cannot be explained by sampling error. Such differences might include some of the contextual factors revealed through the realist review of the evidence. Fixed effects models were used when the Q statistic indicated that the studies were homogeneous and random effects models were used when the Q statistic indicated that the studies were heterogeneous.

As part of the moderator analysis, weighted mean effect sizes were computed for meaningful subgroups. When the moderator variables produced a significant mean effect size, *and* reduced the observed heterogeneity in effect sizes across studies, this was considered to provide insight into (at least some of) the ingredients necessary for a successful intervention.

As mentioned above, there were two dependence structures in the data, 1) hierarchical effects, which referred to geographical areas being nested within studies, and 2) correlated effects, whereby multiple outcome data or follow-up periods were reported within studies. Both of these can downwardly bias the standard errors in a meta-analysis, and result in overly-conservative confidence intervals (Tanner-Smith, Tipton and Polanin, 2016).

Sensitivity analysis was therefore performed to determine if dependency in the data affected the results. For analyses with a sample of studies greater than ten we used robust variance estimator

as an adjustment method (Hedges, Tipton and Johnson, 2010). A different approach was taken for subgroup analyses, which all had small sample sizes. Here the best and worst case scenario were analysed – using (respectively) the most favourable and least favourable effect sizes (from a treatment group perspective) from a single study in the analysis. This was considered superior to using the mean effect across studies, which can become somewhat unstable with very small samples of studies. It also provides an upper and lower bound to our findings.

Publication bias

A well-documented issue that can compromise the reliability of the outcome of any meta-analysis is publication bias (see, for example, Kicinski 2014). Simply put, if evaluations that suggest positive outcomes of interventions are more likely to be published, there is a risk that any positive effect of intervention will be exaggerated. To determine the extent of any publication bias we first compared the mean effect size observed in published studies with that for unpublished studies. Next, we produced a funnel plot, displaying effect sizes against their standard error and used the trim and fill method proposed by Duval and Tweedie (2000) to re-estimate the effect size of intervention accounting for potential bias. The individual effect sizes should be distributed according to a normal distribution and fall, more or less, symmetrically around the overall mean when there is no publication bias. An asymmetric distribution of effect sizes suggests that publication bias is possibly present. In this scenario, the trim and fill method above imputes the effect sizes believed missing and recalculates the mean effect size based on the larger population of studies.

Outlier analysis

Outlying individual outcomes can distort the overall mean effect size estimate. Such outliers are particularly serious if they have extreme values and/or come from large studies that have been heavily weighted in the meta-analysis. Sensitivity analysis was performed to check whether the influence of such outliers might have a problematic influence over our results. This was achieved by visually assessing the presence of extreme values in the forest plots and calculating the mean effect size with and without the inclusion of the extreme value.

Realist analysis

A team of four researchers were involved in the realist review. A detailed code set was created to extract information on Mechanisms, Moderators Implementation and Economics, and was used to theorise how EM would work in reducing reoffending. Two authors visited the EM monitoring centre for the north of England and Wales and spent the day observing the operations and interviewing practitioners about the operational aspects of the implementation of, and possible mechanisms responsible for, EM as an effective crime reduction intervention. Resulting theories were then scrutinised and refined through regular group discussions. Logic models theorising how EM might work in order to achieve particular intended outcomes (often extending beyond reducing re-offending) were constructed to elucidate the way in which elements of EMMIE conceptually interlinked to explain how EM works and under what conditions.

5. RESULTS

Search results and screening

Our search tactics returned over 4,600 potentially eligible records (see Figure 1). A total of 373 records could not be excluded on the basis of their title and abstract and, consequently, the full texts were retrieved. Applying this additional criterion resulted in the full texts of 33 records being read in detail by at least one of the authors. 16 records did not report a quantitative effect size and were therefore retained for the realist analysis. This left 17 studies (reported in 19 documents) eligible for meta-analysis.

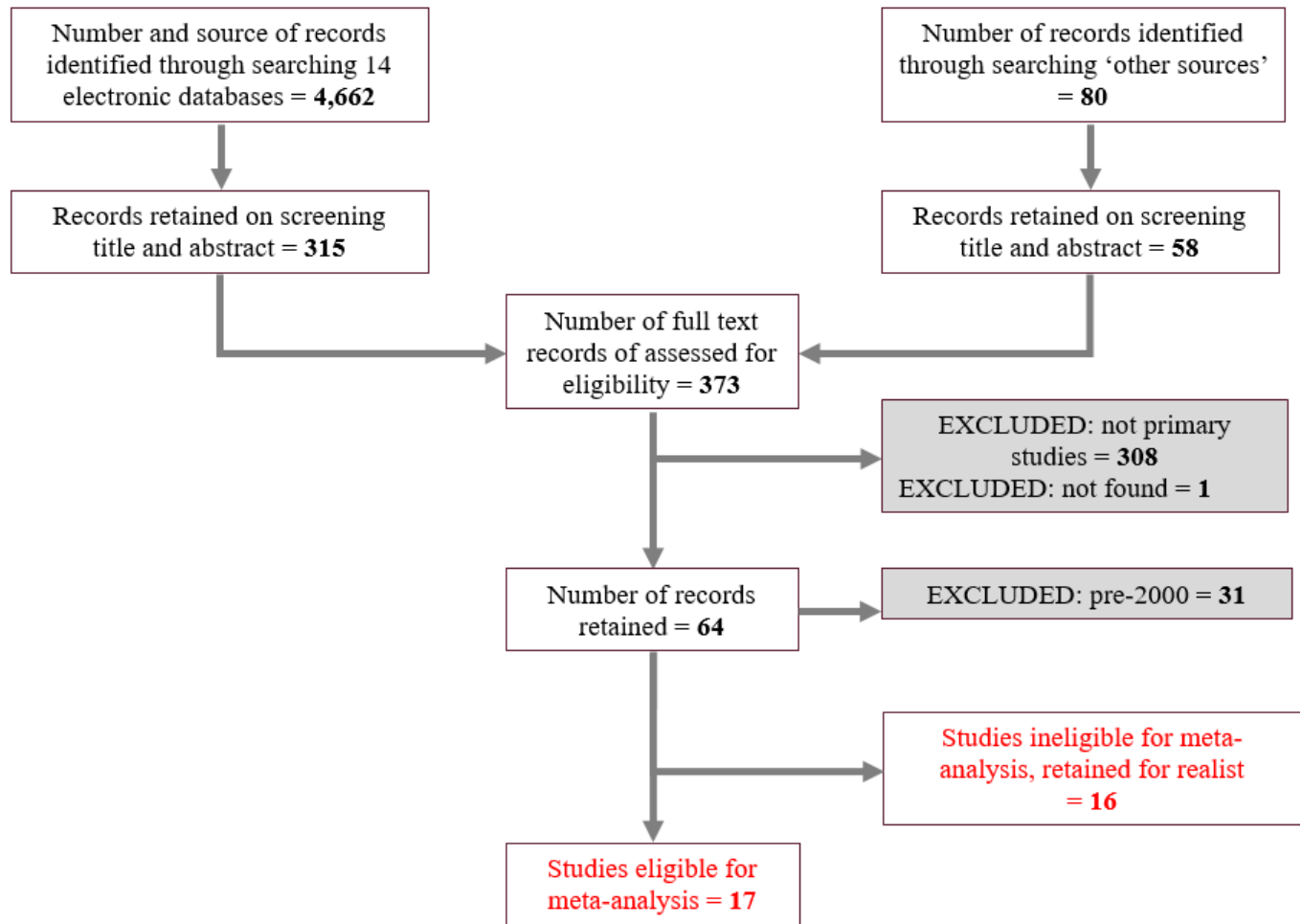


Figure 1 - flowchart of study selection

Profile of studies included in the meta-analysis

The 17 studies included in our meta-analysis differed in their methodological approach. The majority (88%) provided measurements of re-offending rates in the treatment and comparison groups *after* EM had been implemented. These took the form of various constructs of re-offending, such as self-reported offending (n = 1), recorded crime (n=1), re-arrest (n = 3), re-conviction (n = 1), re-imprisonment (n = 2) (see Appendix E). Only two of the 17 studies⁶ provided any information of offending rates prior to the implementation of EM.

⁶ Marklund and Holmberg (2009) and Pearson (2012).

A minority of studies (12%, $n = 2$) employed random assignment to minimise bias, and these used control groups that were unique to each study. The remaining studies were distributed across quasi-experimental designs with a comparable control group, or equivalent⁷ (53%) and quasi-experimental designs with a non-comparable control group (35%).

Three studies⁸ reported data for more than one geographic area. On closer inspection, implementers in each of these areas had responsibility for delivering EM and evaluators collected data specific to each area. The prevailing assumption from this was that the areas represented independent samples within each study; that is, participants were independent across each area within a study. However, as noted in section 4, this introduced some hierarchical effects into the structure of the data. In the results that follow we present these effect sizes first as if they were independent, and then conduct sensitivity analysis to validate whether the results are stable when the dependence structure is accounted for.

Evidence appraisal of the studies included in the meta-analysis

The results of the evidence appraisal assessment for all 17 studies⁹ are displayed in Table 1. The weight of evidence score ranged from 1.75 to 3.94, with the mean score equating to 2.64. Table 1 indicates that there were a small number of very high quality studies in terms of internal validity. Examples include the studies by Killias et al. 2010 and Lapham et al. 2007. The particularly strong features of studies such as these were that they had high quality research designs (these were the only two studies with randomized control designs), and hence were judged not to suffer from selection bias. They also did not suffer from high rates of attrition. Attrition bias is a common concern with offender-based interventions (Olver et al. 2001) and the majority of the studies in Table 1 either didn't report attrition information ($n = 7$) or had some attrition that was not adequately controlled for ($n = 5$). Lapham et al. (2007) was the only study to report sufficient statistical power, which earned them a higher score of 3.5 on this domain.

⁷ The equivalent design was a regression discontinuity design, with propensity score matching, used by Marie 2009 and Marie et al. 2011.

⁸ Baumer et al. (2008), Erez et al. (2012) and Sugg et al. (2001)

⁹ It is worth noting that the three geographic regions in Erez et al. (2012) were studied by different research teams, and the South region employed different methods and reported different data than the other two regions. For this reason we decided to treat each area separately.

	Statistical power and robustness	Sampling bias	Attrition bias	Data collection	Study design	Overall weight of evidence score
Bales et al., 2010	2.50	2.00	1.00	2.00	3.00	2.44
Baumer et al., 2008	2.50	2.00	1.00	1.67	2.00	1.90
Bonta et al., 2000a	2.50	3.00	2.00	3.67	2.00	2.40
Bonta et al., 2000b	2.50	2.00	2.00	3.33	3.00	2.73
Erez et al. 2012 (West)	2.00	3.00	4.00	3.67	3.00	3.08
Erez et al. 2012 (Midwest)	2.00	3.00	4.00	3.67	3.00	3.08
Erez et al. 2012 (South)	2.00	3.00	4.00	3.67	2.00	2.58
Finn & Muirhead-Steves, 2002	2.50	3.00	1.00	3.00	2.00	2.19
Gies et al., 2013	2.50	3.00	3.00	2.00	3.00	2.81
Killias et al., 2010	2.50	4.00	4.00	3.00	4.00	3.69
Lapham et al., 2007	3.50	4.00	4.00	4.00	4.00	3.94
Marie, 2009; Marie et al., 2011	2.50	3.00	2.00	3.67	3.00	2.90
Marklund & Holmberg, 2009	2.50	3.00	2.00	2.67	3.00	2.77
Omori & Turner, 2015	2.50	2.00	1.00	3.67	3.00	2.65
Pearson, 2012	2.00	3.00	2.00	3.00	3.00	2.75
Roy & Barton, 2007	1.50	3.00	1.00	1.67	2.00	1.90
Sugg et al., 2001	2.00	1.00	2.00	2.67	3.00	2.46
Tennessee Board of Probation and Parole, 2007	2.00	2.00	1.00	1.00	2.00	1.75
Turner et al., 2010; 2015	2.50	3.00	1.00	3.67	3.00	2.77

Table 1 – The results from the evidence appraisal assessment. Mean weight of evidence score = 2.64

Note: Scores for individual dimensions equated to judgements of 4) high quality; 3) medium quality; 2) low quality and 1) unclear from reporting. Some dimensions had multi-part answers, thus explaining fractional scores.

At the other end of the spectrum were three studies of weaker internal validity. These include Baumer et al. 2008, Roy and Barton, 2007 and Tennessee Board of Probation and Parole, 2007, all of which were quasi-experimental designs with a non-comparable control group. These studies did not report key information, such as attrition and how their data were collected and over what period, meaning that a clear assessment of data quality could not be made. Due to concerns about statistical conclusion validity (and no reporting of sample size/statistical power) Roy and Barton (2007) received a score of 1.5 for this domain. The other two studies were judged to have medium statistical conclusion validity. All three of these studies suffered from between-group bias between the treatment and control groups, but only Roy and Barton (2007) attempted to acknowledge and adjust for this sampling bias.

EFFECT: Meta-analysis of the impact of electronic monitoring on re-offending

The overall effect of EM has been derived in several ways, whilst ensuring that only comparable effect sizes are ‘pooled’. Five types of data and statistics were reported across the 17 studies¹⁰. Some studies reported multiple types, with two studies¹¹ reporting raw data, logistic regression and hazard ratios. Table 2 displays a summary with the total number of studies reporting each type of data or statistic in the top row and the sub-set of studies which also had other types of data denoted beneath. Two studies had *just* hazard ratios and eight studies had *just* raw or proportional data. Please note that the numbers do not equate to the totals because of the studies reporting multiple data types.

From Table 2 it can be seen that raw data and proportions predominated, with hazard ratios that reported time-to-event (re-offending) being the second most common data type. Four studies reported logistic regression results alongside other data types¹². Means and standard deviations of re-offending frequency was reported alongside proportional data in one study (Pearson 2012). Lastly, one study solely reported regression discontinuity estimates (Marie 2009). No

¹⁰ Raw data were converted into proportions of re-offending across different groups.

¹¹ Finn and Muirhead-Stevens 2002 and Turner 2010; 2015.

¹² One of which was Finn and Muirhead-Stevens 2002 which reported logistic regression, hazard ratios and proportional data

correspondence between study design (e.g. RCT) and outcome data was apparent from the included studies.

	Hazard ratios	Means and standard deviations	Logistic regression	Raw data/ proportions	Regression discontinuity estimates
Total studies with data type	5	1	4	13	1
Subset with hazard ratios	2		2	3	
Subset with means/ s.d.				1	
Subset with logistic regression	2			3	
Subset with raw data/ proportions	3	1	3	8	

Table 2 – A summary of data types reported across the 17 studies eligible for meta-analysis

Since by definition meta-analysis requires multiple effect sizes from more than one study, the results that follow only report on two of these outcomes: hazard ratios and proportions. These were the only two categories where there were more than two studies (the other outcomes were not comparable over studies).

Overall effect using hazard ratios

Figure 2 shows results of studies that report survival data (hazard ratio). It shows that all of the effects across these five studies are non-significant, and indeed, the fixed-effects model overall weighted mean effect size of 0.777 is also non-significant (confidence interval: 0.578 – 1.045). This indicates that, when the hazard ratio studies are considered as a whole, electronic monitoring had no statistically distinguishable effect on time to re-offending.

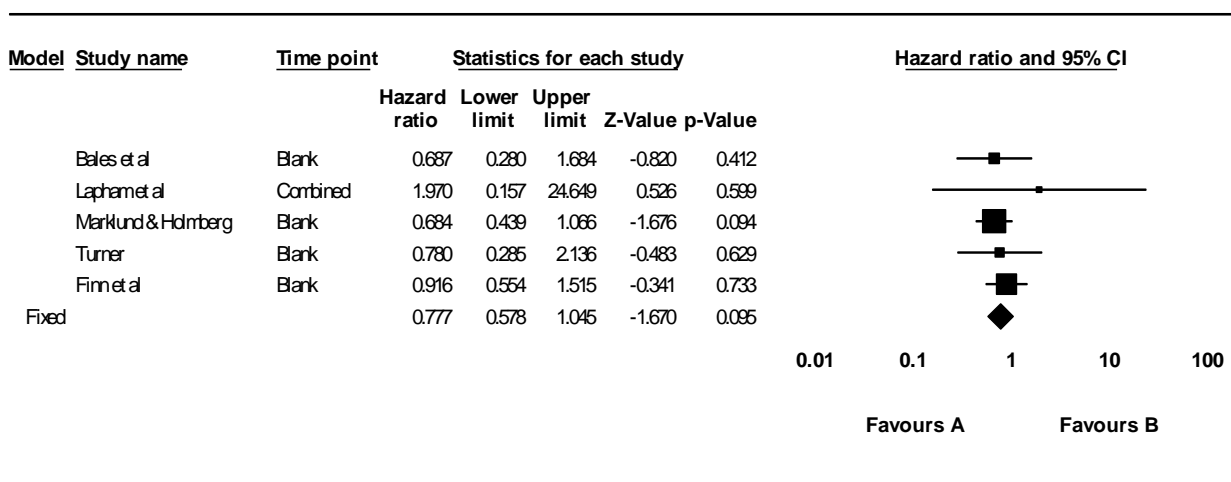


Figure 2 - Forest plot of the effect sizes from studies reporting hazard ratios. Blank = cross-sectional design, combined = repeated measures. A = treatment, B = control

Overall effect using proportions of re-offending

For each study that reported their results in proportions of re-offending in both the treatment and comparison groups, we first estimated the effect of intervention by computing an odds ratio and a confidence interval for each geographic area in the way described in the methods section. The outcome measure chosen from each study was the most stringent estimate of re-offending, so that (say) re-imprisonment was selected over re-arrest (see Appendix E for a hierarchy of different constructs). We also computed an overall weighted mean effect size using a random effect models¹³.

¹³ An estimate of the heterogeneity observed across studies ($Q=58.16$, $df=18$, $p<0.01$) indicated that the use of a random effects model was warranted.

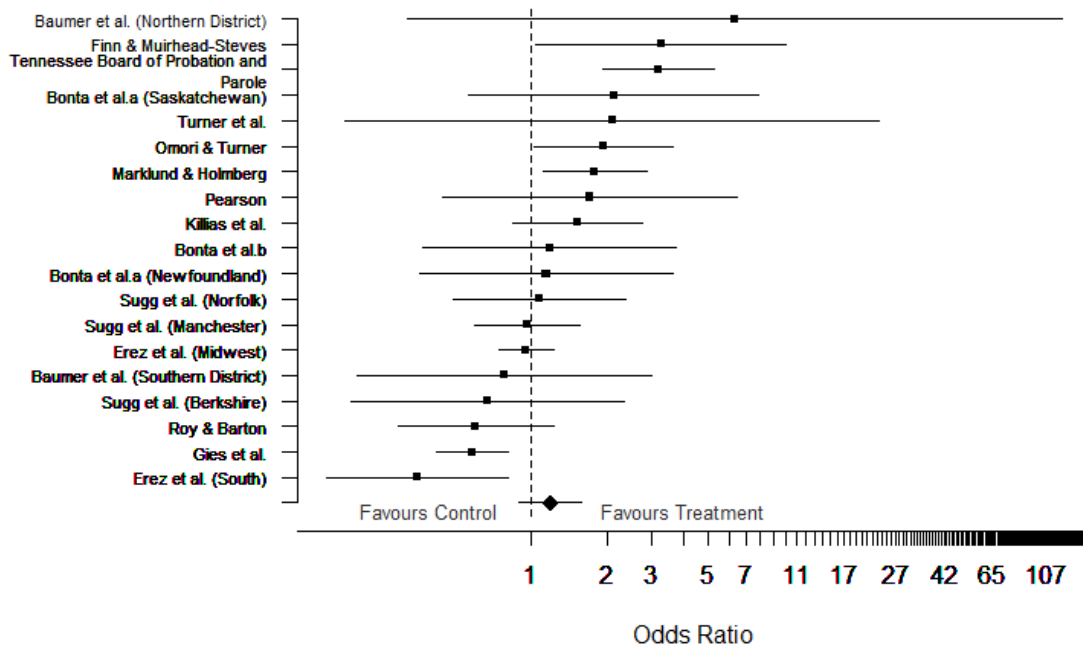


Figure 3 - Forest plot of the effect sizes from studies reporting proportions, for each geographic area, using the most stringent measure of re-offending

The overall weighted mean size in Figure 3 is shown at the bottom with confidence intervals around the black diamond. Since the lower bound of the confidence interval straddles the vertical reference line of 1, the result is non-significant (ES: 1.19, CI: 0.89 – 1.59). Many of the ORs from the individual studies (13 of the 19 effect sizes) are similarly non-significant. Excluding the Baumer et al. (2008) (Northern District¹⁴) outlier did not appreciably change these results¹⁵. Applying robust variance estimation to the studies that contributed multiple effects from different geographical areas similarly produced statistically comparable results¹⁶. This indicates that, when the proportion effect size studies are considered as a whole, electronic monitoring is found to have no statistically distinguishable effect on re-offending rates.

¹⁴ This covered the St. Joseph counties in the US state of Indiana.

¹⁵ A random effects model was justified for this sensitivity analysis ($Q=56.77$, $df=17$, $p<0.01$), with a weighted mean effect size of 1.17 (CI: 0.88-1.57).

¹⁶ Robust variance estimator generated an overall weighted mean effect of 1.22 (CI: 0.85 – 1.75).

Assessing the influence of publication bias

Almost a third of the EM studies used in the meta-analysis could be considered ‘grey literature’ (n = 5) since they were published outside of academic outlets. Moderator analysis using this subgroup of studies produced a non-significant weighted mean effect (0.9, CI: 0.63-1.3, see Appendix F for forest plot). Removing the outlier from Baumer et al. (2008) (Northern District) did not make an appreciable difference to the results.

We next created funnel plots to visualise the distribution of the effect sizes from each geographical area around the mean value (these are included in Appendix F). The results of the trim and fill procedure suggested that two studies were missing. The original point estimate in log units was 0.177 (which, when exponentiated to an odds ratio, was 1.19), confidence intervals -0.12 – 0.47 (exponentiated to 0.89 – 1.59). The adjusted point estimate is 0.120 (OR: 1.13), with confidence intervals of -0.05 – 0.29 (0.95 – 1.33). These results indicate that accounting for possible missing studies does not seem to affect the initial conclusions of the meta-analysis; that is, the adjusted mean effect size persists in demonstrating a non-significant effect of EM on re-offending. The initial and adjusted point estimates are very similar in magnitude. For this review, therefore, it appears that publication bias is not a major concern. This was confirmed by a regression test for funnel plot asymmetry undertaken on the two imputed studies that demonstrated no significant differences between this and a symmetric plot ($t = 1.14$, $df = 17$, $p = 0.269$).

Assessing the influence of evidence quality

Based on the aforementioned evidence appraisal exercise, studies were divided into those of ‘higher’ and ‘lower’ quality evidence, which referred to the strength (or lack thereof) of the internal validity within the studies. All studies with an evidence appraisal score above the mean (which was 2.64) were assigned the status of ‘higher quality’ and those that had a score below the mean were deemed ‘lower quality’. The forest plots for the meta-analytic results for these two subgroups of studies are included in Appendix F. In brief, the weighted mean effect from the higher quality studies¹⁷ was approaching parity at 1.03 (CI: 0.70-1.53) and was non-significant. As might be

¹⁷ Both the higher quality and lower quality subgroups exhibited significant heterogeneity (respectively these were $Q=28.1$, $df=6$, $p < 0.01$ and $Q=24.1$, $df=10$, $p < 0.01$) and thus warranted random effect models.

expected, the lower quality studies produced a higher weighted mean effect size (1.34, CI: 0.87-2.08), but this remained non-significant. Neither of these subgroup analyses were altered by the best and worst case scenario sensitivity analysis¹⁸.

Assessing the influence of alternative controls

Across the 17 studies included in our meta-analysis, the effectiveness of EM was determined through comparisons with various *different* control groups. The majority of evaluation studies (n = 8, with 11 geographical regions) used control groups that received a ‘business as usual’ treatment, which was typically a package of probation conditions, either attached to early release or as an alternative to imprisonment. Several studies had multiple treatment and control groups, some of which represented business as usual and others that represented a deviation from usual (e.g. no treatment or more intensive treatment than normal). From inspection it was discerned that some form of community sentence was used as a control for three studies¹⁹ (across five independent geographical regions). Two studies did not provide clear information on the control group²⁰. The two subgroups of studies that could be used in moderator analysis were thus ‘business as usual’ and ‘community sentences’, both of which used proportional data. Neither demonstrated a statistically significant effect (see Table 3) and the sensitivity analysis to check for issues with data dependency did not alter these results²¹.

¹⁸ For higher quality studies this was ES=1.05 (CI: 0.61-1.82) for the worst case scenario and ES=1.18 (CI: 0.80-1.72) for the best case scenario. For lower quality studies this was ES=1.56 (CI: 0.78-3.12) for the worst case scenario and ES=1.53 (CI: 0.84-2.80) for the best case scenario.

¹⁹ For example, Killias et al. (2010) used community service, Roy and Barton (2007) used day reporting centres and Sugg et al. (2001) described their control group as receiving “combination and community service orders”.

²⁰ Pearson (2012) and Tennessee Board of Probation and Parole (2007)

²¹ For business as usual the RVE results were 1.14 (CI: 0.64 – 2.04). For the community sentences the worst case scenario was ES=0.97 (CI: 0.61-1.55), for the best case scenario ES=1.05 (CI: 0.71-1.56).

Treatment received by the control group	Type of model	Q statistic	Mean ES	CI
‘Business as usual’ (11 observations)	Random effects	33.3, df=10, p<0.01	1.13	0.78-1.64
Community sentences (5 observations)	Fixed effects	4.31, df=4, p>0.1	0.99	0.74-1.34

Table 3 – Summary of meta-analytic results for EM versus alternative treatment

Assessing the influence of outcome measurement

A range of outcome measures were reported in the 17 studies included for meta-analysis (see Appendix E for details). As specified in our inclusion criteria, all of these included a measure of re-offending. Some of the studies also, and *separately*, reported violations of parole or EM conditions. We performed moderator analysis on those categories of outcome measures that had at least three studies. All of these used different forms of proportional data.

As Table 4 and Figure 4 illustrate, a significant weighted mean effect was observed for studies using reconviction and re-imprisonment data (1.35, CI: 1.08-1.69), and this was verified by the sensitivity analysis using worst and best case scenarios²². This suggests that EM is an effective crime reduction measure when measured through this type of outcome data. This was a particularly interesting finding, as these outcomes could be considered the most serious measure of re-offending. No effect was found for re-arrest outcome data or parole violations, even when sensitivity analysis was performed²³.

²² For worst case scenario, ES=1.53 (CI: 1.17-2.00) for best case scenario ES=1.53 (CI: 1.19-1.98).

²³ For re-arrest data, worst case scenario, ES=0.79 (CI: 0.32-1.97) and best case scenario ES=1.20 (CI: 0.87-1.66). For parole violations, worst case scenario, ES=0.85 (CI: 0.56-1.29) and best case scenario ES=1.58 (CI: 0.86-2.91).

Outcome	Type of model	Q statistic	Mean ES	CI
Re-arrest (6 studies)	Random effects	21.8, df=5, p<0.01	0.87	0.59-1.29
Reconviction/ re-imprisonment (7 studies)	Fixed effects	8, df=6, p>0.1	1.35	1.08-1.69
Parole violations (8 studies) [#]	Random effects	69.5, df=7, p<0.01	1.14	0.62-2.09

[#] When specified, these were technical violations, otherwise they were considered generic

Table 4 – Summary of meta-analytic results for EM evaluations by outcome data

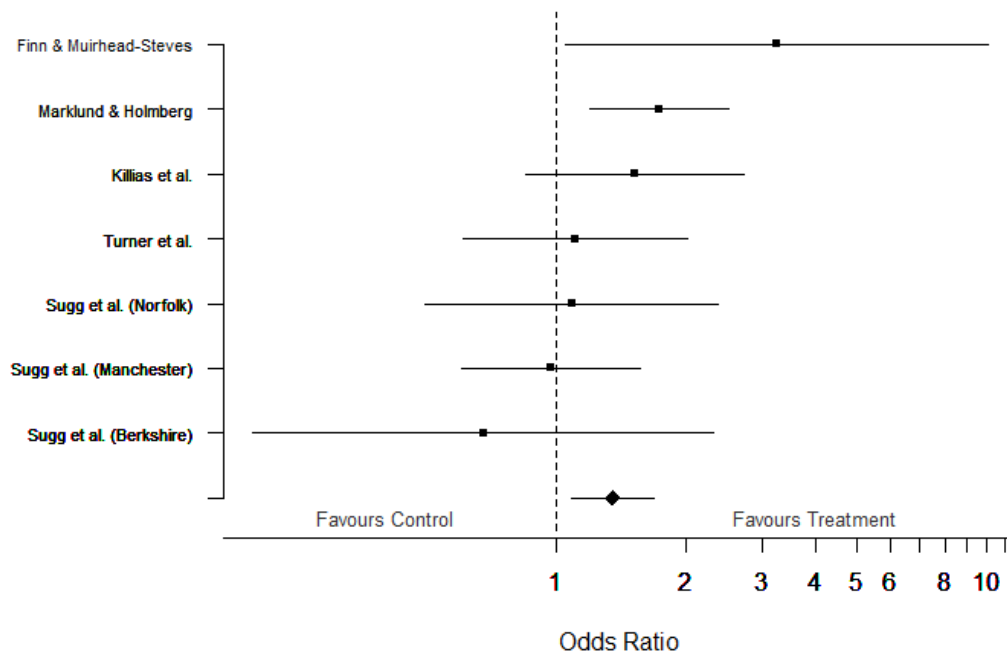


Figure 4 – Forest plot of the studies using reconviction / re-imprisonment outcome data

Assessing the influence of follow up period

Whether EM has short-term and/or long-term effects is noteworthy, and has implications for both the plausibility of different mechanisms and the cost-effectiveness of the intervention. To elaborate, if EM is only effective in reducing reoffending during the period the offender is on EM, then it can be seen as a cost-effective alternative to a custodial sentence, and might imply that there is exclusively a situational mechanism at play. If, however, the effects of EM extend beyond the

EM period – into the months and years that follow the EM treatment – then this suggests that it has a more long-lasting effect in facilitating desistance from offending behaviour. This latter scenario might be more plausibly linked to behavioural and social mechanisms (see next section for a more detailed discussion).

Six of the studies in our sample provided no information on the time period over which the effects of EM were evaluated, which meant that it was not possible to discern whether the effects of EM on reoffending were being measured exclusively for the EM period, or for a follow up period. Some studies collected data beyond the intervention time (i.e. the time the offender was on EM), which was treated in this analysis as a follow-up period. Follow-up periods, when reported, ranged from 150 days to three years.

Six studies reported only one follow up period, and four studies reported multiple follow up periods. Only three of these studies reported raw or proportional data. Moreover, the three studies were not sufficiently similar to analyse quantitatively. To elaborate, Erez et al. (2012) examined the effect of EM on participants who were pre-sentence as opposed to the others which focused on participants post-sentence.

Summary of effect findings

The meta-analysis on the impact of electronic monitoring on levels of re-offending has therefore demonstrated that when considered as two complete data sets using comparable statistical constructs (odds ratios calculated from proportions and hazard ratios) those with EM did not show differences from the control groups. Evaluation of the effectiveness of EM is complicated by the wide variety of outcome measures, control groups and time scales that are employed. Most of the moderator analysis used to explore this found similar outcomes to the wider meta-analysis- that there was no evidence of a statistically significant effect of EM. However, it appears that when analysing re-conviction or re-imprisonment as the outcome measure, there is some evidence that EM demonstrates a positive effect compared to controls. It also appears that there is little evidence that EM becomes effective in the short-term at reducing negative outcomes. There was, unfortunately, insufficient data to undertake moderator analysis exploring the effect of study design, since the two RCTs used different outcome data to one another. Note that in this section statistical moderator analysis has concentrated on matters of internal validity and evaluation

design. Further statistical moderator analysis exploring the role of contextual variations in determining effect sizes will be presented later in the section titled Moderator.

MECHANISM

A number of potential mechanisms by which EM may reduce crime are elucidated in the 33 studies judged eligible for realist review. Twenty-eight of these studies make some reference to mechanisms in varying levels of detail (see Table 5 for details). Mechanisms mentioned in the studies can be split broadly into situational mechanisms and behavioural or social mechanisms. Situational mechanisms, refer to the ways in which manipulation of the immediate environment can bring about reductions in crime, specifically through: increasing the risk, increasing the effort, removing excuses and reducing provocations. Some of these are inferred from the description of the programme and its implementation, rather than being specifically identified by the primary study authors as situational mechanisms. The behavioural or social mechanisms included in the studies refer to, for example, building better relationships with family members, increasing stability in the offender's life (through the structure enforced by the programme such as, gaining and maintaining employment, and participating in therapy), an increase in self-discipline, and being protected from the negative effects of incarceration. Some of these mechanisms are not directly related to the actual devices used to monitor offenders but relate to the wider conditions that participants must meet in order to be on the EM programme. In this section we discuss each group of mechanisms in turn.

Situational mechanisms

EM is a form of situational crime prevention (SCP) (Clarke, 1997). It follows that the common mechanisms through which situational measures are known to produce their effects were also observed in the EM literature reviewed here. The most common situational mechanism mentioned (24 studies) was increasing the risk of offenders being caught if they broke, or attempted to break, the law or the conditions of their monitoring programme. Formal surveillance was strengthened since the offenders' location could be monitored either partially when they were under curfew at home, for example, or round the clock if they were on active GPS monitoring, reducing the potential for anonymity as their whereabouts could be ascertained at all times ((Frost 2002; Turner et al. 2015).

Study	Increasing risk	Increasing effort	Removing excuses	Reducing provocations	Family contact	Employment	Therapy	No drugs/ alcohol	No prison
Armstrong & Freeman (2011)		x	x						
Avdija & Lee (2014)	x								
Bales <i>et al.</i> (2010)	x	x			x	x	x		x
Baumer <i>et al.</i> (2008)	x		x						
Bonta <i>et al.</i> (2000a)	x						x		x
Bonta <i>et al.</i> (2000b)	x				x		x		x
Dierenfeldt (2013)	x	x							
Erez <i>et al.</i> (2012)	x	x			x		x	x	
Finn & Muirhead-Stevens (2002)	x				x	x	x		x
Finn <i>et al.</i> (2001)	x		x						
Florida (2004)	x								
Frost (2002)	x								
Gies <i>et al.</i> (2013)	x	x	x					x	
Gowen (2000)	x								
Harig (2001)		x	x					x	
Hudson and Jones 2016	x				x	x			x
Jannetta, (2006)	x		x				x		
Killias <i>et al.</i> (2010)				x	x				
Lapham <i>et al.</i> (2007)	x		x			x	x	x	
Marklund & Holmberg (2009)	x					x	x	x	
Mortimer (2001)			x	x	x				
Nestleroad (2012)	x								
Omori & Turner (2015)	x	x							x
Pearson (2012)	x	x	x	x	x		x		
Roy <i>et al.</i> (2007)									
Shute (2007)	x	x							
Sugg <i>et al.</i> (2001)									
Tennessee (2007)	x	x						x	
Turner <i>et al.</i> (2010)	x	x						x	
Turner <i>et al.</i> (2015)	x	x						x	
Total	23	12	9	3	7	4	9	8	5

Table 5 - Mechanisms through which EM produces effects by studies

Baumer et al. (2008:18) noted that offenders ‘were not very excited about the possibility of having their whereabouts monitored around-the-clock’ – since an offender’s account of their location at a particular time could be verified or refuted by the GPS system data produced by their device.

Rather than being used to replace supervision by supervisory officers, EM is designed to enhance this supervision, providing information to these officers and judges in cases where the offender is believed to have breached the terms of their monitoring or committed a crime, and effectively extending the network of guardianship over an offender. Offenders themselves are well aware of being tracked. Bales et al.’s. (2010: 132) survey found that 54% of offenders wanted to follow the rules of the EM programme because they felt that they are being watched, while 82% of parole officers felt offenders were following the rules because ‘they know they are being monitored and can be caught easily’ (ibid: 133). It is possible to infer from this that offenders can feel that the risks of being caught are higher when part of an EM programme than if they were not.

As well as increasing the risk of being caught, some studies suggested that EM increased the effort required to commit offences and avoid detection. In order to breach EM conditions to commit a crime, the offender would have to remove the EM device, usually attached to an ankle bracelet, or somehow subvert the system to trick the receiver into thinking that the offender was in one place while they were actually in another. Although offenders have been known to use a number of ‘excuses’ to ‘explain’ why their receiver was not showing they were in the correct place, (such as lack of signal, technical faults, being in the bath, being in the back garden, or unplugging the receiver to do the Hoovering²⁴), EM systems are designed to alert the provider if the ankle bracelet or the receiver is tampered with (Gies et al. 2013; Loble 2000; Tennessee 2007). Furthermore, ankle bracelets are typically robust and hard to cut or destroy, and are not affected by water (Florida 2004; Gies et al. 2013). These measures, therefore, make it difficult for the offender to subvert the system and those that try often get caught, as their ‘excuses’ are rarely justifiable. Similarly, the implementation of exclusion zones for offenders increases the effort required to find suitable targets, for example, ensuring that particular sex offenders cannot be within a certain distance of places in which children congregate (Omori and Turner 2015), or preventing domestic violence

²⁴ Personal communication with EM provider (UK).

perpetrators from coming into contact with places where their victims may be found (Erez et al. 2012).

Another way in which EM may prevent reoffending or technical breaches is by removing excuses which offenders could use to justify their behaviour. EM programmes have a set of rules which are explained to offenders at the start, in order to assist compliance (Erez et al. 2012; Harig 2001; Mortimer 2001), which includes not re-offending and abiding by the EM conditions. In some cases, offenders sign contracts stating that they have read and understood these rules and agree to abide by them (Armstrong and Freeman 2011) especially, in cases where EM is imposed as a stand-alone condition or as a pre-condition for bail. Other programmes that have supervision as part of EM conditions, involve regular updates and meetings with supervisory officers to ensure that these rules are being followed and to reinforce them where necessary. Flexibility in terms of adapting curfews to suit employment patterns have been found to aid compliance (Bales 2010; Erez et al. 2012; Mayer 2003) both by ensuring that the offender is able to be gainfully employed, thus not having the time or need to commit crimes, and also by ensuring they are able to abide by the rules and avoid technical breaches. One programme (Baumer et al. 2008) sent offenders instantaneous notifications of breaches by text, to encourage future compliance. Other programmes (Erez et al. 2012; Harig 2001; Lapham 2007; Turner et al. 2010) insist on abstinence from drugs and alcohol and enforce this through regular and/or random testing, further removing excuses which offenders may try to use for non-compliant or undesirable behaviour. This can also work in combination with behavioural and social mechanisms as discussed below. While this particular example is not linked to EM itself (the monitoring anklet or bracelet does not prevent an offender from consuming illicit substances), it can be part of the wider EM programme.

Reducing provocations is the final situational mechanism which may be activated by EM in order to reduce re-offending. By enforcing curfews and exclusion zones, EM may assist in neutralising peer pressure by removing the influence of criminogenic settings and/or criminogenic peers (Mortimer 2001; Killias et al. 2010). On the other hand, however, EM may lead to an increase in frustration and stress, by confining individuals to their home for long hours and enforcing increased contact with family (Bales et al. 2010; Lobley and Smith 2000; Pearson 2012). It can also lead to difficulties in gaining and maintaining employment due to the stigma attached to wearing the EM ankle bracelet and accompanying restrictions (Bales et al. 2010; Erez et al. 2012;

Mayer 2003; Mortimer 2001; Pearson 2012; Tennessee 2007). This could lead to *increasing* rather than *reducing* provocations, which could ultimately lead to a backfire effect and see an increase in crime or undesirable behaviour, although no study explicitly reported this outcome.

Behavioural and social mechanisms

Many of the behavioural and social mechanisms discussed within the studies in this review are not linked directly to EM itself, but to the wider programme involving EM. Nevertheless, they pertain to re-offending as an ultimate outcome and are, therefore, worth mentioning. While the use of curfews leading to additional time spent in the home may increase stress in some individuals, others undoubtedly benefit from more contact with family members in pro-social settings. Developing better relationships with family members was mentioned by many studies as having a positive effect on the offenders' lives and behaviours, including reducing re-offending (Erez et al. 2012; Finn and Muirhead-Stevens 2002; Killias et al. 2010). EM also provides more stability and structure in offenders' lives, through enforcing curfews and deadlines, and through the need to gain and maintain employment, which is often part of the requirements of the programme (Finn and Muirhead-Stevens 2002; Hudson and Jones 2016; Lapham et al. 2007; Mortimer 2001).

The EM programme can involve a mandatory therapeutic component, with offenders required to attend therapy as part of their EM conditions (Pearson 2012). Ensuring that offenders receive such assistance, including physical and psychological therapy to help with addictions, can encourage and reinforce pro-social behaviours through the broader EM programme. By removing some of the triggers for offending, including substance abuse and anti-social behaviours, it is hoped that re-offending will be reduced. Finally, offenders on EM are protected from the negative effects of incarceration, either completely, if they are placed on EM rather than being imprisoned, or partially, if EM allows for early release from incarceration (Bonta et al. 2000b; Finn and Muirhead-Stevens 2002; Hudson and Jones 2016; Omori and Turner 2015). Avoiding the influence of criminogenic people and prison conditions might be helpful for reducing reoffending rates for these offenders (Finn and Muirhead-Stevens 2002).

Thus, studies included in the review identified a host of situational and social mechanisms by which EM programmes work. Some of these mechanisms were specifically identified as such by the authors of the studies, but others have been inferred. Increasing the risk of getting caught

appears to be the most dominant mechanism mentioned relating to EM programmes, but other social mechanisms associated with wider EM programmes, such as greater time spent in pro-social situations with family members, the benefits of employment, and enforced abstinence from drugs and alcohol could all contribute towards the goal of reducing re-offending.

MODERATOR

The term ‘moderator’ in this section refers to the conditions that are relevant to the activation of mechanisms through which EM produces its effect, or the extent of any effects observed (see Table 7 for a summary of the moderators reported). To elaborate, an intervention may trigger different causal mechanisms in different conditions, leading to variations in outcome patterns (or effect) (see Pawson and Tilley, 1997). Hence moderator/context, as used here, relates to real variations in causal processes created by application of EM and does not include sources of outcome variation to do with research methods or measurement instruments. The statistical moderator analysis in this section of the review is therefore theoretically driven to generate specific hypotheses about how different contextual conditions under which EM is implemented might trigger varying mechanisms and result in particular outcome patterns. Thus, there is close correspondence between moderator and implementation, which trigger the quantity and direction in which particular mechanisms may be triggered.

Moderators or contextual factors have an impact on the effects of an intervention by triggering causal processes identified in the mechanisms section. Based on the 33 studies included in our realist review, contextual factors that could potentially impact on the effect of EM can be categorised as follows:

- 1) Location
- 2) Offender type
- 3) EM technology
- 4) EM conditions – this refers specifically to-
 - a. Dosage and duration of EM, and
 - b. Whether EM is applied on its own as part of a package of interventions
- 5) Timing of the intervention in the criminal justice process.

Assessing the influence of location

Of the 16 studies included in the meta-analysis, eight were situated in the USA, three in Canada, and three in Europe (with one UK study covering three geographic areas). Of the remaining 17 studies included for the realist synthesis, 10 were located in the USA and 6 in the UK. The location of one study was not clear. Overall, the study descriptions indicated that the location of the intervention might be an important contextual factor influencing the effectiveness of EM because of differences in legal regulations and requirements resulting in differences in institutional arrangements about who would be responsible for deciding whether an individual is suitable for being on EM, for day to day monitoring of subjects, for responding to breaches in EM and more generally, socio-political attitudes towards EM as an offender management technique.

The results of the meta-analysis for the USA and Canadian subgroups of studies are summarised in Table 6 (see Appendix F for forest plots), and show that all the weighted mean effect sizes were non-significant. These results were unchanged by the sensitivity analysis exploring the dependency in the data²⁵. The three European studies were deemed too dissimilar, in terms of the jurisdictional differences between the countries, to warrant pooling in meta-analysis.

Study setting	Type of model	Q statistic	Mean ES	CI
USA (11 observations)	Random effects	54, df=10, p<0.01	1.05	0.70-1.57
Canada (4 observations)	Fixed effects	0.66, df=3, p>0.1	1.44	0.78-2.66

Table 6 – Summary of meta-analytic results for study setting

²⁵ For the USA studies RVE adjustment produced ES=1.06 (CI: 0.47-2.40). For the Canadian studies the worst case scenario was ES=1.30 (CI: 0.65-2.59) and the best case scenario was ES=1.58 (CI: 0.77-3.27).

Study	GPS	RFID	Pre-trial	Instead of prison	After prison	24 hour	Specific period	Low/ medium risk	Sex/ violent/ high risk offender	Geographic restrictions	Stand alone	Package
Armstrong (2011)	x				x	x			x	x		
Armstrong et al (2011)					x		x					x
Avdija (2014)								x				
Bales (2010)	x	x			x		x	x	x	x		x
Baumer (2008)	x	x			x	x			x	x	x	
Bonta (2000a)				x				x				x
Bonta (2000b)								x			x	x
Dierenfeldt (2013)					x				x			
Erez et al. (2012)	x	x	x			x	x		x	x	x	x
Finn (2001)		x			x	x			x	x	x	
Finn (2002)					x		x		x			
Florida (2004)	x	x	x			x	x		x			x
Frost (2002)	x			x		x				x		
Gies et al. (2013)	x				x	x			x	x		x
Gowen (2000)		x	x	x	x	x				x		
Harig (2001)	x	x		x								x
Hudson & Jones (2016)	x								x		x	
Jannetta, (2006)	x				x	x			x	x		x
Killias (2010)		x		x			x	x		x		x
Lapham (2007)		x		x			x			x		x
Lobley (2000)				x			x			x		x
Marklund (2009)				x			x		x	x		x
Mayer (2003)												
Mortimer (2001)		x			x		x	x		x	x	
Nestleroad (2012)					x							
Omori (2015)	x				x	x			x		x	
Pearson (2012)	x				x				x	x		x
Roy (2007)				x							x	
Shute (2007)	x	x		x		x	x		x	x		x
Sugg (2001)							x				x	x
Tennessee (2007)	x				x	x			x	x	x	
Turner (2010)	x				x	x			x	x	x	
Turner (2015)	x				x	x			x	x	x	

Table 7 - Moderators mentioned in the studies

Assessing the influence of the type of EM technology

The influence of technology refers to whether monitoring systems use RFID or GPS technology, and whether monitoring is active (continuous monitoring) or passive (EM data is inspected retrospectively at a particular time of the day). While the use of GPS technology is both costlier and more resource intensive than RFID (see Economics section below), they both entail their own sets of implementation issues which will be discussed in the next section. Of the studies included in our review, five reported the use of RFID technology, ten studies reported use of GPS technology and six studies reported that both RFID and GPS were used. It is noteworthy that 12 studies (six of which were included in the meta-analysis) provided no information on the EM technology used.

We tested the impact of both technologies on measures of re-offending for studies included in the meta-analysis²⁶ and found a non-significant impact on re-offending for each - see Table 8. These results were not altered by the best- and worst-case scenario sensitivity analysis²⁷. One study had two U.S. regions where GPS technology was compared to RFID, but this was also non-significant. Disappointingly, the data did not allow for more nuanced testing of the efficacy of active monitoring compared to passive.

Technology	Type of model	Q statistic	Mean ES	CI
GPS (9 observations)	Random effects	47.8, df=8, p<0.01	1.02	0.66-1.59
RFID (7 observations)	Fixed effects	10.9, df=6, p>0.1	1.04	0.78-1.38

Table 8 – Summary of meta-analytic results for type of EM technology

Assessing the influence of EM conditions

Certain conditions imposed in an EM programme might create a context that could have an impact on the mechanisms triggered and, consequently, the effectiveness of the intervention. Two main kinds of conditions imposed as part of the EM regime were identified:

²⁶ Meta-analysis with proportional data included four studies (from seven geographical areas) for RFID, six studies (covering 10 geographical areas) for GPS and three studies (across five geographic areas) had both, although only one (Erez et al. 2012) compared these data across different groups which precluded moderator analysis.

²⁷ For GPS this was ES=1.09 (CI: 0.49-2.43) for the worst case scenario and ES=1.43 (0.78-2.61) for the best case scenario. For RFID this was ES=1.07 (0.49-2.31) for the worst case scenario and ES= 1.13 (0.91-1.41) for the best case scenario.

- a. Dosage and duration of EM:** Offender monitoring could occur over varying lengths of time and either be continuous (24 hour) monitoring or for fixed time curfew periods (most often 7pm to 7am). Decisions about whether an offender would be monitored continuously or for a fixed period were dependent on the type of offender, period deemed acceptable for the risk level, and technology available for EM. Further, the period offenders were on EM saw a huge range with no evidence to indicate the optimum period an offender should be on EM. For instance, some offenders could be on EM, post sentencing after prison, from 0 to 727 days with an average of 204 days (Gies et al. 2013) or from 1 to 30 months with an average of 10.8 months per offender (Armstrong 2011). On the other hand, Killias (2010) reports that offenders could be sentenced to be on EM for up to 3 months instead of prison. Few studies reported or speculated on the logic behind specific recommended duration of EM if the intended aim was to reduce re-offending and not just to reduce costs of keeping them in prison.

One study reported that the longer youths were placed on EM, the less likely were their chances of completing the programme successfully (Pearson 2012) but did not explain why, while another study reported that keeping low risk offenders for long periods on EM can have a backfire effect – that is, it can lead to increases in re-offending (Tennessee 2007 citing Lowenkamp and Latessa 2005) again, without explaining why.

- b. Standalone or a package of interventions:** 12 studies included in this review appeared to have EM as a standalone intervention in at least one location. On the other hand, 16 studies reported EM being part of a suite of measures put in place in an offender management programme. These included, for example, an intensive rehabilitation programme (Bonta 2000a; Jannetta 2006); alcohol abuse treatment and surprise breath tests (Lapham 2007; Harig 2001); counselling and therapeutic interventions (Pearson 2012); and community orders or other service conditions (Sugg 2001). Unfortunately, from the data in the studies, the effect of these programmes cannot be isolated from the measurement of overall effectiveness of EM. Bonta et al. (2000a; 2000b) point out that EM increases compliance with other treatment modalities, insofar that it maximises the benefits of these programmes by reducing the likelihood of offenders dropping out, and therefore could be considered a moderator towards treatment effectiveness. The moderator analysis involving these two subgroups did not generate any significant

effects (see Figure 25 and Figure 26) and these were unaffected by the sensitivity analysis using worst and best case scenarios²⁸.

Assessing the influence of the timing of EM in the CJS process

EM was prescribed at three points in the criminal justice process in the eligible studies; pre-sentencing, post sentencing instead of prison and post sentencing after prison. Moderator analysis was conducted for all of these groups of studies. As mentioned in the section on follow up periods, Erez et al. (2012) evaluated the effect of EM in the pre-sentence period. The weighted mean effect for the three geographical regions from this study was marginally non-significant at the upper-bound (see Table 9), indicating that there was potential for the (domestic violence) offenders on EM to re-offend more during this period than their counterparts, who were classified as being on ‘bonded time’. It was not possible to verify this result through sensitivity analysis since all three effect sizes in this moderator analysis originated from the same study.

When EM was implemented as an *alternative* to a prison sentence, a statistically significant effect was found for a reduction in re-offending (see Table 9 and Figure 5). This moderator analysis did not warrant sensitivity analysis since the three studies contributing the effect sizes were independent. Whilst the number of studies in the meta-analysis would encourage caution in the reliability of this finding, it is still noteworthy. One putative hypothesis for this effect is that the pernicious effects of the prison environment and contact with other prisoners is avoided (Finn and Muirhead-Stevens 2002). A non-significant effect was observed for the weighted mean for the studies using EM post-prison (Table 9), and again the effect sizes contributing to this analysis were all independent.

CJS timing	Type of model	Q statistic	Mean ES	CI
Pre-sentence [#] (domestic violence) (3 observations)	Random effects	7.4, df=2, p<0.05	0.71	0.50-1.02
Post-sentence (instead of prison) (3 observations)	Fixed effects	0.49, df=2, p>0.1	1.62	1.15-2.30
Post-prison (7 observations)	Random effects	38.3, df=6, p<0.01	1.69	0.82-3.5

²⁸ For standalone studies, worst case scenario, ES=1.19 (CI: 0.61-2.35) and best case scenario ES=1.37 (CI: 0.83-2.27). For studies that had EM as a package of measures all contributing effect sizes were independent.

[#] The analysis contained three individual regions from the Erez et al. (2012) study.

Table 9 – Summary of meta-analytic results for EM timing in CJS process

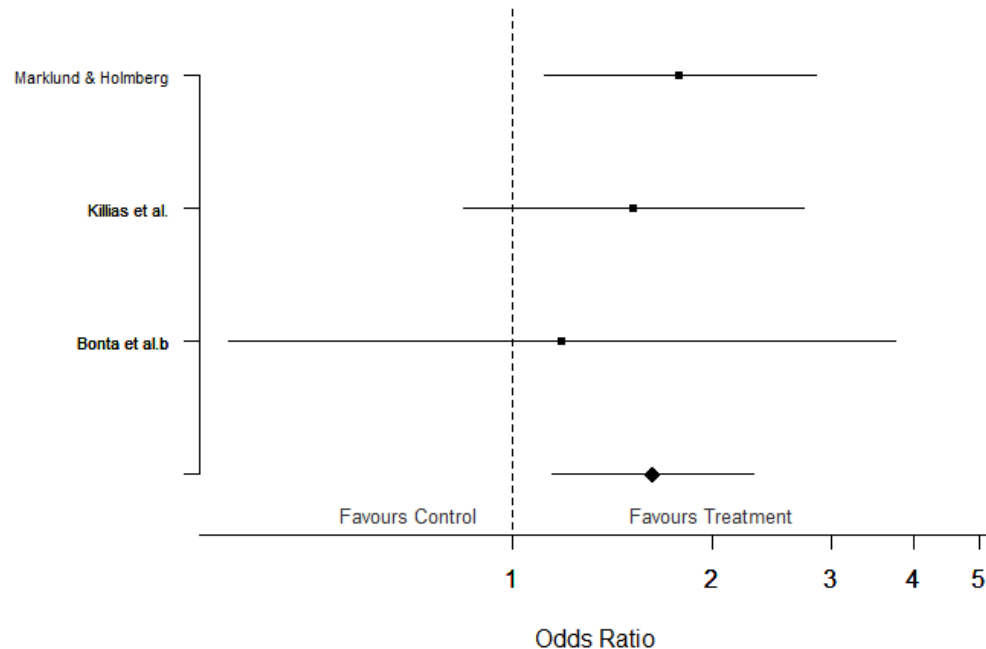


Figure 5 - Forest plot of the studies where EM was implemented post-sentence (instead of prison)

Assessing the influence of offender type

The studies included across the whole review examined high (11 studies), medium (two studies) and low risk offenders (one study), and further included sex offenders (12 studies), violent offenders (seven studies), drink drivers (two studies), domestic violence offenders (two studies), gang members (two studies), drink drivers (2 studies), vehicle thieves (one study), as well as non-violent (one study) and juvenile delinquents (one study). 13 studies included unspecified offenders. Some studies included multiple types of offenders but these were not comparable across studies (for example, with varying age bins).

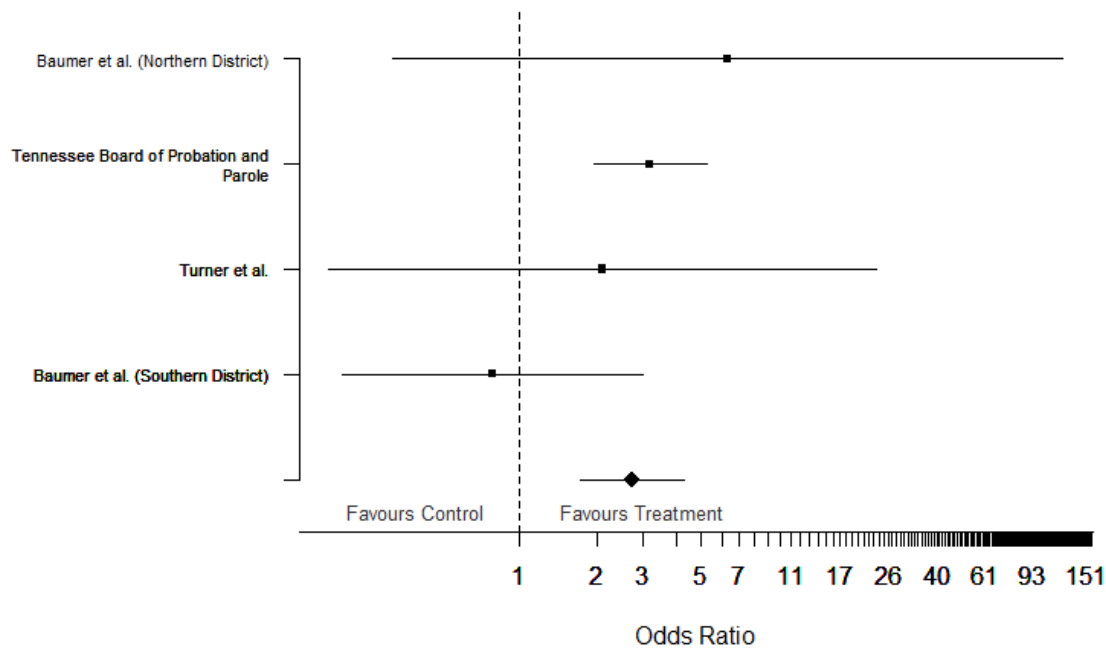


Figure 6 - Forest plot of the studies focusing on sex offenders

For the five studies examining what they termed as high-risk offenders²⁹ (across seven geographical areas), there was a non-significant effect on re-offending (0.81, CI: 0.58-1.12)³⁰. Sensitivity analysis on best and worst case scenarios did not alter this result³¹. A statistically significant weighted mean effect *was* observed, however, for the three studies (with four geographical areas) that examined the effect of EM on sex offenders³² (see Figure 6). With a weighted mean effect of 2.71 (CI: 1.71-4.28)³³ this was a comparatively strong effect. This result was substantiated by the sensitivity analysis testing best- and worst-case scenarios³⁴.

IMPLEMENTATION

Implementation here refers to the practical task of operating an EM scheme. How these practical challenges are met comprises part of the context for mechanism activation. We refer here to five main implementation issues identified across the studies we reviewed:

²⁹ These were loosely defined as ‘high-risk’, often by their risk of reoffending, although the risk assessment tool was not provided. One study referred to sex offenders who were automatically considered high risk.

³⁰ A random effects model was used for this analysis (Q=19.3, df=6, p<0.01).

³¹ Worst case scenario for this subgroup was ES=0.86 (CI: 0.45-1.65), best case scenario was ES=0.99 (CI: 0.65-1.50).

³² One of which was also considered ‘high risk’ (see footnote 29).

³³ A fixed effects model was used for this analysis (Q=4.07, df=3, p<0.1)

³⁴ For worst case scenarios for sex offender subgroup analysis ES=2.65 (CI: 1.67-4.21) and for the best case scenario ES=3.20 (CI: 1.97-5.21).

technological issues, staffing issues, programme design, information and consent issues and, finally, response to breach. A summary of the issues reported by studies can be found in Table 10.

Technological issues

This refers to the technology and accompanying equipment used for EM, the availability and quality of which will affect whether the intervention succeeds in achieving its intended aims. Factors such as equipment malfunction, loss of signal or power, battery failure, lack of communication between various databases, and inadequate broadband capacity were identified as being impediments to successful implementation across the 33 studies.

For example, the use of RFID technology required the presence of a power line, and often a telephone (Gowen 2000; Florida 2004; Killias 2010; Erez et al. 2012). Having telephone contact was vitally important for a variety of reasons. Not just for monitoring presence through RFID technology, but also to maintain contact between the parole/monitoring agent and the offender in case of alerts due to unauthorised absences and/or for clearing false alerts (Gies et al 2013; Baumer 2008; Tennessee 2007; Mortimer 2001; personal communication with EM monitoring staff). Further, telephone contact was required as part of the supervision protocol (Pearson 2012), for parole agents to monitor the progress of the offender (Turner 2012) and even to reinforce themes visited during supervision sessions and providing support to offenders (Erez et al. 2012). In addition, the telephone line could also be used to set up a remote alcohol testing device as part of EM conditions (Lapham et al. 2007).

Study	Technology/ equipment	Training	Resources/ workload	Coordination	Programme objectives	Programme administration	Adequate Information	Impact on family/ social life	Response to breach
Armstrong 2011	x	x				x	x		
Armstrong et al (2011)				x				x	x
Avdija (2014)									
Bales (2010)	x	x	x			x		x	x
Baumer (2008)		x	x		x	x			
Bonta (2000a)									
Bonta (2000b)									x
Dierenfeldt (2013)								x	
Erez (2012)	x		x	x	x			x	x
Finn (2001)	x			x		x	x	x	x
Finn (2002)	x						x		x
Florida (2004)	x								x
Frost (2002)	x		x	x					x
Gies et al. (2013)	x	x	x			x			x
Gowen (2000)	x		x			x			x
Harig (2001)	x	x	x			x	x		x
Hudson & Jones (2016)	x			x				x	
Jannetta, (2006)	x		x						x
Killias (2010)	x					x		x	
Lapham (2007)	x							x	x
Lobley (2000)				x			x		x
Marklund (2009)									
Mayer (2003)	x							x	x
Mortimer (2001)	x						x	x	x
Nestleroad (2012)									x
Omori (2015)			x						
Pearson (2012)	x		x	x	x			x	x
Roy (2007)				x					
Shute (2007)	x	x	x						x
Sugg (2001)									
Tennessee (2007)	x	x	x	x		x			x
Turner (2010)	x	x	x		x				x
Turner (2015)									x

Table 10 – Implementation factors mentioned in the studies

Smooth functioning of GPS could be affected by loss of EM signals in certain locations or places (Bales 2010) and could cause serious problems for offenders, especially if this happened at their place of residence or work (for e.g. Bales et al. 2010; Baumer et al. 2008). Jannetta (2006) reports that lack of adequate wireless capability on laptops meant agents could not track offender movements while out in the field. Problems with equipment and loss of signal, errors in signal, overload of false positives or technical alerts were said to cause agent complacency and failure to act when a real violation occurred (Gies et al. 2013). Overall, while some of these technological shortcomings are surmountable, others are more difficult to overcome.

Staffing issues

Staff involved in running an EM programme comprise personnel from a variety of agencies including prison, probation, the police, monitoring companies and criminal justice agencies. They were identified as being pivotal in the implementation and success of EM. Problems often arose when too much control was exercised by social workers (Mayer 2003); correction officers are less than helpful (Bonta et al. 2000b); and personnel were inflexible by choice or lacked discretion in the enforcement of programme rules (Erez et al. 2012; Pearson 2012). Bales et al. (2010) report that staff buy-in was essential for successful implementation. Unless staff were completely on board, understood the requirements of the programme as well as its aims and objectives, and were willing to co-ordinate effort, it is unlikely that the programme would succeed.

Specialized training requirements identified for EM staff includes training for installing, maintaining and monitoring EM equipment, responding to alerts (genuine or false) and supervising offenders. Without appropriate training, staff can find it difficult to deal with information overload or to synchronise information generated from GPS monitoring with traditional supervision requirements (Turner et al. 2010). Continuous GPS monitoring implies agents have to be on call 24/7 leading to burnouts and rapid turnover of staff (Tennessee 2007). Staff replacement in such cases of burnout and rapid turnover were reported to be a problem. Inadequate staffing and resourcing issues more generally were mentioned as being impediments to the success of EM programmes by a number of studies.

Further, monitoring staff or social workers/probation agents responsible for responding to violations reportedly felt unsafe while conducting home visits - especially at night (Tennessee 2007). Thus, a co-ordinated approach between probation staff, monitoring agents and police is required in order to ensure smooth operation of EM.

Programme administration

Proper care and attention at the planning and design stage was identified as being essential for the success of any EM programme, with objectives, guidelines and expectations developed in advance of implementation (Baumer et al. 2008). Studies highlight the importance of explicit identification of programme goals and objectives, distinct demarcation of roles and responsibilities, and provision of clear lines of communication between various agencies involved in the EM programme.

Identification of programme goals and objectives ought to guide the process of choosing suitable offenders for EM programmes. For example, programmes which include low risk offenders for extended periods have lower chances of being successful operations (Bales et al. 2010; Erez et al. 2012; Pearson 2012) especially if the intended outcome is to reduce re-offending in the longer term.

Clarity of roles and responsibilities for different agencies involved in the programme is considered important for successful implementation. In almost all study sites, EM equipment and technology was reportedly provided by private contractors. Use of different contractors in different sites or over a period of time at the same site resulted in variations in price, equipment used and quality of services provided (Gowen 2000). Greater co-ordination between relevant stakeholders is important especially since where several agencies are involved in implementation, such as corrections agencies, probation or parole officers, social workers, monitoring agents, police and criminal justice officers.

Information, communication and consent issues

Information, communication and consent issues relate to briefings provided to offenders and family members, as well as information exchange between monitoring companies and offender managers. A number of information related issues are important for successful implementation of EM programmes. Mortimer (2001) indicates that early release prisoners need to have information about the scheme and that staff ensure better screening and preparation of potential curfewees, giving them longer notice periods before release, as well as clarification of support available after release. Armstrong et al. (2011) note that both the offender and their family need adequate support to deal with the stress of release, and Armstrong (2011) says that offenders need orientation training. Moreover, good communication between case worker and offender was also deemed crucial for successful implementation (Erez et al 2012). Further, Shute (2007)

reports that communication between offender managers and the monitoring companies was suboptimal. Shute (2007) also notes that there was a mismatch between the monitoring companies' reported expenditure and what offender managers needed to adequately cost the programme.

The issue of consent referred to the offender consenting to being on EM as well as in some cases having the ability to pay for EM (Erez et al. 2012; Bales et al. 2010; Finn and Muirhead-Steves 2002). The use of RFID monitoring requires that the offender have a residence with a landline installed for the express purpose of monitoring the offender's presence during curfew (Finn and Muirhead-Steves 2002; Killias et al. 2010). In some instances this involved getting agreement from family members that they would leave the landline free for the purpose of the monitoring unit having an open line of communication with the offender (Tennessee 2007). However, problems were reported when families felt socially stigmatized, were inconvenienced, and suffered embarrassment (Bales et al. 2010, Erez et al. 2012, Pearson 2012) when a family member was on EM and were, therefore, less co-operative than anticipated. Although increasingly, the use of landlines has been replaced with mobile technology, except in areas with no satellite signal, to overcome this problem.

Response to breach

The final set of moderator conditions that has an impact on how EM worked refers to the swiftness and certainty of response to a breach. Often offenders are allowed a particular fixed pre-determined limit for unavoidable breaches, beyond which there will be serious consequences, to the extent of being returned to prison with no exception (personal communication with EM provider). This ensures that offenders are prompt and responsible in keeping to their curfew times if they do not wish to return to prison.

The efficacy of response is largely dependent on the agency or agencies responsible for overseeing and managing compliance and responding to breaches. In some cases, private contractors provide the equipment, maintain it, and even carry out the day to day monitoring of offenders on EM. In these cases, they are responsible for making the initial contact with the offender in the case of a breach to establish the cause (Harig 2001). In other instances, the police or probation agency are responsible for monitoring individuals and responding to possible breaches.

In conclusion, successful implementation of EM requires good communication between the various agencies responsible for implementation, as well as clear programme design, administrative responsibilities and communication strategies laid out from the outset.

ECONOMICS

Twenty-one studies included in this review provided information about the economic cost of EM, either for offenders or pre-trial accused persons. Most of these studies compared the cost of implementing EM against the cost of imprisonment, albeit without taking into account the costs associated with re-offending while on EM (something that does not happen if the offender is in prison). Only one study calculated the financial impact of crimes committed while offenders were on EM - calculations which included lost quality of life as well as the direct costs of the crimes themselves (Frost, 2002).

Table 11 shows information from 15 studies which provided daily costs of EM programmes per offender, or analyses of the costs of EM versus either traditional probation supervision or imprisonment. This facilitates a comparison of EM programmes with other options for offenders. The table is ordered by year of study, allowing the most recent figures to appear at the top. It is evident that variation in the costs of EM is largely dependent upon the type of monitoring technology employed – programmes using RFID technology are generally cheaper than GPS programmes. The latter also vary depending on whether passive or active monitoring is used. While RFID technology only works for part of the day, it is possible for GPS to allow 24-hour monitoring, which then requires staff members to be available throughout the monitoring period. The most expensive estimate from a US-based study for active GPS is \$33.43 per day (Omori and Turner 2015), while Shute (2007) gives a figure of £42 per day for passive GPS in the UK (no active GPS figures were available). The technology most used in the UK is RFID, which has been estimated at £17-18 per offender per day (Armstrong et al. 2011) – GPS is rarely used for offenders who are being monitored for suspected terrorism offences³⁵.

While GPS is always more expensive than RFID monitoring, both forms of EM are considerably cheaper than a prison sentence alternative. Omori and Turner (2015) give the average daily cost of an offender in prison in the US as \$97.50 compared to \$33.43 on GPS,

³⁵ Personal communication with EM provider (UK).

while Armstrong et al. (2011) compare the weekly cost of prison in the UK of £610 with that of an offender on EM of £126. Bales (2010) estimates that for the cost of one inmate in prison for a year, six could be on GPS and 28 could be on RFID, showing potentially significant cost savings to the state or county running the EM programme. However, the cost of EM is always higher than traditional parole or community supervision. Turner (2015) notes that monitoring a high risk sex offender with EM costs \$4,600 a year *more* than normal intensive caseload supervision without EM technology. Omori and Turner (2015) also note that the cost of parole without EM is almost one third the cost of active GPS monitoring (\$13.51 per day). Gies et al. 2013 corroborates this proportion, showing traditional parole to cost \$7.20 per day compared to \$21.20 for active GPS. Both these studies suggest cost of active supervision on GPS is three times the cost of traditional parole (without EM). Therefore, while EM may be cost saving compared to imprisonment, authorities may need to consider the financial implications of putting offenders on EM rather than traditional supervision or parole – the decision to adopt EM should depend on whether such supervision is essential as it is not only more expensive but may have a net widening effect (discussed on page 7).

Daily cost per offender					
Study	Year	RFID	GPS Passive	GPS Active	Comparison with cost of imprisonment/other supervision
Turner	2015				High Risk Sex Offender monitored by GPS cost an average of \$4,600 per year more than other HRSO supervised on intensive caseloads (without GPS)
Omori & Turner	2015			\$33.43	Average Daily Cost Per Parolee: active GPS \$33.43, other parole \$13.51, prison (including reception centres) \$97.50, jail \$88.94
Gies	2013			\$21.20	Traditional supervision is \$7.20 per day per parolee
Pearson	2012			\$15.50	
Erez	2012		\$6.84 av.	\$11.18 av.	
Armstrong et al	2011	£17-18			The estimated weekly cost of keeping a person in prison is £610 (based on the 2009/10 annual prison place cost of £31,703). This compares to a weekly cost of £126 to manage someone on HDC
Bales	2010	\$2.34 With equip. and staff: \$8.60	\$4.25 With equip. and staff: \$23.66	\$8.97 With equip. and staff: \$11.13	Prison costs \$20,108 per inmate per year, which is 6 times more expensive than active GPS, and 28 times more expensive than RFID
Marklund & Holmberg	2009				The daily cost for a client with EM is almost as high as the cost of a place in an open prison
Tennessee	2007		\$4.25	\$7.70	
Shute	2007		£42		
Jannetta	2006			\$8.75	
Florida Senate	2004	\$2.75	\$4	\$9	GPS monitoring program has saved \$140,640 in incarceration costs
Harig	2001				Placement cost savings for whole programme are estimated to be \$97,000- \$110,000. This is when comparing a total annual cost averaging more than \$78,000 to institutionalize a juvenile delinquent. Net savings calculated based on care days saved at \$215.36 per day.
Mortimer	2001				After 16 months, prison resource savings were £84.6m, net benefits were £49.2m, total 2600 prison places saved
Gowen	2000	\$17.98			Average daily cost of home confinement with EM was \$17.98 compared to \$64.32 in federal custody
Lobley & Smith	2000	£2,500 for 3m, £4,860 for 6m			If EM replaced prison sentences in 40% of cases in which they were made, this could produce an overall cost saving ranging from approximately £1.7 million if the term in prison would have been for 6 months, to a little more than £0.33 million if the displaced prison terms consisted of an equal number of 3 and 6 month terms.

Table 11 – Costs of EM per day and comparison with prison

The daily costs of EM for offenders reported in the studies need to be interpreted cautiously as the cost of an EM programme comprises a number of different factors, some of which may go uncalculated in some studies but be included in others. For example, Bales (2010) reports two sets of figures, the higher set including the cost of equipment and staff and the lower with just the cost of the tag. It should also be noted that active offender tracking requires more resources, including more staffing, making it more expensive than passive systems. Passive tracking on GPS in turn is more expensive than RFID systems. Other factors to consider are the installation and maintenance costs of equipment. This might include replacing equipment which may break, be broken or tampered with by the offender, and require further visits by staff. Pearson (2012) (see Table 12) gives some idea of these costs, including \$1,800 for a new EM device, \$80.25 for a new strap and \$107 for a new charger.

As discussed previously, staff are an important moderator of the effectiveness of EM. Moreover, this review found that staffing is also one of the largest costs associated with any EM programme. Staff are required to initially decide whether an offender is suitable for an EM programme, requiring a full risk assessment. Armstrong et al. (2011) suggest that the full cost of assessment and preparation for an offender to be placed on EM is £602. Probation or parole officers are sometimes required to monitor offenders, whereas other times it is private agencies who carry out this role. Where monitoring centres are set up, these incur staffing costs which are often 24-hours in nature. Additional costs include training costs for staff both initially and at regular periods during employment. Alongside staffing costs come equipment costs within the offices for staff, including mobile phones, laptops, wireless internet facilities and phone lines. Lapham et al. (2007) and Harig et al. (2001) give details of some of these costs in Table 12.

Another staffing cost to be considered is that of the extra supervisory officers required to monitor GPS caseloads – fewer offenders are allocated per parole officer due to the extra supervisory requirements compared to normal probation or parole conditions. Offenders on active GPS programmes require round the clock supervision. This can be even more complicated in cases where inclusion and exclusion zones are applied, as seen in domestic abuse and sexual offending cases. Additionally, staffing costs rarely include police time spent in responding to offender breaches of their curfew when calculating the total cost of implementing EM. This use of police time would not be required if the offender was still incarcerated. Hudson and Jones (2016) however calculate that using GPS monitoring may actually save police time and costs in comparison to traditional parole for prolific offenders.

Study	Offender contributions	Equipment costs	Staff costs	Other costs
Gies <i>et al.</i> 2013		Equipment per parolee per day \$2.95		
Pearson 2012		Device charger (\$107.00); Back plate (\$32.10); New strap (\$80.25); A new EM Device (\$1,800); New cell phone (up to \$430) and charger (\$53.50)	EM staff wages include benefits, standby payments and pension adjustment	Additional Costs to Consider: Police to facilitate an arrest, payroll expenses ongoing training for staff and stakeholders, user acceptance processes, start-up fees, travel expenses
Erez <i>et al.</i> 2012	\$8.68 active GPS per day, \$6.79 passive GPS per day, average of \$8.80 per day across programmes, 16.3% of defendants cannot make payments, some defendants pay up to \$18.50 per day or % of salary.			
Armstrong <i>et al.</i> 2011			HDC administrator for 2 days £176; HDC coordinator for 1 day £120; Unit Manager for half day £125; Total cost of assessment £421 Prisoner officer for 1 day £157; HDC administrator for quarter day £24; Total cost of preparation/ oversight £181 per offender. Combined cost of HDC assessment and preparation £602 per offender	Cost of an installation is £125 Police costs are borne by whichever individual police forces are involved in the 'arrest on suspicion' (and maybe charging) of released prisoners who would not otherwise have been back on the street but for their early release
Bales <i>et al.</i> 2010	Approximately 10%	GPS \$3,263 per year		
Marklund & Holmberg 2009				The prison and probation service is, where required, responsible for providing the inmate with an income.
Lapham <i>et al.</i> 2007	Offenders pay from \$25-\$40 per week for treatment, unless indigent. Providers may impose additional fees according to a sliding scale.	GPS Equipment Costs for 370-380 Offenders Daily \$1,450,337.70	Administrative Costs \$77,290.93 Impact Probation and Parole Officer Positions \$310,386.00; Centre Staff \$88,926.00; Project Director \$43,536.00;	Spent a total of \$77,290.93 on cell phones for officers, digital cameras, equipment cases, supplies, and wireless cards for computers.

		Staff Overtime (through March 2007) \$344,159.33	
Tennessee 2007	Monthly GPS fee of \$50.00		
Shute <i>et al.</i> 2007			This does not include the costs to the probation service or the Youth Offending Teams of supervising the offenders or interpreting the data received from the monitoring companies.
Jannetta <i>et al.</i> 2006	\$8.75 per unit, per day includes the cost of provision of the GPS units, the software, and the monitoring centre services	25 agents are required to supervise 500 GPS parolees, at a total staffing cost of \$2,440,925 annually.	
Florida Senate 2004	Offenders on active GPS monitoring must pay an additional \$8 per day for a total of \$13	An additional 1000 offenders on electronic monitoring would require an additional 25 full time employment positions at an annual cost of \$1,046,775.	
Finn <i>et al.</i> 2001	Parolee is required to pay a one-time \$15 fee and a daily rate based on a percentage of his/her salary using a sliding scale		
Harig <i>et al.</i> 2001	\$27,740 EM equipment	Funding for one full-time probation officer \$37,500, with fringe benefits \$9,627 Travel \$2,438	\$2000 computer equipment \$300 internet service \$2496 drug testing \$9627 fringe benefits
Mortimer 2001		16 months Estimated Costs: Prison staff costs £4.3m; Probation services £3.0m; Contractor costs (actual charges made ex. VAT) £27.9m; Sentence Enforcement Unit £0.2m	The total cost of the risk assessment process to Probation Services was around £1.6 million in the first year, with each risk assessment costing approximately £60

Table 12 - Other costs associated with EM programmes

Such offenders would receive nightly visits by police on traditional parole to confirm their whereabouts, whereas this was not necessary when they were wearing the EM GPS tag, which was estimated to save an average of £850 per offender over a 3 month period.

Total programme costs therefore will often run into the millions of pounds or dollars (Mortimer 2001; Lapham et al. 2007). Studies indicated that start-up costs may vary depending on the type of equipment which must be purchased, or rented from a private contractor who runs the programme. According to Mortimer (2001), private contractors who oversaw the implementation of the EM programme in England and Wales were paid £27.9 million over a period of 16 months. Prison staff, probation services and sentence enforcement unit costs added an extra £7.5 million over the same period. Some of these costs are offset by programmes which require the offender to contribute towards the costs of monitoring, although this seems to be exclusively in the US. (None of the European programmes mentioned this component). Of the six US based studies that mention offender contributions, three suggest that these payments are means tested against the income of the offender. While Tennessee (2007) mentions a monthly fee of \$50, Lapham et al. (2007) records a weekly fee of \$25-40 where the offender can afford to pay. A more recent study suggests that the type of technology used affects the amount involved: with offenders on active GPS monitoring contributing an average of \$8.68 a day compared to those on passive GPS monitoring contributing \$6.79 per day (Erez et al. 2012). Offenders who can afford to, may pay up to \$18.50 per day. Other costs may also be incurred by defendants, such as the cost of travel to attend parole, probation appointments with supervisors, or mandatory therapy fees, if such therapy is part of the conditions of release (Lapham et al. 2007).

Practitioners, law-makers and government officials have multiple considerations when assessing the advantages and costings of an EM programme. If cost-effectiveness is sought, then the longer the offender is in the community rather than incarcerated, the better (Mortimer 2001; Marklund and Holmberg 2009). However, programmes which require lifetime monitoring, such as some sex offender programmes in the US, will only increase costs as more offenders are placed on the programme, since attrition only occurs upon an offender's death (Turner 2015). If initial assessment and setup costs are high, as in the UK (Armstrong et al. 2011), cost savings compared to imprisonment are offset by short sentences. However, if the main reason for the programme is to ensure that offenders do not re-offend while on EM, then true cost implications include the costs of crimes committed while they are on the programme, or indeed costs saved if EM is found to prevent re-offending in the short, medium or long term.

Only one study calculated the benefits of EM by including crimes not committed (Frost 2002). This study concluded that considering the number of crimes prevented by the EM programme, the total cost savings of direct costs (of the crime itself) and indirect costs (including quality of life lost) was over \$17 million per year by the third year of the programme. This was based on the lower numbers of violent crimes committed by offenders who were placed on EM, with an average direct cost estimate of \$3,670 and indirect cost estimate of \$8,905 per crime. Since this is the only study to conduct this kind of cost benefit analysis, it is difficult to assess how this estimate compares with other similar programmes.

Finally, it is useful to re-iterate the difference between EM as an intervention aimed at being financially viable and cost effective alternative to other offender management techniques and as a crime reduction initiative. As we have seen in this review there is limited evidence of effectiveness of EM approaches over reasonable alternative strategies. Given there is no clear winner in terms of effectiveness of one approach over the other, cost perhaps becomes an important concern. For example, if prison and EM are as effective at each other at reducing crime, but EM is cheaper, it makes sense to fully evidence the competing costs of alternatives. It is clear that whilst our main interest is in EM as an intervention to reduce reoffending, clearly some of these programmes were intentionally set up to reduce costs (of prisons) so in the short term, it has the benefit of having cost-saving potential. The tables and discussion above appear to suggest that on balance, reduced costs of EM offer it an advantage over incarceration.

6. DISCUSSION

This systematic review of the literature on the electronic monitoring of offenders, based on the EMMIE framework (Johnson et al. 2015), assessed the available evidence with the aim of answering the following questions: 1) What is the effect of EM on re-offending? 2) How is EM assumed to reduce re-offending? 3) For whom, and under what conditions is EM found to be more or less effective? 4) How is EM implemented? and 5) What are the estimated costs associated with the EM of offenders relative to the anticipated savings from crimes averted?

A systematic search of the published and unpublished literature identified 33 studies that met our inclusion criteria. 17 of these studies contained quantitative data on the effectiveness of EM and were included in our meta-analysis. These studies in combination with a further 16 were also included in our realist review concerned with extracting information relevant to the mechanisms, moderators, implementation and economic costs associated with EM of offenders. Across all 17 studies included in our meta-analysis, we found that overall the EM of

offenders was not associated with a statistically significant reduction in re-offending rates (however measured). These 17 studies did, however, cover a wide range of different EM programmes implemented in different settings and targeted at different types of offenders. When meta-analysing relevant sub-groups we found that EM was associated with statistically significant positive effects for particular kinds of offenders and at particular stages in the criminal justice system (i.e. EM sentencing instead of prison or using reconviction outcome data). A statistically significant backfire effect was observed for studies with a short-term follow up period (either pre-sentence or up to 12 months), but given the small number of studies, and that these predominantly referred to domestic violent offenders pre-trial, this finding should be seen as tentative. It is clear that the impact of EM on re-offending is nuanced and needs to be contextualised.

This review indicates that measuring the effectiveness of EM is complex, firstly because the intended outcomes of EM programmes are not always reducing re-offending, but can be reducing prison overcrowding or simply seen as a more cost-effective offender management method. Our results suggest that *moderators* (conditions under which EM operates) can impact *mechanisms* (how it operates) in achieving the *effect* (intended outcomes) differently depending on *implementation* issues involved. The situation is further complicated by the fact that EMMIE aspects of EM are interrelated in complex and non-linear ways. Sometimes the intended outcomes or *effect* (for example, compliance with probation requirements) and *implementation* issues (namely, resource availability) will dictate the *moderator* conditions (such as choice of type of technology and geographic restrictions). At other times the existence of particular contextual *moderating* factors (c.f. type of offender or legal requirement to be in employment or attend drug and alcohol programmes) will necessitate the triggering of specific (social or behavioural) *mechanisms* to achieve the desired *effect* (rehabilitation) and might be dependent on certain *implementation* requirements being fulfilled (such as attendance monitoring).

There is little evidence in the studies reviewed here to indicate the exact *mechanisms* that produced the effect of EM on re-offending. Although increasing the risk was posited to be the dominant mechanism through which EM was expected to work, surveys and offender feedback in the studies reviewed indicated that social and behavioural mechanisms were also a powerful influence in encouraging prosocial behaviour. While increased exposure to prosocial situations is proposed as being beneficial to reducing likelihood of future offending, in some cases it

caused a great deal of stress for offenders and family members as a result of forced interaction during curfews of up to 12 hours (Armstrong et al. 2011; Pearson 2012; Erez et al. 2012).

However, single contextual factors may not be sufficient to produce the sought after effects, but could do so when combined in complex and interdependent ways. An example is when the type of offender (sex offender for example) would merit active 24-hour monitoring which can only be provided with GPS technology. Thus, all three moderating conditions exist because they are interdependent and the effect of each one individually is difficult to disentangle, especially since implementation issues would differ from situation to situation. In an effort to conceptually combine the dimensions of EMMIE, two high level models to indicate the complexity of determining the effectiveness of EM, given the plethora of starting points (*moderators*), available resources (*implementation and economics*) and the interaction that might trigger different mechanisms for the intervention to achieve intended outcomes (*effects*) are now detailed.

Figure 7 shows just one particular configuration of a pathway for EM of an offender (in this case a sex offender) under given contextual factors or moderator conditions (outlined in blue) and implementation issues (outlined in green) that might affect the activation of particular mechanisms (outlined in yellow) to achieve certain intended outcomes (grey boxes outlined in red). In Figure 7, the only mechanism activated is increasing the risk, but modifying the moderator conditions (adding probation conditions) or the implementation issues (not enough buy-in from family) might activate different mechanisms and pathways to achieve the intended outcomes (or not). Thus, when the evidence indicates that EM had a significant positive result on sex offenders it could be because, more often than not, EM was combined with a therapeutic component. Given the available evidence, it is difficult to attribute the positive impact to EM or the fact that being on EM compelled attendance at these mandated activities, which actually led to the reduction in re-offending behaviour.

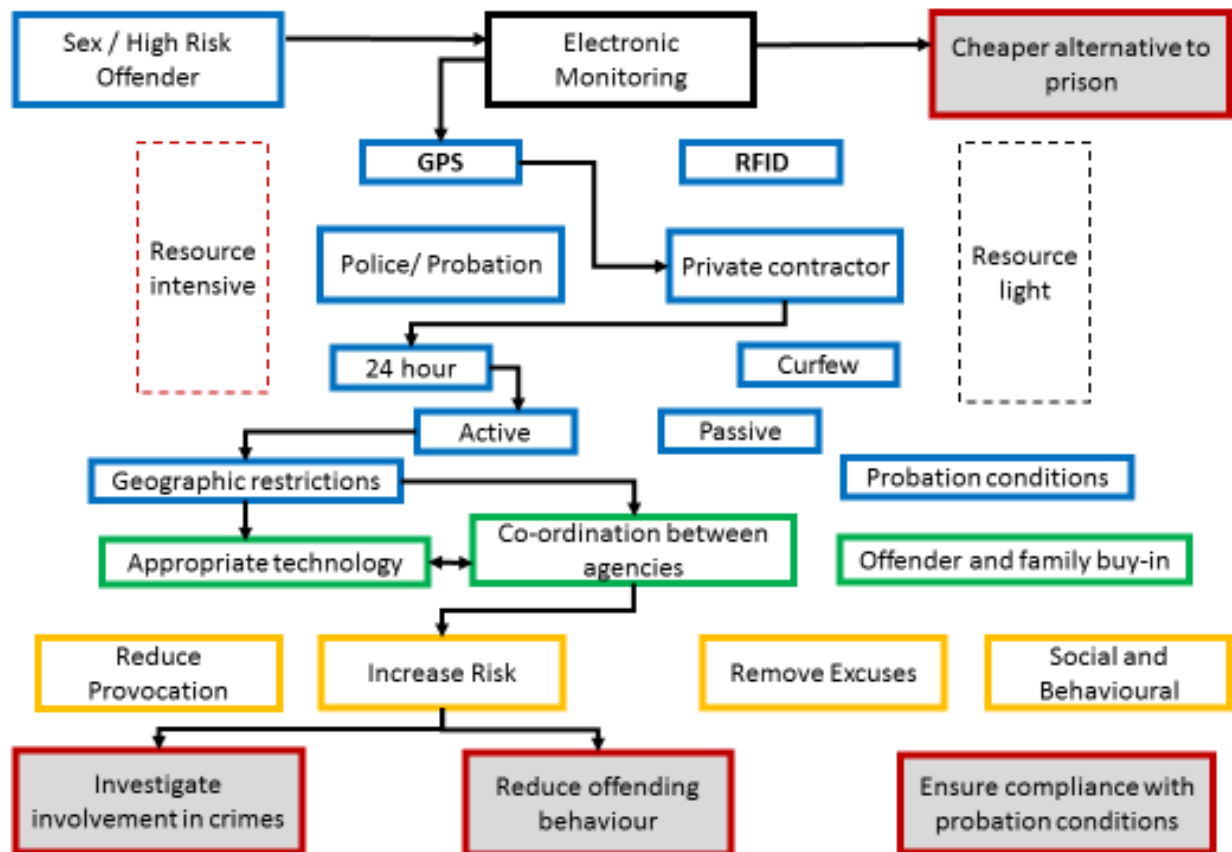


Figure 7 – Logic model for a Sex offender on 24-hour active monitoring using GPS technology operating within inclusive geographic zones only

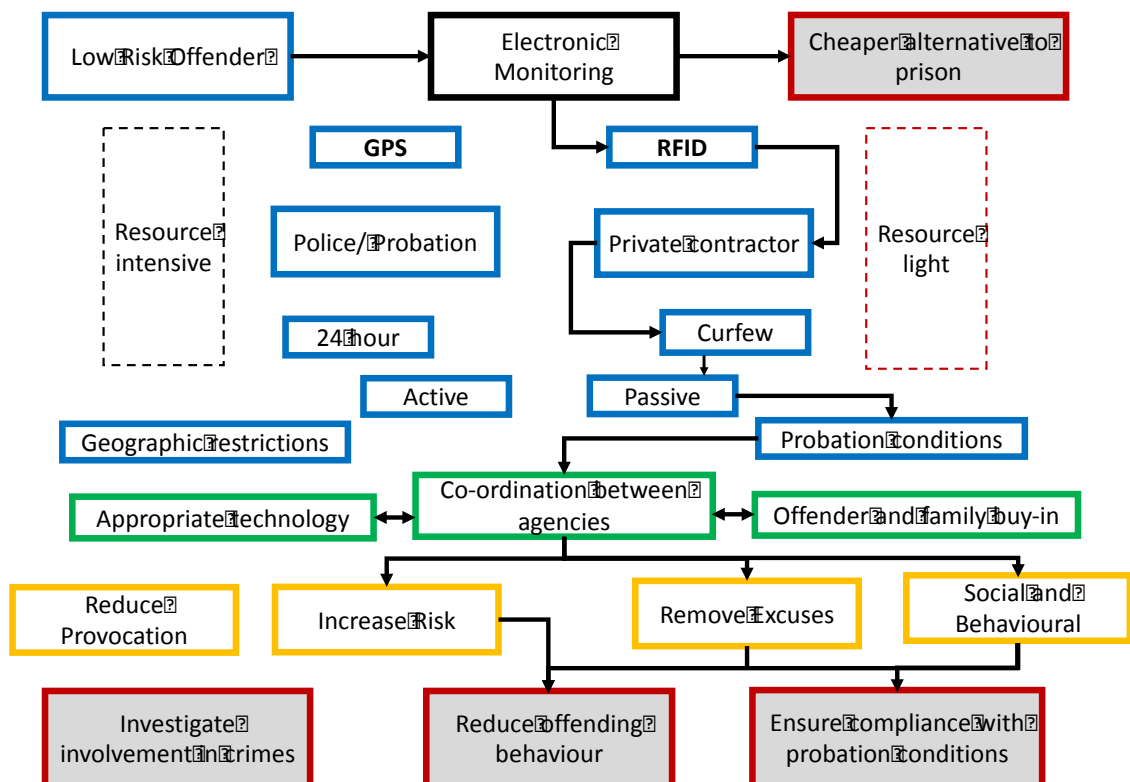


Figure 8 - Logic model for a low risk offender on a 12-hour curfew passive monitoring using RFID technology and having to comply with probation conditions of employment

Similarly, Figure 8 shows a different configuration that might have different starting points and activate other causal pathways to achieve the intended outcomes. A full evaluation would map out a logic model for each particular programme, identify the potential pathways and specific causal mechanism/s that might putatively be activated, identify and measure interim outcomes associated with particular mechanisms (through surveys or observation) and use the data to ascertain how and whether the intervention is successful (and if not, why not).

7. CONCLUSION

Many countries are increasingly investing in the EM of offenders, in an effort to both reduce re-offending and as a proposed cost-effective alternative to prison. The increased dependence on GPS technology to monitor high risk offenders involves a huge investment in resources and equipment and the evidentiary value of EM data is still unestablished across many jurisdictions. Using EM to increase the risk of getting caught and convicted requires necessary legal provisions and provision of appropriate training to staff and members of the criminal justice system.

This systematic review of the literature and evidence indicates that EM has been shown to produce positive effects for certain offenders (such as sex offenders), at certain points in the criminal justice process (post-trial instead of prison), and perhaps in combination with other conditions attached (such as geographic restrictions) and therapeutic components. It may not work so well for other subgroups or under different conditions. Future research should focus on understanding and measuring the impact of stand-alone EM programmes compared to EM programmes that combine other treatments and interventions. To improve our understanding of how and when EM is most effective, future evaluation studies might usefully look to collect data on the various elements of the programme as well as contextual factors to measure the impact of the component factors and identify effective causal mechanisms that achieve the intended outcomes.

Further, it is important to note that EM programmes can be inequitable, especially those that require that the offender contribute to the cost, have a permanent residence, and the necessary support structure (in the form of a landline where necessary, and agreement from family members to EM). Expansion of EM as an offender management technique should ensure fair

and equitable treatment in the interests of avoiding litigation and upholding basic principles of procedural justice for all.

8. ACKNOWLEDGEMENTS

This review was undertaken as part of the *Commissioned Partnership Programme: the What Works Centre for Crime Reduction*. We would like to extend our gratitude to David Aaberg for his research assistance, Phyllis Schultz for her expertise in searching the grey literature, Karla Dhungana-Sainju for her help in identifying sources, and to John Eck and Anthea Hucklesby for commenting on an earlier draft of this review. Our grateful thanks to Julia Hartless Jones of the Ministry of Justice for organising a visit for the research team to the Electronic Monitoring Centre at Manchester, UK and to the helpful staff at the Centre who provided valuable practitioner perspective and allowing us to observe the working of the Centre for a day. The opinions stated in this article are solely those of the named authors, and are not necessarily shared by other academics or organizations (such as the College of Policing) involved in the What Works Centre for Crime Reduction. The research reported here was funded by the Economic and Social Research Council (ESRC) grant ES/L007223/1, and the College of Policing

9. APPENDICES

Appendix A – Electronic databases searched

ASSIA (Applied Social Sciences Index and Abstracts)

Criminal Justice Abstracts

Criminal Justice Periodicals

ERIC (Education Resources Information Centre)

IBSS (International Bibliography of Social Sciences)

NCJRS (National Criminal Justice Reference Service)

ProQuest theses and dissertations

PsycINFO

PsycEXTRA

SCOPUS

Social Policy and Practice

Sociological Abstracts

Web of Science

CINCH

Appendix B – An example of the search syntax used in electronic databases

"electronic monitor*" OR tag* OR curfew* OR "random calling" OR "verifier
anklet" OR "verifier wristlet" OR "verifier bracelet" OR ((house OR home
) W/1 (arrest OR detention OR confinement OR incarceration)) AND
"crim* OR illegal* OR illicit* OR delinqu* OR offend* OR parole* OR proba
te* OR incarcerate* OR recidivism* OR convict* OR felon* OR misdemeano*
OR bail*"

Appendix C – List of sources searched for grey literature

In collaboration with an information specialist, we have also searched the publications of the following government, research and professional agencies:

- Centre for Problem-Oriented Policing (Tilley Award and Goldstein Award winners)
- Institute for Law and Justice
- Vera Institute for Justice (policing publications)
- Rand Corporation (public safety publications)
- Police Foundation
- Police Executive Research Forum
- The Campbell Collaboration reviews and protocols
- Urban Institute
- European Crime Prevention Network
- Swedish National Council for Crime Prevention
- UK Home Office
- UK Ministry of Justice
- UK College of Policing (Polka)
- Australian Institute of Criminology
- Swedish Police Service
- Norwegian Ministry of Justice
- Canadian Police College
- Finnish Police (Polsi)
- Danish National Police (Politi)
- The Netherlands Police (Politie)
- New Zealand Police
- US National Institute of Justice

The following resources have also be utilised:

- Google
- Google Scholar

- Academic Search Premier (EBSC)
- ProQuest Sociology
- Rutgers Criminal Justice Grey Literature Database
- OSCE Polis Digital Library

Appendix D – The formulae used in the meta-analysis

The odds ratio is computed as follows:

$$OR = \frac{T_{not-reoffending} \times C_{reoffending}}{T_{reoffending} \times C_{not-reoffending}} \quad (1)$$

As suggested by Lipsey and Wilson (2001: 53) in equation 3:28, when one of the cells in the above formulae was zero, 0.5 was added to all cells.

For computational reasons, the standard error (SE) is calculated for the logarithm of the OR (LOR = loge(OR)) rather than the raw OR. The formula for computing the SE for LOR is as follows:

$$SE_{LOR} = \sqrt{\frac{1}{T_{not-reoffending}} + \frac{1}{T_{reoffending}} + \frac{1}{C_{not-reoffending}} + \frac{1}{C_{reoffending}}} \times 2 \quad (2)$$

Confidence intervals are then computed in the usual way (by multiplying the SE_{LOR} by 1.96 and adding and subtracting this value from the LOR to get the upper and lower estimates of the interval) and the estimates are then exponentiated as logged values can be difficult to understand.

To estimate the mean effect size we first computed the inverse variance weights for each OR using the formula:

$$\omega_i = \frac{1}{SE^2}$$

Where,

ω_i is the inverse variance weight, and

SE_i is the standard error of the estimate

The weighted mean effect size is then simply:

$$\overline{OR} = \frac{\sum(\omega_i \times LOR_i)}{\sum \omega_i}$$

The above (fixed effects) formula assumes that any variation in effect sizes observed across locations (or studies) is due to sampling error alone. However, it is also possible that there is real variation across studies (e.g. due to contextual differences). To account for this, we use a random effects model to compute the weighted mean effect size (see, Lipsey and Wilson, 2001), this essentially serves to increase the estimated standard error of the estimates.

To estimate the effect size of individual studies reporting survival data (Hazard ratio), we employ the following formulae:

$$HR = \frac{H^T}{H^C} = \frac{O^T/E^T}{O^C/E^C}$$

Where,

O_i represents the observed number of events (e.g. re-offend) in group i,
E_i represents the expected number of events (e.g. re-offend) in group i, and
H_i represent the overall hazard rate for the ith group.

To calculate the mean effect we use the following equation:

$$\text{Mean } \ln HR = \frac{\sum \ln HR / V^*}{\sum 1/V^*}$$

Where,

$\ln HR$ is the log hazard ratio and V^* is the variance of the log hazard ratio

Appendix E – Hierarchy of outcome data types of re-offending for the studies eligible for meta-analysis

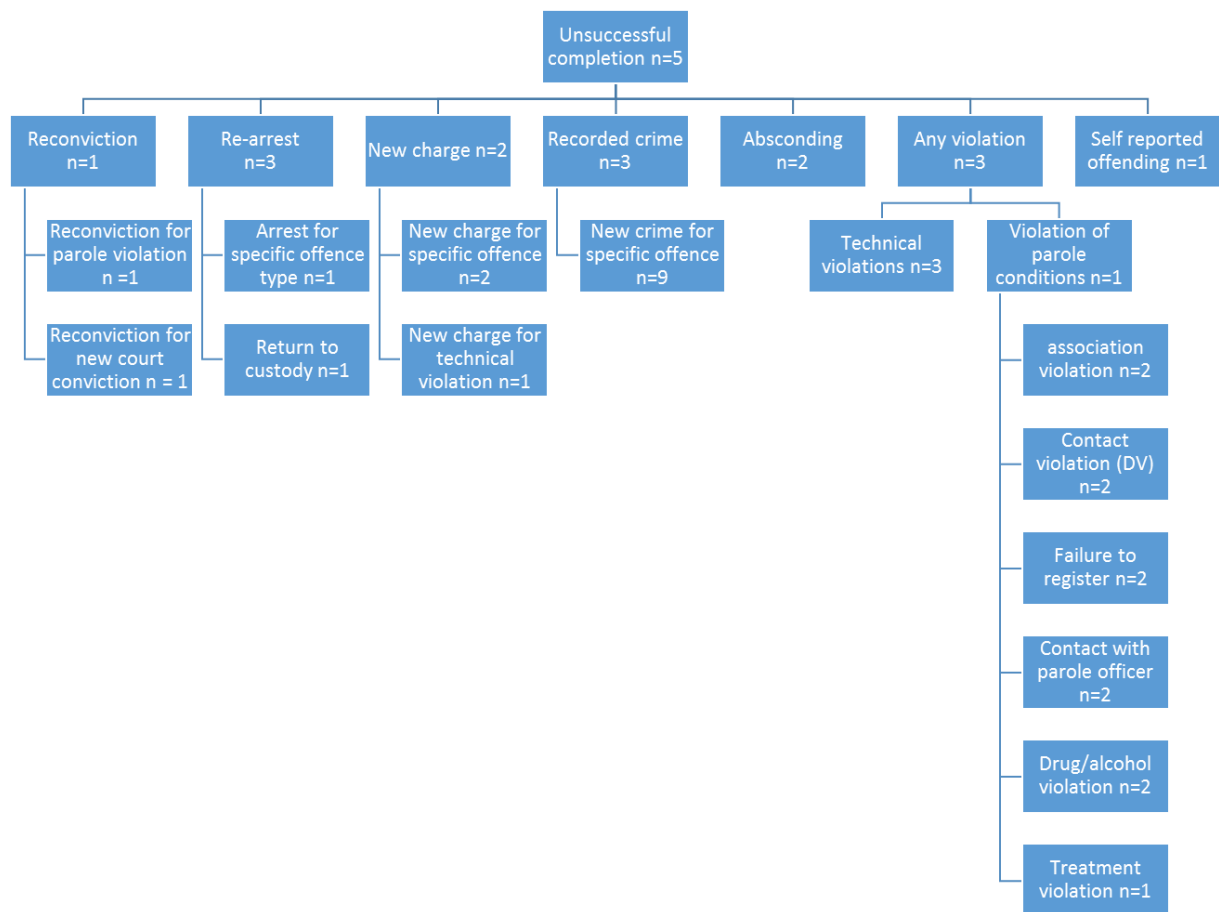


Figure 9 – Hierarchy of outcome data types in the sample of studies eligible for meta-analysis

Appendix F – Additional forest plots

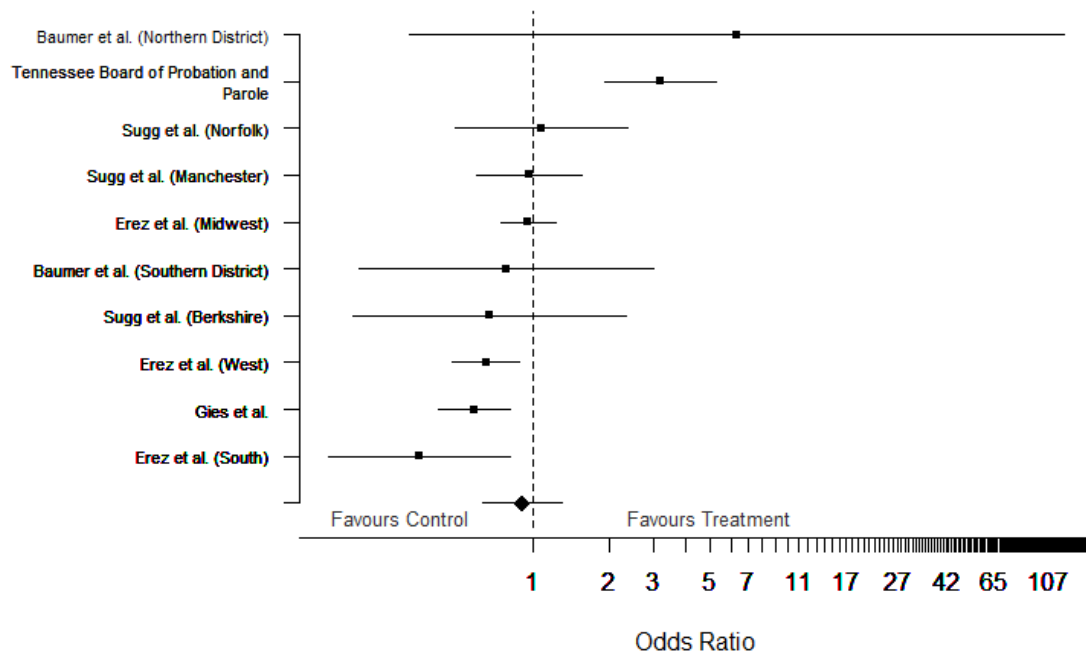


Figure 10 - Forest plot of the grey literature studies

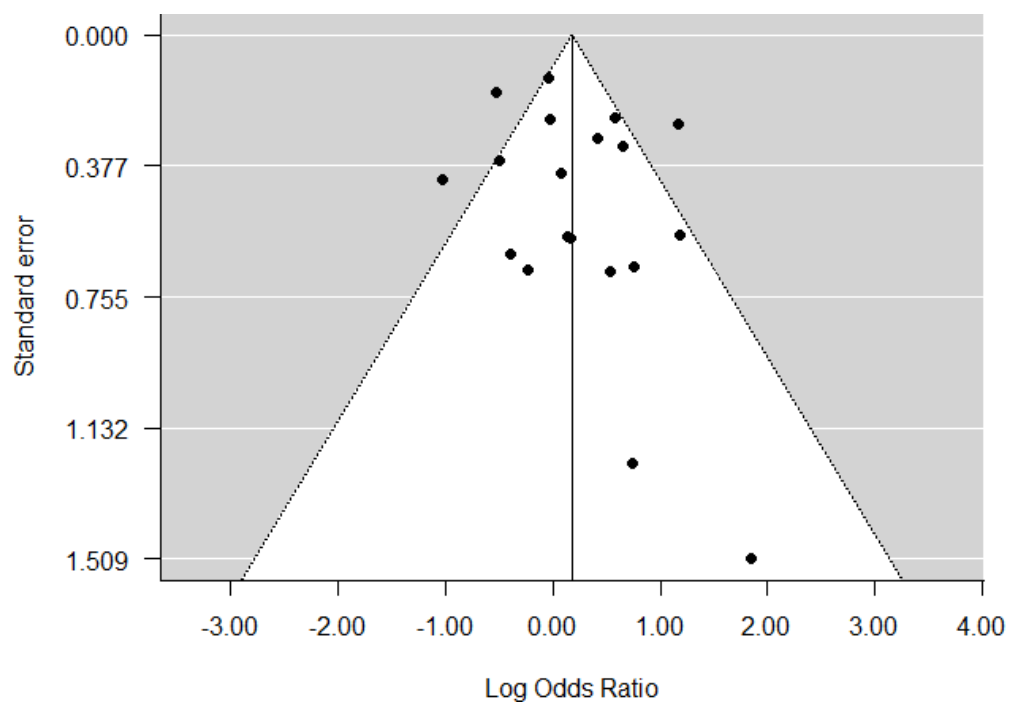


Figure 11 – Funnel plot of standard errors against log odds ratios for the 19 geographical areas used in the meta-analysis

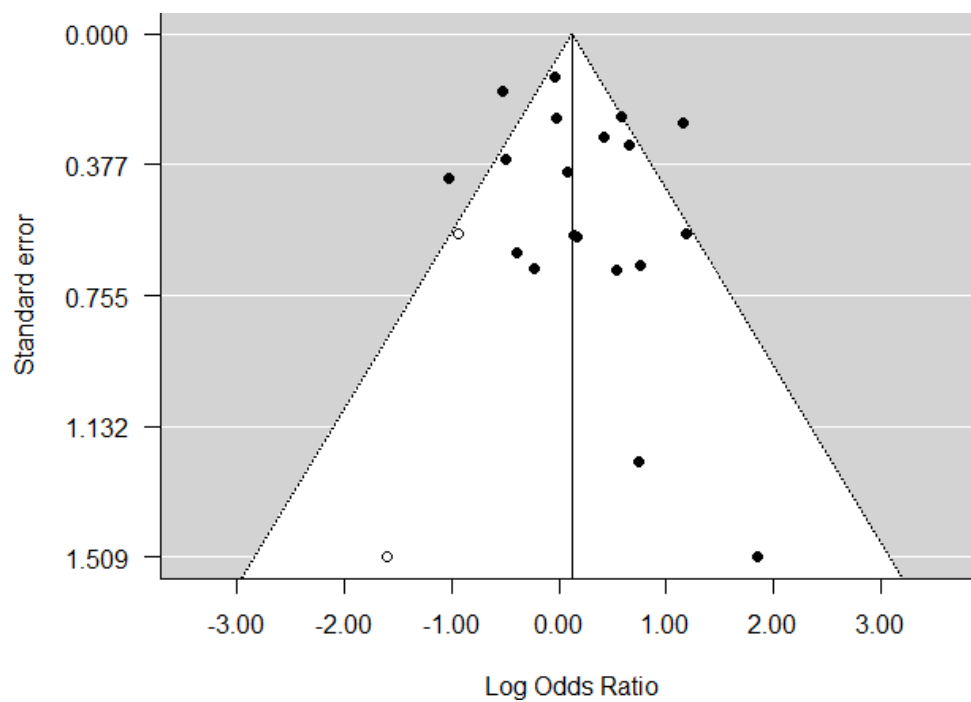


Figure 12 - Funnel plot with two further imputed studies (white dots)

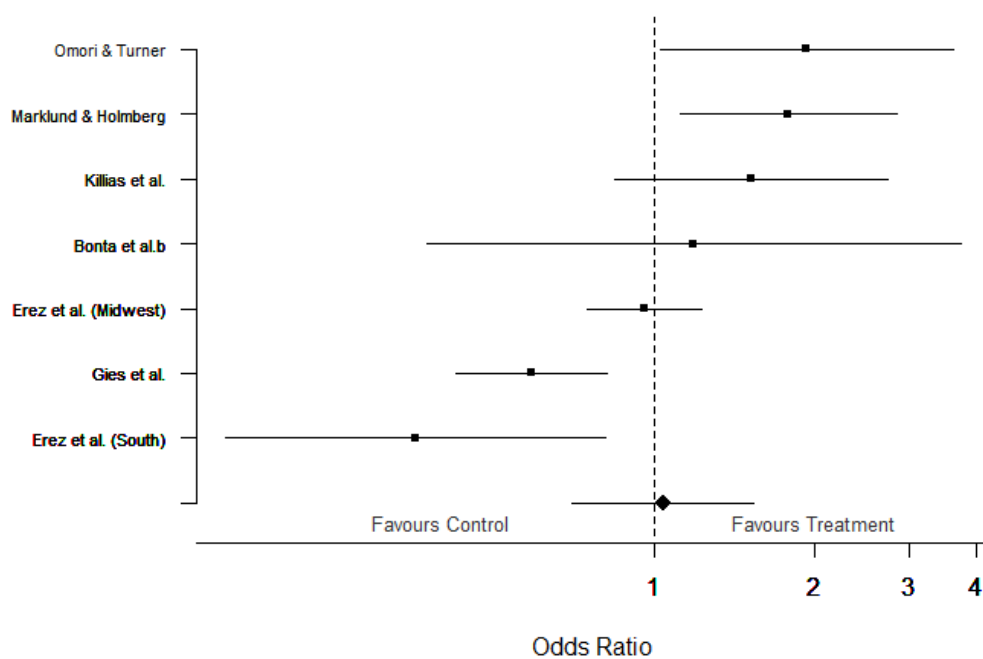


Figure 13 - Forest plot of the effect sizes from higher quality studies

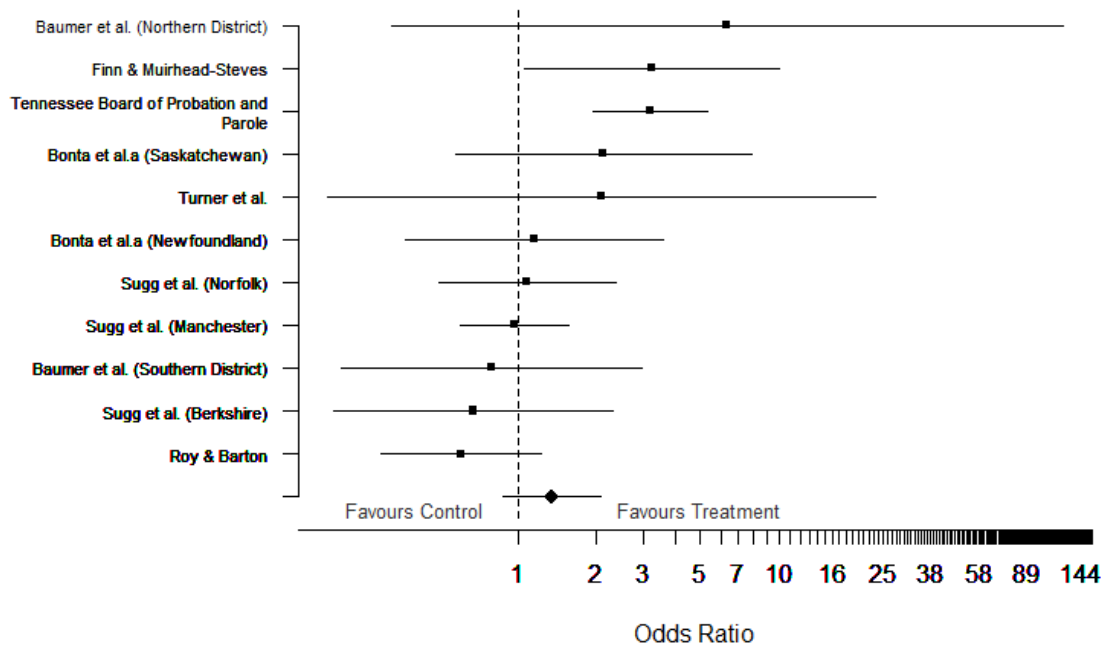


Figure 14 - Forest plot of the effect sizes from lower quality studies

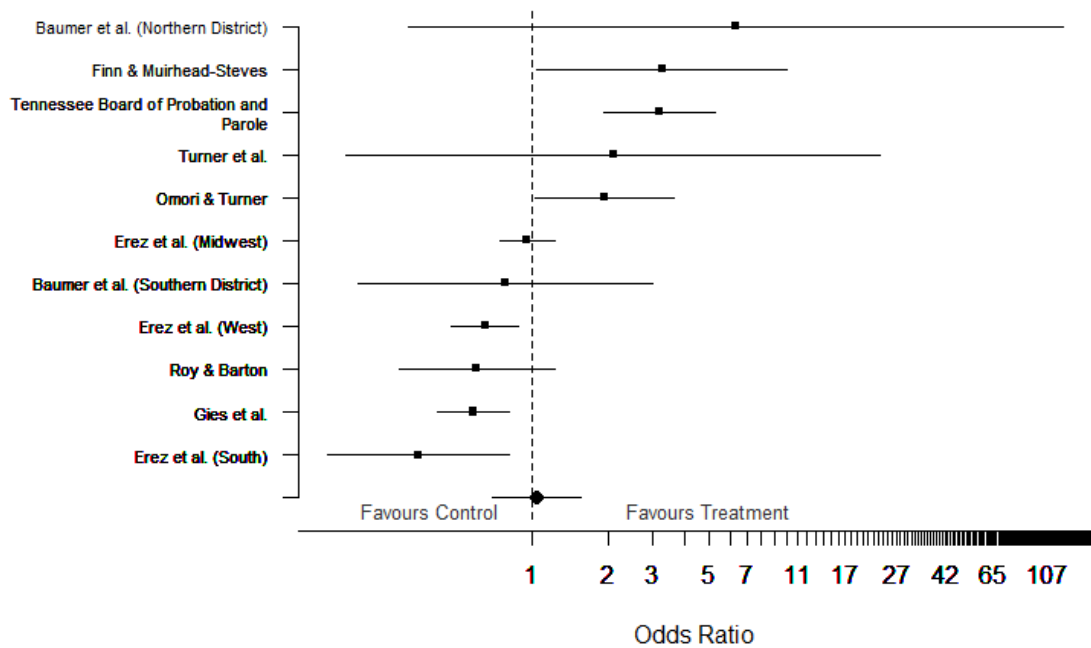


Figure 15 - Forest plot of the USA studies

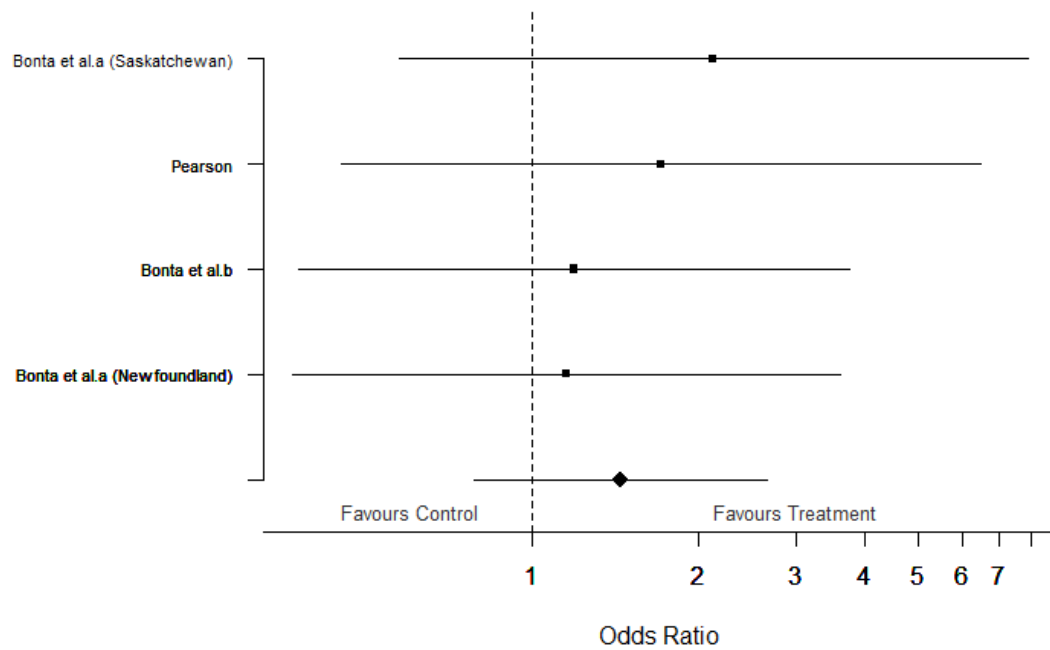


Figure 16- Forest plot of the Canadian studies

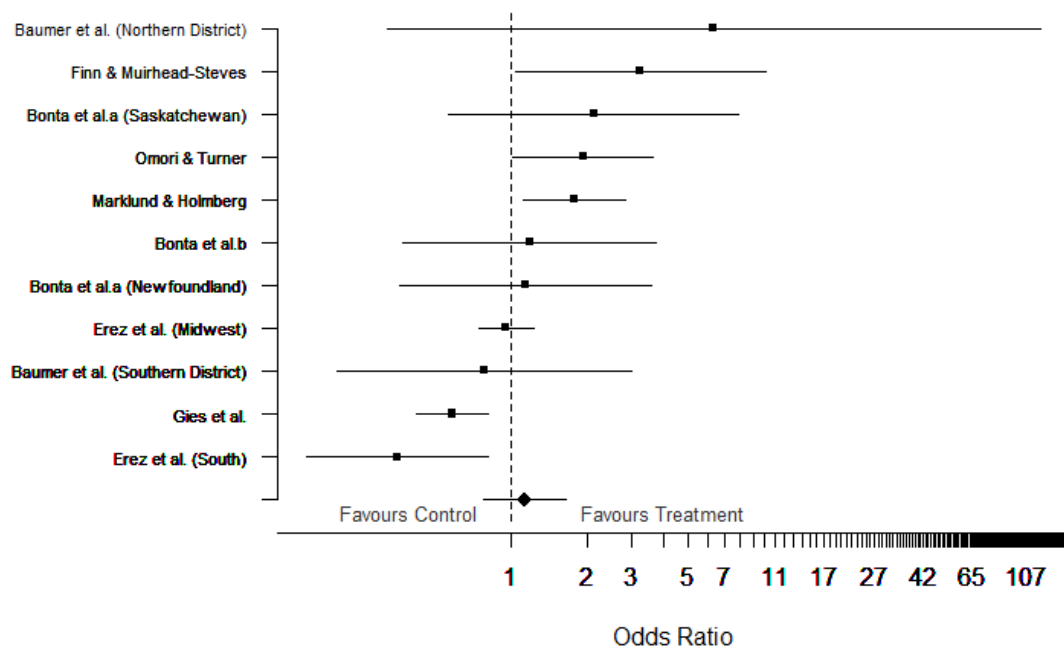


Figure 17- Forest plot of the studies with EM compared to 'business as usual'

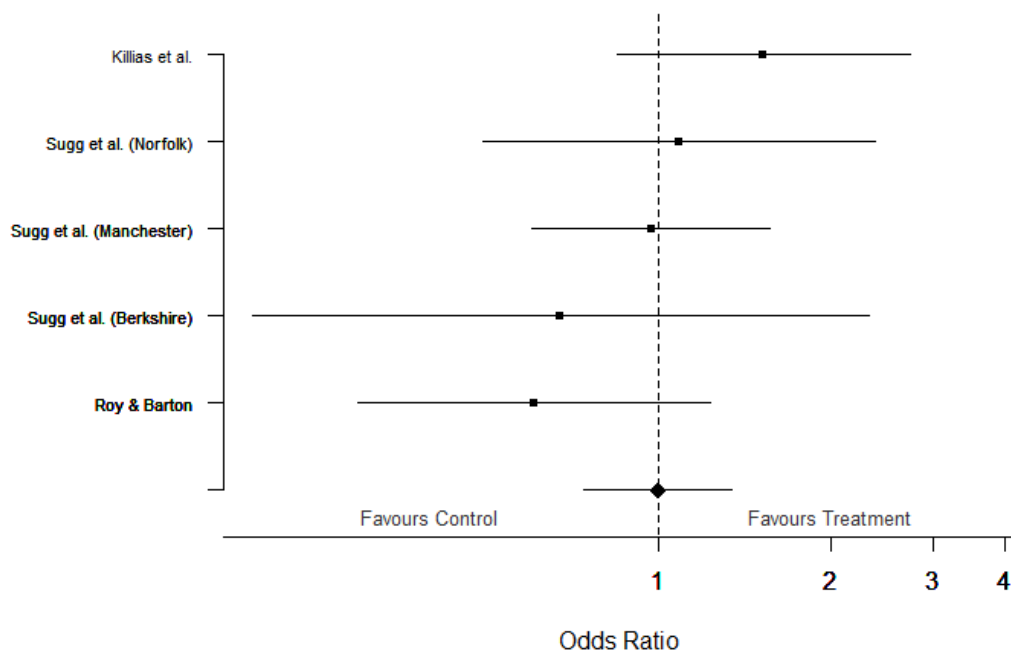


Figure 18 - Forest plot of the studies with EM compared to community sentences

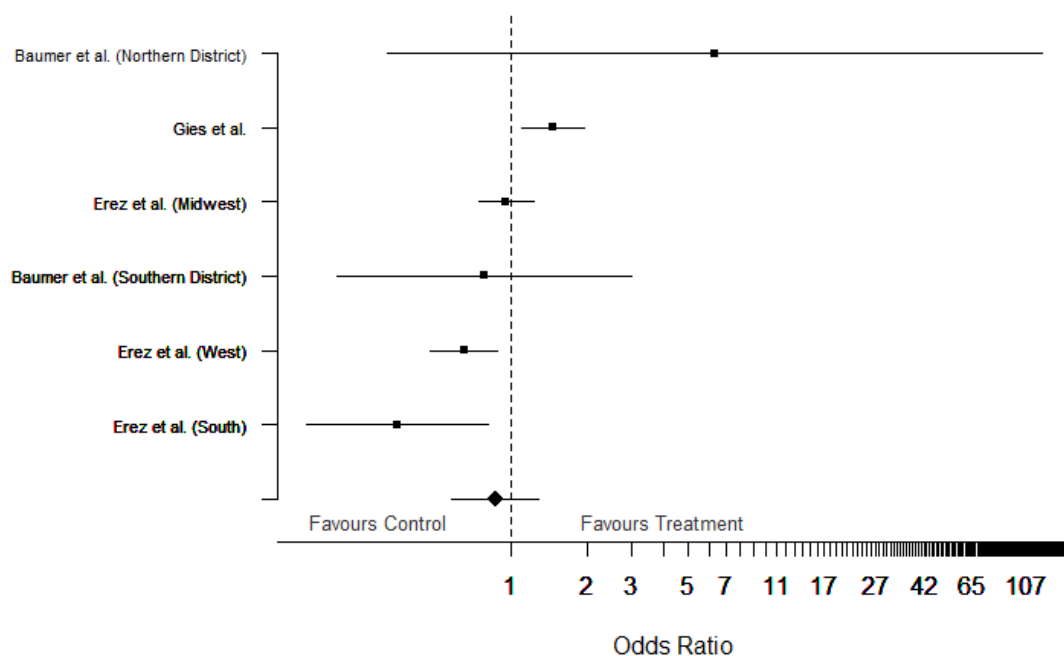


Figure 19 - Forest plot of the studies using re-arrest as the outcome

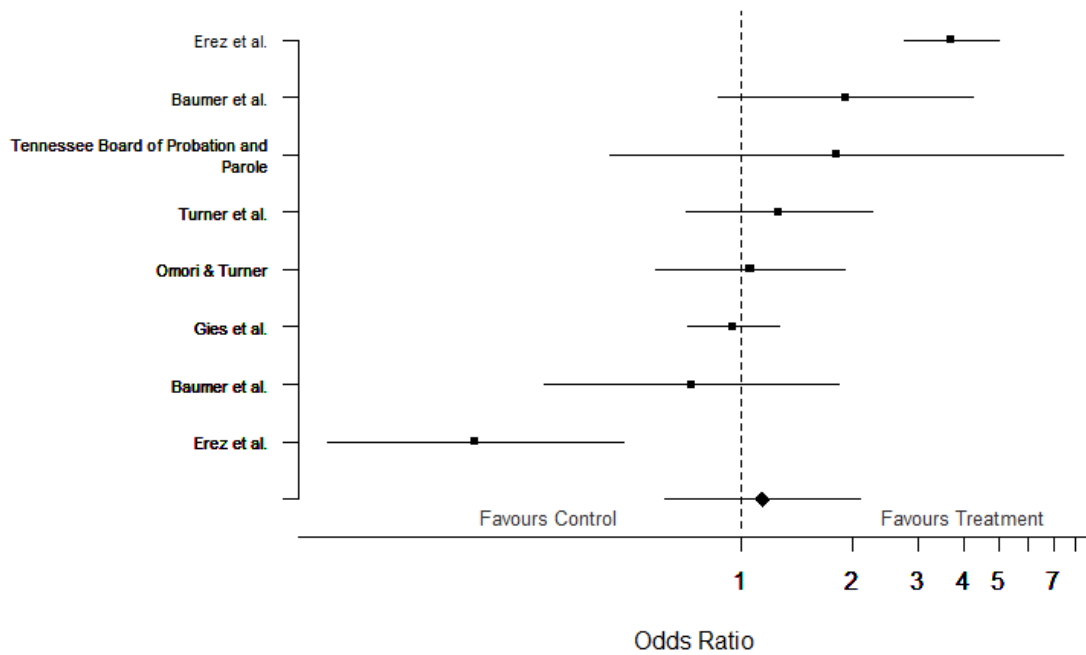


Figure 20 - Forest plot of the studies using parole violations as the outcome

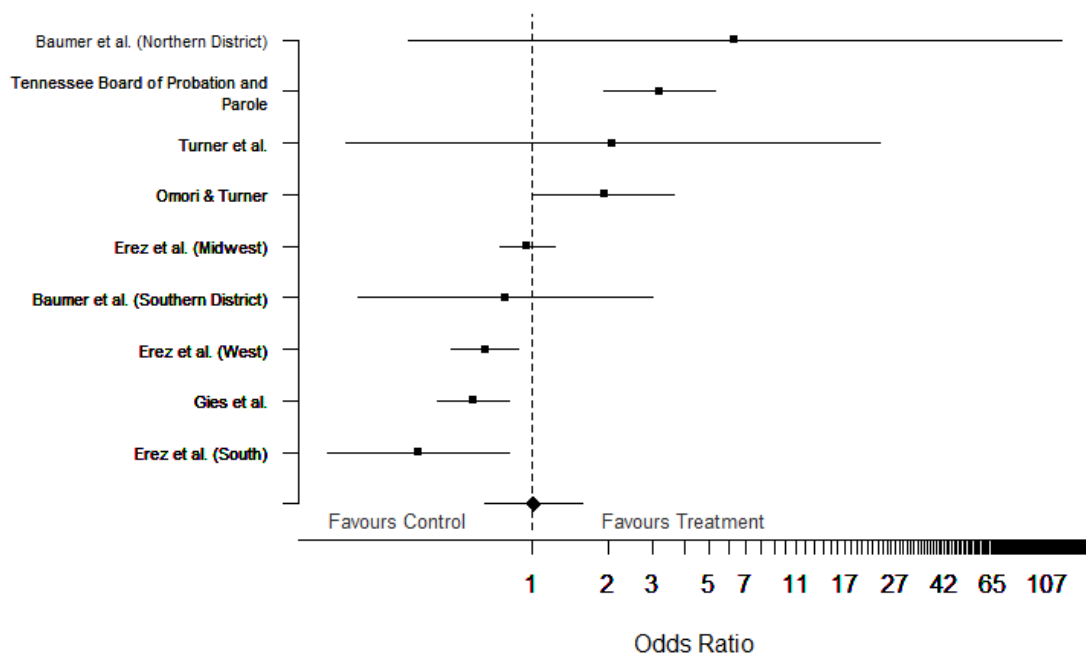


Figure 21 - Forest plot of the studies using GPS technology

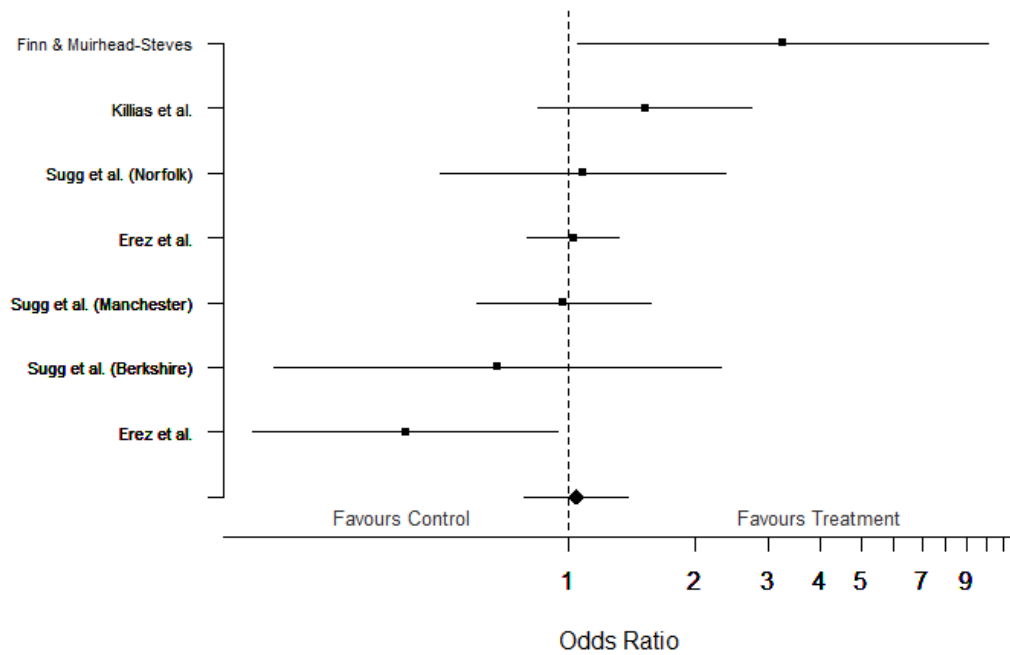


Figure 22 - Forest plot of the studies using RFID technology

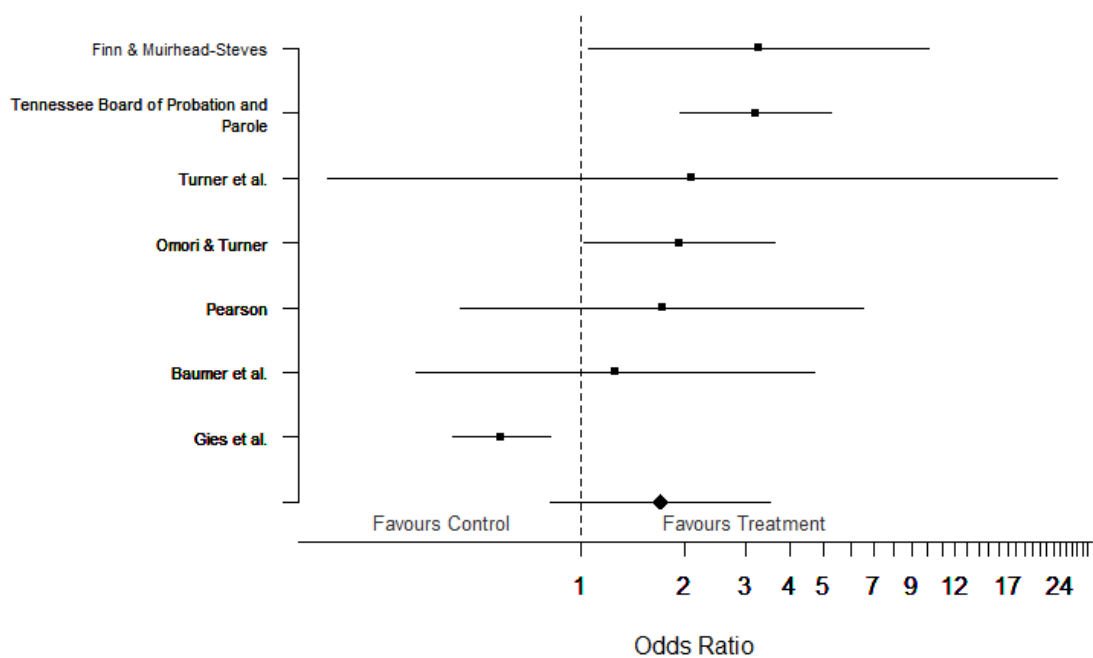


Figure 23 - Forest plot of the study areas where EM was implemented post-prison

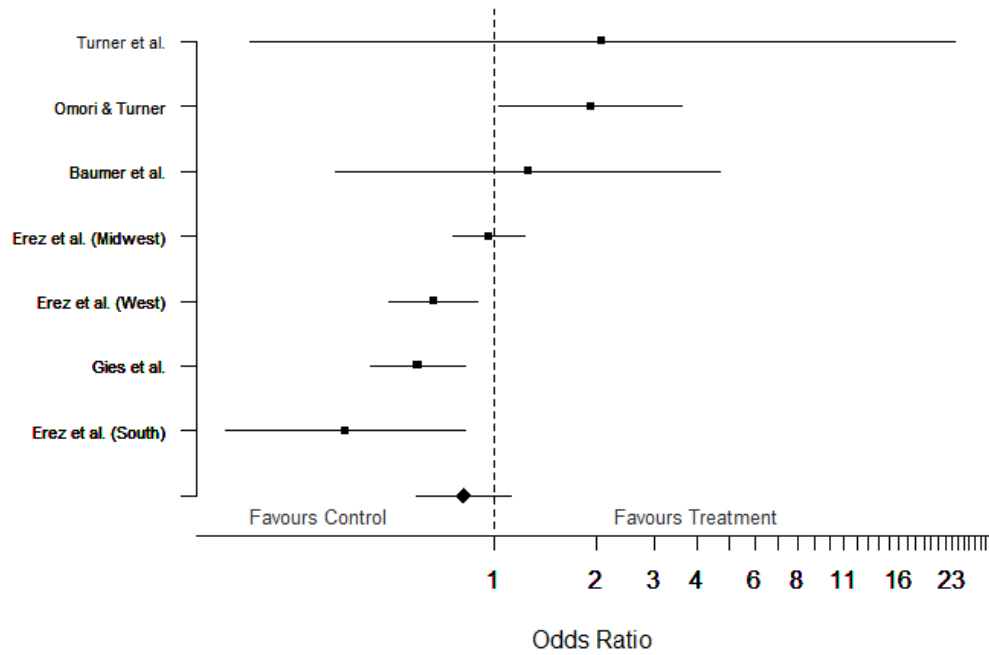


Figure 24 - Forest plot of the studies with high-risk offenders
 N.B. The Baumer et al. study combined the two geographical areas

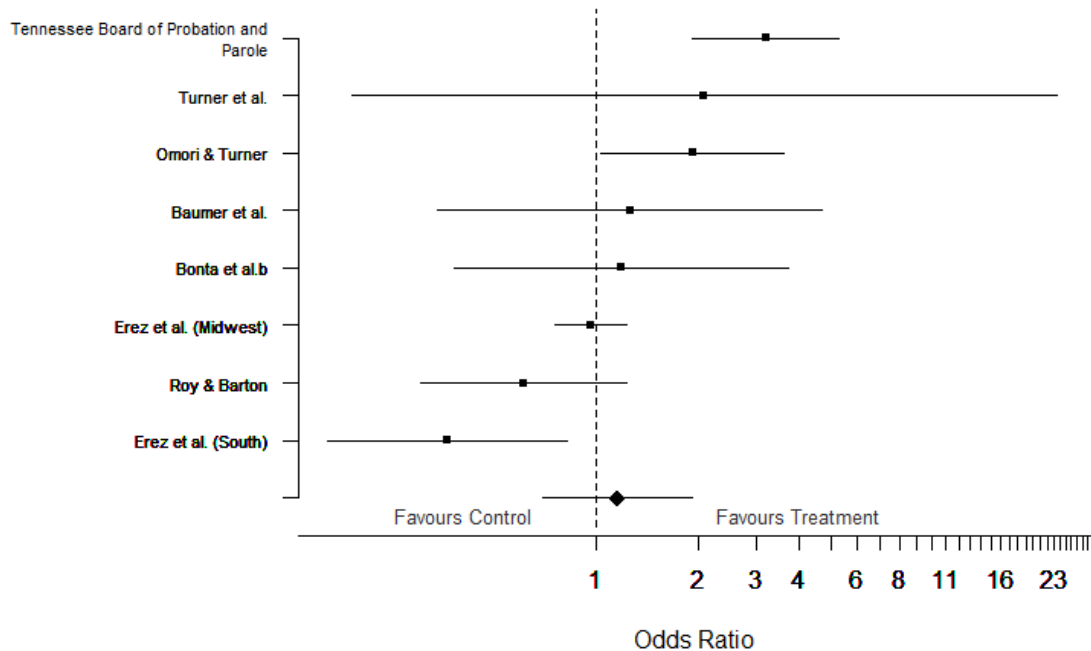


Figure 25 - Forest plot of the studies with EM implemented as a standalone intervention

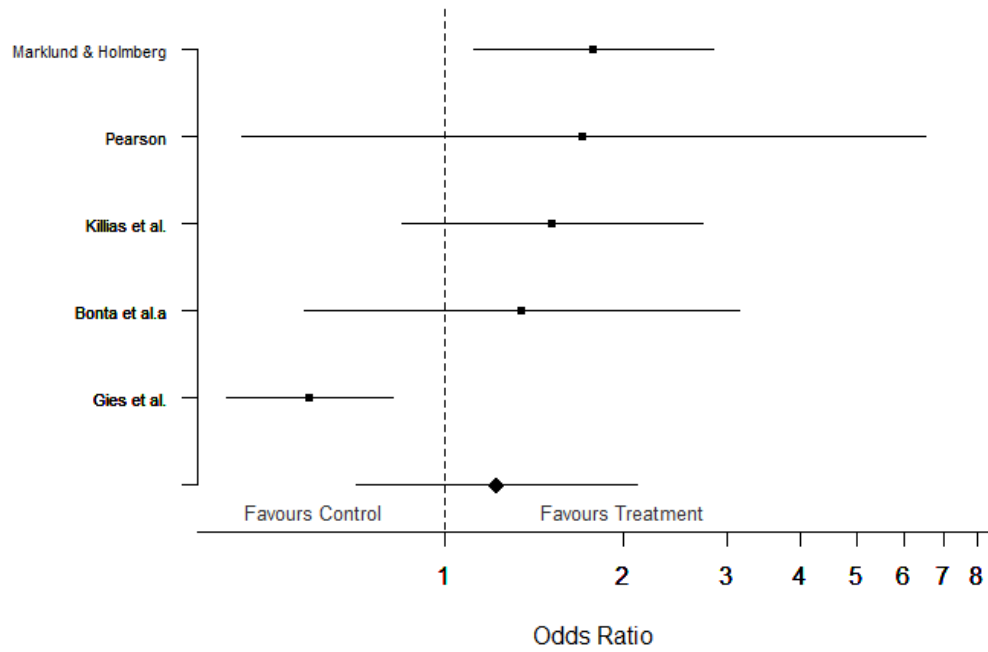


Figure 26 - Forest plot of the studies with EM implemented as a package of interventions

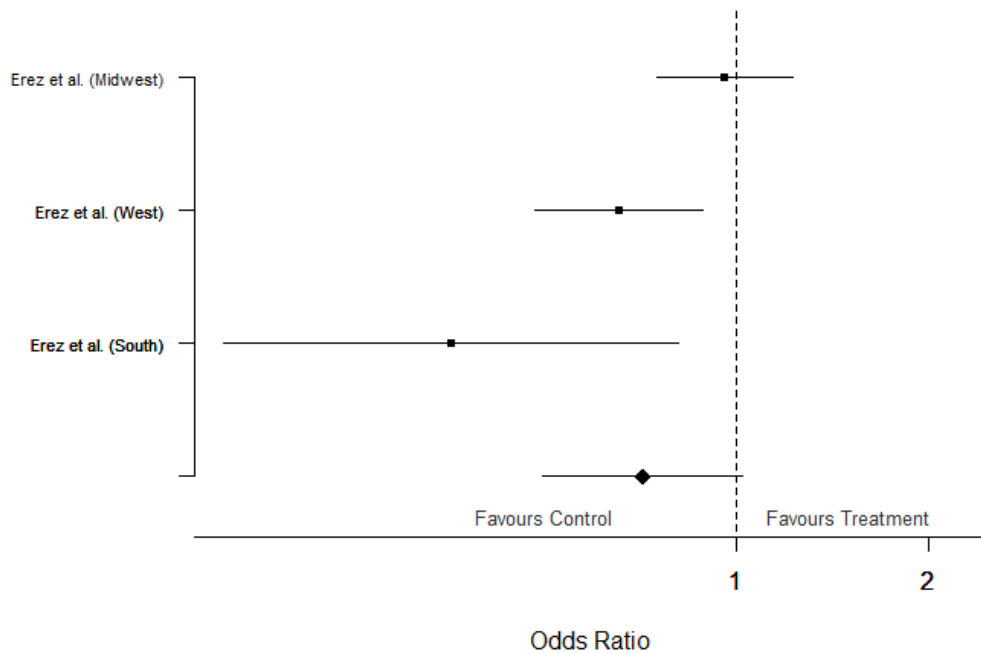


Figure 27 - Forest plot of the study areas where EM was implemented pre-sentence

Appendix G – Summary table of studies included in the meta-analysis

Study	Study setting	Method	Sample size	Data period	Data	Outcomes	Offender type	Comparison group
Bales et al., 2010	Florida, USA	Quasi-experimental 1 (comparable groups)	Treatment group = 87, Comparison group = 47 (across both areas)	Shorter than EM period	Cox's regression and hazard ratios	absconding from supervision, revocations for technical violations, and revocations for misdemeanour or felony arrests	Medium- and high-risk offenders	Non-EM probation conditions
Baumer et al., 2008	Indiana, USA	Quasi-experimental 2 (non-comparable groups)		Unclear	Proportions	Arrests, non-compliance with EM conditions, 'not successful'	Sex offenders	Non-EM probation conditions
Bonta et al. 2000a	BC, SK, NL, Canada	Quasi-experimental 2 (non-comparable groups)	Treatment group = 262, Comparison group = 256 (across all areas in prison) and 30 (non-EM probation)	Longer than EM period	Proportions	Reoffending	Non-specific	1) in prison and 2) non-EM probation conditions
Bonta et al. 2000b	NF, Canada	Quasi-experimental 1 (comparable groups)	Treatment group = 54, Comparison group = 100 (released from prison) and 17 (non-EM probation)	Longer than EM period	Proportions	Reconvictions (police data)	Non-specific	1) released from prison and 2) non-EM probation conditions

Erez et al. 2012	USA ('West', 'Midwest' and 'South' regions)	Quasi-experimental 1 (comparable groups)	Treatment (GPS EM) group = 1,087, RF EM comparison group = 632, Non-GPS comparison group = 437, in prison comparison group = 1,223, Non-EM probation = 725 (across all areas)	Longer than EM period	Proportions	Arrests and non-compliance with EM conditions	Domestic violence offenders	1) RF EM group, 2) Non-GPS EM group, 3) in prison, 4) non-EM probation conditions
Finn & Muirhead-Steves (2002)	Georgia, USA	Quasi-experimental 2 (non-comparable groups)	Treatment group = 128, comparison group = 158	Longer than EM period	Raw data, logistic regression and hazard ratios	Reimprisonment	Violent male offenders (inc. sex offenders and homicide)	Non-EM probation conditions
Gies et al. (2013)	California, USA	Quasi-experimental 1 (comparable groups)	Treatment group = 392, comparison group = 392	Unclear	Proportions	Arrests, non-compliance with EM conditions, return to custody	High risk gang offenders	Non-EM probation conditions
Killias et al. (2010)	Switzerland	Randomised control trial	Treatment group = 115, comparison group = 117	Unclear	Proportions	Reconvictions, self-reported offending	Non-specific	Community service
Lapham et al. (2007)	Oregon, USA	Randomised control trial	Standard DISP = 118, standard DISP no EM = 118, standard DISP no vehicle sale = 116, standard DISP no EM no vehicle sale = 120	Longer than EM period	Hazard ratios	Re-arrest	Drink drivers (repeat offenders)	Four treatment groups: with and without EM, with and without mandatory vehicle sale

Marie (2009); Marie et al. (2011)	UK	Quasi-experimental 1 (regression discontinuity with comparable groups)	Treatment group = 63,584, comparison group = 126,906	Longer than EM period	Regression estimates	Conviction (Courts data)	Non-specific	Released from prison
Marklund & Holmberg (2009)	Sweden	Quasi-experimental 1 (comparable groups)	Treatment group = 260, comparison group = 260	Unclear	Proportions, hazard ratios	Reconvictions		Non-EM probation conditions
Omori & Turner (2015)	California, USA	Quasi-experimental 1 (comparable groups)	Treatment group = 94, comparison group = 91	Longer than EM period	Proportions	New offence, non-compliance with EM/probation conditions	High risk sex offenders	Non-EM probation conditions
Pearson (2012)	Winnipeg, Canada	Quasi-experimental 1 (comparable groups)	Treatment group = 45, comparison group = 42	Longer than EM period	Mean and sd of offences	Charges for vehicle theft, other crime and for non-compliance	Young vehicle-theft offenders	Non-EM probation conditions
Roy and Barton (2007)	Indiana, USA	Quasi-experimental 2 (non-comparable groups)	Treatment group = 118, comparison group = 51	Unclear	Proportions	New offence, non-compliance with EM/probation conditions	Drink drivers	Day release centre
Sugg et al. (2001)	Berkshire, Manchester and Norfolk, UK	Quasi-experimental 1 (comparable groups)	Treatment group = 261, comparison group = 51 (across all areas)	Longer than EM period	Proportions	Reconvictions	Non-specific	Combination and community service orders
Tennessee Board of Probation	Tennessee, USA	Quasi-experimental 2 (non-comparable groups)	Treatment group = 493, comparison group = 370	Unclear	Raw data	New charge, non-compliance with EM conditions	Sex offenders	Non-EM probation conditions

and Parole (2007)								
Turner et al. (2010; 2015)	California, USA	Quasi-experimental 1 (comparable groups)	Treatment group = 94, comparison group = 91	Longer than EM period	Proportions	New offence, non- compliance with EM/probation conditions	Sex offenders	Non-EM probation conditions

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