

Can electronic monitoring reduce crime for moderate to high-risk offenders?

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Abstract. Electronic monitoring (EM) of offenders has been in use for just over two decades and motives for using it remain diverse. Some agencies that use EM attempt to deliver humane and affordable sanctions while others seek to relieve jail crowding or to avoid the construction of new jails. Nonetheless, all EM programs aim to suppress the criminal behavior of offenders being monitored and its advocates have always hoped EM could be instrumental in reducing long-term recidivism. This review investigates the history of EM and the extent to which EM empirically affects criminal behavior in moderate to high-risk populations. All available recidivism studies that included at least one comparison group between the first impact study in 1986 and 2002 were considered for the review. Although variants such as GPS tracking and continuous testing for alcohol in perspiration have recently emerged, no studies of these technologies were found that met the review's inclusion criteria. Studies are examined and combined for meta-analysis where appropriate. Given its continued and widespread use and the dearth of reliable information about its effects, the authors conclude that applications of EM as a tool for reducing crime are not supported by existing data. Properly controlled experiments would be required to draw stronger conclusions about the effects of EM.

Key words: electronic monitoring, evaluation, house arrest, house detention, meta-analysis, offender, parole, probation, recidivism, re-offending, review, systematic review, tagging

Introduction

Electronic monitoring (EM) is either in routine use or has been piloted on every inhabited continent. Overwhelmingly, prison overcrowding and the cost of building new prisons are cited as reasons for using EM. But today there are about 100,000 people in the United States being electronically monitored (Conway 2003: 5) and Europe is currently experiencing a wave of EM growth akin to that which swept the United States in the late 1980s (albeit with more attention to planning, quality of implementation, and attention to evaluation). By mid-2004, the number of offenders in Europe who had experienced EM exceeded 150,000. The daily caseload was just under 9,000, of whom 77% were in England and Wales.¹

For some people on EM, monitoring represents a true alternative to prison; without EM, some people who are monitored would be incarcerated. But children are on EM, people who refuse to pay child support are on EM, and so are tax cheats, drunk drivers, child molesters, and paroled killers.

While EM has been implemented in similar ways around the world, its use varies consistently between low-risk and high-risk offenders. In low-risk populations, EM may be used by itself or in conjunction with other forms of

low-contact monitoring. In moderate to high-risk populations, EM is more likely to be one part of a program that involves human contact and supervision, drug treatment, or other services. For these offenders, EM might be used as a true prison diversion program, thereby addressing overcrowding. But it is not known if EM is the best way to address the precipitating issues for this population. EM seems to be included as a solution to prison crowding largely because the public tolerates it.

This review is the first of two reviews of EM. In this work, the authors examine the impact of EM on recidivism for moderate to high-risk offenders. Most of the offenders in this review have been arrested several times, arrested at an early age and convicted of a serious offense. For these offenders, EM may serve its purported role as an alternative to incarceration. A second review will examine low-risk offenders whose crimes and characteristics differ from the offenders included in this review and for whom incarceration is not a likely sanction.²

As this and other reviews find, EM has not demonstrated superiority to options such as penal code reform, intensive probation, or psychotherapy in reducing the burden of imprisonment or in reducing recidivism among moderate to high-risk offenders.

Background

History of the intervention

In the 1960s, a research group at Harvard worked on the development of medical telemetry and tracking systems. As part of their experiments, a few volunteer offenders were electronically monitored and one of the investigators proposed that the equipment could be used as an adjunct to psychotherapy and to enhance accountability (Schwitzgebel 1967). Although there was discussion of the constitutional implications of such tracking during the 1970s, no new applications were attempted during the decade.

House arrest *without* electronically-aided enforcement, used since biblical times, underwent significant expansion in the late 1970s and early 1980s, largely consequent to institutional population pressures. Despite large-scale use, many agencies were uneasy about offender compliance with what was also known as “home detention” or “home confinement.”

Continuous signaling (CS) technology

By the early 1980s, three companies were experimenting with monitoring systems that consisted of ankle-worn radio transmitters and programmable receivers placed in offenders’ homes connected to hardwired telephone lines. Because the devices worn by offenders were constantly monitored, these were frequently called “continuous signaling” systems (CS). At defined intervals and whenever an unauthorized absence or other suspect event occurred, the receivers would automatically place calls to monitoring agencies. The agency could be either a public criminal justice agency or a private contractor that would relay violation results to the responsible public agency. Violating offenders could be taken into custody or

otherwise sanctioned. Although called “continuous signaling” technology, the devices usually monitored only presence/absence at a single location. Sporadic use was made of dual home/work monitoring units as well as “drive-by” units that could pick up the ankle transmitter’s signal at work, educational institutions, or treatment programs. Although offenders could cut the transmitters off from their ankles, various “tamper alert” systems assured that such violations were discovered. Over time, drive-by units were adopted by more and more agencies and used on a regular basis not only to check compliance at scheduled locations outside of the home, but also during sweeps of “hot” violation zones, such as bars and areas known for drug sales.

It is important to note that early systems frequently needed repair and generated abundant “false positives” of offender curfew violations. In many instances, it is impossible to know whether a “monitored” group actually received monitoring to the extent intended. It is also impossible to specify when technical improvements and increased agency competence resulted in acceptable program integrity. While later research is not exempt from technical problems or user competence problems, according to Peggy Conway, editor of *The Journal of Offender Monitoring*, by the late 1990s technical problems had become tertiary to cost and workload issues. All EM research, but particularly that done before 1990, should be examined for treatment delivery problems; the degree to which EM was used as it was meant to be used must be considered.

Random calling (RC) technology

Other machines were not in continuous contact with a device worn by an offender but, instead, used random calling (RC) to track offenders. To verify that the offender was answering the telephone, a variety of systems were used. Marketed first and most popular was an ankle-worn locked band that contained a magnetic key, which had to be mated with a wand connected to a telephone attachment. Identity verification systems included slow-scan picture phones, electronic voice analysis, and code emitting wristwatches. Remote breath-testing for alcohol was developed by the late 1980s and is a variant of RC technology.

Recent developments

In late 1997, two vendors began marketing systems that mated CS, wireless phone, and Global Positioning System (GPS) technologies. Although GPS tracking is limited by cellular network coverage and blockage of satellite coverage by structures, agencies were attracted by the ability to track offenders in real-time. As of 2004, GPS tracking appears to be gaining market share at the expense of CS systems. In 2001, a demonstration project began on a system that linked GPS tracking with police crime-mapping databases. If applied to large numbers of offenders, police could identify offenders in proximity to a reported crime or provide an “electronic alibi” for offenders who were not in the vicinity of the crime. GPS-based loggers that record offender movements but do not relay

movements in real-time to a monitoring agency have also been tried; data from these systems are typically uploaded daily through a modem.

Over the years, several types of home-installed RC systems that test for alcohol use have been introduced with mixed results. In 2003, CS equipment was introduced that can perform up to two tests per hour for alcohol emitted through the skin. Research is underway that may result in remote testing, with or without instant agency notification, for other drugs via traces found in sweat, characteristic eye movements, voice changes, or muscle tremors.

Although all of the emergent technologies have found marketplace acceptance, as of the cutoff for this review, none had been studied relative to reoffending using minimally acceptable methodologies. All of the studies reviewed in this report used either RC or CS monitoring.

Applications of electronic monitoring

In moderate to high-risk populations, EM is often intended as a diversion program; it is used in lieu of jail or prison to relieve overcrowding or to reduce the need for new prisons and jails. EM may also be used at the end of a prison sentence with the intent of helping prisoners transition into their communities. But other prison diversion programs exist; while the impacts of EM on reoffending might be compared to the impacts of prison, EM must also be compared to other programs.

No definitive reports of EM's effects on crime exist, yet it is extremely important to examine the effects of EM on crime for several reasons.

First, EM may have positive, negative or neutral effects on offending during its use. Compared to unsupervised release, EM might suppress crime during the monitored period, but when it is applied to offenders who would otherwise be incarcerated, EM might expose communities to risk during the period of monitoring.

Second, EM may have positive, negative or neutral effects on criminal behavior after its completion.³ Again, EM must be considered relative to other options. For example, compared to EM, prison might be relatively criminogenic while drug treatment might reduce recidivism.

Finally, because the use of EM varies by population and because the impact of EM in low-risk populations may differ from its impact in high-risk populations, it is critical that researchers examine the effects of EM in each group and, if it is to be used at all, determine how EM is most effectively used with particular populations of offenders.

While EM may reduce spending on prisons and jails and while it may affect criminal behavior, EM might be applied in other innovative ways. In moderate to high-risk populations, EM could be used to reduce the burden of monitoring on probation and parole officers. Although other monitoring would continue, some parts of routine monitoring could become "automated" through the use of EM. EM could also be used as an early warning system to distinguish offenders able to function in the community from offenders for whom reincarceration is needed. In such a system, breaches of EM protocol would result in the return of recidivists (or people expected to recidivate) to prison and the release of reformed offenders into the community. Some recidivism would be expected among the EM completers, but

one would expect their rate of recidivism would be lower than the rate of recidivism for EM dropouts or comparable offenders not subjected to a period of EM.

Prior review results

Corbett and Marx (1991), Mainprize (1996), MacKenzie (1997), Schmidt (1998), Gendreau et al. (2000), and Whitfield (2001) all have done careful reviews of the literature about EM's effects. MacKenzie focused on two studies using random assignment while Gendreau et al. did a meta-analysis of 140 studies that included six studies of EM and a total of 1,414 offenders. No positive effects on recidivism for EM were claimed by any of the reviewers. In fact, Gendreau et al. (2000) noted a 6% recidivism rate for EM studies as compared to 4% for the comparison group, a difference not statistically significant. Gendreau et al. did note a 10% recidivism reduction for studies that included a "modicum" of treatment in addition to the primary interventions of intensive supervision programs, arrest, fines, restitution, boot camps, scared straight, drug testing, and electronic monitoring. Unfortunately, they found insufficient information in the studies to address issues of treatment quality.

In addition to the review articles, several research reports contain excellent syntheses of prior work, notably works by Klein-Saffran (unpublished data), Bonta et al. (1999), Gainey et al. (2000), and Finn and Muirhead-Steves (2002).

None of the reviews that examined the methodology of the reviewed studies were able to substantiate any general effect on post-EM recidivism.

The authors of this review improve upon previous efforts in seven aspects:

1. Following the Campbell Collaboration approved protocol (Renzema 2003), the search strategy is both more clearly defined and intensive than most previous reviews. In particular, efforts have been made to obtain agency reports and other unpublished studies in order to minimize publication bias.
2. Inclusion/exclusion criteria are specified and transparent.
3. Program integrity issues are considered in inclusion/exclusion decisions.
4. The extension of the review period through 2002 allows consideration of large studies and studies that are methodologically superior to previously reviewed work.
5. Where possible, outcomes are assessed at both the termination of EM and during a longer follow-up period in recognition of the hypothesis that EM might suppress crime during its application but not in the long run.
6. The authors code the presence/absence of several treatment elements that may co-occur with EM.
7. Although the work resulting in this review is ongoing and includes the evaluation of all applications of EM, given the work summarized in Cullen and Gendreau (2000) on the futility of diffuse interventions with low-risk offenders, the authors focus their initial analysis on moderate to high-risk populations.

Objectives

Considering the number of EM programs around the world and the wide range of potential EM outcomes, it is urgent that we understand what actually happens

when an offender is given EM rather than another intervention. In this review, the authors examine the effect of EM on crime both for the duration of EM and after the discontinuation of monitoring in moderate to high-risk populations.

Criteria for including studies in this review

Types of interventions

For the purpose of this review, electronic monitoring was defined as any technology that “records the location of an offender within the community at particular places and times without human observation and transmits these data electronically to a central monitoring station, or uses an electronic device to detect the presence of a prohibited substance in the body (or to monitor other physiological functions) of an offender living in the community and transmits those data to a central location” (Renzema 2003). This definition excludes ignition interlocks but includes GPS tracking, logging, and emerging drug-testing technologies.

Types of offenders

This review investigates the effectiveness of EM for moderate to high-risk adult (18+) offenders.

Developing a criterion for “moderate to high-risk” proves a bit troublesome in the absence of standard risk assessment instrument scores for most of the samples examined here. Included as “moderate to high-risk” are probationers and others for whom recidivism measures exceed 30% during the study’s criterion period, typically one to three years. This is arbitrary given the variety of recidivism definitions, follow-up periods, offender mixes, and policy variations across jurisdictions.

Offenders at the “back end” of the criminal justice system, i.e., parolees, early releasees, and divertees who have served some institutional time are also defined here per se as “moderate to high-risk.”⁴

Comparison groups

To be considered, a study must have included one or more appropriate comparison groups receiving:

1. Traditional probation or parole
2. Intensive supervision probation or parole
3. Incarceration
4. An intervention other than parole or incarceration

Group assignment

To be considered, studies must have used one of the following methods of group assignment:

1. Random allocation—offenders in the EM group and control group are placed in groups without any attempt by researchers, judges, prosecutors, etc., to match them with offenders in another condition or to otherwise influence assignment.

2. Matching—offenders in the EM group are matched with a contemporary group of subjects that has the same risk of recidivism and is highly similar in most recent crime committed, criminal history, and demographic variables.
3. Historical matching—offenders in the EM group are compared to matched subjects from a comparable time period before EM was implemented in the area where the study takes place.

Outcome measures

To be considered for the review, a study must have included at least one primary outcome measure or one secondary outcome measure.

Primary outcomes:

1. Release condition violations resulting in reincarceration
2. Arrest for a new crime
3. Conviction of a new crime

Secondary outcomes:

1. Violations not resulting in a return to prison
2. Employment
3. Restitution
4. Substance abuse as measured by testing

Search strategy

The lead author attempted to obtain all research, published and unpublished, concerning the impact of EM on offender behavior. Electronic searches were conducted, reference lists and conference reports were examined, government agencies in the U.S., Canada, England, France, the Netherlands, and Sweden were contacted, equipment producers were surveyed, and leading researchers were asked for leads. No language restrictions were applied; studies were found in English, French, Dutch, Swedish, and German. For a more detailed description of the search, see Renzema (2003: 12–16).

In all, 381 articles or abstracts on EM were reviewed. Of these, 154 appeared to include evaluations. At this writing, one of the 154 remains fugitive but would probably *not* be included as the abstract makes no reference to a comparison group (Schafer and Martin 2001). The lead author designed a spreadsheet that includes the key characteristics of the 119 studies that were accurately classified as evaluations of EM.⁵ Of the 119 studies, 100 were immediately and clearly eliminated as not meeting the inclusion criteria. The remaining studies were independently examined by both authors with the inclusion/exclusion decisions reached jointly. Those selected for inclusion were independently coded; coding differences were reconciled in conference (Figure 1).

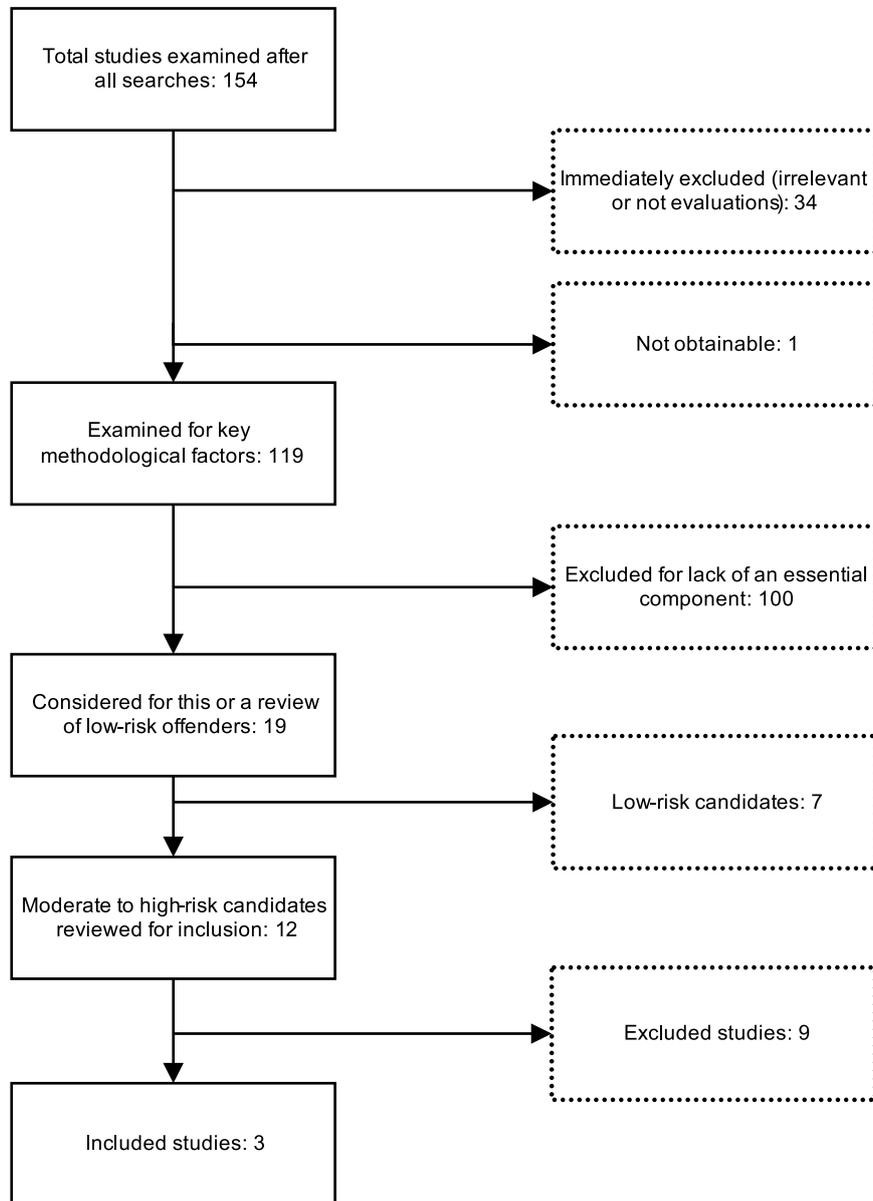


Figure 1. Study search flow chart.

Results

Excluded studies

The nine studies described below were considered for inclusion in this meta-analysis but were eventually excluded.

Petersilia and Turner (1990) conducted one of only four randomized trials discovered during the search. It was one of two randomized controlled trials (RCTs) that considered recidivism as an outcome variable. The authors attempted to study the marginal value of EM as an adjunct to probation; that is, they tried to understand the impact of EM on offenders already under intense human supervision.⁶ While many reports detail the cost and purported benefits of electronic monitoring, few studies examine its value as an instrument used in conjunction with other crime suppressing tools that can be used independently. Petersilia and Turner aimed to do exactly this in Los Angeles, but they encountered barriers to implementation that made their results impossible to interpret with any confidence. As a result of those barriers, only 44% of the offenders assigned to the EM group were ever monitored electronically. Additionally, offenders in EM and intense supervision groups were supposed to receive ten contacts per month during the follow-up period; most received four. Poorly implemented human surveillance resulted in a difference between the groups. The authors estimate that 40% of the offenders receiving EM received a medium (32%) or high (8%) level of face-to-face contact whereas only 28% of the intense supervision group received a medium (22%) or high (6%) level of face-to-face contact. These contacts, a slightly higher number of telephone and collateral contacts, and law enforcement checks of the EM group likely caused the resultant differences in recorded violations.

Though not included in the selection criteria above, one post-hoc hypothesis, that EM needs to be delivered to be effective, seemed reasonable. The study was excluded because treatment integrity was considered insufficient (both by Petersilia and Turner and by the reviewers)⁷ to support any conclusions about EM.

The present authors regret that neither Petersilia and Turner (1990) nor this review add to our immediate understanding of EM's costs and benefits as an adjunct to parole or probation. One can only conclude that high-quality research of the marginal value of EM (the value of EM as an addition to existing methods of supervision) is desperately needed.

Austin and Hardyman (unpublished data) studied the early release of prisoners in Oklahoma through the Pre-Parole Conditional Supervision Program. Between 1989 and 1991, EM was tested as an additional component of an established pre-release program.

Unlike other studies that assign offenders who can meet the conditions of monitoring to EM and assign those offenders who cannot meet these conditions to other groups, Austin and Hardyman (unpublished data) screened participants for their abilities to participate in EM (they were required to have a residence with a phone) and then randomly assigned only those subjects who met the inclusion criteria. Methodologically, this design is laudable and exceptionally rare, but some

of those who were randomized did not actually meet the criteria as anticipated. Offenders who were randomized to the EM group but could not receive EM were put into a "No Phone" group. The study would have benefited by similarly screening the control participants after randomization, but this problem was unforeseen. Still, data collection continued and the authors presented all of the data. Unfortunately, the experimental group may have been creamed despite an otherwise superb effort to obtain comparable groups. That is, those offenders in the experimental group may have been more likely to succeed than offenders in the comparison group.

Despite the potential bias, the study is of comparable or better quality than other studies included in this review. It is not included in the meta-analysis because, apparently as a result of chance, the follow-up periods differed greatly between the EM group and the control group. On average, offenders in the control group were followed for 105.4 days and offenders in the EM group were followed for 126.6 days (20% longer). Rates of recidivism in the EM group were higher than rates of recidivism in the control group, but the difference may be related to the follow-up period; the re-arrest rate for offenders receiving EM (13.9%) was 25% higher than the rate of re-arrest in the control group (11.2%). Accounting for the follow-up period, more technical violations were recorded in the EM group than in the control group, but this did not appear to be related to new crimes.

Austin and Hardyman conclude that EM ought not to be used with all offenders and that the ability of EM to assist in monitoring those parolees who are at highest risk of parole violation is worth examining. If one's goal is to detect technical violations, this appears to be correct. If one wants to reduce crime, this study shows no value for EM in addition to another form of monitoring.

Dodgson et al. (2001) considered EM as an early release program in the U.K. The authors examined a group of 118 prisoners released to home detention curfew (HDC), which was intended to ease the transition of prisoners into society and to reduce recidivism. During the period studied, an additional 558 prisoners were eligible for HDC based on statistical indicators but were denied release during a subjective evaluation.

The study was considered because the offenders studied had already served a custodial sentence and because the risk of recidivism for the group as a whole was moderate. But those offenders who were granted HDC had a much lower likelihood of recidivism than their peers who were denied HDC.

Recognizing that the released group had been creamed, Dodgson and her colleagues decided not to compare the results of the group granted HDC to another population. Instead, they combined the HDC group with the group not granted HDC, and compared the results to a historical control that would have been eligible for consideration had HDC been operating at the time. This resulted in a group of 676 offenders who had been granted (118) or denied HDC (558), and a historical comparison of 6,723 offenders.

Logically, if HDC had a strong effect on recidivism, the more recent group would have demonstrated a reduced rate of recidivism. The group granted HDC, however, represented the lowest risk group in the sample and, at six months, had a

rate of recidivism of only 9.3%. Hypothetically, if this represented a 50% decline in recidivism (11 recidivists rather than 22), the rate of recidivism in the larger group would have declined by only 1.6%. Even the most optimistic proponent of EM would expect a much smaller decrease in recidivism. Lowering the rate of recidivism in the treated group from 11.6% to 9.3%, or decreasing it by 20%, would represent a raw decrease from 14 recidivists to 11 recidivists. By diluting the treatment group, this very significant decline would become undetectable.

The study was excluded because there is no reliable way to determine the effect of EM on the treated group. While it would be inappropriate to include such a design in a meta-analysis, the design used by Dodgson et al. is not without merit. The design is logically defensible, but a much larger sample would be required to detect even a strong effect of EM on recidivism in this population during a short period.

One study (Florida 1987), which appeared superficially to be an RCT, did not appear, on closer examination, to be of high enough methodological quality to be included in the review.

Five studies (Jolin 1987; Jolin and Stipak 1992; Jones and Ross 1997; Klein-Saffran, unpublished data; Quinn and Holman 1991) were excluded because the reviewers concluded that the comparison groups were inadequately matched. The reviewers considered the potential for bias in judging the degree to which control groups matched experimental groups, but decided that potential biases in study selection were outweighed by biases in studies thought to be severely flawed. Reasons for exclusion are listed in Table 1.

In the studies excluded due to poorly matched comparison groups, some variables (e.g., age, number of prior convictions, risk scores) could be coded and considered through statistical analysis, but other variables that influenced group assignment could not be quantified and appeared to affect recidivism. For example, Klein-Saffran (unpublished data) considered two groups that differed on important variables, but were similar in most respects. However, offenders assigned to halfway houses could not find suitable accommodation on their own. Despite other statistical similarities, the reviewers believed that this difference would be impossible to control through any amount of statistical adjustment.

The reviewers note that all matched studies in this field are likely to include groups that are different in some way. Even well-matched historical controls may differ on one important variable. Still, only those studies that met the relatively strict inclusion criteria outlined above were included because the reviewers felt that only studies with the specified characteristics would provide real evidence of the effect of EM on recidivism.⁸ While some might argue that other studies should have been included in this review and meta-analysis despite the objections outlined here, the reviewers doubt that biases or errors in judgment influenced the final result. It is noteworthy that among the studies excluded for poorly matched control groups, results exist both in favor of EM and in favor of the comparison group. It is also worth noting that the outcomes of these studies are consistently in the direction one would predict at baseline given the characteristics of the groups.

Table 1. Excluded studies.

<i>Study</i>	<i>Summary of results</i>	<i>Reason for exclusion</i>
Austin and Hardyman (1991)	Accounting for differences in length of monitoring, EM showed no effect as an adjunct to another form of monitoring. EM did, however, appear to increase the detection of parole violations.	As a result of chance, follow-up periods between groups were too different to allow reasonable comparisons. While the study was well designed and included a safeguard against "creaming" that was not present in any other study examined, the experimental group may have been "creamed."
Dodgson et al. (2001)	After six months, those selected for release to EM had a low rate of recidivism (9.3%) compared to eligible offenders not granted release (40.5%) and an historical comparison group (30.0%).	Offenders placed on EM were carefully selected based on statistical and subjective analyses. Consequently, an appropriate comparison group could neither be found nor formed post hoc for inclusion in meta-analysis.
Florida (1987)	Apparent prison divertees on two types of monitoring were compared to a no-EM condition. EM paid more restitution.	The study was initially misclassified as an RCT. It says that assignment was "generally random," but no details concerning group assignment are offered.
Jolin (1987)	The EM group, a subset of current work releasees, was matched with past work releasees. The EM group had a lower rate of re-arrest than the comparison group.	The sentence length and follow-up periods differed between groups (6–18 <i>versus</i> 18–36 months) and the EM group may have been "creamed."
Jolin and Stipak (1992)	EM was compared to work release and a drug treatment program. Rates of re-arrest were compared. Even controlling for those differences that made inclusion in meta-analysis inappropriate, no reliable conclusions are possible due to baseline differences.	The EM and work release groups differed greatly in convictions for (1) felonies and (2) drug-related offenses. Comparisons to the drug treatment program were impossible for those and other reasons, notably a significant difference in age, another known predictor of recidivism.
Jones and Ross (1997)	The rate of fingerprinted re-arrest within two years after assignment to EM was compared to a group assigned to boot camp. While the EM group showed a much higher rate of recidivism than the boot camp group, this difference reflects baseline differences in risk. Evidence presented here does not support any conclusions concerning the relative merits of the programs.	The groups differed in previous and current convictions for (1) violent or sexual felonies, (2) violent misdemeanors, and (3) property offenses. EM participants were at risk of recidivism while offenders in boot camps were confined. Data were not available for failure on EM and failure after its completion. Approximately 40% of subjects were under 16 years old and nearly all (98%) were less than 23 years old.

Table 1. Continued.

<i>Study</i>	<i>Summary of results</i>	<i>Reason for exclusion</i>
Klein-Saffran (unpublished data)	Offenders were assigned halfway houses or EM as part of Southern Florida's Community Control Project. Within one year of release, those assigned to EM were less likely than offenders placed in halfway houses to be arrested or revoked. This difference likely resulted from baseline differences.	Offenders placed in halfway houses were refused EM by community corrections managers or parole officers, most often because they did not have suitable accommodation or because they "had a need for halfway house services." Offenders placed in halfway houses were convicted of their first offenses four years before the EM offenders, were thrice as likely to be black, and had a higher risk of recidivism.
Petersilia and Turner (1990)	Offenders assigned to EM were more likely than intensely supervised offenders to be jailed or arrested during their probation. Given the failure to actually implement EM, this difference probably resulted from higher levels of contact with probation officers in the EM group. No differences in recidivism appeared after one year.	This was a well-designed trial with random assignment. Implementation was so poor, however, that the reviewers judged that it had to be excluded. Of 52 subjects assigned to EM, only 23 (44%) were ever monitored. Furthermore, there was poor and highly dissimilar implementation of intense supervision probation (ISP), which was intended to be a common feature of the EM group and the comparison group.
Quinn and Holman (1991)	Violating probationers and parolees placed on EM were matched with demographically similar non-violating probationers and parolees. Violators failed twice as often.	In this study, the comparison group was "creamed." Consequently, offense and occupational status differences between the two groups were both logically and statistically significant.

Included studies

Only three studies of moderate to high-risk offenders met the inclusion criteria for the review. All three studies had unique methodologies. While comparisons are informative, the studies merit individual examination; the authors urge caution in interpreting the combined results, except insofar as one may conclude that there are virtually no data supporting the use of EM.

Finn and Muirhead-Steves (2002)

Of the included studies, only Finn and Muirhead-Steves (2002) reported outcomes at multiple times. They compared EM to an historical control for high-risk, violent male offenders in Georgia. As demonstrated in Table 2, Finn and Muirhead-Steves (2002) suggest that EM has a modest impact for its duration, but its effect is

Table 2. EM outcomes over three time periods among male parolees with violence history.

<i>Outcome</i>	<i>Proportions recommitted</i>	<i>Percent C recommittal >E (%)</i>	<i>Odds ratio (fixed)</i>	<i>Lower limit</i>	<i>Upper limit</i>	<i>Z</i>	<i>p Value</i>
Recommitted within 150 days	E: 0/128; C: 4/158	2.53	0.134	0.007	2.505	-1.346	0.178
Recommitted within one year	E: 4/128; C: 15/125	6.93	0.308	0.099	0.951	-2.047	0.041
Recommitted within three years	E: 30/128; C: 37/158	-0.02	1.001	0.577	1.736	0.004	0.997

Limits are for 95% CI; mean duration of EM was 87.4 days with a range of 6–153 days.
Source: Finn and Muirhead-Steves (2002: 303–304).

transient; after EM ends, monitored offenders “catch up” to those who did not experience it. Within three years of release, 23.4% of the EM group ($n = 128$) and 23.4% of an historical comparison group ($n = 158$) were returned to prison.⁹

For one subgroup in Georgia, sex offenders, EM may have reduced recidivism; however, there is another plausible explanation for the observed effect and the study design precludes any definitive conclusions about the unique effects of EM.

Although program details are sketchy, during the study period Georgia was beginning implementation of the “containment model” of sex offender management, an empirically-based highly intensive treatment and surveillance approach described by English et al. (1996). In an e-mail to the first author, John Prevost, Associate Director of Research and Technology at the Georgia Board of Pardons and Parole, described sex offender treatment during the study period as “scattered local initiatives in selected parts of the state.”¹⁰ He noted that the addition of a planned minimum of 90 days of EM was a new element in the treatment package but that treatment was not universal for sex offenders in either time period. Most of the offenders in the control group probably did not receive polygraph exams; a few of the offenders in the EM group may have. Prevost also indicated that there were early concerns about the quality and availability of contracted psychotherapeutic services and contracted polygraph examiners.

In other words, later released (the EM group) sex offenders may have received more extensive and more competent overall treatment than the historical controls released during the previous year. It is reasonably clear from the agency’s 1998 annual report¹¹ that by the end of the study period there was a high level of program integrity, but there may be some historical bias that would tend to reduce later-released sex offenders’ recidivism with or without EM.

The reviewers also caution readers who accept the hypothesis that either EM or the improved treatment of sex offenders reduced recidivism in that group. For this to be true, one must also accept that to result in the identical overall results that were

Table 3. Return to prison with three years for Georgia sex offenders.

Outcome	Treatment			
	EM + other		Comparison (other without EM)	
	Percentage	n	Percentage	n
Not returned	94.3	33	70.4	31
Returned	5.7	2	29.6	13

$p=0.0088$ (Fisher's exact test), $C=0.29$.

Source: Finn and Muirhead-Steves (2002), additional data supplied by Finn.

observed, EM may have *increased* the rate of recidivism among the remaining offenders.

Of sex offenders in the EM group, two of 35 were returned to prison; 13 of 44 sex offenders in the comparison group were returned to prison. As shown in Table 3, this percentage difference is statistically significant ($p=0.0088$) using Fisher's exact test.

Bonta, Wallace-Capretta and Rooney (2000)

Bonta and his colleagues found that EM combined with court orders tended to improve compliance with a treatment program (Bonta et al. 2000b). They also found that the combination was associated with significantly lower recidivism for a group of moderate to high-risk prison divertees, but the same program failed to produce results for lower risk offenders.

Table 4, from Bonta et al. (2000b), illustrates the strength of the relationship but includes both EM + treatment prison divertees (54) and treated probationers (17) and compares them with an untreated matched group of prisoners.¹²

In Newfoundland, Bonta et al. examined EM in conjunction with a treatment program, which was also offered to control subjects. Members of the experimental group averaged 71.4 days of EM and were required to attend a cognitive behavioral

Table 4. Bonta et al.'s recidivism as a function of risk level and treatment.

Risk level	Treatment			
	Yes (IRS) ^a		No (Prison)	
	Percentage	n	Percentage	n
Low	32.3	10	14.5	8
High	31.6	12	51.1	23

^aIncludes participants in "LDP," a CBT program of whom 54 were divertees with EM and 17 were probationers without EM.

Source: Bonta et al. (2000b: 324).

Table 5. Longest term outcome of EM discussion and policy implications.

Study or sub-category	EM n/N	Comp n/N	OR (fixed) 95% CI	Weight %	OR (fixed) 95% CI
Bonta 1999	17/54	6/17		7.46	0.84 [0.27, 2.66]
Finn 2002	30/128	37/158		30.24	1.00 [0.58, 1.74]
Sugg 2001	190/261	192/261		62.30	0.96 [0.65, 1.42]
Total (95% CI)	443	436		100.00	0.96 [0.71, 1.31]
Total events: 237 (EM), 235 (Comp)					
Test for heterogeneity: $\text{Chi}^2 = 0.07$, $\text{df} = 2$ ($P = 0.97$), $I^2 = 0\%$					
Test for overall effect: $Z = 0.23$ ($P = 0.82$)					

0.1 0.2 0.5 1 2 5 10
Favours treatment Favours control

therapy program for nine hours per week. Control subjects, probationers without EM, were not subject to revocation for failure to attend the program. Only 52.9% of the unmonitored probationers completed the therapeutic program. Of the divertees who were required to attend and also on EM, 87% completed the program.

Although suggestive, it is impossible to gauge whether higher completion rates were due to EM or due to the threat of revocation. Given these data, it is impossible to determine whether EM had an independent contribution to the lowered recidivism of the higher risk offenders. Still, whether or not it occurred in this case, this study does demonstrate one application of EM as a means of increasing participation rates in other programs.

Sugg, Moore and Howard (2001)

Evaluating an emerging EM program in Manchester, Reading and Norfolk, Sugg et al. examined EM compared to combination and community service orders because “previous research has shown that, had curfew orders not been available, offenders would have received community sentences seen by sentencers as an alternative to custody.” The report published by the Home Office offers few specific details about the program. Within two years of being “curfewed” and subjected to EM, 72.8% of the offenders in the study had been reconvicted.

Combined results

Given the results of the individual studies, it should not be surprising that the combined results are equally grim. As Table 5 shows, there was no overall impact on recidivism at the longest follow-up period for each study, periods which ranged from one to three years.

Discussion and policy implications

After 20 years, it is clear that EM has been almost desperately applied without adequate vision, planning, program integration, staff training, and concurrent research. It has punished, perhaps more humanely and cheaply than otherwise possible, and it has been an element in the avoidance of prison crowding and prison construction,¹³ but it is not free and it is not without unintended effects.

Is EM simply another fad, another example of what Latessa et al. (2002) call “correctional quackery?” If one looks at gross recidivism rates for moderate to high-risk offenders, it would seem so. Through this review, the authors failed to identify any methodologically sound evaluation comparing EM to incarceration and they failed to find any convincing evidence that EM is superior to other prison diversion programs.

Yet there *may* be a depression of the rate of offending during the monitored period. Could some of the lessons in “relapse prevention” learned by those who treat substance abuse be applied here? Would an extension of the monitoring period for some offenders so that they “age out” be useful? The programs to test these ideas have not been evaluated and, for the most part, evaluations are not being done.¹⁴

The authors of this review found only two studies in which EM effects were plausible, but in both cases, effects were only observed in small subpopulations of offenders: Georgia sex offense parolees and Newfoundland prison divertees (Finn and Muirhead-Steves 2002; Bonta et al. 2000a). The reviewers considered the possibility that programs for these subgroups might be what Sherman and Strang (2004) call “light bulbs” and that the reviewers should “look for outliers rather than averages.”¹⁵ But in both cases, the reviewers found evidence that EM may not have caused the observed differences. The reviewers caution readers to consider other causal variables and to remember that systematic reviews may identify statistically different subgroups that differ only as a result of chance (Counsell et al. 1994).

Using the results of the long line of treatment impact studies that began in 1979 with Gendreau and Ross’s *Effective Correctional Treatment: Bibliotherapy for Cynics*, general principles of “what works” have been distilled, refined, and published repeatedly. The two EM programs in which effects were noted had several of the “what works” characteristics listed by Latessa et al. (2002). Paraphrasing Latessa, there appeared to be appropriate organizational culture, research-based programs, and client risk and needs assessments. Both programs had several components that addressed offender needs or traits directly related to criminal behavior and had a cognitive behavioral component.¹⁶ By contrast, EM did not appear to reduce recidivism among the remainder of the Georgia parolees or the offenders in the study by Sugg et al. (2001), who received minimal non-EM supervision and services.

One can only speculate why the two programs in which EM was coupled with another treatment did better than the relevant comparison groups. In Newfoundland, it is conceivable that EM was a useless addition to an effective treatment package that would have produced an impact even if it had not been included and that the divertees had a relatively high program completion rate because of the threat of return to prison. For the Georgia parolees, the chronology of treatment implementation is hazy; perhaps the non-EM sex offenders paroled in 1995 simply encountered a less-effective, less-organized treatment package than those who were released in 1996. There is some evidence that, even in those studies where it appeared to have some impact, EM was not the variable responsible for change.

Given the theoretical rationales for EM enumerated elsewhere (Renzema 2003: 6–8) and the meta-analytic studies of “what works” in corrections of Bonta, Cullen, Gendreau, Latessa, Ross, Sherman, and others over the past two decades, it is hardly surprising that recidivism has not been reliably reduced by an intervention that is typically quite short, applied in a standard fashion, and applied to a diverse group of offenders for whom it may or may not have any relevance to their motives for offending.

Practical advice for politicians and policy makers

What should policy makers do given the paucity of good information about the impact of EM? The reviewers have a few suggestions:

- Consider other options. If governments continue to use EM as they have for the past 20 years, EM will not reduce demands on parole officers nor will EM make our communities safer. Although fewer prisons may be built and filled because of EM’s use, EM is not the only prison diversion program. Other paths may be more effective in lowering costs and securing public safety.
- Treat underlying problems. Odds of success improve when EM is used as part of an evidence-based correctional package. Although EM may suppress crime for its duration, EM is *not* a “treatment” that directly changes values or teaches skills. Used in isolation, EM should not be expected to produce enduring effects for moderate to high-risk offenders. If EM is going to be used to address a budget crisis, to relieve prison crowding, or to increase offender accountability, EM should be coupled with programs that are likely to reduce recidivism.
- Use EM logically to accomplish realistic goals. Rather than as a knee-jerk reaction to crime, overcrowding, and high costs of running correctional systems, EM ought to be used in a sensible manner to accomplish clearly defined and realistic objectives. One might use EM to facilitate evidence gathering or to quickly return high-risk offenders to custody with the hope of minimizing risk to communities. One might use GPS technology to disrupt criminogenic associations. One might use EM in lieu of contact with parole officers. But one must use EM in a manner that is logically related to the objective at hand. EM will not necessarily lead to any desirable outcomes. Though the reviewers are uncertain of EM’s full effects, they are certain that it is not a panacea.
- Do not make it impossible for offenders to “succeed.” Technical violations and failure to pay the fees associated with EM and probation can, in some cases, result in incarceration. The costs and benefits of incarcerating people for such offenses should be weighed. Policy makers should also consider what offenders are meant to do while on EM. How will they spend their time? With whom will they interact? EM is necessarily a part of a larger program that should encourage lawful behavior and create opportunities for reform.
- Study the effects of EM. Little evidence about the impact of EM is available and, if governments continue to use it, they have an obligation to show that it creates public value. Even when one cannot randomly allocate offenders to EM or another program, one should find records from a group of similar offenders

and one should either invite outside research (preferred) or undertake research within the agency that oversees the EM program.

Conclusion

All studies of EM in moderate to high-risk populations have serious limitations and matched studies of EM in moderate to high-risk populations are of very low quality. After 20 years of EM, we have only a few clues as to its impact—we should know more by now. Government-approved experimental research may be the only way to determine if EM achieves its goals.

If EM continues to be used as it has been used, shortsighted governments will continue to waste taxpayer dollars for ideological reasons and political gain. Governments that choose to use EM in the future ought to use it to enhance other services that have a known effect on crime reduction. Those governments *must* test the marginal effects of EM, publish the results, and discontinue use of EM if it fails to provide quantifiable public benefits. Money spent on EM could be spent on empirically-tested programs that demonstrably protect our communities.

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Notes

- 1 E-mail to Marc Renzema on 21 August 2004 from Dick Whitfield, EM consultant in England.
- 2 The reviewers also felt that a single review of EM could result in misleading statistical analyses because a small effect on a rare outcome would be very difficult to detect. That is, the failure to detect a decrease in recidivism in a sample of offenders of whom 50% are expected to recidivate could provide evidence that EM does not work; failure to detect an effect in an equally sized sample of offenders with a 5% rate of recidivism might say very little about the true effects of EM.
- 3 In a Campbell Collaboration protocol for a review of EM, Renzema (2003) surveys several criminological and psychological theories and finds some support for expecting

- crime suppression during the monitoring period and less for expecting post-monitoring crime-free behavior. He found less theoretical support for the hypothesis that EM would increase recidivism.
- 4 The reviewers are aware of jurisdictions where fewer than a fifth of probationers “fail” and places where more than eight in ten have at least one violation of probation rules during their terms. A federal study of the outcomes of 1994 parolees in 15 states (Bureau of Justice Statistics 2002) showed parolee re-arrest rates at 6, 12, and 24 months from release of 29.9%, 44.1%, and 49.1%. The same study showed reconviction rates of 10.6%, 21.5%, and 36.4% in the same time periods. Thus, setting a mean failure rate minimum of 30% for inclusion as “moderate to high” risk accomplishes the primary goal here, which is to segregate the lowest risks for a separate analysis.
 - 5 This updated version of this spreadsheet is available at <http://www.renzema.net/META-DOCS/C2REVIEWCANDIDATES.pdf>.
 - 6 It seems logical that increased supervision of offenders should aid monitoring of drug use, criminal activity, and probation violations; a finding that closely supervised offenders on EM are more likely than virtually unsupervised offenders to be recalled during the period of monitoring would shed little light on the true effects of EM.
 - 7 Petersilia and Turner are clear about their methods and are transparent throughout their statistical analysis. They give an honest assessment of the data and suggest ways to improve future research in the field. While it had to be excluded from this review, this study provides valuable insights for anyone interested in doing experimental research of EM.
 - 8 Compared to criteria used for meta-analyses in medicine and psychology, these criteria are not strict at all. However, because experimental research is rarely done in social sciences other than psychology, previous meta-analyses have included large numbers of studies and sought to handle low-quality studies through sophisticated statistical analyses. The present authors suspected from the outset that a medical approach would produce an empty review (it would have) and that more open inclusion criteria would have generated more heat than light. The authors hoped that the criteria employed would return studies with some value while excluding those with more substantial sources of bias.
 - 9 Return to prison is one way of estimating reoffending, but the reviewers note that it probably underestimates the actual number of offenses committed.
 - 10 E-mail to Renzema, 24 August 2004.
 - 11 See http://www.pap.state.ga.us/results_driven_supervision.html.
 - 12 In our recidivism analysis (see Table 5), we did not use the prison group, which was not comparable to the experimental group. Instead, we considered the possibility that EM has a marginal impact on a reasonably intensive treatment program.
 - 13 Many studies (mostly outside the universe considered for this review) suggest that prison costs may be reduced and construction costs may be avoided because jurisdictions are able to divert offenders to EM in lieu of incarceration. To the extent that diversions have been possible only because the public will tolerate diversions to EM more than they will tolerate other prison diversion programs, these studies make sense. But most such studies fail to consider EM as one of many diversion programs, some of which may be cheaper, less intrusive, and/or of proven utility in reducing recidivism. Further, many analyses fail to consider the costs and benefits of EM as an addition to other forms of monitoring.
 - 14 Several states in the U.S. authorize lifetime probation or indeterminate civil commitment post prison for certain offenders, primarily those who have committed sex crimes. Under these statutes, EM could be used for a long time, but there is no

- evidence that it is being used for periods beyond six months except in the most unusual and extreme cases. Renzema and Skelton (1990) found an average duration of 80 days nationwide and the work reviewed here does not suggest that this has changed much.
- 15 Sherman and Strang suggest that, in some cases, social scientists should think like inventors who embrace outliers and try to replicate them. "Thomas Edison was not interested in the average life of all previous versions of the lightbulb..." (Sherman and Strang 2004). The reviewers find this idea compelling, but as a tool for reducing recidivism, the reviewers believe that EM remains unproven and not very promising.
 - 16 Information about the type (and availability) of psychotherapy received by Georgia sex offenders at the beginning of the study period is incomplete; however by its end it was based on cognitive-behavioral principles.

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