HM Prison & Probation Service

An evaluation of a brief intervention to reduce reoffending among women serving short sentences

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Thank you.

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1. Summary

This report documents the results of a randomised control trial testing a brief intervention that aimed to reduce reoffending among women serving short sentences in one prison in England.

Over the course of a year, 255 women serving sentences of under 12 months, and who were in the last 6-8 weeks of their sentence, agreed to take part in the trial. Participants were randomly assigned to an intervention or control task. The intervention task consisted of three exercises: i) a daily goal-setting task, which required women to set a goal they could achieve that day, and to review it, and set another the following day, ii) a "Best Possible Self" task, which asked women, in the week prior to their release, to articulate what their life would look like in five years' time if everything had gone as they would like it to, including the steps they would have taken to achieve this, and iii) providing peer support to ("buddying") a fellow participant, helping them to complete their tasks during the intervention period. Women in the control group were asked to complete a list, daily, of the things they had done that day.

Two-hundred and twenty-eight women went on to start the trial and of these, 28-29% in both the experimental and control groups dropped out. Most commonly women chose to drop out; the second most common reason for attrition was early release on home detention curfew. Randomisation was successful, creating two comparable groups, and for the most part, the trial was implemented as intended. However, due to logistical issues on site, less than half of the women (48.8%) in the experimental condition took on the role of the 'buddy' for someone else. In addition, while just under half of the women who took part in the daily review (n = 39) stuck to using this as a listing task, 41 women used the task as a diary, including some reflection and emotional expression, as well as some (limited) goal setting.

Wherever possible, analysis proceeded on an *intent-to-treat* basis, comparing the outcomes of those assigned to each condition, regardless of whether or not they received or completed the intervention. This approach minimises the risk that it is differences between the characteristics or situation of the experimental and control groups that are responsible for any difference in outcome, rather than the intervention. The analyses involving data from post-treatment questionnaires, however, included only a small number of non-completers (n = 6); the majority of non-completers did not complete the second set of questionnaires, therefore were excluded from analyses that used these data. Analyses compared women in the experimental and control groups on measures of future orientation (the extent to which someone is engaged with the notion of their future and a future self), self-efficacy (believing

one can achieve one's goals), self-esteem, self-reported resettlement plans, and official records of proven reoffending a year after release.

Results suggest that the brief intervention led to more comprehensive resettlement planning, and had a small impact on time offence-free up to 18 months post release. The rates of proven reoffending across both groups of women was high, with just over 70% going on to commit another offence, in line with the national average for women serving short sentences.

While, for the whole sample, the comprehensiveness of resettlement plans was not related to one-year proven reoffending (frequency or rates), women who had no fixed address to go to on release, who did not feel that their accommodation was safe and secure or who did not report any family contact, had statistically significantly higher proven reoffending rates than those who did. Women reporting a substance use issue just prior to release had much higher rates of proven reoffending than those who did not; 81.3% of the women who participated in the trial reported a problem with substance use. Reconviction for a new offence within a year of release was predicted by number of previous convictions, whether education, training or employment was set up in advance of release, lack of family contact and level of future orientation just prior to release.

The findings highlight the importance and security of accommodation, family contact, proper and sustained support for substance use problems, and education, training and employment for women seeking to (re)integrate into the community following a prison sentence. The trial suggests that a brief intervention for women serving short sentences can have merit in the short-term by improving the breadth of resettlement planning, but that any impact on time offence-free is small. Brief psychological interventions for women in prison, while promising, may struggle to make a difference in the face of the structural disadvantages they can face on release. The study emphasises the need for comprehensive and continued support to help women manage the transition from prison to the community.

2. Introduction

2.1 Context

The number of women in prison more than doubled between 1993 and 2010, since when the population has remained relatively stable (Prison Reform Trust, 2019). In December 2019 there were 3,703 women held in 12 prisons in England, accounting for around 5% of the prison population in England and Wales. The most common offence for which women were serving prison sentences at the end of 2019 was violence (30%), followed by theft (18%), drugs offences (13%) and robbery (11%) (Ministry of Justice, 2020).

In 2017, women serving custodial sentences of under 12 months made up just under a fifth (18%) of the women's prison population in England and Wales and had the highest rate of proven reoffending of all women in prison; just under three quarters (73%) of these women were reconvicted of a crime within a year of release (Ministry of Justice, 2018a). While the Female Offender Strategy committed to a move away from short custodial sentences for non-violent offences, and towards community-based solutions women do, and for the foreseeable future will continue to, serve sentences of under 12 months in jail (Ministry of Justice, 2018b). This study aimed to test a novel approach to reducing reoffending among women convicted of crime.

2.2 Review of the literature

What can be done to reduce the likelihood that women serving short sentences will reoffend? Recent research on women serving sentences in England and Wales indicates that binge drinking, lack of closeness with family and poor temper control are risk factors that are particularly influential in women's (proven) reoffending (Travers & Mann, 2018), In addition, women face a number of immediate challenges related to release from prison, including securing accommodation, employment or financial support, healthcare, and for many substance use treatment, mental health support, child support services and help to exit and be protected from domestically violent or exploitative relationships (Richie, 2001). These important issues are often neither easily nor quickly resolved and short prison sentences can provide insufficient time in which to engage in structured and/or intensive interventions. However, research into women's desistance from crime suggests that helping women to think of themselves differently, to believe in their ability to live a purposeful, safe and crime-free life, and to see themselves as *good* people with value, can improve the chances that they will be able to navigate and overcome the obstacles and setbacks they almost inevitably face when attempting to (re)integrate into society on release from prison (e.g., Giordano,

Cernkovich & Rudolph, 2002; Brown & Ross, 2010; Berg & Cobbina, 2017; Michalsen, 2019; Larsson, 2019).

Brief interventions

Alongside the desistance research, there is a growing body of evidence that suggests that very brief interventions, if properly targeted, can bring about long-term changes to the way people think about themselves and their world. In early 2014, social psychologist Gregory Walton coined the term 'wise' intervention, to describe these brief activities, which he suggested have two things in common: (1) they target a specific psychological process that harms a particular outcome for people, and (2) they rely on recursive processes; that is, processes which become self-perpetuating (Walton, 2014). The term 'wise' is used to reflect the fact that such interventions are rooted in a precise understanding of an individual's psychological reality; that is, how they understand themselves and their social world. The focus of wise interventions is on the way people interpret and process information and interactions, and how this then impacts on their view of themselves and others. In addition, wise interventions take advantage of opportune moments for change, aiming to make the most of circumstances that usually prompt reflection and consideration of change.

Examples of wise interventions include: brief writing exercises that help people give meaning to traumatic experiences, directing them away from blaming themselves or viewing the world or other people as dangerous (Pennebaker, 1997) and asking mothers considered as at-risk of poorer outcomes for their children about the cause of problems with their child, until they come up with a non-child blaming and non-self-blaming reason (Bugental et al., 2002). Each of these brief interventions has led to significant and long-term improvements in outcomes, relative to control groups. The writing exercise is linked to fewer doctors' visits, better grades at school and less time off work (Frattaroli, 2006). At-risk mothers in the treated group had lower rates of depression, and fewer of the infants of those in this group were physically abused by their mothers (4%) than in the control group (25%) (Bugental et al., 2002).

The power of such simple interventions to affect complex social problems seems rather remarkable. However, it is through the act of changing the way people think about things, and to be specific, people's personal narratives – the way they understand themselves and others - that these interventions are thought to succeed. In each of these examples, individuals were directed away from internalising and interpreting difficulty as a problem with themselves or other people. Instead individuals were provided with a credible alternative interpretation, which helped them to see themselves and others more positively. Walton

argues that, “the psychological processes targeted by these interventions act as levers in complex systems that give rise to social problems” (Walton, 2014 pp. 73).

Future orientation, pro-social identity and self-efficacy

The desistance literature suggests that one of the key differences between those who successfully desist from crime, and those who reoffend, is a sense of agency and the development of a more positive, noncriminal identity. The evidence of the effectiveness of wise interventions and the desistance literature together suggest that an effective way to change behaviour could be to change the way someone sees themselves, both now and in the future. Indeed, there is evidence that people who engage in criminal behaviour are low on *future orientation*, having trouble engaging with the idea of, and thinking of themselves as someone who has, a future (e.g., Mulvey, Schubery & Piquero, 2014). It may also be the case that poverty and other disadvantage causes a tendency to live in the now, as well as disrupting the realisation of future selves.

Research suggests that desistance is related to the way people view themselves and the formation of a ‘prosocial identity’, and self-efficacy (Giordano et al., 2002; Oselin, 2014). Research into the process of desistance from crime indicates that *generative activity* (activities that promote a concern for others beside the self and family, or that nurture or contribute to the next generation) can form an important part of the desistance narrative, forming a key dimension of a more prosocial identity (Maruna, 2001; LeBel, Richie & Maruna, 2015).

2.3 Purpose and aims of this study

This randomised control trial aimed to test a brief intervention with women in prison who were close to release. In line with the desistance literature, the interventions aimed to encourage and strengthen a prosocial, noncriminal identity in women, as well as to increase their future orientation, by asking them to engage with the notion of a ‘future self’ through a simple writing exercise. In tandem, women were asked to take on some peer-support tasks, which aimed to encourage and strengthen a view of the self as a good person who helps others. To promote a sense of agency and self-efficacy, participants were asked to engage in a daily five minute goal-setting and review task, in the weeks before their release. This exercise, which was developed and has been trialled with drug users in Germany, improved drug treatment success by increasing motivation and compliance with the regime, and led to fewer drug relapses (Dau et al., 2011). It aims to increase expected self-efficacy by providing experiences of success in self-management and planning.

This study aimed to evaluate this set of brief interventions, as they were trialled in a women's prison in England, and examines the impact of these three interventions on future orientation, self-efficacy, resettlement plans, and one-year proven reoffending rates. In addition, the study aimed to determine whether self-esteem was affected by the intervention or related to the short (resettlement plans) and mid-term (one-year proven reoffending) outcomes, as low self-esteem is often cited as a prevalent need among women who commit crime, but which has limited empirical support (e.g., Travers & Mann, 2018).

2.4 Research questions and hypotheses

1. **Will a brief intervention with female prisoners increase their future orientation?**

Hypothesis a: The wise intervention set will increase future orientation. That is, those who undertake the 'wise' interventions will have a significantly greater increase in their future-orientation pre- to post-intervention, than those in the control group.

2. **Will a brief intervention with female prisoners increase self-efficacy beliefs?**

Hypothesis b: The wise intervention will increase self-efficacy. That is, those who undertake the wise intervention will have a significantly greater increase in their self-efficacy pre- to post-intervention, than those in the control group.

3. **Will a brief intervention with female prisoners increase levels of self-esteem?**

Hypothesis c: The wise intervention will increase self-esteem. That is, those who undertake the wise intervention will have a significantly greater increase in their self-esteem pre- to post-intervention, than those in the control group.

4. **Will a brief intervention with female prisoners lead to more comprehensive release planning?**

Hypothesis d: The brief intervention will improve release planning. That is, those who undertake the intervention will have more complete release plans than those in the control group.

5. **Will a brief intervention with female prisoners lead to lower rates of proven reoffending/frequency of reoffending over a one-year follow-up?**

Hypothesis e: The brief intervention will reduce one-year proven reoffending rates/frequency of reoffending. That is, those who undertake the intervention will have lower rates of proven reoffending/fewer reoffences one-year post release than those in the control condition, and intervention condition will be a significant predictor of time to reoffending or time offence-free, in a model predicting this outcome.

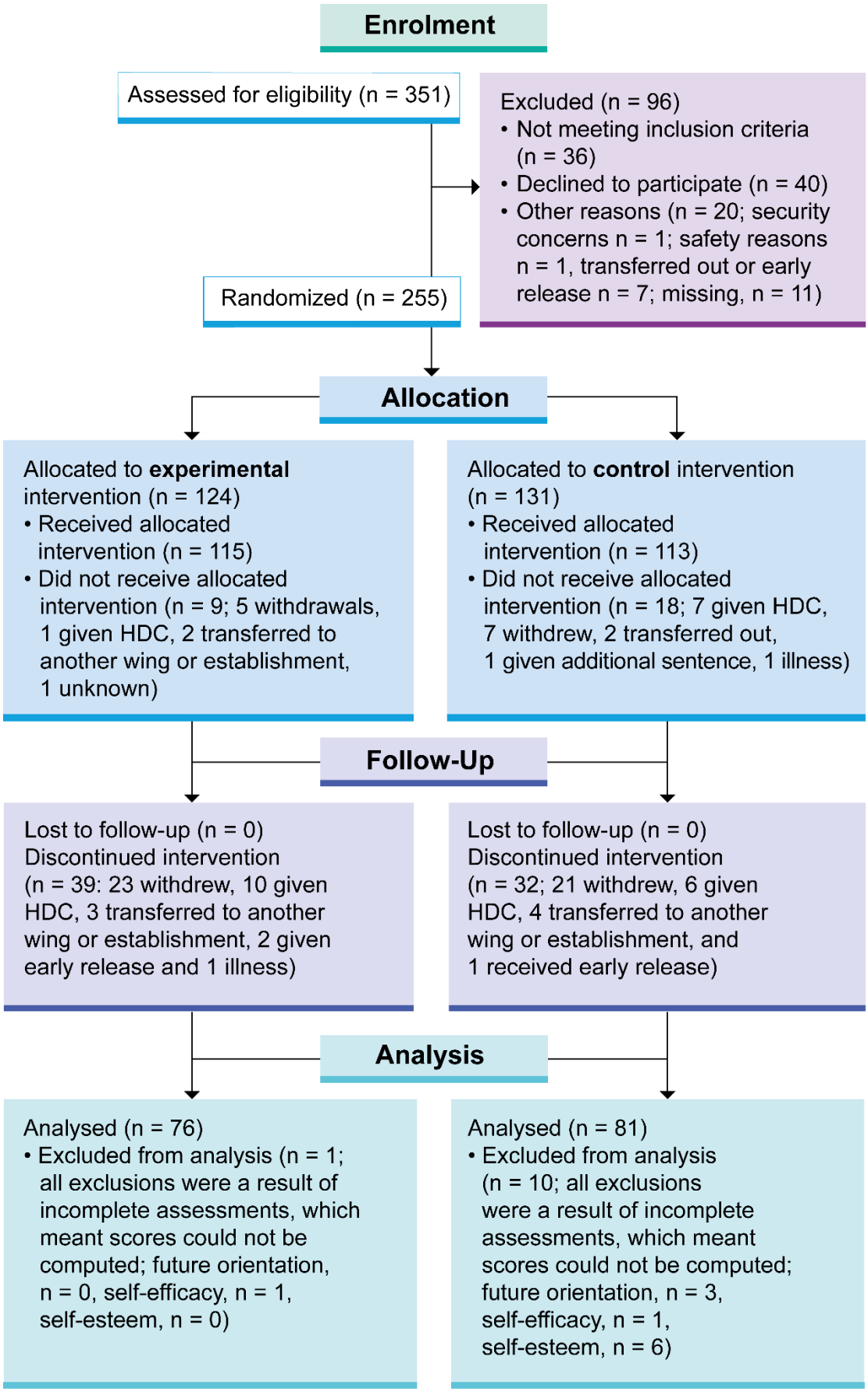
3. Method

We used an experimental design to test the efficacy of this brief intervention. The study was not double-blind, as staff involved in the intervention were aware of the conditions to which participants were assigned. However, steps were taken to blind participants to the conditions of the trial, and review of qualitative feedback from nearly all participants at the end of the trial suggest that these appeared to be effective (*see efficacy of blinding, below).*

3.1 Sample

The sample consisted of 255 women released from a women's prison in England between November 2015 and September 2016 (see Figure 1). Participants were recruited from the three residential units housing mainly women with short sentences, as the trial focussed on those who had between 6 and 8 weeks left to serve. Those who were on the longer-term drug recovery landing, or in the Mother and Baby Unit, were excluded from the trial, as were those on remand and those for whom it was not possible to determine a release date. Women with shorter sentences were selected to enable quicker follow-up on release, and because those in this group were unlikely to receive significant input or to access intensive interventions. This group also tends to be of higher risk of reoffending than those serving longer sentences making it easier to detect possible longer-term effects of the intervention with this group.

Figure 1. Flow diagram of sample attrition at each stage of the trial



During the 12-month trial period, using a consecutive sampling strategy, all eligible women (those who met the inclusion criteria and who were not excluded due to security reasons, transfer or early release) were approached to take part in the trial (N = 296). Two hundred and fifty-five agreed to take part. Of the 41 who did not take part, all but one declined. One potential participant was excluded for safety reasons.

The characteristics of the sample are presented in table 1. The groups differed significantly in the index offences (split into acquisitive offences, violent offences, all other offences and not recorded, to produce groups large enough for this analysis) for which they were serving a sentence (χ^2 (3, 1) = 10.73, $p < .05$, $r = .19$). Compared to those who declined to take part, a greater proportion of those who participated were serving sentences for acquisitive or motoring offences, while a larger proportion of those who declined were serving sentences for drug offences or were missing information on offence. There was no statistically significant difference in the ethnicity (split into three groups, BAME and non-BAME, plus those whose ethnicity was not recorded, due to the small number of people in some of the ethnic minority groups) of those who took part and those who did not (χ^2 (2, 1) = 6.40 p = 0.04), while independent samples Mann Whitney U tests indicated no significant difference between those who agreed and those that declined to take part in age (U = 4148.50, p = .06, r = .11), sentence length (U = 4745.00, p = .50, r = .04) or number of previous convictions (U = 4366.50, p =. 49, r = .08).

Table 1. Characteristics of those who agreed and declined to participate in the research

	Consented (N = 255) ***Mean (Standard deviation)***	**Declined (N = 40)** ***Mean (Standard deviation)***
Age	33.0 (8.0)	37.5 (12.3)
Sentence length (months)	8.9 (7.8)	9.2 (7.0)
Number of previous convictions	21.1 (19.4)	24.4 (21.8)
	n (%)	*n* (%)
Ethnicity		
White	228 (89.4%)	31 (77.5%)
Black	6 (2.4%)	3 (7.5%)
Asian	2 (0.8%)	1 (2.5%)
Mixed Race	9 (3.5%)	3 (7.5%)
Other	1 (2.5%)	1 (2.5%)
Not recorded	1 (0.4%)	1 (2.5%)

	Consented (N = 255) *Mean (Standard deviation)*	Declined (N = 40) *Mean (Standard deviation)*
Index offence		
Acquisitive	131 (51.4%)	12 (30%)
Drugs	10 (3.9%)	3 (7.5%)
Motoring	7 (2.7%)	0 (0%)
Robbery	5 (2.0%)	1 (2.5%)
Sexual	0 (0%)	0 (0%)
Violent	57 (22.4%)	9 (22.5%)
Not recorded	15 (5.9%)	7 (17.5%)

3.2 Measures

Women in the trial completed a questionnaire asking for some basic demographic information and comprising measures of future orientation, self-efficacy, self-esteem and resettlement planning.

Future Orientation Scale (Steinberg, Graham, O'Brien, Woolard, Cauffman & Banich, 2009)

Future orientation was measured using a 15-item self-report scale, developed by Steinberg, Graham, O'Brien, Woolard, Cauffman and Banich (2009). The scale presents respondents with a series of 15 pairs of statements, separated by the word 'But' and asks them to choose the statement which best describes them (e.g., ''*Some people would rather be happy today than take their chances on what might happen in the future BUT Other people will give up their happiness now so that they can get what they want in the future''*). Respondents are then asked to indicate whether the chosen statement is "really true" or "sort of true" of them. These responses are coded on four-point scale from 1-4, and averaged. Higher scores are indicative of greater future orientation. The scale has demonstrated good psychometric properties, including excellent internal consistency (α = .80) (Steinberg et al., 2009).

The New General Self-Efficacy (NGSE) scale (Chen, Gully & Eden, 2004)

The NGSE is an eight-item measure of self-efficacy with good psychometric properties (Chen, Gully & Eden, 2001; Scherbaum, Cohen-Charash, & Kenr, 2006). The scale uses a 5-point Likert Scale response format (1 = *Strongly disagree*, 5 = *Strongly agree*), and items include, "*Even when things are tough, I can perform quite well*". Bandura (2006) suggests, however, that self-efficacy is domain specific – that is, someone can feel efficacious in one domain (e.g., singing) but not in another (e.g., parenting). Bandura (2006) recommends adding questions to any self-efficacy measure, about the domain of interest (in this case

desistance), without sacrificing the integrity of the test. As such, two questions about an individual's belief in their ability to desist from offending, and in keeping with wording of the other items, were added to the NGSE scale, "*I am confident that when I leave prison, I can live an offence-free life*", and "*Even if there are setbacks, I think I will succeed in not committing another offence*". Some items are reverse scored, and scores range from 10-50, with higher scores indicating higher levels of self-efficacy. Chen et al. (2004) report moderate test retest reliability of the tool, with r = .66 over a period of 46 days, which is similar to the interval between first and second testing in the current study.

Self-esteem (Rosenberg, 1965)

The Rosenberg scale is a popular measure of self-esteem, and has been tested in 53 countries, demonstrating good psychometric properties with a range of populations (Schmitt & Alek, 2005). The scale comprises 10 statements, such as "*At times I think I am no good at all*", and "*I feel that I have a number of good qualities*". Respondents indicate the extent to which they agree or disagree with each statement on a four-point Likert Scale (1 = Strongly agree, 2 = Agree, 3 = Disagree and 4 = Strongly disagree). A number of items are reverse scored. Scores range from 10-40, with higher scores equating to higher self-esteem. Normal range self-esteem scores are 15-25, with scores under 15 signifying low self-esteem (Schmitt & Alek, 2005).

For the analyses, individuals' scores on each psychometric measure were averaged.

Resettlement planning

In addition to these psychometric scales, women were asked, in the week prior to release to respond Yes (1) /No (0) to the following statements, which appeared at the end of the questionnaire:

- I have a fixed address that I will go on to release
- The place that I'm going to live on release is safe and secure
- I have firm plans for training/education or employment on release
- I have support set up to deal with my drug or alcohol problems (answer only if you have a drug or alcohol problem)
- I have contact with my family who can support me on release
- I have contact with friends who can support me on release

These questions were developed by the researchers to capture key criminogenic factors which could impact on chances of desistance, and intended to measure the participants'

resettlement plans. These were summed and to produce a resettlement score. Scores could range from 0 to 6 and higher scores equated to more complete resettlement plans.
A further, *substance problem*, variable was created based on participants' answers to the question about drug and alcohol support. Those who answered this question, either yes or no, were coded as having a substance problem (0), while those who did not answer this question were coded as not having a substance problem (1).

One-year Proven Reoffending

Reconviction status was obtained from the Police National Computer (PNC), which holds official sanctions data. Official records spanning 18 months from the date of participants' release was used, to provide a six month "buffer" period, to enable any offences committed during the 12 months after release to come to light, be processed and entered into the PNC database. Participants were coded as having been reconvicted if records indicated they had been convicted during the follow-up for another offence that occurred after the date of their release. Historical offences pre-dating date of release (pseudo offences) were not coded as a new reconviction. Frequency of proven reoffending was calculated as the number of offences, excluding pseudo offences, which had been officially recorded during the follow-up period.

3.3 Procedure

Every week for the 12-month duration of the trial, those women on the eligible residential units who were due to be released within the next 6-8 weeks, were identified on prison management systems and asked by the on-site research manager, whether they would agree to take part in the trial. They were briefed on the nature of the study and provided with the information necessary to ensure that consent was informed.

Those who agreed to take part were randomly allocated to receive the 'wise intervention' tasks, or a control task. Random allocation was co-ordinated by a researcher in Her Majesty's Prison and Probation Service (HMPPS), who used a random number generator to determine the size of 'blocks' which were assigned to condition 1 (intervention) or 2 (control). Small to moderate samples (for example, less than 50 per group), can benefit from "block" and/or "stratified" randomisation techniques (Kendall, 2003). These methods can balance the groups in relation to the number of participants in each, and the distribution of potential confounding variables. While stratified randomisation by risk of reoffending would have been optimal, as this ensures that this potential confounding factor is measured at the start of the experiment, and is evenly distributed between the groups, risk information was not available for the majority of women serving short sentences. Instead, we used block randomisation,

predetermining a set number of people (a ‘block’, e.g., 8, 10, 12) who were then assigned to a condition (e.g., treatment). Those in the next block were all assigned to the other condition (e.g., control), and so on throughout the trial. As the experiment was not double blind (i.e., staff knew which was the intervention and which was the control condition), the block sizes varied randomly, so that the researcher did not know to which condition the last allocations in a block were to be assigned.

Those in the brief intervention condition were assigned a buddy; another prisoner who was part of the trial who was trained to facilitate the ‘best possible selves’ task, and the ‘five minute daily’ task. The face-to-face training was facilitated by the project manager (an officer at the prison) and a psychologist who was overseeing the clinical integrity of the trial. The buddy would find an appropriate time with the participant to ask them to set themselves a simple, positively oriented goal (focusing on doing something measurable, rather than negatively-oriented goals, which focus on avoidance of a particular behaviour), that they could achieve within a day or a weekend. The next day, the buddy would help the participant to review how successful the participant had been in achieving that goal, to rate their success on a scale of 0-100, and to then set themselves another goal for the next day. In the last week before the participant’s release, the buddy facilitated the ‘best possible selves’ task, which required the participant to consider for around 15 minutes a day for four consecutive days, what their life would be like in five years’ time if everything went as well as it could, and to articulate how this ‘best possible self’ would have achieved those things. Participants in the intervention condition were also trained by their buddy to become a buddy for someone else, and took on this role, facilitating these two tasks, for other participants in the intervention condition.

Participants in the control condition were given a ‘daily review’ activity to do, so that women in both conditions experienced a change in regime. Those in this condition were instructed, in the last four weeks of their stay, to record by writing down or getting a buddy to write down what they had done each day, and to rate their mood at the end of each day. Those who agreed to take part, regardless of the condition to which they are assigned, were issued with a certificate which stated that they had taken part in the project. At the end of the trial, daily reviews were coded for adherence to task. Two of the authors coded independently each participants’ set of daily reviews as either completed as intended (listing the things they had done that day), or not as intended (any deviation from the task, e.g., use of the review as a reflective diary). The ratings of the coders agreed on all cases.

The ideal study is “double blind”. That is, neither the participants nor the people implementing the conditions know what the experimental and what the non-experimental condition is. We were unable to achieve this, but did make attempts to blind the study so that participants were unaware of which condition they were assigned to. The on-site research manager was instructed not to indicate which condition the groups were in and to brief all women taking part in the same way. Staff on the wing were not told which tasks formed the experimental and which formed the control conditions.

Regular checks took place to ensure that the trial was proceeding as intended. The protocol remained the same throughout the trial (that is, the experimental and control conditions remained unchanged, as did the process for group allocation and outcome measurement). Women taking part in the study filled in the psychometric tests at two points; once when they agreed to take part in the trial, around 6-8 weeks prior to their release, and once again in the week prior to their release from the prison. The same member of staff (the on-site research manager) administered the assessments pre- and post-trial, under the same conditions.

Women who took part in the trial and were released but returned to Eastwood Park on recall or on a new sentence during the trial period, were included in the study, but their outcomes were based *only* on what happened after their first release during the trial period. They were not permitted to take part in the trial following return to custody.

3.4 Analysis

Wherever possible, analysis proceeded on an ‘intent to treat’ basis, comparing the outcomes of those assigned to each condition, regardless of whether or not they received or completed the intervention. Analyses involving data from post-treatment questionnaires, however, included only a small number of non-completers (n = 6); the majority of non-completers did not complete the second set of questionnaires, therefore were excluded from analysis that used these data.

A series of mixed ANOVAs were used to determine change over time in future orientation, self-efficacy and self-esteem, and any impact of experimental condition on that change. In order to determine whether or not the brief intervention had an impact in the short-term on resettlement planning, multiple regression analysis was performed using number of resettlement plans in place at the end of the trial as the dependent variable. Those variables that had a significant relationship with the dependent variable, as identified using bivariate correlational analysis (which was also used to identify multicollinearity between independent variables), were included as independent variables in the regression.

Information on whether or not the participants in both conditions completed the tasks assigned to them and adhered to the treatment protocol was used to compare those in the treatment and control groups who completed the tasks, and those who did not.

Correlational and Chi square analyse were used to determine any differences between treatment and control groups on one-year proven reoffending rates and frequency of proven reoffending, while Cox Proportional Hazards survival analyses was used to determine whether experimental condition predicted time to proven reoffending or time (proven) offence-free. Finally, Binary logistic regression analysis was used to determine which psychological and social variables predicted one-year proven reoffending.

Evaluators were blind to the condition to which the participants were assigned, when scoring the assessments and when conducting the analysis.

3.5 Limitations

This is a relatively rare example of a randomised control trial of a prison-based intervention, which is widely recognised as a strong evaluation design. Nevertheless, there are some limitations which warrant attention. Examination of the control task products indicated that some of the women in the trial did not complete this as intended. Rather than listing daily activities, they were using the task as a way to keep a diary of their thoughts and feelings about the day's events, and for some this task prompted goal setting and reflection. While we tested for any differences between those who completed the task in the manner intended, and those who used the control task in other ways, these tests were statistically underpowered, due to the small numbers of women in each of these groups, so it is not possible to say with any certainty whether or not the groups differed in their outcomes. It is also possible that the control task prompted reflection in those women who completed the task as intended, but that these reflections were not recorded in their task sheets. In addition, some of the staff were not blind to the condition to which women were assigned, which could have resulted in differential treatment of the women in either condition.

The lack of a 'treatment as usual' (i.e. no intervention) group is a key limitation of this study, as this means it was not possible to determine whether any change over the period of the trial was a result of some impact of taking part/the tasks completed, or whether women's future orientation and self-esteem improves the closer they get to release, without intervention. The vast majority of women who took part in the trial indicated that this was a positive experience, and some of them attributed this to the help and support they received from the project manager, who was a prison officer on site. It is possible that it was this extra,

supportive, contact that was responsible for the improvements in women's psychological functioning over the period of the trial, but without a 'treatment as usual' group, who received no such contact, we cannot be sure.

In addition, the resettlement factors were all measured by self-report, and in particular the substance problem variable was inferred from the answer to another question, which asked participants only to respond if they had a substance issue. This may therefore be an unreliable measure, although its strong relationship with proven reoffending suggests it does have some validity. While we expect randomisation to have generated equivalent groups on both measured and unmeasured variables, because resettlement issues were measured post-trial only, we cannot be sure that the groups were comparable on these factors prior to the trial. This means we cannot be certain that any differences between the experimental and control groups in resettlement planning were not a function of pre-existing differences between women assigned to these two groups prior to taking part in the study. Finally, one of the key factors that was not taken into account in this study was the mental health of the women who took part. Recent research points to the importance of mental health issues, in particular depression, associated with experience of childhood trauma, in women's recidivism (Tripodi et al., 2019). Future research should incorporate measures of mental health, and in particular depression, to determine what sort of relationship this may have with the recidivism of women serving short sentences, and whether this moderates the impact of interventions on resettlement support and recidivism.

4. Results[1]

4.1 Efficacy of randomisation

Comparison of those assigned to the experimental and control conditions confirmed that randomisation was successful. Independent samples *t*-tests indicated that the two groups did not differ in age (t (155) = 1.19, p = .24, r = .09), sentence length (t (155) = -0.39, p = .70, r = .03) nor on number of previous convictions[2] (t (151) = 1.52, p = .13, r = .12). There were also no differences between the two groups in ethnicity (χ^2 (5,1) = 7.04, p = .22, r = .21), or offence type (χ^2 (6,1) = 8.82, p = .18, r = .24).

Table 2. Characteristics of those in experimental and control conditions

	Experimental (N = 76) ***M (SD)***	**Control (N = 81)** ***M (SD)***
Age	33.8 (7.13)	32.3 (8.64)
Sentence length (months)	9.0 (8.86)	9.5 (7.58)
Number of previous convictions	22.0 (16.65)	18.8 (14.39)
Future orientation (pre-trial)	36.96 (7.98)	36.06 (7.61)
Self-efficacy (pre-trial)	37.91 (5.17)	36.46 (5.89)
Self-esteem (pre-trial)	25.45 (5.14)	25.19 (4.52)
	n (%)	*n* (%)
Ethnicity		
White	67 (88.2%)	69 (85.2%)
Black	3 (3.9%)	1 (1.2%)
Asian	0 (0%)	2 (2.5%)
Mixed Race	5 (6.6%)	3 (3.7%)
Other	1 (1.3%)	5 (6.2%)
Not recorded	-	1 (1.2%)
Index offence		
Acquisitive	34 (46.6%)	40 (50.0%)
Drugs	3 (4.1%)	2 (2.5%)
Motoring	0 (0%)	3 (3.8%)
Robbery	0 (0%)	3 (3.8%)
Sexual	-	-
Violent	17 (23.3%)	19 (23.8%)
Not recorded	8 (11.0%)	3 (3.8%)

[1] The Bonferroni correction was applied to compensate for the increased likelihood, as a result of multiple comparisons, of a type one error (a false positive).

[2] One participant had a much higher number of previous convictions than the other participants, and this outlier (number of previous convictions datum only) was removed from the analysis.

Independent samples t-tests indicated no difference between those in the experimental and control groups in pre-trial scores on the measure of future orientation (t (152) = .73, p = .47, r = .06), self-efficacy (t (152) = 1.67, p = .10, r = .13) or self-esteem (t (152) = .40, p = .69, r = .03).

4.2 Efficacy of blinding

Participants' post-trial feedback indicated that blinding was successful. The vast majority of participants described benefits to participation (95.1%), although this information was only gathered from those who completed the trial. There was no difference between those in the control and experimental group in whether they perceived there to be a benefit of the tasks to which they were assigned (Fisher's Exact test, p = .12, Cramer's V = .16). Of all the women that took part, only one reported a cost to participation, citing that the daily review task caused her to relive a bad day by having to write about it.

4.3 Efficacy of Implementation

Logistical problems at the prison meant that less than half of the women (48.8%) in the experimental condition took on the role of the 'buddy' for someone else.

Just under half of the women who took part in the daily review (n = 39) stuck to using this as a listing task. The other half (n= 41) used the task as a diary, including some reflection and emotional expression, as well as some (limited) goal setting.

4.4 Attrition

Just under a third (31.1%) of women dropped out of the trial (n=71). There was no difference in the proportion of those who, after starting the intervention, failed to complete the control tasks (33.9%) and of those who dropped out of the experimental condition (28.3%), χ^2 (1,1) = 0.832, p = .36, r =.06). Nevertheless, it is noteworthy that attrition rates were high across both conditions, pointing to problems with implementing even relatively simple, low level interventions with women serving short sentences in prison. Figure one provides a full breakdown of reasons for attrition. The most common reason for attrition was self-imposed withdrawal, which accounted for 60% and 66% of all attrition in the experimental and control group respectively. Just over a quarter of those who failed to complete the experimental tasks did so because they were released from the prison on home detention curfew, as did 19% of those in the control condition. A small number of women in both conditions were transferred out of the wing or establishment before they could finish the tasks (n = 3 in the experimental and n = 4 in the control condition).

4.5 Change in future orientation, self-efficacy and self-esteem over time

Analysis included all of those women who started the intervention, regardless of whether they received or completed the full intervention or completed the tasks in the way intended. To determine whether participants' scores on measures of future orientation, self-esteem and self-efficacy changed over time, we used a mixed model ANOVA (for scores on these measures pre- and post-treatment see table 3). As all three of these variables had only two levels (time one and time two) sphericity was not an issue. The assumption of homogeneity of variance was met for all three variables at times one and two.

Table 3. Comparison of scores between experimental and control groups on future orientation, self-efficacy and self-esteem, and resettlement planning

	Brief intervention ***Mean (Standard Deviation)***	**Control** ***Mean (Standard Deviation)***	**All** ***Mean (Standard Deviation)***
Pre-treatment future orientation score	2.47 (0.53) (*n* = 78)	2.42 (0.50) (*n* = 73)	2.45 (0.59) (*n* = 151)
Post-treatment future orientation score	2.52 (0.44) (*n* = 78)	2.59 (0.59) (*n* = 73)	2.56 (0.52) (*n* = 151)
Pre-treatment self-efficacy score	3.82 (0.53) (*n* = 78)	3.66 (0.61) (*n* = 73)	3.74 (0.57) (*n* = 151)
Post-treatment self-efficacy score	3.87 (0.61) (*n* = 78)	3.82 (0.70) (*n* = 73)	3.84 (0.65) (*n* = 151)
Pre-treatment self-esteem score	1.56 (0.52) (*n* = 78)	1.57 (0.44) (*n* = 73)	1.56 (0.48) (*n* = 151)
Post-treatment self-esteem score	1.80 (0.55) (*n* = 78)	1.75 (0.52) (*n* = 73)	1.78 (0.54) (*n* = 151)
Resettlement score	4.47 (1.53) (*n* = 78)	3.63 (1.74)* (*n* = 81)	4.04 (1.69) (*n* = 159)

**p < .01*

Time had a statistically significant but small effect on participants' scores on the measures, (F (1, 147) = 13.31, $p < .05$, $r = .29$), For the whole sample, self-reported future-orientation (F (1, 149) = 9.43, $p < .05$), and self-esteem (F (1, 149) = 35.91, $p < .000$) increased significantly over the course of the trial, while self-efficacy did not (F (1, 149) = 3.64, $p = .06$, $r = .15$). Effect size calculations indicated that time had a small effect on future orientation ($r = .24$) and a moderate effect on self-esteem ($r = .44$).

4.6 Change in future orientation, self-efficacy and self-esteem by intervention condition

Contrary to our hypotheses whether or not women were assigned to the brief intervention or to the control task made no difference to their scores on the measure of future orientation (F (1, 149) = 0.02 p =.90, r = .01), self-efficacy (F (1, 149) = 1.58, p = .21, r = .10) or self-esteem (F (1, 149) = 0.04, p = .84, r = .02). This held true when comparing the subgroup of women who completed the full experimental intervention, including buddying, (n = 52), with all those who completed the control task, or with only those who completed the control task as intended (n = 39). However, there is a risk that the small samples and differing group sizes in this analysis meant it lacked sufficient power to detect any effect.

4.7 Impact of intervention condition on resettlement planning

Resettlement planning was measured post-trial only. An independent samples t-test indicated that those in the experimental group had statistically significantly better scores than the control group on the measure of resettlement planning (t (157) = 3.25, p < .01). The brief intervention had a moderate effect on resettlement score (r =.24). Correlational analysis indicated a significant positive correlation (p < .001) between resettlement scores and post-trial self-esteem (r = .38) and self-efficacy (r =.31). However, post-trial future orientation scores were not correlated with resettlement scores (r = .10, p = .22). Change in self-efficacy (r = .07, p = .37), self-esteem (r = .12, p = .15) and future orientation (r = -.06, p = .51) were not correlated with resettlement planning, nor was age (r = .03, p = .72) nor number of previous convictions (r = -.01, p = .87).

Table 4 reports the results of a hierarchical multiple regression using the control condition as the reference category. Testing the assumptions of regression analysis indicated that the number of previous convictions variable violated the assumption of the linearity of the logit. As a result, the log of number of previous convictions was used in all subsequent regression analyses.

Table 4: Predictors of number of resettlement planning issues addressed

	Unstandardised co-efficients		Standardised co-efficients			
	b	***Std. Error***	***β***	***t***	***p***	***SD***
(Constant)	-.48	1.28		-0.37	.71	
Age	-.00	0.02	-.02	-0.21	.83	7.98
Log of previous convictions	.03	0.14	.02	0.20	.84	0.97
Post-trial self-esteem score	1.01	0.33	.29	3.09	.00*	0.50
Post-trial future orientation score	-.21	0.29	-.06	-.74	.46	0.50
Post-trial self-efficacy score	.48	0.28	.16	1.71	.09	0.59
Experimental condition	.96	0.27	.28	3.53	.00*	

*NB: *statistically significant*

Age and log of previous convictions were entered as a first step, followed by post-trial scores on self-esteem, self-efficacy and future orientation, and experimental condition. This suggested that intervention condition and mean post-trial self-esteem score were statistically significant predictors of the strength of resettlement plans (F (6,135) = 6.49, p<.001, R^2 =.23, adjusted R^2 = .20). However, a large amount of variance in resettlement planning was unaccounted for in this model, suggesting that other factors had an influence on women's resettlement plans. Looking at the standardised beta coefficients (table 4), we can see that experimental condition had the largest effect on resettlement planning scores, followed by post-trial self-esteem. Being in the experimental condition increased resettlement planning score by just under 1 point (b = 0.96).

Examination of the items that make up the resettlement planning score (see Table 5) indicated that the experimental and control groups significantly differed only on whether they felt that they had safe and secure accommodation prepared for their release (χ^2 (1, 140) = 6.30, p < .01, r = .21). Calculating the risk ratio indicated that women were 1.28 times as likely to report that they had safe and secure accommodation on release if they were in the experimental condition rather than in the control condition. There were no significant differences between the two groups in whether they had a fixed address to go to on release (χ^2 (1, 157) = 0.00, p = .99, r = .00), had arranged education, training or employment for release (χ^2 (1, 154) = 3.18, p = .08, r = 14), had substance support in place for release (χ^2 (1, 126) = 1.70, p = .19, r = .12), had family contact (χ^2 (1, 153) = 2.15, p = .14, r = .12), or had supportive friends in the community (χ^2 (1, 151) = 3.22, p = .07, r = .15).

Table 5: Proportion and number of women in each trial condition who indicated particular resettlement plans were or were not in place for their release at the end of the trial

In place for release		Control group % (n)	Brief intervention % (n)
Fixed accommodation	No	35.0% (28)	35.1% (27)
	Yes	65.0% (52)	64.9% (50)
Accommodation is safe and secure	No	34.7% (25)	16.2% (11)
	Yes	65.3% (47)	83.8% (57)
Education, training or employment	No	55.7% (44)	41.3% (31)
	Yes	44.3% (35)	58.7% (44)
Substance abuse support	No	17.5% (11)	9.5% (6)
	Yes	82.5% (52)	90.35% (57)
Family contact	No	25.6% (20)	16.0% (12)
	Yes	74.4% (58)	84.0% (63)
Supportive friends	No	32.9% (25)	20.0% (15)
	Yes	67.1% (51)	80.0% (60)

There was no statistically significant difference in the number of women in the control or experimental condition who reported a substance problem in the week before release (χ^2 (1, 155) = 0.70, p = .40, r = .07). A high proportion of women in both groups felt they had a substance issue; 84% of women in the experimental condition, compared with 79% of women in the control condition.

4.8 Reconviction analysis

Of the 228 women who started the trial, 71.1% (n =162) went on to be convicted of another offence in the year after their release. Most commonly, reconvictions were for acquisitive offences (which accounted for 56.1% of the index offences of the women who were reconvicted during the follow-up). Just over a fifth of reconvictions were for a violent offence (21.1%), 7.5% for “other” offences, 7.0% for drugs related offences. 3.9% motoring, 2.2% breaches of licence/order, 0.4% for sexual offences and 0.4% for robbery. 1.3% was missing this information.

Dropping out of the trial was not associated with a higher rate of reconviction (χ^2 (1, 228) = 0.65 p = .42, r = .05). Three quarters (74.6%) of non-completers went on to be convicted of another offence during the follow-up period, compared with 69.4% of completers. Similarly, failure to complete the trial was not associated with a higher frequency of reoffending (Drop outs M = 12.42 SD = 14.13, Completers M = 12.82 SD = 15.08, t (226) = -0.19, p = .85, r = .02).

However, a different picture emerged when examining drop outs by trial condition. While there was no difference in the proportion of women who failed to complete the control (29.2%) and experimental (28.7%) conditions, the one-year reconviction rate of those who dropped out of the experimental condition was significantly higher (87.9%) than the rate of reconviction of those who completed it (68.8%), (χ^2 (1, 113) = 4.48, p = .03, r = .20). There was no difference in the reconviction rate of women who completed (65.9%) and women who did not complete (72.7%) the control condition (χ^2 (1, 115) = 0.51, p = .48, r = .07). Of those who completed the conditions, 70.4% of those in the control condition (57/81) and 68.4% (52/76) of those in the experimental condition, went on to commit a proven reoffence at a one-year follow-up; this difference is not statistically significant (χ^2 (1, 157) = 0.07, p = .79)

Reconviction and resettlement issues

Chi square tests indicated that having a substance use problem prior to release was associated with reconviction (χ^2 (1, 155) = 6.98, p = .01, r = .21). Seventy percent of those who had a substance problem went on to be reconvicted within a year, compared with 45% of those without. Not having a secure and safe home on release was also associated with reconviction (χ^2 (1,140) = 3.84, p = .05, r = .17) as was a lack of family contact (χ^2 (1, 153) = 7.26 p = .01, r = .22). There was no association, however, between having a fixed address on release (χ^2 (1, 157) = 3.41, p = .07, r = .15), having education, training or employment secured prior to release (χ^2 (1, 154) =.01, p = .94, r =.01), or having supportive friends (χ^2 (1, 151) = 0.30, p = .58, r = .04) and one-year proven reoffending (appendix A).

Similarly, not having a fixed address (r = -.17, p = .05), not having safe and secure accommodation (r = -.18, p = .05), having substance use support (r = .34, p = .01), and lacking family contact (r = -.19, p = .05), all had a significant relationship with number of reoffences, as did having a substance use problem (r = -.26, p = .001).
However, there was no association between overall resettlement scores and reconviction status, (t (155) = 0.73, p = .46). Similarly, there was no correlation between number of reoffences and resettlement score (r = -.07, p = .39).

Reconviction, psychological variables and intervention condition

Correlational analysis indicated that one-year reconviction status was associated with pre- and post-treatment future orientation (pre tx r = -.-.27, p = .01, post tx r= -.29, p = .01), self-efficacy (pre tx r = -.25, p = .01, post tx r= -.24, p = .01), and self-esteem (pre tx r = -.27, p = .05, post tx r = -.20, p = .05), prior to release. Of all of the psychological variables, post-treatment future orientation had the strongest relationship with reconviction.

Analyses found no difference in reconviction rates or number of reoffences between the experimental and control groups, or those who completed the conditions as intended (see appendix B).

Differences between the two groups in time from release to proven reoffending (for those who reoffended) or time at risk (for those who had no proven reoffences during the follow-up period) using Cox regression analysis was also examined. (Table 6). The model, containing age, log of number of previous conditions, experimental condition (with brief intervention as the reference group), and mean pre-treatment psychometric assessment scores, was significant (-2LL = 1528.81, χ^2 (6, 228) = 45.66 *p* = .00).

Table 6. Summary of Cox Proportional Hazards regression analysis predicting time at risk or to proven reoffence

	b	**Std. Error**	**Exp (B) (95% CI)**	**Wald**	***p***
Age	-0.03	0.01	0.97 (0.95-0.99)	6.10	.01
Log of previous convictions	0.51	0.10	1.67 (1.37-2.03)	29.95	.00
Experimental condition	0.31	0.16	1.36 (0.99-1.86)	3.65	.06
Pre treatment future orientation	0.14	0.18	0.87 (0.62-1.22)	0.62	.43
Pre treatment self-efficacy	0.10	0.18	0.90 (0.64-1.28)	0.32	.57
Pre treatment self-esteem	-0.33	0.20	0.72 (0.48-1.07)	2.59	.11

Age and the log of the number of previous convictions were significant, while experimental condition approached significance (*p* = .056). As expected, younger age decreased time to reoffence or at risk without (proven) reoffending, as did a higher number of previous convictions. Being in the experimental condition decreased the hazard of having a proven reoffence at any time during the follow-up period by 26% (mean follow-up = 404.7 days, *SD* = 393.4).

Predictors of proven reoffending

Table 7 presents the results of a logistic regression analysis to examine which psychological variables (post-treatment Future Orientation, Self-efficacy and Self Esteem scores) and social variables (having a fixed abode, education training or employment, absence of a substance misuse problem, family contact and supportive friends, in place for release) predicted whether women were reconvicted within one-year post release, using age and the log of number of previous convictions, as covariates. To include as many women in the

analysis as possible, the substance problem variable was included in the model, as opposed to the substance support variable, the latter of which was only available for those women who reported a substance problem.

The model was significant, (χ^2 (10, 127) = 36.98, p = .001). -2LL = 128.14, and the NagelKerke R square statistic indicated that this model accounted for around 35% of the variance in predicting one-year reconviction rates, although this is a rough estimate and should be interpreted with caution.

Table 7. Summary of logistic regression analysis predicting one-year proven reoffending

	b	**Std. Error**	**Exp (B) (95% CI)**	**Wald**	***p***
(Constant)	1.91	2.28	6.75	0.70	.40
Age	-0.00	0.03	0.99 (0.95-1.05)	0.01	.93
Log of previous convictions	0.81	0.27	2.25 (1.33-3.78)	9.25	.00
Post treatment future orientation	-1.14	0.53	0.32 (0.11-0.91)	4.60	.03
Post treatment self-efficacy	-0.25	0.43	0.98 (0.42-2.27)	0.03	.95
Post treatment self-esteem	-0.11	0.59	0.90 (0.28-2.85)	0.03	.86
No fixed address on release	0.44	0.51	1.55 (0.57-4.23)	0.73	.39
Lack of ETE on release	-1.08	0.50	0.34 (0.13-0.90)	4.70	.03
Lack of substance problem	0.29	0.70	1.34 (0.34 – 5.32)	0.17	.68
Lack of family contact	1.70	0.74	5.53 (1.30-23.49)	5.36	.02
Lack of supportive friends	-0.95	0.55	0.39 (0.13-1.13)	3.03	.08

Note: ETE = Education, training or employment

The log of the number of previous convictions, having education, training or employment on release, having family contact and post-treatment future orientation score, significantly predicted one-year proven reoffending outcome. In each case, the confidence intervals of the odds ratios, while wide, did not cross 1.0, indicating we can be confident of the direction of these effects. The largest effect was seen for a lack of family contact prior to release, which was associated with over a five-fold increase in the probability of proven reoffending in the first year following release.

5. Discussion and implications

This randomised control trial examined whether, by improving future orientation and self-efficacy, a brief intervention could improve the resettlement planning and reduce the rates of proven reoffending of women serving short sentences in prison, relative to a control task. The results suggest that a brief intervention was successful in the short-term at improving resettlement plans, and that it had a small impact on time to proven reoffending in the year after release. However, the results indicate that any effect of the intervention was not associated with increases in future orientation, self-efficacy or self-esteem. Post-treatment levels of self-efficacy and self-esteem did emerge however, as statistically significant predictors of resettlement planning; a large part of the variance in this outcome remained unaccounted for, suggesting other factors important to resettlement planning were not captured in this study.

Higher levels of future orientation post-treatment reduced one-year proven reoffending. A large-scale study examining pathways to desistance among young people (the vast majority of whom were male) in America, found future orientation to have a significant relationship with desistance (Steinberg, Cauffman & Monahan, 2015). The findings of the current research support the potential relevance of future orientation as a target for rehabilitative efforts with women convicted of crime, and suggest further research in this area is warranted. This study also highlights the importance of education, training and employment for women seeking to (re)integrate into the community following a prison sentence, as this too was a significant predictor of one-year reconviction for this sample. The results also emphasise the importance of fixed and secure accommodation, appropriate and sustained support to address problematic substance use, and of the potential protective effect of family contact, all of which were associated with significantly lower levels of proven reoffending within a year of release. This is in line with a recent review in which family ties were described as “utterly indispensable” to women who seek to desist from crime (Farmer, 2019).

Contrary to the study hypotheses, women who took part in the control task demonstrated as much change in future orientation, self-esteem and self-efficacy as those who took part in the brief intervention. In fact, all the women demonstrated statistically significant increases in future orientation and self-esteem over the course of the trial, regardless of the condition to which they were assigned. It may be that simply getting closer to release prompts more reflection on and engagement with the future, and a boost to self-esteem. Alternatively, it may be that involvement in the trial, which necessitated regular contact with a prison officer who was well-regarded by those who took part, proved beneficial for participants over and

above the tasks to which they were assigned. The important role of staff-prisoner relationships in maintaining prison safety, facilitating change and improving well-being, has been underscored in several studies (e.g., Liebling & Price, 1999; Liebling, 2011; Liebling et al., 2019). Another explanation is that both the control and the brief intervention were successful in facilitating change among the women, but without a no-treatment comparison group, it is not possible to test this hypothesis. Women's responses to the post-trial survey suggests that the vast majority of the women believed that they derived benefit from all of the tasks, both control and experimental. The task most often mentioned as helpful was the daily goal setting, which participants described as helping them to focus on what they wanted and to get things done. The lack of change in self-efficacy over time, however, suggests that this task did not translate to a greater sense of control over their circumstances.

The one-year reconviction rates of those who took part in the study indicated that the vast majority of women, regardless of intervention condition, went on to commit another (proven) offence in the year following release. Reoffending rates were in line with the national average for women serving short sentences in 2017 (Ministry of Justice, 2018), with over 70% going on to be convicted of another offence within a year of release from prison. Of all those who took part, women who dropped out of the intervention condition had the highest reoffending rates. This parallels the findings from longer criminal justice interventions and is usually interpreted as being a sign that the people who dropped out are generally more impulsive and unable to complete tasks, translating to higher dynamic risk and higher likelihood of offending.

It is particularly surprising that women who reported having drug or alcohol support in place for release were far more likely to be reconvicted within a year of release, than those women with a substance problem who did not have support in place. It may be that those who lacked support had lower levels of need and therefore were not prioritised for treatment/support. In addition, the number of women who did not have support in place was small (n = 17), and as such, this finding may have been a statistical artefact. Future research should distinguish between drug and alcohol use and between different levels of need in these areas, to aid our understanding of this issue.

Short prison sentences can disrupt important factors that can reduce the likelihood of further crime; stable and secure accommodation, employment, support networks and relationships with children and families (Richie, 2001). This research suggests that, for women in one prison in England, a brief intervention that improved resettlement planning had only a small impact on recidivism rates once released. Wise interventions can redirect people's personal

narratives, which lead them to interpret interactions and experiences differently, which in turn, leads to more permanent changes in how they understand themselves and their social world (Walton, 2014). Walton also stresses however, that this only works if the intervention is taking place in a context within which positive experiences facilitate positive outcomes. The structural, social and economic disadvantages faced by some women released from prison may interfere with these recursive processes and inhibit the chances that such interventions will be effective.

Finally, while the group sizes are too small to draw conclusions about the effect of buddying, further research is warranted into the potential impacts of taking on a peer mentor role, given the prevalence of these sorts of initiatives across the prison estate. What can be said with certainty is that the women who took part were very open to engaging in the tasks, reported high levels of motivation to do something constructive with their short time in prison, and felt that their experience was a positive one. However, the fact remains that despite any change observed over the trial period, a sizeable minority of women dropped out of the trial, and rates of proven reoffending one year after release remained unacceptably high.

5.1 Conclusion

A brief intervention for women serving short prison sentences in one establishment had a small effect on reducing rates of proven reoffending over a one-year follow-up. The findings suggest that the structural disadvantages women face on release from prison can outweigh any psychological changes they may manage to make during a short sentence. The study highlights the pressing need for effective services in accommodation, family support and substance use, starting in prison and continuing on release, in supporting women to live crime-free lives.

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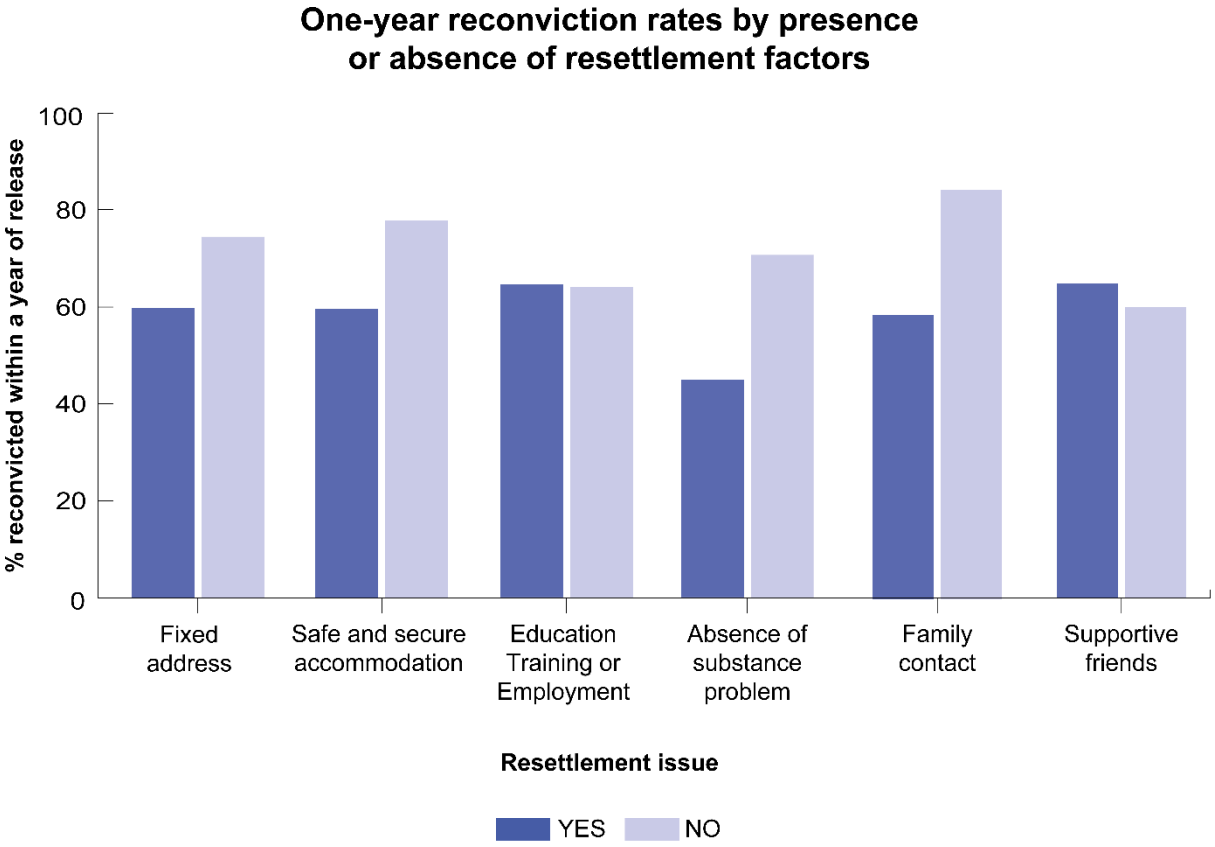
7. Appendices

7.1 Appendix A: Figure 2. Comparison of one-year proven reoffending rates for women with and without different resettlement needs

Figure 2. Comparison of one year proven reoffending rates for women with and without different resettlement issues



7.2 Appendix B: Details of reconviction analysis

Chi square tests indicated that there was no difference in reconviction rates of women who started the trial, who were assigned to the experimental or the control conditions (χ^2 (1, 228) = 1.17, p = .28, r = .07). 67.8% (experimental) 74.3% (control). Similarly, t-tests indicated no difference between the number of reoffences of those in the experimental (M = 13.55, SD= 14.93) and those in the control conditions (M = 11.83, SD= 14.60), (t (226) = 0.88, p =.38, r = .06). Comparing those who completed the experimental task as intended, with those who completed the control task as intended indicated that there were no significant differences in reconviction status between the two; 68.4% (n = 52) of those in experimental condition were reconvicted, compared with 70.4% (n = 57) of those in the control condition, (χ^2 (1,157) = 0.71 p = .79, r = .07). Similarly, a t-test indicated no difference in number of proven

reoffences between those who completed the experimental condition (*M* = 14.08, *SD* =15.60) and those who completed the control task as intended (*M* =9.46, *SD* = 10.46), *t* (89) = 1.60, *p* =.11, *r* = .17.

Those in the experimental group who completed all aspects of the intervention, including buddying another participant, had a 71.2% (*n*= 52) reconviction rate, compared with 65.2% (*n* = 23) of those who completed all tasks except buddying, however, this difference was not significant (χ^2 (1,75) = 2.64 *p* = .61, *r* = .19).

7.3 Appendix C: Survival Curve

The survival curve (figure 3) shows that those in the brief intervention and control conditions followed a largely similar pattern, being most likely to reoffend within the first few months of release. However, those in the brief intervention condition reoffended (based on records of proven reoffending), more slowly and at a lesser rate than those in the control condition.

Figure 3. Cox Proportional Hazards survival curve showing time in days at risk following release from prison until either first proven reoffence or time at which proven reoffending data was collected, for participants in the experimental and control groups