

# BMJ Open Efficacy of brief intervention for drug misuse in primary care facilities: systematic review and meta-analysis protocol

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## ABSTRACT

**Introduction** Drug misuse is associated with significant global morbidity, mortality, economic costs and social costs. Many primary care facilities have integrated drug misuse screening and brief intervention (BI) into their usual care delivery. However, the efficacy of BI for drug misuse in primary care has not been substantiated through meta-analysis. The aim of this systematic review and meta-analysis is to determine the efficacy of BI for drug misuse in primary care settings.

**Methods and analysis** We will include all randomised controlled trials comparing primary care-delivered BI for drug misuse with no intervention or minimal screening/assessment and usual care. Primary outcomes are (1) drug use frequency scores and (2) severity scores at intermediate follow-up (4–8 months). We will retrieve all studies through searches in CENTRAL, Embase, MEDLINE and PsycINFO until 31 May 2020. The reference list will be supplemented with searches in trial registries (eg, [www.clinicaltrials.gov](http://www.clinicaltrials.gov)) and through relevant existing study reference lists identified in the literature. We will conduct a random-effect pairwise meta-analysis for primary and secondary outcomes. We will assess statistical heterogeneity through visual inspection of a forest plot and calculate  $I^2$  statistics. We will assess risk of bias using the Cochrane Risk of Bias Tool V.2 and evaluate the certainty of evidence through the Grading of Recommendations Assessment, Development and Evaluation (GRADE) approach. Sensitivity analyses will account for studies with control group variations and studies with a high risk of bias. If heterogeneity is present, subgroup analyses will consider patient variables of age, sex/gender, race/ethnicity, per cent insured, baseline severity and primary drug misused.

**Ethics and dissemination** This study will use published aggregate data and will not require ethical approval. Findings will be disseminated in a peer-reviewed journal.

## BACKGROUND

Drug misuse is associated with significant global morbidity, mortality, economic costs and social costs.<sup>1</sup> Drug misuse is defined by hazardous use of any non-alcohol-related drug (eg, cannabis, opioids and stimulants) beyond legal and medical guidelines.<sup>2</sup> Drug

## Strengths and limitations of this study

- Our systematic review and meta-analysis will be the first to examine the efficacy of brief intervention (BI) for drug misuse in primary care.
- This study will investigate the efficacy of BI for drug use frequency and severity at multiple time points.
- Limitations of individual studies will be judged with the revised Cochrane Risk of Bias Tool V.2.
- This study will not investigate treatment referral and is limited to screening and BI in primary care.
- Significant heterogeneity will be assessed through subgroup analyses by prespecified patient characteristics.

misuse may increase the risk of harmful consequences in mental, physical and social domains, regardless of any diagnosed substance use disorder (SUD).<sup>1</sup> Additionally, drug misuse may indicate a need for treatment as part of a diagnosable SUD characterised by risky use, impaired control, social impairment, tolerance or withdrawal for a specific substance.<sup>3</sup> In 2017, it was estimated that globally, about 5% of adults report illicit drug use and 0.4% fit criteria for a drug-related SUD.<sup>1</sup> However, only about 10% of people with an SUD receive treatment.<sup>4,5</sup> Most patients who receive specialised SUD treatment are referred through criminal justice or self-referral sources,<sup>6</sup> which means that 10% who receive treatment are likely to be highly motivated or compelled to change. This leaves a 90% disparity known as the treatment gap. The treatment gap led to using primary care facilities as a method of identifying those who may benefit from treatment.

Those who demonstrate drug misuse are likely to present in primary care facilities, are more likely to be admitted and have higher odds of substance-related health complications than those who do not misuse

drugs.<sup>7 8</sup> However, healthcare professionals historically demonstrate some of the lowest referral rates to SUD treatment.<sup>9 10</sup> Ideally, drug misuse would be universally screened and treated in primary care facilities. While drug misuse screenings have improved, follow-up interventions may still be lacking. For example, a good 75% of those who see a healthcare provider report being screened for alcohol use.<sup>11</sup> Unfortunately, when patients do report substance misuse, providers rarely follow-up with advice (20%) or SUD treatment referral (7%).<sup>11</sup>

The current trend of SUD and intervention research focuses on training programmes, counselling and motivational interviewing (MI).<sup>12</sup> To address the SUD treatment gap and referral inconsistency in primary care, the Substance Abuse and Mental Health Services (SAMHSA) developed and funded a large-scale programme called screening, brief intervention and referral to treatment (SBIRT).<sup>13</sup> SBIRT identifies patients in primary care who would not normally seek treatment, provides a short motivational intervention and, if needed, refers to outside specialty treatments. Many resources, including millions of dollars and healthcare professional time, have been invested on research and service implementation of SBIRT programmes across the globe.<sup>14 15</sup> Cost analysis suggests it is worth the price to initiate patient change behaviours.<sup>16</sup> However, the significant bodies of research investigating SBIRT findings have been mixed, with supportive findings for alcohol misuse, but lacking evidence for drug misuse.<sup>13 17–19</sup> Additionally, there is insufficient evidence to support the efficacy of the SBIRT referral to treatment component due to poor referral follow-ups.<sup>20</sup> The incongruent evidence for substance misuse outcomes and lack of evidence for referral efficacy is thought to be due to issues in data collection, intervention facilitation uniformity and site biases.<sup>21</sup> These mixed SBIRT findings suggest focused investigations of the brief intervention (BI) component for drug misuse are required.<sup>19 22</sup>

Many primary care facilities have integrated drug misuse screening and BI into their usual care delivery; both SAMHSA-related SBIRT and non-SBIRT programmes. Because many people visit primary care facilities regularly, they are an optimal setting to screen for drug misuse and to follow up with BI on site. BI in primary care targets patients who otherwise may not seek treatment. However, the efficacy of BI for drug misuse was not well substantiated prior to a major SAMHSA-funded BI programme deployment in 2004 and remains unclear over 15 years after its inception.<sup>23</sup> A systematic review and meta-analysis is necessary to determine BI efficacy for drug misuse in primary care.

### Brief intervention

BI consists of one to five individual sessions lasting 5–60 min each for low to moderate misuse, usually conducted on site as soon as possible.<sup>24</sup> A meta-analysis showed the median BI length is 25 min.<sup>25</sup> Extended BIs may include additional sessions or a follow-up telephone

call of 5–30 min ('booster') to improve motivation after discharge from primary care.<sup>18 26</sup> A meta-analysis showed the median extended BI length is 100 min over multiple sessions.<sup>25</sup> BI can be conducted by physicians, comedical professionals, trained laypersons and through computer programmes. BI is undergirded by a significant number of efficacy and effectiveness studies for alcohol use in many primary care facilities.<sup>21</sup>

BI was developed out of social, cognitive and behavioural approaches to psychotherapy.<sup>26</sup> In a systematic review and meta-analysis of BI for alcohol misuse, Kaner and colleagues<sup>26</sup> outlined key BI components to include (1) feedback about personal use and harm, (2) risks of continued use, (3) benefits of reducing one's use, (4) advice on how to reduce one's use, (5) motivational enhancement and (6) developing a plan to reduce use. BI is grounded in the transtheoretical model of change.<sup>27</sup> BI uses the FRAMES approach: feedback, responsibility to change, advice, menu of treatment options, empathy and supporting of self-efficacy.<sup>18 23 26 28</sup> In addition, BI relies heavily on MI<sup>29</sup> and motivational enhancement therapy (MET),<sup>30</sup> which are directive, client-centred and aimed at resolving ambivalence through provider feedback to elicit behaviour change. MI and MET are both evidence-based treatments for SUDs with strong research support.<sup>31</sup>

BI in primary care works by first identifying drug misuse through screening. Patients present to primary care facilities seeking medical care. Patients are screened as part of admission/sign-in procedures. If unhealthy substance use is reported, further assessment determines the appropriate level of care, either no treatment, BI or referral to outside specialty treatment. Screenings may consist of a simple prescreen to determine any drug use. If positive, a formal screening would be activated. For example, some programmes use the drug abuse screening test (DAST)<sup>32 33</sup> to screen for drug misuse. The National Institute on Drug Abuse<sup>34</sup> outlined the DAST-10 as scored from 0 to 10 and divided into zones indicating appropriate intervention levels. Scores from 0 to 2 suggest no/low level problems and may indicate monitoring, feedback or no treatment. Scores of 3–8, moderate/substantial problems, suggest further assessment and BI may be indicated. Scores of 9–10, severe problems, suggest intensive assessment and may indicate referral to specialty treatment providers.

### Gap in the knowledge

BI in primary care is well documented to be effective, but only for reducing alcohol use frequency and severity.<sup>18 26 28 35</sup> Screening and BI for alcohol have been given a B GRADE recommendation from the US Preventive Services Task Force.<sup>36</sup> Additionally, alcohol findings may not generalise to the treatment of other drug misuse. Evidence from BI for drug misuse studies remains equivocal.<sup>17 18 22 23 28 35 37 38</sup> For instance, a multisite observational study reported significant reductions in drug use outcomes.<sup>18</sup> Conversely, the US Preventive Services Task Force concluded there was insufficient evidence to assess the risks and benefits of primary care screening

or BI for drug misuse.<sup>39</sup> Yet, no meta-analyses have been conducted. Furthermore, patient variables such as sex,<sup>18</sup> age<sup>40</sup> and primary problem substance<sup>41 42</sup> moderate drug use outcomes have not been evaluated in a systematic way. In response to a call for further research into drug misuse BI,<sup>22 43</sup> several randomised controlled trials (RCTs) have been completed in the past 10 years, and a meta-analysis would now help to determine BI efficacy in primary care.

### Present study

This study aimed to address the gap in the research base by investigating the efficacy of BI for drug misuse frequency and severity in primary care settings through a pairwise systematic review and meta-analysis. Findings will help to determine if the use of BI for drug misuse in primary care is warranted or if changes to treatment protocols are needed. We aimed to evaluate the overall efficacy of BI compared with no intervention through (1) drug use frequency at intermediate follow-up (4–8 months) and (2) drug use severity at intermediate follow-up (4–8 months). Secondary outcomes under investigation aimed to discover BI efficacy in terms of frequency and severity at short-term follow-up (<4 months) and long-term follow-up (>8 months). Additionally, outcome comparisons by patient characteristics will be assessed for age, sex/gender, race/ethnicity, insurance and primary problem drug.

## METHODS

We submitted this study protocol to PROSPERO (CRD42020157733). Methods for this systematic review and pairwise meta-analysis follow the Preferred Reporting Items for Systematic Review and Meta-Analysis Protocols.<sup>44</sup>

### Criteria for considering studies

#### Types of studies

Studies will be limited to RCTs, including cluster randomised trials, comparing BI with control conditions. Control conditions may be considered usual care, no intervention, minimal screening/assessment or informational pamphlets on drug use. Quasi-randomised trials, RCTs without control conditions and implementation studies only investigating provider adherence or cost effectiveness will be excluded.

#### Participants

Participants in the study include people presenting in primary care facilities for physical health concerns who are screened for and report drug misuse. Drug misuse includes legal cannabis, illicit substances, over-the-counter drugs and prescription drugs. Alcohol misuse will not be included. No restrictions will be made in terms of age, sex/gender or country.

#### Intervention

We aimed to include BI studies with a range of one to five sessions, with each session lasting from 5 to 60 min. The intervention may be composed of any one treatment

or a combination of the following delivery modalities: in-person, phone, mailed letter or computer. The intervention may be facilitated by any healthcare provider of any education level.

### Comparators

We will include ‘no intervention’ controls consisting of minimal screening/assessment only. No intervention may also be referred to as usual care and may include drug screening and an information pamphlet about drug misuse and self-help resources. Comparators that include more than screening/assessments needed for study inclusion and randomisation, or an information pamphlet, will be included in the main analysis but excluded in a sensitivity analysis.

### Outcomes

Our outcomes are past 30 days’ drug use frequency or severity. Primary outcomes are (1) drug use frequency and (2) severity at intermediate follow-up (4–8 months). Secondary outcomes are (1) frequency and (2) severity at short-term follow-up (<4 months) and (3) frequency and (4) severity at long-term follow-up (>8 months). If studies use multiple severity measures, we will prioritise based on the majority to have a more congruent outcome. This will not be necessary for frequency, which is a count of days in the past month, and different measures are on the same metric. Typically, studies report follow-ups in intervals of 3, 6, 9 or 12 months. If a study reports two separate follow-ups that both fall within our intermediate range (ie, 4 and 6 months), we will prioritise the follow-up closest to the midpoint (ie, 6 months).

#### Outcomes include

**Frequency (count)**—Frequency is measured as the number of days in the past month the primary problem substance was used. Multiple instruments may be appropriate for analysis and will include but will not be limited to the timeline followback (TLFB)<sup>45</sup> and the Addiction Severity Index (ASI)-Lite Frequency Score.<sup>46</sup>  
**Severity (continuous)**—Severity is measured as a composite score which accounts for past 30 days’ use frequency and includes measures of cravings, withdrawal and drug use consequences in physical, psychological, social and vocational domains. Again, multiple instruments may be appropriate for analysis and will include, but will not be limited to, the Alcohol, Smoking and Substance Involvement Screening Test (ASSIST)<sup>47</sup> and the ASI<sup>46</sup> composite scores.

### Setting

Following previous research, primary care is defined as immediately accessible and general care to a broad range of health concerns in the community or hospital settings.<sup>25 26</sup> Examples include emergency departments, family practice, women’s health clinics and community clinics.

### Search strategy

We will search studies in CENTRAL, Embase, MEDLINE and PsycINFO until 31 May 2020. The preliminary search strategy for MEDLINE (see online supplementary appendix 1) will be adapted for other database terms. Searches will be supplemented with trial registries (eg, www.clinicaltrials.gov) and through relevant existing literature review and RCT reference lists. We will not place any restrictions on language, date, document type or publication status for inclusion. We will use keywords drug misuse, intervention, setting and study method. A database search professional will be enlisted and consulted for the initial search. We will evaluate similar systematic and non-systematic reviews to find potentially eligible trials. Search queries, dates and identified records will be made available in the online supplementary appendix 1 and will be represented in a Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) flowchart.

### Study selection

Reviewers will independently examine titles and abstracts of studies identified in the initial search. Irrelevant or duplicate reports will be removed. Reviewers will then meet to form a consensus on study inclusions. Next, we will obtain full texts of potentially relevant articles. Teams will independently assess each paper for inclusion per criteria, then consensus will be made on conflicting assessments. Coauthors not involved in selection will supervise and form final judgements if consensus cannot be made. If it is still not possible to determine study eligibility, study authors will be contacted for further information. We will report the inter-rater reliability for study inclusion. Multiple published papers related to the same study will be collated and included in the report. The study selection process will be recorded in detail to produce a PRISMA flowchart.

### Data extraction

Reviewers will independently summarise the study details to include methods, participants, interventions, referral information, comparator interventions, treatment outcomes, dropouts and potential biases using a standardised data extraction form. We will develop and pilot the data extraction form for this review. For inconsistencies, review teams will discuss lacking information. Two senior researchers will provide mediation for reporting discrepancies. Data will be entered into Review Manager V.5 software and all authors will independently check entries for inconsistencies.

### Assessment of risk of bias in included studies

Two review authors will independently assess the risk of bias for each study using the Cochrane Risk of Bias Tool V.2 (RoB 2).<sup>48</sup> Risk of bias will be rated for each domain at three levels: high, low and unclear. Evidence will be provided by means of a quotation exemplar. Evidence and justification for judgement of risk of bias will be presented in a table. Studies providing few or no details

about randomisation or blinding will be contacted for clarification. We will evaluate studies providing outcomes on bias due to randomisation process, deviations from intended interventions, missing outcomes, measurement of outcome and selection of reported results.<sup>48</sup> We will report the inter-rater reliability for assessment of bias. Study classification per RoB 2<sup>48</sup> are as follows:

Low risk of bias: All domains are rated at low risk of bias.

Some concerns: At least one domain is rated as concerning, and no domain is rated at high risk of bias.

High risk of bias: At least one domain is rated to be at high risk of bias, or multiple domains are concerning.

### Measures of treatment effect

#### Data synthesis

For studies assessing drug use frequency on the same outcome metric (eg, days used per TLFB or ASI), we will calculate the mean difference (MD) with 95% CIs. Because different instruments measure the same count metric, they can be combined and compared with MD. For studies assessing drug use severity using different outcome measures (eg, severity per ASI or ASSIST), we will calculate the standardised MD with 95% CIs. The SMD is interpreted as small effect (0.2), moderate effect (0.5) and large effect (0.8).<sup>49</sup> ASI and ASSIST have been shown to have strong concurrent validity ( $r=0.73-0.82$ ,  $p<0.001$ ) for derived composite severity scores.<sup>50-52</sup> Studies with more than two treatment arms can cause problems in pairwise meta-analysis. When multiple arms are reported in a single study, we will include only relevant arms. If two treatment arms are relevant, the control will be split and compared with each arm separately.<sup>48 53</sup>

We will analyse results from cluster RCTs<sup>48</sup> by taking account of intracluster correlation coefficients (ICCs). When the ICC is not reported, we will contact the authors. If the ICC is unavailable, we will use comparable study estimates as a correction, in accordance with the *Cochrane Handbook for Systematic Review of Interventions*.<sup>48</sup> In cluster RCTs, we will extract data that account for the clustering. If the trial accounted for cluster level effects by using multilevel modelling or generalised estimating equations, we will directly extract the data. If the study has not statistically accounted for cluster effects, we will use the generic inverse variance approach. The inverse variance approach weights studies to account for small samples, such as when studies are cluster randomised.<sup>54</sup> Cluster analysis decisions will be made based on the number of clusters, outcome data accounting for cluster design and ICC by average cluster size.<sup>55</sup>

If data are missing, we will contact study authors to request the data where needed. We will attempt to use data from intention-to-treat (ITT) analyses. We will present ITT analyses for all primary outcomes using data corrected by mixed-effect models for repeated measures. If studies imputed missing data using last observation carried forward (LOCF), we will include them in the main

meta-analysis but will exclude them during the sensitivity analysis due to uncertainty of reliability in LOCF methods.

### Statistical heterogeneity

Given the potential heterogeneity between studies, we will conduct a random-effect pairwise meta-analysis for all analyses. A random-effect model assumes that the effects are different but sampled from a related representation of a larger population distribution, improving generalisability.<sup>48 56</sup> We will assess statistical heterogeneity using CIs for individual trials visually represented in a forest plot. We will calculate the  $I^2$  statistic with the suggested interpretation: 0%–40% may not be important; 30%–60% moderate heterogeneity; 50%–90% substantial heterogeneity; 75%–100% considerable heterogeneity.<sup>54</sup>  $I^2$  heterogeneity intervals overlap to allow for subjective decision-making that accounts for (1) effect magnitude and direction, (2) strength of evidence (ie, p value size) and (3) detection of significant but very small effects in highly powered studies.<sup>54</sup>

### Assessment of reporting biases

If the analysis contains 10 or more studies, data from included RCTs will be represented in a funnel plot to assess asymmetry. We will consider possible reasons of funnel plot asymmetry, including publication bias.

### Sensitivity analysis

We will conduct sensitivity analyses to examine results robustness from main analyses. We will assess the effects of excluding studies with, and controls receiving more than usual care, studies using LOCF and studies with a high risk of bias in the areas of randomisation process, deviations from intended interventions, missing outcomes, measurement of outcome and selection of reported results.

### Subgroup analyses

We will conduct subgroup analyses to examine differences between subsets of the population of interest and to understand heterogeneity. We will assess differences based on the following patient and trial characteristics: age, sex/gender, race/ethnicity, per cent insured, baseline severity and primary drug misused.

### Patient and public involvement

No patient was involved in the protocol development nor will be involved in the review.

### Ethics and dissemination

This study uses published aggregate data and does not require ethical approval. Findings will be disseminated in a peer-reviewed journal.

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**Contributors** ES and TAF conceived the study. ES, NW and TAF provided substantial contribution to the design of the study during its development. ES, MS, RT, CH, KY, YL, NW and TAF revised the study protocol critically for important intellectual content and contributed to its improvement. ES planned and conducted the search. TAF supervised the statistical analysis planning. ES, MS, RT, CH, KY, YL,

NW and TAF gave the final approval to publish the current protocol. All the authors agreed to be accountable for all aspects of the work in ensuring that questions related to the accuracy or integrity of any part of the work are appropriately investigated and resolved.

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**Competing interests** TAF reports personal fees from Mitsubishi-Tanabe, MSD and Shionogi, and a grant from Mitsubishi-Tanabe, outside the submitted work; TAF has a patent (2018-177688) pending. All the other authors report no competing interest.

**Patient and public involvement** Patients and/or the public were not involved in the design, conduct, reporting or dissemination plans of this research.

**Patient consent for publication** Not required.

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