Exploring the impact of psychological interventions for alcohol use disorder on sleep

A thesis submitted in partial fulfilment of the requirements for the degree of

Doctorate in Clinical Psychology

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Declaration

I, the author, confirm this thesis is my own work and that I am aware of the University of Sheffield guidance on unfair means (www.sheffield.ac.uk/new-students/unfair-means). This thesis is submitted in partial fulfilment of the requirements for the degree of Doctorate in Clinical Psychology. This work has not been submitted for any other degree or to any other institution. No funding has been received for this thesis. No conflicts of interests stated. For any enquiries about data or code sharing, please contact shussain33@sheffield.ac.uk or matt.field@sheffield.ac.uk.

Structure and Word Count

Section One: Literature Review

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Section Two: Empirical Project

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Lay summary

Alcohol Use Disorder (AUD) is a major public health concern affecting millions of people. Many individuals with AUD experience sleep difficulties which persist long after stopping alcohol and are linked to higher risk of relapse. Despite this, the impact of psychological treatments for AUD on sleep is not well understood.

The first chapter explores how sleep changes during and after psychological interventions for AUD. A search of studies identified sixteen studies. Findings showed that interventions specifically targeting sleep such as CBT for insomnia (CBT-I) led to clear improvements in sleep quality and insomnia symptoms. Broader alcohol-focused treatments also improved sleep, even when sleep was not the main focus. However, improvements in sleep and reductions in alcohol use did not always happen together, suggesting sleep should be treated as a separate but related part of alcohol recovery. More research is needed using objective sleep measures and longer follow-up periods to better understand the lasting impact of these interventions.

The second chapter explores how patterns of alcohol consumption relate to changes in sleep quality amongst highly active users of the “Drink Less” app during the first 30 days of app use. Four distinct drinking groups were identified: “light drinking throughout”, “moderate drinking, decreasing”, “heavy drinking, decreasing”, and “heaviest drinking, decreasing”. All groups showed improvements in sleep quality over time, although the rate of improvement differed between groups. Contrary to hypotheses, the “light drinking throughout” group showed the greatest improvements in sleep quality, while the three groups with heavier drinking patterns showed relatively smaller rates of improvement over time.

These findings suggest that reductions in alcohol during the early stages of using an alcohol reduction app do not correspond to improvements in sleep over the same time period. Future research should explore whether digital alcohol interventions may benefit from incorporating targeted sleep-focused support.

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Section One: Literature Review

Sleep outcomes during and after psychological interventions for alcohol use disorder: A Systematic Review

Abstract

Background: Sleep disturbances are common among individuals with Alcohol Use Disorder (AUD) and often persist into recovery. Although psychological interventions such as Cognitive Behavioural Therapy for Insomnia (CBT-I) and Motivational Interviewing (MI) are well-established treatments for AUD, their impact on sleep outcomes remain underreported. This systematic review aims to explore how psychological interventions for AUD impact sleep outcomes during and after treatment; and whether improvements in sleep outcomes precede improvements in alcohol outcomes, or vice versa.

Method: A systematic review was conducted. Scopus, PsycInfo and Medline were searched in February 2025. Included studies involved adults with AUD who underwent psychological interventions for AUD, with sleep outcomes measured over time. Sixteen studies met the inclusion criteria and underwent quality appraisal using the Critical Appraisal Skills Programme (CASP) checklists.

Results: Psychological interventions varied, but CBT-I consistently produced significant improvements across multiple sleep parameters, although these did not translate into improvements in alcohol-related outcomes. Broader AUD treatments were associated with improvements in both alcohol and subjective sleep quality. Integrated treatments addressing comorbid PTSD or depression improved sleep indirectly, often through symptom reduction. However, the direction and mechanisms linking sleep and alcohol outcomes remained unclear.

Conclusion: Psychological interventions for AUD can improve sleep, particularly with insomnia-specific approaches like CBT-I. However, improvements in sleep do not consistently translate into improvements in alcohol outcomes. Future research

should prioritise integrated treatments, objective sleep measures, and long-term follow-up.

Practitioner points:

- Sleep disturbances are highly prevalent in individuals with AUD and therefore should be routinely assessed during treatment.
- Sleep-focused components (e.g., CBT-I) should be considered for individuals with AUD and comorbid insomnia, given their effectiveness on sleep.
- Further research should evaluate long-term sleep outcomes and integration of sleep-focused interventions within AUD treatment.

Keywords: *Alcohol Use Disorder, sleep, insomnia, cognitive behavioural therapy, psychological intervention, systematic review*

Introduction

Background

Although alcohol consumption is common worldwide, harmful alcohol use represents a major public health concern (Heath, 2012; Rehm, 2011). The Alcohol Use Disorders Identification Test (AUDIT; Saunders et al., 1993) is a widely used screening tool for identifying hazardous and excessive alcohol use. A score of ≥8 typically indicates excessive alcohol consumption and potential risk for Alcohol Use Disorder (AUD).

AUD as defined by the Diagnostic and Statistical Manual of Mental Disorders, (DSM-5; American Psychiatric Association, 2013), is diagnosed when an individual meets at least two of eleven criteria for problematic alcohol use within a 12-month period. These criteria include impaired control over drinking, social or occupational impairment, risky use, and physiological dependence, including tolerance and withdrawal. AUD severity is classified as mild, moderate, or severe, depending on the number of criteria met.

According to the World Health Organization (2024), alcohol contributes to 2.6 million deaths each year, representing 4.7% of all global deaths, and is a causal factor in more than 200 disease and injury conditions. AUD is particularly associated with liver disease, various cancers, cardiovascular problems, and a wide range of mental health disorders, including depression and anxiety (Rehm et al., 2017; Boden & Fergusson, 2011).

Beyond the individual, alcohol misuse generates a significant societal burden, including increased healthcare costs, loss of productivity, criminal justice involvement, and familial and interpersonal harm (Sacks et al., 2015).

While alcohol-related harms are well documented, recovery remains complex. Many individuals do not seek or receive treatment and relapse rates following intervention are high (Miller & Wilbourne, 2002; Witkiewitz & Marlatt, 2007).

AUD And Sleep

Sleep disturbances are highly prevalent among individuals with AUD, with estimates suggesting that up to 70% experience clinically significant sleep problems (Brower, 2003; Chakravorty et al., 2016). Common complaints include difficulty falling or staying asleep, non-restorative sleep, irregular sleep-wake patterns, and shortened overall sleep duration (Brower, 2003; Chakravorty et al., 2016). These disturbances are not merely side effects of intoxication or withdrawal but are often deeply entrenched, resulting from the neurobiological impact of chronic alcohol use on the sleep-regulating systems. Over time, these changes can disrupt natural sleep cycles, making sleep disruption a long-lasting concern (Brower, 2001).

Paradoxically, while alcohol may initially induce sedation and aid sleep onset, long-term use contributes to persistent sleep fragmentation and poor sleep quality. Importantly, these disturbances often persist beyond detoxification and abstinence, sometimes lasting for months, and have been associated with increased risk of relapse (Brower et al., 2001; Arnedt et al., 2007). Sleep difficulties may thus act as both a precipitating and maintaining factor in AUD, creating a bidirectional relationship that complicates recovery.

Beyond its relationship with alcohol use, sleep disturbances affect psychological, cognitive, and physical functioning. In both clinical and general populations, untreated insomnia has been associated with significant impairments in mood regulation, including increased irritability, depressive symptoms, and heightened emotional reactivity (Baglioni et al., 2011). Sleep disruption is also linked to cognitive deficits, in attention, memory, and executive functioning, domains often compromised in individuals with AUD (Fortier-Brochu et al., 2012; Harrison & Horne, 2000), and may compromise an individual's ability to engage in or benefit from treatment (Harvey, 2011; Roehrs & Roth, 2001).

Interventions for AUD

Globally, treatment for AUD is delivered across healthcare settings, including specialist addiction services and general medical practice (Rombouts et al., 2020). Treatment encompasses medical and support-based interventions aimed at reducing alcohol consumption, managing withdrawal, and supporting long-term recovery. Pharmacological treatments include disulfiram, acamprosate, and naltrexone, which target different mechanisms of alcohol dependence and cravings (Jonas et al., 2014; Soyka & Müller, 2017; NICE, 2014).

Detoxification services, often delivered in inpatient or outpatient settings, provide medical supervision during the acute withdrawal phase (Brower, 2003). Alongside medical treatment, peer-support groups such as Alcoholics Anonymous (AA) are widely accessed and offer long-term community-based support through shared experiences and structured recovery programs (Kelly et al., 2020).

Psychological interventions for AUD

Psychological interventions are also a key component in the treatment of AUD (Ghosh et al., 2024). They offer individuals the tools to better understand, manage, and ultimately change their relationship with alcohol (Jhanjee, 2014). Alcohol psychoeducation, including information on how alcohol affects the brain, body, decision-making, and long-term health, can provide meaningful insight for individuals experiencing AUD (Heather et al., 2006). Psychoeducational approaches often aim to increase awareness of drinking patterns, triggers, and consequences, supporting individuals in recognising the need for change (Magill et al., 2021).

Among the most widely researched and implemented approaches is Cognitive Behavioural Therapy (CBT). CBT focuses on identifying and modifying maladaptive thoughts, behaviours, and emotional responses related to alcohol use (Magill & Ray, 2009). CBT for AUD often includes skills training in coping strategies, identifying high-risk situations, managing cravings, cognitive restructuring to challenge beliefs that sustain drinking behaviour and relapse prevention (Kadden, 1995).

Another key intervention is Motivational Interviewing (MI). MI is a client-centred approach designed to enhance intrinsic motivation for change by resolving ambivalence around alcohol use (Miller & Rollnick, 2012). MI is particularly effective in the early stages of treatment and can be used as a standalone approach or integrated with CBT to support long-term behaviour change (Lundahl et al., 2010).

Digital and remote psychological interventions, mainly based on CBT and MI principles, have been developed to increase accessibility and engagement (Kiluk, 2019). These include mobile applications, online therapy modules, and guided self-

help programs, which have demonstrated short-term effectiveness in reducing alcohol consumption (Riper et al., 2018).

While CBT and MI are established and effective treatments, emerging evidence supports the use of additional psychological interventions such as mindfulness-based approaches (Garland et al., 2010), acceptance and commitment therapy (ACT) (Lee et al., 2015), and dialectical behaviour therapy (DBT) (Wilks et al., 2017) in treating AUD.

However, despite the overall efficacy of psychological interventions, challenges remain particularly around treatment retention and long-term relapse prevention (Witkiewitz & Marlatt, 2009). High dropout rates and difficulties sustaining behaviour change over time highlight the need for more personalised, flexible, and ongoing care models (Moos & Moos, 2006).

Rationale

While psychological interventions are widely used for AUD, existing systematic reviews primarily focus on reducing alcohol consumption and preventing relapse (Kaner et al., 2018). These reviews demonstrate consistent evidence that psychological therapies can reduce drinking; however, less attention is paid to broader outcomes such as sleep. Despite the high prevalence of sleep disturbances in AUD and the link between poor sleep and relapse, the impact of psychological interventions on sleep remains poorly understood. Understanding this relationship is important, as improvements in sleep may represent one mechanism through which individuals reduce or stop drinking, and reductions in alcohol use may produce beneficial downstream effects on sleep.

To the author's knowledge, no systematic review has explored how sleep changes during and after psychological treatment for AUD and how these changes relate to alcohol outcomes. By synthesising evidence, this review can improve understanding of how psychological interventions for AUD affect sleep and contribute to more holistic, person-centred approaches to treatment.

Aims

This systematic review aims to explore how psychological interventions for AUD impact sleep outcomes during and after treatment; and if improvements in sleep outcomes precede improvements in alcohol outcomes, or vice versa.

Method

Protocol Registration

The protocol for this systematic review was registered on the international prospective register of systematic reviews (PROSPERO; Reference: CRD420250637967).

Study eligibility

The full inclusion and exclusion criteria are outlined in Table 1. Only studies involving adult participants (≥18 years) were included, as individuals under the age of 18 are typically excluded from clinical interventions for AUD.

Studies were included only if participants had AUD. Studies must have either reported that participants had formal diagnoses of AUD or used validated diagnostic or screening measures indicative of AUD (e.g., AUDIT, DSM-based diagnostic interviews, Severity of Alcohol Dependence Questionnaire, Alcohol Dependence Scale). Studies that had recruited participants from specialist alcohol treatment, detoxification, or residential rehabilitation services were also considered eligible, on the basis that entry into such services typically require a clinical diagnosis of AUD or alcohol dependence.

Eligible studies were required to be a study of treatment for AUD and incorporate a psychological component, such as CBT, motivational interviewing, or mindfulness-based approaches. Specifically, a treatment for AUD was defined as any structured intervention delivered to a population with a diagnosis of AUD that aimed to reduce alcohol use or to target psychological or behavioural factors known to maintain or exacerbate problematic drinking or relapse risk. Sleep-focussed interventions were also included where they were delivered to this population. An

intervention was considered to include a psychological component if it explicitly described the intervention as psychological or therapeutic in nature, or if it described structured techniques such as cognitive restructuring, behavioural change strategies, skills training, motivational interviewing, exposure-based techniques, or relapse prevention.

Studies must have assessed sleep outcomes throughout the course of treatment. No restrictions were placed on the date of publication.

To ensure methodological rigour, only peer-reviewed research articles published in peer-reviewed journals were included. Grey literature was excluded to enhance the credibility and quality of studies (Pappas & Williams, 2011).

Table 1

Inclusion and Exclusion Criteria

Inclusion	Exclusion
Adult participants only	Articles that do not meet any of the aforementioned inclusion criteria
Study participants must have AUD	Animal studies or non-human research
Studies must focus on evaluating treatment interventions for AUD	Studies with full texts not available in English
Interventions must include a psychological component	Study protocols or articles that do not report primary findings
Studies must include sleep outcomes measured over the course of treatment	
All study designs (qualitative and quantitative)	

Search strategy

A systematic search was conducted across three databases, (Scopus, Medline via Ovid and PsycInfo via Ovid) in February 2025, in order to identify the

literature investigating the impact of psychological interventions for AUD on sleep outcomes. The Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidance was followed in order to structure and report the identification, screening, eligibility and inclusion of studies (Page et al., 2021). The final completed PRISMA checklist is detailed in Appendix A.

To improve the searching process, guidance was sought from the university liaison librarian to check search terms. During the consultation, the librarian provided valuable support in refining the search strategy, and use of Boolean operators such as AND and OR to structure more precise and comprehensive search queries. They also assisted in how to appropriately map keywords to subject headings and use MeSH terms within each database.

The PRISMA diagram for this flow of the strategy can be found within Figure 1 and search terms for the review are detailed in Appendix B.

Following systematic searching, research papers from the three databases were extracted, and duplicates were removed using EndNote v.21 software (Clarivate, 2023). Titles and abstracts were then screened by the author against the inclusion criteria. Studies that met the criteria proceeded to a full-text review, which served as the second stage of screening for eligibility. To enhance the reliability of the study selection process, approximately 25% of the full-text reviewed studies (*n* = 10) were independently cross-checked by the research supervisor. This sample consisted of five studies randomly selected from those initially included and five from those initially excluded. No disagreements were found between reviewers during this cross-checking process.

Data Extraction

Study characteristics were extracted from the papers, which included author and date, sample size, participant demographics, location, intervention type and comparator (where applicable), how alcohol and sleep were measured and the key findings that relate to the review question. A summary of study characteristics is available in Table 4.

Data Synthesis

A narrative synthesis approach was used to synthesise findings across the included studies following guidance for synthesis without meta-analysis (Campbell et al., 2020). This approach was selected due to substantial heterogeneity across studies. Specifically, included studies varied widely with regards to the type of psychological interventions implemented, delivery formats employed, and treatment contexts. In addition, sleep and alcohol outcomes were measured using a range of subjective and objective instruments and follow-up durations also varied considerably.

Given this heterogeneity, pooling effect sizes through meta-analysis was not considered appropriate, as it would risk producing misleading summary estimates and obscuring clinically meaningful differences between interventions. Instead, studies were grouped according to the primary intervention type, and findings were summarised by identifying and comparing within and across these categories.

Quality appraisal

The quality of the included studies was assessed using the Critical Appraisal Skills Programme (CASP) checklist, a widely used tool for evaluating methodological rigour and the trustworthiness of research evidence (CASP, 2018). The CASP

checklist appropriate to each study type (e.g., Randomised Controlled Trials or Cohort Studies) was applied to ensure a consistent and structured appraisal process. Copies of the appraisal tools used are provided in Appendix C and D.

The CASP framework allows for a nuanced appraisal of each study's validity, transparency, methodological robustness, and overall contribution to the evidence base. Each study was reviewed against the relevant CASP criteria, with responses categorised as ‘Yes’ (criterion clearly met), ‘No’ (criterion not met), or ‘Can’t Tell’ (unclear or insufficient information provided).

The purpose of the quality appraisal was not to assess studies for inclusion/exclusion, rather, in line with previous guidance, quality assessment was conducted to inform interpretation (Carroll & Booth, 2015).

To ensure reliability and consistency, approximately 25% of the studies (*n* = 5) were independently appraised by both the primary author and the research supervisor. Discrepancies (*n* = 3) were resolved through discussion and consensus. Following discussion and clarification of these discrepancies, 100% agreement was reached for the quality assessment. The remaining studies were appraised by the primary author only (*n* = 11).

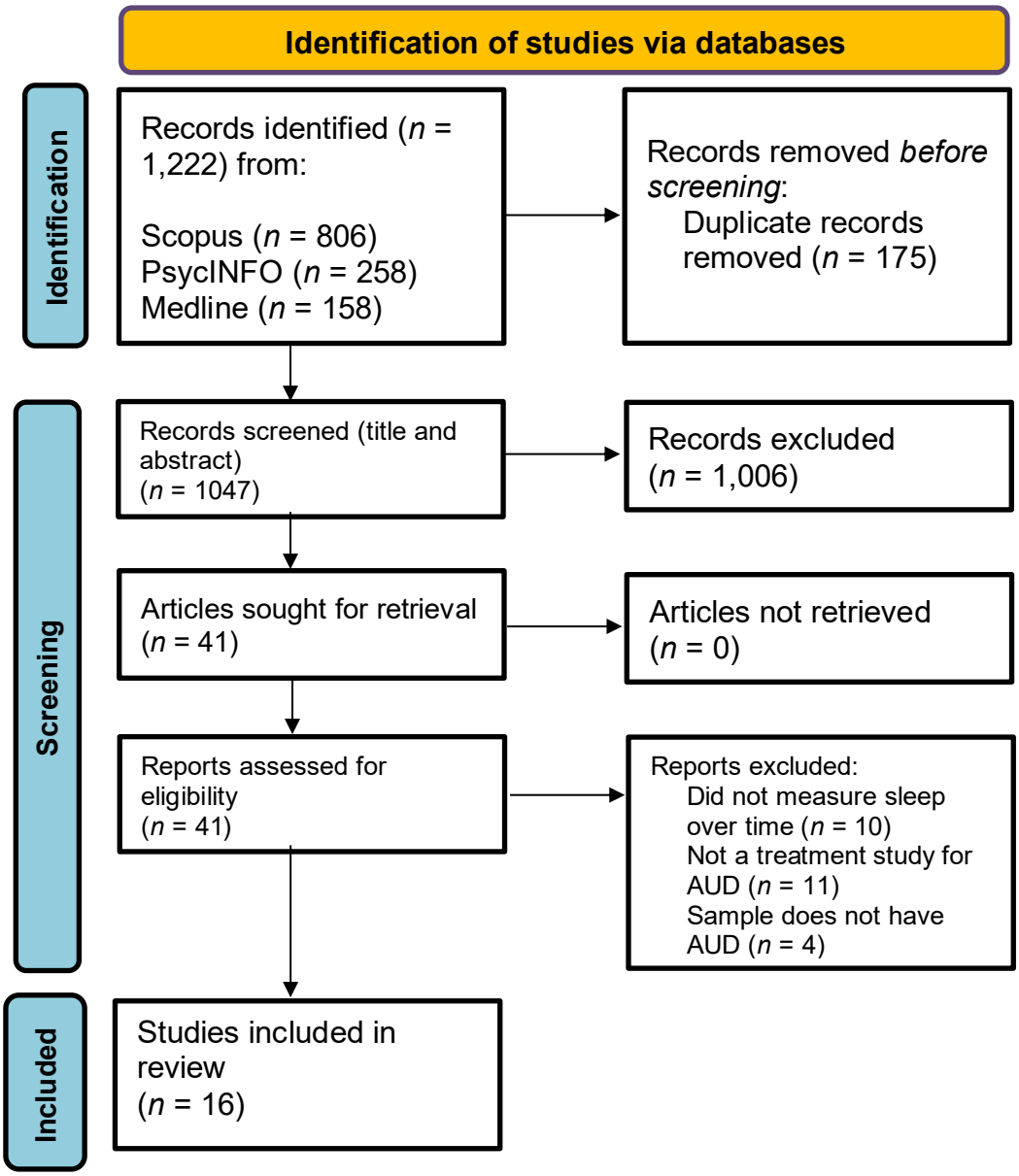
The quality assessment of included studies can be found in Table 3 and 4. As CASP tools do not provide a standardised category system, a simple rating framework was developed to aid in the appraisal process. A point was awarded for each checklist item marked as "Yes," and studies were then categorized as having low (0-5), moderate (6-10), or high (10+) methodological quality based on their total score. This approach allowed for a more structured comparison of study rigor across the included papers.

Results

Figure 1 outlines the PRISMA diagram and search outcomes. 1,222 records were initially identified. After duplicates were removed, 1047 records remained. Initial screening excluded 1,006 records and 41 records met the criteria for a full-text review. At this stage, 25 studies were excluded, with the primary reasons for exclusion presented in Appendix E. Subsequently 16 records were included in this review.

Figure 1

PRISMA Diagram



Study Characteristics:

Table 4 outlines a summary of characteristics of included studies. All included papers were peer-reviewed and published in a range of journals. No date restrictions were applied, allowing for a thorough synthesis of the available evidence.

The studies included within this review were either cohort studies (*n* = 6) or randomised controlled trials (*n* = 10), which explored how psychological interventions for AUD impact sleep outcomes during and following treatment. It is important to note that the psychological interventions examined across the included studies varied considerably. Some interventions were delivered alongside pharmacological treatments such as disulfiram, naltrexone, or acamprosate, whilst others focused only on psychological therapy.

Importantly, several psychological interventions were not limited to addressing alcohol use alone, instead, they also targeted comorbid conditions such as insomnia and PTSD. For example, some studies used CBT-I, while others used modified prolonged exposure therapy to treat co-occurring PTSD and AUD. Interventions also differed in delivery, ranging from structured individual or group therapy to self-help formats, such as online CBT modules or psychoeducational booklets.

Study Settings:

Regarding geographic distribution, the majority of studies (*n* = 12) were conducted in the United States. The remaining studies took place in a range of international settings: one in Canada (Currie et al., 2004), one in China (Huang & Chen, 2021), one in Finland (Laaksonen et al., 2013), and one in Spain (Grau-Lopez et al., 2020).

Sample characteristics:

Sample sizes varied substantially across studies, from seven participants (e.g. Arnedt et al., 2007) to 481 participants (e.g. Grau-Lopez et al., 2020).

Participants were recruited from a variety of different settings: three studies recruited from inpatient settings (Brooks et al., 2021; Grau-Lopez et al., 2020; Huang & Chen, 2021), five recruited from residential treatment clinics (Arnedt et al., 2007; Arnedt et al., 2011; Kolla et al., 2014; Peck et al., 2018; Cameron et al., 2022), four from veteran affair clinics (Buckheit et al., 2022; Chakravorty et al., 2019; Chakravorty et al., 2025; Colvonen et al., 2020), two from outpatient clinics (Currie et al., 2004; Laaksonen et al., 2013) and two from community settings (Hernandez et al., 2004; Miller et al., 2004).

As per the inclusion criteria all participants had a diagnosis of alcohol dependence or AUD, however some studies included participants with co-morbid diagnoses including PTSD, insomnia, major depression, and other substance use disorders (SUD).

Measures of Alcohol and Sleep:

The included studies measured alcohol use and sleep outcomes in a variety of ways reflecting significant variability across the literature. However, despite the variability, all studies employed validated instruments to measure alcohol and sleep outcomes, as detailed below.

Alcohol use was most commonly assessed using the Timeline Follow-Back (TLFB) method (Sobell & Sobell, 1992), which recorded metrics such as drinking frequency, percent days abstinent, percent of heavy drinking days, and drinks per drinking day. Other studies used validated self-report scales including the Alcohol

Dependence Scale (Skinner & Allen, 1982), the Alcohol Use Disorders Identification Test (Saunders et al., 1993), the Severity of Alcohol Dependence Questionnaire (Stockwell et al., 1979), Short Index of Problems (Miller, 1995), the Penn Alcohol Craving Scale (Flannery et al., 1999), Craving Visual Analogue Scale (Hayes, 1921), the Michigan Alcoholism Screening Test (Selzer, 1971), the Alcohol Craving Questionnaire (Singleton et al., 1994), the Brief Addiction Monitor (Cacciola et al., 2013), the Alcohol Withdrawal Symptom Checklist (Banger et al., 1992), the Alcohol Abstinence Self-Efficacy Scale (DiClemente et al., 1994), the Clinical Institute Withdrawal Assessment for Alcohol (Shaw et al., 1981), the Clinical Institute Withdrawal Assessment for Alcohol Revised (Sulivan et al., 1989) and the Drinker Inventory of Consequences (Miller, 1995). Some also incorporated biological measures like urinalysis and alcohol breath tests.

Similarly sleep outcomes were assessed using a variety of methods. Common self-report measures included the Insomnia Severity Index (Bastien et al., 2001), the Pittsburgh Sleep Quality Index (Buysse et al., 1989), and daily sleep diaries (Carney et al., 2012). These captured aspects such as sleep onset latency, wake after sleep onset, total sleep time, sleep efficiency, sleep quality, and frequency of nighttime awakenings. Other sleep-related questionnaires included the Dysfunctional Beliefs and Attitudes about Sleep (Morin et al., 2007), Multidimensional Fatigue Inventory (Smets et al., 1995), Epworth Sleepiness Scale (Johns, 1991), the Sleep Hygiene Index (Mastin et al., 2006), the PROMIS-Sleep Disturbance scale (Cella et al., 2010), Self-Efficacy for Sleep Scale (Lacks, 1987), Sleep-Related Behaviors Questionnaire (Ree & Harvey, 2004), and the 7-item Sleep Impairment Index (Morin, 1993). A number of studies also used objective measures like actigraphy (Ancoli-Israel et al., 2015), which recorded sleep duration, sleep efficiency, sleep onset latency, wake

after sleep onset, and total sleep time. One study also used the European Quality of Life (Group, 1990) to assess quality of life related to sleep and health.

Table 2

Data extraction

Source and Year	Aim	Design	Location	Sample (mean age, gender, diagnosis)	Intervention	Control group	Outcome measures	Key findings
Arnedt et al. (2007)	Evaluate the efficacy of cognitive-behavioral treatment for insomnia in recovering alcoholic patients	Cohort	USA	N = 7 Mean age: 38.6 Male: 3 Female: 4 Diagnosis: AD and Insomnia	8-week CBT for insomnia in alcohol recovery (CBTI-AD), plus ongoing alcohol treatment	No control	*Alcohol:* TLFB = PHDD and DDD; ADS *Sleep:* Sleep diary = SOL, WASO and SE; ISI; DBAS-SF	*Effects of treatment on sleep:* Significant improvements in ISI and DBAS-SF scores. Significant improvements in SOL, WASO and SE. *Other outcomes:* All subjects were abstinent, and no relapses reported. No TLFB data reported.
Arnedt et al. (2011)	Compare the efficacy of CBTI-AD vs. behavioural placebo treatment in improving sleep, daytime	RCT	USA	N = 17 Mean age: 46.2 Male: 11 Female: 6 Diagnosis: AD and Insomnia	8-week CBTI-AD	Behavioural Placebo Treatment (BPT)	*Alcohol:* TLFB = PDA, PHDD and DDD; ADS *Sleep:* Sleep diary = SE, TST, SOL, WASO,	*Effects of treatment on sleep:* CBTI-AD group showed significantly greater improvements in SE and WASO vs BPT.

	symptoms, and reducing relapse in individuals recovering from AD.						nighttime awakenings, sleep quality; ISI	Significant effects were found for SOL and nighttime awakenings in CBTI-AD group but no group differences. CBTI-AD group showed significant improvements in ISI scores vs BPT. *Other outcomes:* No significant differences in relapse rates between groups. Significant correlations were found between baseline drinking variables (PDA, PHDD and DDD) and SE and TST.
Brooks et al. (2021)	Examine how sleep-related behaviours, cognitions, and sleep quality predict relapse in individuals	Cohort	USA	N = 149 Mean age: 47.30 Male: 99 Female: 50 Diagnosis: AUD	Inpatient treatment of alcohol withdrawal, psychosocial management, and an	No control	*Alcohol:* TLFB = PDA, PHDD, DDD; PACS; CIWA *Sleep:*	*Effects of treatment on sleep:* Relapse was significantly associated with higher PSQI, DBAS-SF, SRBQ scores

	with AUD during the transition from inpatient to outpatient care.				educational treatment program.		PSQI; ESS; DBAS-SF; SE-S; SRBQ; Actigraphy = SE, WASO, TIB Diaries tracked sleep and alcohol use.	and lower SE-S scores post discharge. No significant differences in actigraphy sleep data between those who relapsed and those who did not relapse. *Other outcomes:* Individuals who relapsed had significantly higher PACS score post-discharge.
Buckheit et al. (2022)	Test baseline insomnia as a predictor of changes in PTSD symptoms, alcohol use, and treatment engagement in a web-based intervention for veterans. (Secondary data analysis)	RCT	USA	N = 162 Mean age: 31.81 Male: 74 Female: 88 Diagnosis: Hazardous alcohol use and PTSD.	12-week self-guided online CBT program (Thinking Forward) with 12 core modules on trauma and alcohol use, plus 12 optional modules including insomnia.	Primary care TAU, including medical, behavioral health, pharmacy, and weight management.	*Alcohol:* TLFB = PDD and PHDD *Sleep:* ISI *PTSD:* PCL-M	*Effects of treatment on sleep:* Improvements in ISI scores significantly predicted lower PHDD. However, changes in ISI did not predict changes in PDD. Higher baseline ISI scored predicted lower treatment engagement.

								Other outcomes: Baseline PDD and baseline ISI scores significantly predicted PDD post treatment. Higher baseline ISI scores significantly predicted lower PDD.
Cameron et al. (2022)	Examine factors associated with QOL, assess changes in QOL and compare outcome between patients with AUD and OUD.	Cohort	USA	N = 118 Mean age: 38.2 Male: 65.2% Female: 34.8% Diagnosis: AUD and/or SUD.	Residential treatment program providing individualized, holistic care.	No control	*Alcohol:* BAM *Sleep:* BAM	*Effects of treatment on sleep:* Patients reported significant reductions in sleep disturbances from admission to discharge. *Other outcomes:* Patients reported significant reductions in drug/alcohol cravings from admission to discharge.
Chakravorty et al. (2019)	Evaluate the efficacy of 8-week CBT-I in improving insomnia in Veterans with AD during early	RCT	USA	N = 22 Mean age: 54.5 Male: 22 Diagnosis: AD and Insomnia	CBT-I	Standard care with monitoring only (MO); no specific therapy provided	*Alcohol:* TLFB = PDA; PACS *Sleep:* Sleep diary = SL, WASO, nighttime	*Effects of treatment on sleep:* CBT-I group showed significant improvements in SL, ISI, DBAS-SF, and SHI vs MO.

	recovery, including 3- and 6-month follow-up.					for sleep or alcohol use.	awakenings, TST; ISI; DBAS-SF; SHI; ESS	*Other outcomes:* Both groups showed improvements in PDA but no significant group differences. No significant changes in PACS scores.
Chakravorty et al. (2025)	Evaluate the efficacy of CBT-I for improving insomnia, alcohol outcomes, and daytime functioning, and assess whether insomnia improvement is linked to reduced alcohol use.	RCT	USA	N = 63 Mean age: Not reported Male/Female: Not reported Diagnosis: AUD	CBT-I	Quasi-Desensitization Therapy (QDT)	*Alcohol:* TLFB = PDA, DDD, PNHDD; PACS; SIP *Sleep:* Sleep diary: SOL, WASO, TST, SE; ISI;	*Effects of treatment on sleep:* CBT-I group showed significant improvements in WASO at 6 months follow up vs QDT group. Both groups showed improvements in ISI, SOL, WASO, TST and SE but no significant group differences. *Other outcomes:* Both groups showed improvements in PDA but no significant group differences.

Colvonen et al. (2020)	Evaluate whether changes in PTSD and heavy drinking predict changes in sleep among Veterans receiving treatment for PTSD and AUD.	RCT	USA	N = 119 Mean age: 41.6 Male: 107 Female: 12 Diagnosis: AUD and PTSD	Concurrent Treatment for PTSD and Substance Use Disorder Using Prolonged Exposure (COPE) Vs. Integrated coping skills therapy, Seeking Safety (SS)	No control	*Alcohol:* TLFB = PHDD *Sleep:* Sleep diary = SOL, TST, WASO, SE; Actigraphy = TIB, TST, WASO, SE; ISI	*Effects of treatment on sleep:* All pps showed significant reduction in ISI scores, and there was a significant 6-minute reduction in Actigraphy WASO. Greater reduction in PTSD symptoms significantly predicted improvements in ISI. *Other outcomes:* Neither alcohol outcomes (PHDD) nor treatment group predicted change in any sleep outcomes.
Currie et al. (2004)	To test the efficacy of a cognitive–behavioural approach to treating disturbed sleep in abstinent alcoholics.	RCT	Canada	N = 60 Mean age: 43.3 Male: 42 Female: 18 Diagnosis: AD and Insomnia	Individual Therapy for Insomnia (IT) OR Self-help with telephone support (SHTS)	Wait-list control (WLC)	*Alcohol:* TLFB = Relapses, drinking patterns *Sleep:* Sleep diary = TST, SOL, nighttime awakenings, SE, WASO, SQL; Actigraphy = mean	*Effects of treatment on sleep:* Pps in active treatment conditions showed significant improvements in SE, SOL, SQL, nighttime awakenings, PSQI and SII scores vs WLC.

							activity level; PSQI; SII;	*Other outcomes:* No significant difference in relapse rates or drinking patterns across groups.
Grau-Lopez et al. (2020)	To compare sociodemographic, clinical, and psychopathological characteristics of SUD patients undergoing a detoxification program, by comorbidity and insomnia symptoms.	Cohort	Spain	N = 481 Mean age: 40.9 Male: 72.3% Female: 27.7% Diagnosis: Substance Dependence	Pharmacological and psychological interventions to treat SUD.	No control	*Alcohol:* Urinalysis and alcohol breath test *Sleep:* Insomnia symptoms evaluated by psychiatrist via daily interviews based on criteria; sleep logs completed by nurses	*Effects of treatment on sleep:* At discharge, insomnia symptoms were alleviated in 63.8% of patients.
Hernandez-Avila et al. (2004)	Evaluate the effects of nefazodone on mood, anxiety, and drinking behavior in individuals with co-occurring depression and AUD.	RCT	USA	N = 41 Mean age: 42.9 Male: 49% Female: 51% Diagnosis: AD and Major Depression	Nefazodone plus manual-guided supportive psychotherapy delivered over eight sessions.	Placebo plus manual-guided supportive psychotherapy delivered over eight sessions.	*Alcohol:* TLFB = PHDD, DDD; DrInC = Alcohol related problems *Sleep:* PSQI	*Effects of treatment on sleep:* The Nefazodone group showed significant improvements in overall sleep quality, and sleep disturbance vs placebo.

								Other outcomes: The Nefazodone group had significantly fewer DDD and PHDD Vs placebo.
Huang and Chen. (2021)	To evaluate the feasibility and efficacy of intensive group music therapy focused on emotion-regulation skills for treating male inpatients with alcohol dependence.	RCT	China	N = 36 Mean age: 40.8 Male: 36 Female: 0 Diagnosis: AD	Group music therapy	TAU involving drug therapy, psychological, and health education treatment.	*Alcohol:* MAST; CIWA; CVAS *Sleep:* PSQI	*Effects of treatment on sleep:* No significant differences in sleep quality between groups. *Other outcomes:* No significant differences in alcohol cravings between groups.
Kolla et al. (2014)	To examine the course and clinical correlates of sleep problems during early alcohol recovery.	Cohort	USA	N = 119 Mean age: 50.6 Male: 68 Female: 51 Diagnosis: AD	Comprehensive residential treatment program including group therapy, pharmacotherapy, AA meetings, sleep hygiene, and psychiatric consultations.	No control	*Alcohol:* Daily breath alcohol levels; Urine drug screens *Sleep:* PSQI	*Effects of treatment on sleep:* Sleep quality significantly improved with improved PSQI scores and reduced sleep disturbance. Older age significantly predicted improvements in PSQI scores.

								Other outcomes: All participants remained abstinent.
Laaksonen et al. (2013)	To examine how the combination of medication and a brief cognitive behavioral intervention for alcohol dependency can affect patients' quality of life, symptoms of depression and smoking habits.	RCT	Finland	N = 243 Mean age: 43 Male: 71% Female; 29% Diagnosis: AD	Disulfiram OR Naltrexone OR Acamprosate All pps also received a booklet to work through based on principles of CBT.	No control	*Alcohol:* AUDIT; SADD *Sleep:* EQ-5D	*Effects of treatment on sleep:* Sleep improved significantly, but no significant differences between groups. *Other outcomes:* All groups showed significant reductions in drinking.
Miller et al. (2022)	To compare the feasibility, acceptability, and preliminary efficacy of CBT-I in adults with co-morbid insomnia and SUD.	Cohort	USA	N = 22 Mean age: 39.5 Male: 32% Female: 68% Diagnosis: SUD	CBT-I – Half the sample were also receiving SUD treatment elsewhere	No control	*Alcohol:* SIP *Sleep:* Sleep diary = SOL, TST, WASO; DBAS-16; ISI	*Effects of treatment on sleep:* Both groups had significant improvements in DBAS scores, ISI scores and SOL.
Peck et al. (2018)	Characterize adults with comorbid AUD,	RCT	USA	N = 126 Mean age: 33.64 Male: 52.8%	Modified prolonged	Healthy lifestyle control	*Alcohol:* ACQ-Now	*Effects of treatment on sleep:*

	OUD, and PTSD and examine whether opioid use influences treatment-related changes in key outcomes.	Female: 47.2% Diagnosis; AD, PTSD, other SUD	exposure (mPE)	*Sleep:* ISI	Both groups showed significant reductions in insomnia severity, with no significant differences between the groups post-treatment. *Other outcomes:* Both groups showed significant reductions in alcohol craving with no significant differences between the groups post-treatment.

1

[1] AD – Alcohol dependence; DDD – Number of drinks per drinking days; PDA – Percent days abstinent; PHDD – Percent of heavy drinking days; PNHDD – Percent non-heavy drinking days; SE – Sleep efficiency; SOL – Sleep onset latency; SUD – substance use disorder; SQL – Sleep Quality; TIB – Time in bed; TST – Total sleep time; WASO – Wake after sleep onset;

AASE – Alcohol Abstinence Self-Efficacy Scale (DiClemente et al., 1994); ADS – Alcohol Dependence Scale (Skinner & Allen, 1982); ACQ-Now – Alcohol Craving Questionnaire Now (Singleton et al., 1994); AUDIT – Alcohol Use Disorders Identification Test (Saunders et al., 1993); AWSC – Alcohol Withdrawal Symptom Checklist (Banger et al., 1992); BAM – Brief Addiction Monitor (Cacciola et al., 2013); CIWA - Clinical Institute Withdrawal Assessment (Shaw et al., 1981); CIWA-AR – Clinical Institute Withdrawal Assessment for Alcohol Revised (Sulivan et al., 1989); CVAS – Craving Visual Analogue Scale (Hayes, 1921); DBAS-SF – Dysfunctional Beliefs and Attitudes about Sleep (Morin et al., 2007); DrInC – Drinker Inventory of Consequences (Miller, 1995); ESS – Epworth Sleepiness Scale (Johns, 1991); EQ-5D – Modified European Quality of Life (Group, 1990); ISI – Insomnia Severity Index (Bastien et al., 2001); MAST – Michigan Alcoholism Screening Test (Selzer, 1971); PACS – Penn Alcohol Craving Scale (Flannery et al., 1999); PCL-M – PTSD checklist Military version (Weathers et al., 1993); PROMIS – Patient-Reported Outcomes Measurement Information System, Sleep Disturbance scale (Cella et al., 2010); PSQI – Pittsburgh Sleep Quality Index (Buysse et al., 1989); SADD – Severity of Alcohol Dependence Questionnaire (Stockwell et al., 1979); SE-S – Self-Efficacy for Sleep Scale (Lacks, 1987); SHI – Sleep Hygiene Index (Mastin et al., 2006); SII – Sleep Impairment Index (Morin, 1993); SIP – Short Index of Problems (Miller, 1995); SRBQ – Sleep-Related Behaviours Questionnaire (Ree & Harvey, 2004); TLFB – Timeline Follow-Back Interview (Sobell & Sobell, 1992

Table 3

CASP tool checklist for Randomised Controlled Trials

Authors (Date)	CASP Checklist Questions for Randomised Controlled Trials												Overall Rating
	1	2	3	4a	4b	4c	5	6	7	8	9	10	
	Clearly formulated research question?	Participants randomised to interventions?	All participants accounted for at its conclusion?	Participants, 'blind' to the intervention?	Investigators 'blind' to the interventions?	People assessing/analysing outcome/s 'blinded'?	Study groups similar at the start?	All groups receive the same care?	Intervention effects reported comprehensively?	Precision estimates reported?	Benefits outweigh the harms and costs?	Results applied to your local population/in your context?	
Arnedt et al. (2011)	Y	Y	Y	Y	N	CT	Y	Y	Y	N	CT	CT	Moderate
Buckheit et al. (2022)	Y	Y	Y	CT	CT	CT	Y	Y	Y	N	CT	CT	Moderate
Chakravorty et al. (2019)	Y	Y	Y	N	N	CT	Y	Y	Y	Y	CT	CT	Moderate
Chakravorty et al. (2025)	Y	Y	Y	N	N	Y	Y	Y	Y	N	CT	CT	Moderate

Colvonen et al. (2020)	Y	Y	Y	N	N	Y	Y	Y	Y	N	CT	CT	Moderate
Currie et al. (2004)	Y	Y	Y	N	N	N	Y	Y	Y	N	CT	CT	Moderate
Hernandez-Avila et al. (2004)	Y	Y	Y	Y	Y	Y	Y	Y	N	N	CT	CT	Moderate
Huang & Chen. (2021)	Y	Y	Y	N	CT	Y	CT	Y	Y	N	CT	CT	Moderate
Laaksonen et al. (2013)	Y	Y	Y	CT	CT	CT	CT	Y	Y	N	CT	CT	Low
Peck et al. (2018)	Y	Y	Y	CT	CT	CT	Y	Y	Y	Y	CT	CT	Moderate

[2]

[2] Y = Yes; N = No; CT = Can't Tell

Table 4

CASP tool checklist for Cohort Studies

Authors (Date)	CASP Checklist Questions for Cohort studies													Overall Rating
	1	2	3	4	5a	5b	6a	6b	7	8	9	10	11	
	Clearly formulated research question?	Cohort recruited in an acceptable way?	Exposure accurately measured to minimise bias?	Outcome accurately measured to minimise bias	Have all confounding factors been identified?	Confounding factors considered?	Follow up of subjects complete enough?	Follow up of subjects long enough?	Were results reported comprehensively?	Precision estimates reported?	Do you believe the results?	Results applied to your local population/in your context?	Do results fit with other available evidence?	
Arnedt et al. (2007)	Y	Y	Y	Y	CT	CT	Y	N	Y	N	Y	CT	CT	Moderate
Brooks et al. (2021)	Y	Y	Y	Y	CT	CT	Y	CT	Y	Y	Y	CT	CT	Moderate
Grau-Lopez et al. (2020)	Y	Y	N	CT	N	N	CT	CT	Y	Y	CT	CT	CT	Low
Kolla et al. (2014)	CT	Y	Y	Y	N	N	CT	CT	Y	Y	Y	CT	CT	Moderate

Miller et al. (2024)	Y	Y	Y	Y	CT	CT	Y	Y	Y	N	Y	CT	CT	Moderate
Cameron et al. (2022)	Y	Y	Y	CT	CT	CT	CT	N	Y	N	CT	CT	CT	Low

[3]

[3] Y = Yes; N = No; CT = Can't Tell

Randomised Controlled Trials:

Ten randomised controlled trials were assessed using the CASP RCT checklist. Across the included RCTs, the overall methodological quality was predominantly moderate (*n* = 9), with one study rated as low (Laaksonen et al., 2013).

Common strengths observed among these trials included clearly formulated research questions, appropriate randomisation procedures, and accounting for all participants at study conclusion. The studies also consistently showed similar baseline characteristics between groups and equivalent care was provided across study arms.

However, several methodological limitations were consistently observed across the RCTs. Blinding procedures often presented the most significant challenge, with participant blinding achieved in only two studies (Arnedt et al., 2011; Hernandez-Avila et al., 2004). This can often be expected in psychological intervention research where blinding can often be impractical due to the nature of treatment delivery.

Precision reporting was another common shortfall, with only two studies providing adequate confidence intervals or other precision measures (Chakravorty et al., 2019; Peck et al., 2018), limiting interpretability of results. Assessment of whether benefits outweighed harms and costs and applicability of results to local populations was particularly challenging due to the inherently subjective nature of these criteria. None of the included studies explicitly addressed these issues, and all received either "no" or "can't tell" ratings. This highlights the difficulty of using

standardised appraisal tools for criteria that rely on value-based or context-dependent judgments, which are often omitted or underexplored in clinical research.

Cohort Studies:

Six cohort studies were assessed using the CASP cohort study checklist. Four achieved moderate quality ratings, while two studies were rated as low quality (Grau-Lopez et al., 2020; Cameron et al., 2022).

The cohort studies generally demonstrated appropriate study design and conduct. Five studies formulated clear research questions, with one receiving a “can’t tell” rating as this was ambiguous (Kolla et al., 2014). All studies employed acceptable recruitment methods and measured exposures appropriately to minimise bias. Outcome measurement was adequate in most studies, with comprehensive reporting of results achieved in all studies.

However, common weaknesses included incomplete follow-up, limited consideration of confounding variables, and underreporting of precision estimates. None of the studies considered or identified relevant confounding variables and all received either "no" or "can't tell" ratings.

Follow-up varied considerably, with some studies achieving complete follow-up while others were significantly limited. Follow-up duration was also frequently insufficient, with only one study demonstrating an adequate follow-up period of six weeks (Miller et al., 2024). Precision estimates were inconsistently reported, with only three studies providing appropriate confidence intervals or statistical precision measures (Brooks et al., 2021; Grau-Lopez et al., 2020; Kolla et al., 2014).

Overall, the quality of evidence was predominantly moderate across both study designs. Although none of the studies achieved a “high quality” score under

the adopted framework, the included studies still provide valuable evidence. Importantly, quality ratings were not used as exclusion criteria but served to inform interpretation and contextualise findings.

The common methodological limitations were inadequate blinding procedures, insufficient control for confounding factors, poor reporting of precision estimates, and limited assessment of applicability to broader populations. These limitations suggest that whilst meaningful synthesis is achievable, results should be interpreted with consideration of these methodological constraint

Narrative Synthesis:

This section presents a narrative synthesis of findings from the studies examining sleep outcomes during and after psychological interventions for AUD. Given the heterogeneity of study aims, design, intervention, populations, and outcome measures, the synthesis is organized thematically to highlight key patterns.

Interventions for insomnia in AUD populations:

Several studies specifically focused on interventions for insomnia, in individuals with comorbid AUD and insomnia. This is most commonly through targeted interventions such as CBT-I. Although the primary focus of these studies was treating insomnia, they also integrated psychological strategies aimed at facilitating AUD recovery.

Arnedt et al. (2007) conducted a small pilot study (*n* = 7) evaluating 8-week CBTI-AD for insomnia in alcohol recovery. Treatment was an integrated CBT-I approach adapted for individuals in alcohol recovery (CBTI-AD). This combined sleep-focused behavioural strategies with psychological approaches to treat AUD. All participants showed significant improvements in a range of sleep quality measures

whilst remaining abstinent. These findings suggest that interventions specifically targeting sleep such as CBTI-AD can lead to improvements in sleep in individuals who are already abstinent. However, due to the study design, it was not possible to determine whether changes in sleep precede changes in alcohol use or vice versa. Generalisability of findings and interpretations are also limited given the small sample size, lack of control group, and absence of follow-up data.

Arnedt et al. (2011) expanded this work by comparing CBTI-AD to a behavioral placebo treatment (BPT) in a RCT. The intervention group demonstrated significantly greater improvements in sleep outcomes compared to BPT group, however, no significant differences were found in relapse rates between groups. Although the internal validity of this study can be seen as stronger than that of the previous study due to the RCT method employed, the sample size remains small (*n* = 17), which may have limited the power to detect differences in relapse rates. This suggests that whilst CBTI-AD may effectively improve sleep, the findings are insufficient to conclude that it has no effect on relapse.

Chakravorty et al. (2019) examined CBT-I in veterans with comorbid AD and insomnia during early recovery (*n* = 22*)*. Alongside CBT-I, the treatment for AD in this study was Motivational Enhancement Therapy based group therapy. The CBT-I group showed significant improvements in a wide range of sleep outcomes compared to the monitoring-only control. A subsequent study by Chakravorty et al. (2025) with a larger veteran sample (*n* = 63) compared CBT-I to quasi-desensitization therapy (QDT). Both groups showed improvements in different measures of sleep quality. The consistency of sleep-related improvements across these studies strengthens confidence in the robustness of the sleep-related findings. However, whilst both studies showed improvements in some alcohol outcomes, no

significant group differences were found although the studies may have been underpowered to detect between-group differences. Taken together, these findings suggest that insomnia focused interventions may primarily exert their effects through direct mechanisms targeting sleep but may not be sufficient alone to have beneficial effects on alcohol outcomes.

Miller et al. (2024) examined CBT-I in adults with comorbid insomnia and substance use disorder including AUD. Half the sample were receiving substance use disorder (SUD) treatment elsewhere, alongside the CBT-I. Although the sample sizes are too small (*n* = 22) for strong conclusions, both groups did show significant improvements in different measures of sleep quality, with no clear between-group differences in substance use outcomes. However, given the concurrent engagement in SUD treatment among half of the participants, lack of control group, and lack of randomisation means we cannot be certain that improvements in sleep are attributable to CBT-I. It is also unclear whether improvements in sleep were associated with changes in alcohol use, limiting conclusions regarding the relationship between sleep and alcohol outcomes.

One study (Currie et al., 2004) compared individual therapy for insomnia vs self-help telephone support vs wait list controls in abstinent individuals with AD and insomnia. The treatment groups both contained components of treatment for AUD such as psychoeducation about alcohol's impact on sleep. Participants in the active treatment conditions showed significant improvements in a range of sleep outcomes vs wait list controls. However, consistent with other studies there were no significant differences in relapse rates or drinking patterns across groups. This suggests that improvements in sleep are not necessarily associated with improvements in alcohol outcomes. This study provides stronger evidence for the effectiveness of insomnia-

focused interventions on sleep outcomes, given its comparatively larger sample (*n* = 60), and randomised study design.

Collectively, this evidence suggests that insomnia focused interventions are consistently associated with improvements in a range of sleep outcomes. The interventions in these studies incorporated psychological components to treat AUD, however, despite the robust sleep improvements, the interventions showed limited impact on alcohol related outcomes, with most studies finding no significant differences in relapse rates or drinking patterns between treatment and control groups. This strengthens confidence in the conclusion that while interventions focused on insomnia appear effective for addressing sleep disturbances in AUD populations, their role in supporting recovery from AUD remains unclear. However, these findings should be interpreted with caution as many of the studies had small sample sizes and were therefore likely underpowered to detect meaningful differences in alcohol-related outcomes. Short follow-up periods also reduce confidence in long term effects.

Treatments for AUD not specifically targeting insomnia:

The following studies evaluated psychological or multi-modal interventions designed primarily to treat AUD, with sleep outcomes included as secondary outcome measures. These studies provide valuable insight into whether sleep improves indirectly as a result of alcohol-focused treatment, even when sleep is not explicitly targeted.

Brooks et al. (2021) examined the transition from inpatient to outpatient care and found that individuals who relapsed reported significantly worse sleep outcomes post-discharge. In contrast, objective actigraphy data did not reveal significant group

differences. Given the observational design of this study, these findings may reflect associations between sleep disturbance and relapse rather than direct effects of treatment on sleep outcomes. Importantly, it is not possible to determine the direction of association; whether poor sleep caused relapse, relapse to drinking caused sleep to worsen, or whether other factors such as anxiety or psychological distress influenced both relapse and worsening of sleep. Nevertheless, the relatively large sample (*n* = 149) strengthens confidence in the observed association, suggesting that subjective sleep quality may be more predictive of relapse than objective sleep measures.

A couple of studies examined sleep as secondary outcomes within comprehensive residential treatment settings. For example, Cameron et al. (2022) examined participants undergoing a residential treatment program for AUD and/or other SUD (*n* = 118). Participants reported significant reductions in both drug/alcohol cravings and sleep disturbances from admission to discharge.

Similarly, Kolla et al. (2014) examined sleep outcomes in individuals participating in a comprehensive residential treatment program (*n* = 119). Results showed significant improvements in sleep quality and all participants remained abstinent.

Although sleep was not a primary focus in either of these two studies, the findings suggest that holistic alcohol focused treatment approaches may be associated with improvements in sleep. This supports the possibility that sleep may improve indirectly during treatment for AUD. However, although improvements in sleep and alcohol outcomes appear to co-occur in these studies, the study designs mean it is not possible to determine whether improvements in sleep caused changes

in alcohol outcomes, or vice versa; or whether both alcohol use and sleep were influenced by broader aspects of treatment.

While the relatively larger sample in these studies enhances confidence in the improvements, the absence of control groups and the complexity of these multi-component interventions limit the ability to determine which components contributed to sleep. Improvements could also reflect natural recovery processes following alcohol cessation, or structured daily routine rather than specific psychological mechanisms.

Grau-Lopez et al. (2020) examined sleep disturbance symptoms in individuals undergoing a detoxification program for SUDs (*n* = 481). Participants had a formal clinical diagnosis of SUD, with AUD representing the primary substance diagnosis and accounting for 42.4% of the sample. Treatment consisted of pharmacological and psychological interventions. At discharge, insomnia symptoms were reported to be alleviated in 63.8% of patients overall. However, as with the studies above, the improvements in sleep may reflect general recovery related factors such as withdrawal effects and increased structure rather than specific intervention effects limiting insight into the specific mechanisms underlying changes in sleep. Additionally, sleep outcomes at discharge were not reported separately for the AUD subgroup, reducing the extent to which improvements can be attributed specifically to individuals with AUD.

Overall, findings from studies where psychological or multi-modal interventions were primarily aimed at treating AUD, suggest that meaningful improvements in sleep can occur even when not directly targeted. Interventions such as residential treatment programs, and detoxification programs were associated with

reductions in insomnia severity and improvements in subjective sleep quality. However, the strength of this evidence is variable given that study designs without control groups limit causal inference. In particular, where sleep and alcohol outcomes improve in parallel, it is not possible to determine if improvements in sleep lead to reductions in alcohol use, whether reductions in alcohol use led to improved sleep or whether both are driven by broader aspects of treatment. While the consistency of sleep improvement across residential, and detoxification contexts is noteworthy, it remains unclear whether these changes are attributable to specific psychological components, reductions in alcohol use, structured environments or a combination of these.

Pharmacological and Psychological Interventions:

The following section reviews studies examining pharmacological treatments for AUD delivered alongside psychological support. Although these studies did not primarily aim to address sleep disturbances, sleep-related outcomes were assessed as secondary measures.

Hernandez-Avila et al. (2004) conducted a RCT examining nefazodone plus manual-guided supportive psychotherapy versus placebo plus psychotherapy in individuals with comorbid AD and major depression. The nefazodone group showed significant improvements in sleep quality and fewer drinking and heavy drinking days compared to the placebo. No significant differences in depressive symptoms were found. These findings suggest that sleep improvements may arise through multiple pathways including direct pharmacological effects and indirect effects of reduced alcohol consumption. However, given the small sample size (n = 41) the study was likely underpowered, and therefore conclusions regarding these pathways remain

unclear, as it is not possible to infer whether improvements in sleep led to reductions in alcohol use or whether reductions in alcohol use led to improved sleep, or whether both were influenced by other factors.

Laaksonen et al. (2013) examined different medications combined with brief CBT for AD. All groups showed significant improvement in sleep and reductions in drinking with no significant differences between medication groups. This comparatively larger sample (*n* = 243) enhances confidence in the stability of these findings. However, as all participants received both pharmacological and psychological support, it is not possible to determine whether sleep improvements were attributable to medication effects, psychological intervention, alcohol reduction, or natural recovery during abstinence.

Together, these findings suggest that pharmacological treatments combined with psychological support can contribute to improved sleep outcomes. Both studies reported significant improvements in sleep quality alongside reductions in alcohol use, highlighting the value of multi-modal treatment approaches when addressing both alcohol use and sleep disturbances. However, as sleep was assessed as a secondary outcome and mechanisms of change were not directly examined, the specific contribution of pharmacological versus psychological components remain tentative.

Integrated interventions:

Colvonen et al. (2020) examined sleep outcomes amongst participants receiving two integrated psychological treatments for PTSD and SUD (*n* = 119). Whilst changes in alcohol were not clearly reported, both groups demonstrated significant improvements in sleep. However, changes in alcohol use did not predict

improvements in sleep. Instead, reductions in PTSD symptoms were significantly associated with improved sleep, suggesting that sleep improvements may occur independently of changes in alcohol use. This is noteworthy as it provides insight into the potential mechanisms underlying changes in sleep, which are not directly examined in most other studies in this review. It also contrasts with the broader pattern observed across other studies where improvements in sleep often co-occur with improvements in alcohol outcomes, indicating that within integrated interventions, sleep improvements may be driven more by changes in co-occurring psychological symptoms (e.g. trauma symptoms) than by reductions in alcohol use.

Similarly, Buckheit et al. (2022) evaluated an integrated web-based CBT program designed for individuals with co-occurring PTSD and AUD (*n* = 162). Reductions occurred in both heavy drinking and overall drinking frequency, however only reductions in heavy drinking were significantly associated with improvements in sleep. This suggests some integrated interventions may lead to small improvements in sleep, though these improvements are more closely linked to changes in harmful drinking patterns rather than overall alcohol use.

In contrast, Peck et al. (2018) examined sleep in adults with comorbid SUD and PTSD (*n* = 126). Individuals received treatment for PTSD vs healthy lifestyle controls. However, all participants received treatment as usual for their SUD. Both groups showed significant reductions in insomnia severity and alcohol craving with no between group differences. This suggests that sleep improvements may occur during psychological treatment for AUD even without direct sleep-focused interventions and may reflect broader non-specific effects of engaging with structured treatment.

Collectively, integrated interventions suggest that sleep can improve meaningfully even when it is not the primary treatment target. However, mechanisms remain unclear and vary across studies. While some studies indicated sleep improvements may occur independently of alcohol use and instead be linked to changes in PTSD symptoms, others found improvements in sleep to be linked with specific alcohol outcomes such as heavy drinking.

Alternative approaches:

Huang and Chen. (2021) evaluated group music therapy focused on emotion-regulation skills for male inpatients with AD. Both groups showed improvements in sleep quality and alcohol craving, however, no significant differences were found between music therapy and treatment as usual. These findings suggest that improvements in sleep and alcohol craving may reflect general aspects of inpatient care rather than the specific effects of music therapy. However, given the small sample size (*n* = 36) and single-site inpatient context, findings should be interpreted with caution.

Importantly, across all studies the order of changes in alcohol use, sleep, and PTSD symptoms could not be determined, limiting conclusions regarding direction of effects, suggesting these improvements may occur in parallel rather than through a linear pathway.

Discussion

This systematic review aimed to synthesise evidence on how psychological interventions for AUD impact sleep outcomes during and after treatment and to examine whether improvements in sleep outcomes precede improvements in alcohol outcomes or vice versa. The findings revealed insight into the relationship between sleep disturbances and alcohol recovery. Overall, the findings indicate that while sleep improvements are achievable through various intervention approaches, the relationship between sleep and alcohol outcomes is nuanced and context dependent.

The most robust evidence emerged from interventions specifically targeting insomnia in AUD populations, particularly through CBT-I and its adaptations. These interventions consistently demonstrated significant improvements across multiple sleep parameters and validated insomnia severity measures (Arnedt et al., 2011; Currie et al., 2004; Chakravorty et al., 2019). These findings align with a broader body of research demonstrating CBT-I efficacy across diverse populations and clinical contexts (Trauer et al., 2015).

CBT-I is widely recognised as the first-line treatment for insomnia with a large body of research supporting its effectiveness. Studies have shown that CBT-I leads to significant improvements in similar sleep outcomes (SOL, WASO) and improves overall sleep quality in approximately 70-80% of individuals (Edinger & Means., 2005; Trauer et al., 2015). Its efficacy has also been found to extend beyond individuals with primary insomnia, with evidence demonstrating comparable outcomes in populations with comorbid chronic conditions (Currie et al., 2000; Garland et al., 2014; Manber et al., 2008).

Beyond AUD, there is growing evidence supporting the use of CBT-I across other SUDs, where insomnia is highly prevalent (Angarita et al., 2016). For example, Soleimani et al. (2015) found that an eight-week CBT-I programme significantly improved sleep quality among individuals receiving methadone maintenance treatment for opioid use disorder compared with behavioural placebo therapy. Similarly, Arnedt et al. (2023) found that a CBT-I programme tailored for individuals who used cannabis to facilitate sleep showed significantly greater improvements in insomnia severity. Miller et al. (2024) also reported significant improvements in insomnia severity among individuals with alcohol and other SUDs (including cannabis, opioid, methamphetamine and cocaine) receiving CBT-I, further supporting the applicability of CBT-I across diverse clinical populations. Collectively, these findings suggest that the benefits of CBT-I extend across a range of SUD populations. However, consistent with the findings of the present review, CBT-I appears to demonstrate its greatest benefits in improving sleep outcomes, whereas its impact on substance use outcomes remains less consistent.

Several mechanisms have been proposed to explain why interventions such as CBT-I may be beneficial for treating insomnia. Cognitive behavioural models suggest insomnia is maintained through maladaptive beliefs about sleep, conditioned arousal and behaviours such as excessive time in bed, irregular sleep schedules and daytime sleeping (Harvey, 2002; Perlis et al., 2005). CBT-I aims to targets these maintaining factors using strategies such as cognitive restructuring, stimulus control, sleep restriction and sleep hygiene (Edinger & Means, 2005). In individuals with AUD, improving sleep using interventions such as CBT-I, may reduce fatigue, emotional dysregulation and cognitive impairments associated with craving and relapse (Brower, 2003; Chakravorty et al., 2016).

However, despite these theoretical mechanisms, the present review found that improvements in sleep did not consistently correspond with improvements in alcohol-related outcomes. This suggests that although sleep disturbance represents an important treatment target, recovery from AUD is multifactorial, and addressing insomnia alone is unlikely to be sufficient to reduce alcohol use. Instead, CBT-I may be most valuable as an adjunctive intervention alongside evidence-based psychological treatments targeting alcohol use directly.

However, despite these improvements in sleep, most studies found little to no effect on alcohol consumption or relapse rates when insomnia was the primary treatment target. Chakravorty et al. (2025), for instance, reported significant sleep improvements with CBT-I but no corresponding changes in alcohol outcomes. This mirrors results from Currie et al. (2004), who also found that although participants receiving CBT-I experienced significant improvements in sleep, there were no corresponding changes in alcohol-related outcomes. This finding challenges the idea of a simple, two-way relationship between sleep and alcohol use, highlighting the complex nature of AUD.

In contrast, the evidence for sleep improvements as secondary outcomes of AUD focused treatments presents a more complex but encouraging picture that aligns with broader literature on secondary health benefits of addiction treatment. The residential treatment studies (Cameron et al., 2022; Kolla et al., 2014) are particularly noteworthy as they employed multi-modal approaches incorporating medical supervision, psychological support, complementary therapies, and spiritual care. They reported significant improvements in subjective sleep quality which reflect the benefits of addressing multiple recovery domains simultaneously. This aligns with broader research which suggest that comprehensive substance use treatment

approaches often produce broad-spectrum improvements in physical health, mental health, and psychosocial functioning (McLellan et al., 2000; Volkow et al., 2016).

These improvements, however, were not always corroborated by objective sleep outcome measures. Brooks et al. (2021), for example, found discrepancies between subjective reports of poor sleep and actigraphy data, highlighting the influence of psychological and emotional factors on perceived sleep quality. These findings align with research by Roehrs and Roth (2001), who noted that alcohol withdrawal may exacerbate sleep misperception in early recovery.

Integrated treatment approaches for co-occurring AUD and PTSD offered further nuance. In studies by Colvonen et al. (2020) and Buckheit et al. (2022), sleep improvements appeared to be more strongly linked to reductions in PTSD symptoms than to changes in alcohol use. These findings support Germain et al.'s (2007) work suggesting that trauma-related hyperarousal may sustain sleep disturbances independently of substance use. Interestingly, in the study by Buckheit et al. (2022), improvements in ISI scores were associated with reductions in heavy drinking, though not in overall drinking frequency. The complex interplay between trauma symptoms, alcohol use, and sleep disturbances indicates that while sleep and alcohol outcomes may interact, their trajectories do not always align.

The limited evidence from medication trials combined with psychological support included in the review suggests potential synergistic effects on sleep outcomes. Hernandez-Avila et al. (2004) and Laaksonen et al. (2013) both found various AUD medication with supportive psychotherapy led to improvements in sleep quality alongside reductions in alcohol use. These findings align with broader addiction literature emphasizing integrated treatment approaches for co-occurring

disorders. For example, combining antidepressants with psychotherapy and substance use interventions has consistently shown better outcomes than pharmacological or psychosocial treatment alone (Nunes & Levin, 2004).

Notably, the observation that different AUD medications (disulfiram, naltrexone, acamprosate) paired with brief CBT all produced similar sleep improvements, highlights the centrality of the psychological component for sleep related outcomes. This is consistent with evidence that psychological interventions often result in longer-lasting improvements in sleep by targeting maladaptive beliefs, behaviours, and sleep-related self-efficacy (Edinger & Means, 2005; Manber et al., 2008).

These findings collectively reinforce that while psychological interventions can meaningfully improve sleep in individuals with AUD, the type and focus of the intervention matter greatly. Insomnia-focused interventions, particularly CBT-I, consistently yielded the most robust sleep outcomes. However, their limited impact on alcohol consumption suggest it is not the reduction in alcohol that results in the improvements in sleep outcomes. Whilst, alcohol focused treatments may also yield sleep benefits, particularly in structured, holistic settings, these benefits are not guaranteed and may depend on broader psychosocial improvements.

Strengths and limitations:

This review has several strengths. Firstly, it represents the first systematic synthesis focused specifically on sleep outcomes during and after psychological treatment for AUD, addressing a gap in the literature. The inclusion of both randomised controlled trials and cohort studies allowed for a comprehensive overview of different treatment modalities and real-world clinical practices enhancing

ecological validity. Furthermore, the application of a structured appraisal framework ensured consistency in evaluating methodological quality across studies.

However, several limitations should be considered when interpreting the findings of this review. Due to substantial heterogeneity across interventions, outcome measures, and study designs, a meta-analysis was not appropriate limiting the precision and inferential strength of the conclusions that can be drawn. Whilst a narrative synthesis allowed for meaningful comparison, it is inherently more subjective and does not allow pooled effect sizes to be calculated or provide an overall estimate of the magnitude of intervention effects. As a result, conclusions regarding effectiveness of interventions are based on subjective patterns across studies rather than statistically aggregated estimates and should be interpreted with caution.

In addition, many studies used self-report sleep measures without accompanying objective data, which could introduce bias due to sleep misperception or expectancy effects. Follow up durations were also often short, making it difficult to assess the sustainability of observed improvements in sleep or drinking outcomes. Finally, the absence of detailed qualitative feedback in studies limits insight into participants subjective experience of sleep, perceived mechanisms of change and the factors influencing engagement and adherence.

Clinical implications:

The findings of this systematic review have several important implications for clinical practice in the treatment of individuals with AUD and comorbid sleep disturbances.

Sleep disturbances should be routinely assessed and addressed in AUD treatment, given the high prevalence of insomnia in this population and their potential impact on treatment outcomes (Brower et al., 2001). Given the finding that subjective sleep quality may be more predictive of relapse than objective measures (Brooks et al., 2021), regular monitoring of both objective and subjective sleep measures may provide a more comprehensive picture of recovery progress and help identify individuals at higher risk for relapse.

Interventions such as CBT-I, which have demonstrated strong efficacy across populations, should be considered as adjuncts to AUD treatment, particularly for individuals with persistent sleep complaints. However, for individuals in whom alcohol recovery is the primary goal, the evidence suggests that comprehensive, multi-modal approaches may benefit both alcohol and sleep. This may be particularly relevant for individuals with less severe sleep disturbances or those where treatment resources are limited.

The finding that residential treatment programs and other comprehensive approaches can produce meaningful sleep improvements without specific sleep-focused interventions suggests that addressing broader recovery-related factors may have positive downstream effects on sleep quality. However, clinicians should not assume that reductions in alcohol use will automatically lead to improvements in sleep. Instead, sleep interventions should be embedded within broader treatment frameworks that address the multifaceted nature of addiction.

Ultimately, treatment selection should be guided by primary treatment goals, patient presentation, and an understanding of the complex relationships between different symptom domains.

Further research

Future research should prioritise integrated treatment protocols targeting both insomnia and AUD. Research should also use standardised and objective sleep measures such as actigraphy or polysomnography to validate self-reported improvements and reduce bias, including sleep misperception and expectancy effects. Qualitative feedback could further explore experiences of sleep change, treatment acceptability, barriers to engagement, and perceived mechanisms of improvement. Integrating qualitative and quantitative approaches would provide a more comprehensive understanding of effectiveness, strengthening the clinical implications.

Additionally, future reviews should include a broader range of studies exploring web-based interventions for alcohol use, as digital platforms are increasingly common and accessible. These interventions often incorporate behavioural strategies that could influence sleep yet remain underrepresented in the literature. Evaluating their impact on alcohol and sleep-related outcomes could help identify scalable, cost-effective treatment options.

Finally, the limited follow-up periods in most studies represent a significant gap in the literature. Given the chronic nature of both sleep disturbances and AUD, research examining long-term outcomes is crucial for understanding the sustained effectiveness of different intervention approaches and the long-term relationships between sleep and recovery outcomes.

Conclusion

This systematic review provides important insights into sleep outcomes during and after psychological interventions for AUD and whether sleep improvements precede improvements in alcohol outcomes. The evidence demonstrates that sleep improvements are achievable with sleep-targeted interventions showing the most consistent benefits. This is consistent with the extensive literature on CBT-I and its effectiveness across diverse populations.

However, the relationship between sleep and alcohol recovery appears more complex than a simple bidirectional model, with better alcohol outcomes not necessarily translating to sleep improvements and vice versa.

These findings provide a nuanced understanding of the relationship between sleep and alcohol use, supporting the recognition that sleep is a critical, yet often overlooked, aspect of AUD treatment. The evidence suggests that sleep should be treated as a distinct domain, with targeted interventions, while acknowledging its interconnection with broader recovery processes.

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Appendices

Appendix A: Preferred Reporting Items for Systematic Reviews

Section and Topic	Item #	Checklist item	Location where item is reported
TITLE			
Title	1	Identify the report as a systematic review.	Page 2
ABSTRACT			
Abstract	2	See the PRISMA 2020 for Abstracts checklist.	Page 3
INTRODUCTION			
Rationale	3	Describe the rationale for the review in the context of existing knowledge.	Page 9
Objectives	4	Provide an explicit statement of the objective(s) or question(s) the review addresses.	Page 10
METHODS			
Eligibility criteria	5	Specify the inclusion and exclusion criteria for the review and how studies were grouped for the syntheses.	Page 12
Information sources	6	Specify all databases, registers, websites, organisations, reference lists and other sources searched or consulted to identify studies. Specify the date when each source was last searched or consulted.	Page 12
Search strategy	7	Present the full search strategies for all databases, registers and websites, including any filters and limits used.	Page 12
Selection process	8	Specify the methods used to decide whether a study met the inclusion criteria of the review, including how many reviewers screened each record and each report retrieved, whether they worked independently, and if applicable, details of automation tools used in the process.	Page 13-14
Data collection process	9	Specify the methods used to collect data from reports, including how many reviewers collected data from each report, whether they worked independently, any processes for obtaining or confirming data from study investigators, and if applicable, details of automation tools used in the process.	Page 13-14
Data items	10a	List and define all outcomes for which data were sought. Specify whether all results that were compatible with each outcome domain in each study were sought (e.g. for all measures, time points, analyses), and if not, the methods used to decide which results to collect.	Page 13-14
	10b	List and define all other variables for which data were sought (e.g. participant and intervention characteristics, funding sources). Describe any assumptions made about any missing or unclear information.	Page 13-14
Study risk of bias assessment	11	Specify the methods used to assess risk of bias in the included studies, including details of the tool(s) used, how many reviewers assessed each study and whether they worked independently, and if applicable, details of automation tools used in the process.	Page 14
Effect measures	12	Specify for each outcome the effect measure(s) (e.g. risk ratio, mean difference) used in the synthesis or presentation of results.	
Synthesis methods	13a	Describe the processes used to decide which studies were eligible for each synthesis (e.g. tabulating the study intervention characteristics and comparing against the planned groups for each synthesis (item #5)).	N/A
	13b	Describe any methods required to prepare the data for presentation or synthesis, such as handling of missing summary statistics, or data conversions.	Page 14
	13c	Describe any methods used to tabulate or visually display results of individual studies and syntheses.	Page 14
	13d	Describe any methods used to synthesize results and provide a rationale for the choice(s). If meta-analysis was performed, describe the model(s), method(s) to identify the presence and extent of statistical heterogeneity, and software package(s) used.	N/A
	13e	Describe any methods used to explore possible causes of heterogeneity among study results (e.g. subgroup analysis, meta-regression).	N/A
	13f	Describe any sensitivity analyses conducted to assess robustness of the synthesized results.	N/A
Reporting bias assessment	14	Describe any methods used to assess risk of bias due to missing results in a synthesis (arising from reporting biases).	N/A
Certainty assessment	15	Describe any methods used to assess certainty (or confidence) in the body of evidence for an outcome.	N/A

Section and Topic	Item #	Checklist item	Location where item is reported
RESULTS			
Study selection	16a	Describe the results of the search and selection process, from the number of records identified in the search to the number of studies included in the review, ideally using a flow diagram.	Page 15
	16b	Cite studies that might appear to meet the inclusion criteria, but which were excluded, and explain why they were excluded.	Page 15
Study characteristics	17	Cite each included study and present its characteristics.	Page 19-29
Risk of bias in studies	18	Present assessments of risk of bias for each included study.	Page 30-33
Results of individual studies	19	For all outcomes, present, for each study: (a) summary statistics for each group (where appropriate) and (b) an effect estimate and its precision (e.g. confidence/credible interval), ideally using structured tables or plots.	N/A
Results of syntheses	20a	For each synthesis, briefly summarise the characteristics and risk of bias among contributing studies.	Page 34-41
	20b	Present results of all statistical syntheses conducted. If meta-analysis was done, present for each the summary estimate and its precision (e.g. confidence/credible interval) and measures of statistical heterogeneity. If comparing groups, describe the direction of the effect.	N/A
	20c	Present results of all investigations of possible causes of heterogeneity among study results.	Page 34-41
	20d	Present results of all sensitivity analyses conducted to assess the robustness of the synthesized results.	N/A
Reporting biases	21	Present assessments of risk of bias due to missing results (arising from reporting biases) for each synthesis assessed.	N/A
Certainty of evidence	22	Present assessments of certainty (or confidence) in the body of evidence for each outcome assessed.	Page 41-43
DISCUSSION			
Discussion	23a	Provide a general interpretation of the results in the context of other evidence.	Page 44-47
	23b	Discuss any limitations of the evidence included in the review.	Page 41-43
	23c	Discuss any limitations of the review processes used.	Page 48
	23d	Discuss implications of the results for practice, policy, and future research.	Page 48-50
OTHER INFORMATION			
Registration and protocol	24a	Provide registration information for the review, including register name and registration number, or state that the review was not registered.	Page 11
	24b	Indicate where the review protocol can be accessed, or state that a protocol was not prepared.	Page 11
	24c	Describe and explain any amendments to information provided at registration or in the protocol.	N/A
Support	25	Describe sources of financial or non-financial support for the review, and the role of the funders or sponsors in the review.	N/A
Competing interests	26	Declare any competing interests of review authors.	N/A
Availability of data, code and other materials	27	Report which of the following are publicly available and where they can be found: template data collection forms; data extracted from included studies; data used for all analyses; analytic code; any other materials used in the review.	N/A

From: Page MJ, McKenzie JE, Bossuyt PM, Boutron I, Hoffmann TC, Mulrow CD, et al. The PRISMA 2020 statement: an updated guideline for reporting systematic reviews. BMJ 2021;372:n71. doi: 10.1136/bmj.n71. This work is licensed under CC BY 4.0. To view a copy of this license, visit https://creativecommons.org/licenses/by/4.0/

Appendix B: Search Syntax

Construct	Search terms
Alcohol	"Alcohol Abuse" OR "Alcohol Use" OR "Alcohol Use Disorder" OR "Alcoholism" OR "Binge drinking" OR "Alcohol addict" OR "Alcohol dependence" OR "Alcohol consumption" OR "Alcoholics" OR "Alcohol misuse"
Treatment	"Psychotherapy" OR "Psychotherap*" OR "Behavio* therap*" OR "Cognitive behavio* therap*" OR "CBT" OR "Motivational Interviewing"
Outcome	"Sleep depriv*" OR "Sleep quality" OR "Insomnia" OR "Sleep"

Note: Individual search terms each construct were combined with the Boolean operator 'OR'; constructs were combined with the Boolean operator 'AND'.

Appendix C: CASP checklist for Cohort Studies

CASP Checklist:
For Cohort Studies

Reviewer Name:	
Paper Title:	
Author:	
Web Link:	
Appraisal Date:	

During critical appraisal, never make assumptions about what the researchers have done. If it is not possible to tell, use the "Can't tell" response box. If you can't tell, at best it means the researchers have not been explicit or transparent, but at worst it could mean the researchers have not undertaken a particular task or process. Once you've finished the critical appraisal, if there are a large number of "Can't tell" responses, consider whether the findings of the study are trustworthy and interpret the results with caution.

Section A: Are the results valid?	
1. Did the study address a clearly focused issue?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: *A question can be 'focused' in terms of* • *the population studied* • *the risk factors studied* • *is it clear whether the study tried to detect a beneficial or harmful effect* • *the outcomes considered*	
2. Was the cohort recruited in an acceptable way?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: • *Look for selection bias which might compromise the generalisability of the findings:* • *was the cohort representative of a defined population* • *was there something special about the cohort* • *was everybody included who should have been*	
3. Was the exposure accurately measured to minimise bias?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: *Look for measurement or classification bias:* • *did they use subjective or objective measurements* • *do the measurements truly reflect what you want them to (have they been validated)* • *were all the subjects classified into exposure groups using the same procedure*	
4. Was the outcome accurately measured to minimise bias?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: *Look for measurement or classification bias:* • *did they use subjective or objective measurements* • *do the measurements truly reflect what you want them to (have they been validated)* • *has a reliable system been established for detecting all the cases (for measuring disease occurrence)* • *were the measurement methods similar in the different groups* • *were the subjects and/or the outcome assessor blinded to exposure (does this matter)*	
5. (a) Have the authors identified all important confounding factors?	☐ Yes ☐ No ☐ Can't Tell

CONSIDER: • *list the ones you think might be important, and ones the author missed*	
b) Have they taken account of the confounding factors in the design and/or analysis?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: • *look for restriction in design, and techniques e.g. modelling, stratified-, regression-, or sensitivity analysis to correct, control or adjust for confounding factors*	
6. a) Was the follow up of subjects complete enough?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: • *the persons that are lost to follow-up may have different outcomes than those available for assessment* • *in an open or dynamic cohort, was there anything special about the outcome of the people leaving, or the exposure of the people entering the cohort*	
b) Was the follow up of subjects long enough?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: • *the good or bad effects should have had long enough to reveal themselves*	
Section B: What are the results?	
7. What are the results of this study?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: • *what are the bottom line results* • *have they reported the rate or the proportion between the exposed/unexposed, the ratio/rate difference* • *how strong is the association between exposure and outcome (RR)* • *what is the absolute risk reduction (ARR)*	
8. How precise are the results?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER:	

• *look for the range of the confidence intervals, if given*	
9. Do you believe the results?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: • *big effect is hard to ignore* • *can it be due to bias, chance or confounding* • *are the design and methods of this study sufficiently flawed to make the results unreliable* • *Bradford Hills criteria (e.g. time sequence, dose-response gradient, biological plausibility, consistency)*	
Section C: Will the results help locally?	
10. Can the results be applied to the local population?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: • *Is a cohort study the appropriate method to answer this question* • *If the subjects covered in this study could be sufficiently different from your population to cause concern* • *If your local setting is likely to differ much from that of the study* • *If you can quantify the local benefits and harms*	
11. Do the results of this study fit with other available evidence?	☐ Yes ☐ No ☐ Can't Tell
12. What are the implications of this study for practice?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: • *one observational study rarely provides sufficiently robust evidence to recommend changes to clinical practice or within health policy decision making* • *for certain questions, observational studies provide the only evidence* • *recommendations from observational studies are always stronger when supported by other evidence*	

Appendix D: CASP checklist for Randomised Controlled Trials (RCTs)

CASP Checklist:
For Randomised Controlled Trials (RCTs)

Reviewer Name:	
Paper Title:	
Author:	
Web Link:	
Appraisal Date:	

During critical appraisal, never make assumptions about what the researchers have done. If it is not possible to tell, use the "Can't tell" response box. If you can't tell, at best it means the researchers have not been explicit or transparent, but at worst it could mean the researchers have not undertaken a particular task or process. Once you've finished the critical appraisal, if there are a large number of "Can't tell" responses, consider whether the findings of the study are trustworthy and interpret the results with caution.

Section A Is the basic study design valid for a randomised controlled trial?	
1. Did the study address a clearly formulated research question?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: *Was the study designed to assess the outcomes of an intervention?* *Is the research question 'formulated' in terms of:* • *Population studied* • *Intervention given* • *Comparator chosen* • *Outcomes measured?*	
2. Was the assignment of participants to interventions randomised?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: • *How was randomisation carried out? Was the method appropriate?* • *Was randomisation sufficient to eliminate systematic bias?* • *Was the allocation sequence concealed from investigators and participants?*	
3. Were all participants who entered the study accounted for at its conclusion?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: • *Were losses to follow-up and exclusions after randomisation accounted for?* • *Were participants analysed in the study groups to which they were randomised (intention-to-treat analysis)?* • *Was the study stopped early? If so, what was the reason?*	
Section B Was the study methodologically sound?	
4. (a) Were the participants 'blind' to intervention they were given?	☐ Yes ☐ No ☐ Can't Tell

(b) Were the investigators 'blind' to the intervention they were giving to participants?	☐ Yes ☐ No ☐ Can't Tell
(c) Were the people assessing/analysing outcome/s 'blinded'?	☐ Yes ☐ No ☐ Can't Tell
5. Were the study groups similar at the start of the randomised controlled trial?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: • *Were the baseline characteristics of each study group (e.g. age, sex, socio-economic group) clearly set out?* • *Were there any differences between the study groups that could affect the outcome/s?*	
6. Apart from the experimental intervention, did each study group receive the same level of care (that is, were they treated equally)?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: • *Was there a clearly defined study protocol?* • *If any additional interventions were given (e.g. tests or treatments), were they similar between the study groups?* • *Were the follow-up intervals the same for each study group?*	
Section C: What are the results?	
7. Were the effects of intervention reported comprehensively?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: • *Was a power calculation undertaken?* • *What outcomes were measured, and were they clearly specified?*	

• *How were the results expressed? For binary outcomes, were relative and absolute effects reported?* • *Were the results reported for each outcome in each study group at each follow-up interval?* • *Was there any missing or incomplete data?* • *Was there differential drop-out between the study groups that could affect the results?* • *Were potential sources of bias identified?* • *Which statistical tests were used?* • *Were p values reported?*

8. Was the precision of the estimate of the intervention or treatment effect reported?	☐ Yes ☐ No ☐ Can't Tell

CONSIDER: • *Were confidence intervals (CIs) reported?*

9. Do the benefits of the experimental intervention outweigh the harms and costs?	☐ Yes ☐ No ☐ Can't Tell

CONSIDER: • *What was the size of the intervention or treatment effect?* • *Were harms or unintended effects reported for each study group?* • *Was a cost-effectiveness analysis undertaken? (Cost-effectiveness analysis allows a comparison to be made between different interventions used in the care of the same condition or problem.)*

Section D: Will the results help locally?

10. Can the results be applied to your local population/in your context?	☐ Yes ☐ No ☐ Can't Tell

CONSIDER: • *Are the study participants similar to the people in your care?* • *Would any differences between your population and the study participants alter the outcomes reported in the study?* • *Are the outcomes important to your population?*

• *Are there any outcomes you would have wanted information on that have not been studied or reported?* • *Are there any limitations of the study that would affect your decision?*	
11. Would the experimental intervention provide greater value to the people in your care than any of the existing interventions?	☐ Yes ☐ No ☐ Can't Tell
CONSIDER: • *What resources are needed to introduce this intervention taking into account time, finances, and skills development or training needs?* • *Are you able to disinvest resources in one or more existing interventions in order to be able to re-invest in the new intervention?*	

APPRAISAL SUMMARY: *List key points from your critical appraisal that need to be considered when assessing the validity of the results and their usefulness in decision-making.*		
Positive/Methodologically sound	**Negative/Relatively poor methodology**	**Unknowns**

Appendix E: Studies excluded from the review

Author	Paper Title	Exclusion Reason
Berger et al. (2013)	Efficacy of Acamprosate for Alcohol Dependence in a Family Medicine Setting in the United States: A Randomized, Double-Blind, Placebo-Controlled Study	Did not measure sleep over course of treatment
Berman et al. (2015)	Changes in Mental and Physical Well-Being Among Problematic Alcohol and Drug Users in 12-Month Internet-Based Intervention Trials	Subjects did not have AUD
Cucciare et al. (2011)	Characterizing binge drinking among U.S. military Veterans receiving a brief alcohol intervention	Did not measure sleep over the course of treatment and subjects did not have AUD
Dwyer et al. (2024)	Latent profiles of regulatory flexibility in alcohol use disorder: Associations with delay discounting and symptoms of depression, anxiety, and stress	Not a treatment study for AUD and subjects did not have AUD
Farren et al. (2009)	A double-blind, placebo-controlled study of sertraline with naltrexone for alcohol dependence	Did not measure sleep over course of treatment
Fucito et al. (2017)	Using Sleep Interventions to Engage and Treat Heavy-Drinking College Students: A Randomized Pilot Study	Subjects did not have AUD
Greef & Conradie. (1998)	Use of Progressive Relaxation Training for Chronic Alcoholics with Insomnia	Not a treatment study for AUD
Johnson et al. (1996)	Ritanserin in the treatment of alcohol dependence – a multi-center clinical trial	Did not measure sleep over course of treatment
Killeen et al. (2004)	Effectiveness of Naltrexone in a Community Treatment Program	Did not measure sleep over course of treatment

Milanak et al. (2023)	A Transdiagnostic group therapy for sleep and anxiety among adults with substance use disorders: Protocol and pilot investigation	Not a treatment study for AUD
Miller et al. (2021)	Cognitive behavioral therapy for insomnia among young adults who are actively drinking: a randomized pilot trial	Not a treatment study for AUD and subjects did not have AUD
Miller et al. (2021)	Insomnia treatment effects among young adult drinkers: Secondary outcomes of a randomized pilot trial	Not a treatment study for AUD and subjects did not have AUD
Miller et al. (2022)	Cannabis use as a moderator of cognitive behavioral therapy for insomnia	Not a treatment study for AUD and subjects did not have AUD
Miller et al. (2025)	Daily Associations between Sleep and Alcohol Use among Veterans: Acute and Cumulative Effects	Not a treatment study for AUD
Morgenstern et al. (2016)	Within-Person Associations Between Daily Motivation and Self-Efficacy and Drinking Among Problem Drinkers in Treatment	Not a treatment study for AUD
Oslin et al. (2008)	A Placebo-Controlled Randomized Clinical Trial of Naltrexone in the Context of Different Levels of Psychosocial Intervention	Did not measure sleep over course of treatment
Palfai et al. (2014)	Personalized Feedback as a Universal Prevention Approach for College Drinking: A Randomized Trial of an e-Mail Linked Universal Web-Based Alcohol Intervention	Did not measure sleep over the course of treatment and subjects did not have AUD
Sessa et al. (2021)	First study of safety and tolerability of 3,4-methylenedioxymethamphetamineassisted psychotherapy in patients with alcohol use disorder	Did not measure sleep over the course of treatment
Smith et al. (2014)	Sleep Related Beliefs and their Association with Alcohol	Not a treatment study for AUD

	Relapse Following Residential Alcohol Detoxification Treatment	
Soyka et al. (2002)	Use of Acamprosate and Different Kinds of Psychosocial Support in Relapse Prevention of Alcoholism	Did not measure sleep over the course of treatment
Strid et al. (2018)	The influence of hazardous drinking on psychological functioning, stress and sleep during and after treatment in patients with mental health problems: a secondary analysis of a randomised controlled intervention study	Not a treatment study for AUD and subjects did not have AUD
Verlinden et al. (2023)	Effects of a digital cognitive behavioral therapy for insomnia on sleep and alcohol consumption in heavy drinkers: A randomized pilot study	Not a treatment study for AUD and subjects did not have AUD
White et al. (2007)	Long-Term Effects of Brief Substance Use Interventions for Mandated College Students: Sleeper Effects of an In-Person Personal Feedback Intervention	Did not measure sleep over the course of treatment
Wyant et al. (2023)	Acceptability of Personal Sensing Among People With Alcohol Use Disorder: Observational Study	Not a treatment study for AUD
You et al. (2025)	Evaluating a 30-day alcohol abstinence challenge in heavy-drinking individuals with and without chronic pain: Feasibility, safety, and perceived benefits	Not a treatment study for AUD

Section Two: Empirical Project

Trajectories of alcohol consumption and changes in sleep quality amongst highly active users of the Drink Less app

Abstract

Background: Alcohol Use Disorder (AUD) is associated with difficulties. Digital interventions offer tools for alcohol reduction, yet limited research has examined impacts on sleep. This study aims to identify drinking trajectories among Drink Less app users and explore how changes in alcohol consumption relate to changes in sleep.

Method: A secondary data analysis was conducted using data from app users over a 30-day period (N = 15,457). Latent class analysis was used to identify drinking trajectories, and linear mixed models were used to examine changes in sleep.

Results: Four drinking trajectories were found: “light drinking throughout (37.7%)”, “moderate drinking, decreasing (7.8%)”, “heavy drinking, decreasing (20.3%)”, and “heaviest drinking, decreasing (34.1%)”. Groups showed improvements in sleep over 30-days $F(1, 158{,}459) = 539.93$, $p < .001$, however, the rate of improvement differed between trajectories $F(3, 158{,}459) = 20.86$, $p < .001$. The “light drinking throughout” group had better sleep at baseline and demonstrated the steepest improvements ($\beta = 0.032$, $p < .001$), whereas moderate and heavier drinking groups demonstrated comparatively shallower rates of sleep improvement over time.

Conclusion: Users demonstrated improvements in sleep over 30 days of app use, with the steepest rate of improvement among light drinkers. Future research should explore whether integrating sleep-focused interventions within digital interventions enhances both sleep and alcohol-related outcomes.

Practitioner points:

- Sleep may improve during digital alcohol reduction interventions, although the rate of improvement will vary depending on the rate of changes in drinking over time.
- App users who maintain light drinking throughout the first month report better sleep at baseline and a steeper rate of improvement in sleep over time, relative to other groups, although group differences are small.
- Future research should explore whether digital alcohol interventions may benefit from targeted sleep-focused support

Keywords: *Alcohol Use Disorder, digital intervention, behavioural change, sleep*

Introduction

Alcohol consumption is a long-standing part of many cultures worldwide; however, patterns of harmful alcohol use pose a serious problem for public health (WHO, 2024; Balakrishnan et al., 2009). Alcohol use disorder (AUD) as indicated by a score of ≥8 on the Alcohol Use Disorders Identification Test (AUDIT) (Appendix A) shares a high rate of co-morbidity with numerous mood and anxiety disorders, with symptoms of depression being the most common (Boden & Fergusson, 2011). AUD is also associated with disruptive effects on sleep patterns and quality. Whilst alcohol may initially induce drowsiness, it disrupts the normal sleep cycle, making it harder to fall asleep, stay asleep, and transition between sleep stages (Roehrs & Roth, 2001). Excessive alcohol use has also been associated with sleep fragmentation, reduced restorative sleep, and greater daytime fatigue (Chakravorty et al., 2016).

Such sleep disturbances have also been found to impair cognitive functioning and daily performance. Sleep impairment has consistently been associated with poorer attention, reduced concentration, impaired memory consolidation, slower reaction times, and difficulties with executive functioning (Killgore, 2010; Durmer & Dinges, 2005). Chronic poor sleep has also been linked to reduced mental clarity, impaired decision-making, and decreased emotional regulation (Walker & Van Der Helm, 2009). Consequently, the combined effects of excessive alcohol use and impaired sleep may substantially hinder functioning across multiple areas of life including work and daily tasks. Frone, (2008), found that high-risk drinkers had reduced job performance, increased workplace accidents, and increased absenteeism.

Within the existing literature, there is mixed evidence regarding the impact of interventions for AUD on sleep outcomes. Whilst some studies reported improvements in sleep patterns, reduced sleep disturbances and increased sleep duration, these findings are not consistent across interventions and populations (Brower, 2003; see systematic review above). This inconsistency may reflect differences in individual's responses to treatment.

Alcohol use is unlikely to remain constant over time, with individuals often demonstrating different patterns of change throughout recovery (Staudt et al., 2018). Examining average changes in alcohol consumption may therefore obscure clinically meaningful differences between individuals. Identifying trajectories of drinking behaviour addresses this by recognising distinct subgroups of individuals who follow similar patterns of drinking behaviour over time, providing greater insight into the heterogeneity of treatment response. Many people receiving treatment for AUD experience difficulty sustaining abstinence (Witkiewitz, 2008), while reductions in alcohol consumption, even without complete abstinence, have been associated with significant improvements in how individuals with AUD feel and function (Witkiewitz et al., 2018). Consequently, understanding drinking trajectories may help inform person-centred interventions by identifying individuals who are at greater risk of relapse, or in need of additional support.

Several studies have investigated trajectories of drinking across a range of clinical and non-clinical populations. Within clinical populations, Witkiewitz and Masyn (2008) examined drinking trajectories over a 12-month period following alcohol treatment among adults with AUD and identified three distinct post-lapse drinking trajectories: (1) infrequent moderate drinking, (2) prolapse drinkers (heavier drinking with decreased frequency over time) and (3) frequent heavy drinking. Most

individuals either returned to abstinence or maintained low-level drinking, while only a small proportion continued to drink heavily and frequently. Higher alcohol dependence was associated with membership of the frequent heavy drinking trajectory, while an earlier first lapse was associated with membership of the prolapsing trajectory. Changes in coping were also associated with drinking behaviour within trajectories, with better coping over time linked to less frequent and lighter drinking following the first lapse.

A further study by, Witkiewitz et al. (2017) examined drinking trajectories in adults during treatment for AUD and had follow-up data for up to 12 months post treatment. Seven distinct drinking trajectories were identified ranging from persistent heavy drinking to abstinence. Individuals with lower alcohol dependence severity, fewer baseline drinks, fewer negative mood symptoms, and fewer heavy drinkers in their social networks had a higher probability of membership in low-risk drinking classes.

Similarly, Cranford et al. (2014) examined drinking trajectories among adults with AUD over approximately a three-year period and identified five distinct drinking trajectories ranging from stable abstinence to persistent heavy drinking. Factors associated with trajectory memberships characterised by a decline in drinking included involvement with Alcoholics Anonymous and greater purpose in life.

Collectively these findings demonstrate that diverse drinking trajectories are observed across clinical treatment populations whether alcohol use is examined following an initial lapse, during treatment, or over longer-term recovery. The variation in number and characteristics of drinking classes likely reflect the differences in populations studied, stage of recovery, and follow-up period.

Whilst the majority of research on drinking trajectories has focused on clinical populations, fewer studies have explored this in non-clinical populations. For example, Baumann et al. (2022) identified drinking trajectories over a three-year period among adults from the general population. Participants were grouped into at-risk and low-risk drinkers, and separate trajectories were explored for these two groups. Three distinct categories were identified for the at-risk drinkers (light-stable, medium-stable, and high-decreasing), and two were identified for the low-risk drinkers (very light to slightly increasing, and light to increasing) suggesting that patterns of alcohol consumption can differ according to baseline drinking severity.

Taken together, these findings suggest that heterogeneous drinking trajectories are observed across both clinical and non-clinical populations, indicating that changes in alcohol consumption are rarely uniform. However, previous research has primarily focused on patterns of alcohol consumption and predictors of trajectory membership, with little attention given to whether distinct drinking trajectories are associated with changes in broader health outcomes, such as sleep.

Kock et al. (2024), found approximately 4% of increasing and higher-risk drinkers ($\geq$8 AUDIT) receive brief interventions in primary care to support alcohol reduction, with key barriers to delivery including practitioners' lack of time to address alcohol problems and limited training in how to address them. Digital interventions could solve some of these problems as they have the capability to reduce excessive alcohol consumption (Kaner et al., 2017). They can reach large numbers of high-risk drinkers for low incremental cost of provision, offer convenience and privacy for users, and may reduce the stigma associated with help seeking in person (West & Michie, 2016).

Many alcohol reduction apps are available (Crane et al., 2015; Weaver et al., 2013), however the majority are developed with no reference to scientific evidence or theory (Crane et al., 2015). Drink Less is the first evidence and theory-based alcohol reduction app designed to help increasing and higher risk drinkers reduce their alcohol consumption (Garnett et al., 2019). The development of the app was guided by the Capability, Opportunity, Motivation–Behaviour (COM-B) model and the Theoretical Domains Framework, which suggest that behaviour change arises through interactions between an individual's capability, opportunity and motivation to change behaviour (Michie et al., 2011; Cane et al., 2012).

The app features a variety of intervention modules, each grounded in specific Behaviour Change Techniques (BCTs). It is comprised of eight modules helping users change their drinking behaviour: Goal Setting, Self-Monitoring and Feedback, Normative Feedback, Cognitive Bias Retraining, Action Planning, Behaviour Substitution, Information about Antecedents, and Insights (Garnett et al., 2018; 2021).

A previous RCT carried out by Oldham et al. (2024), sought to evaluate the Drink Less app by exploring how effective the app is for reducing alcohol consumption compared with usual digital care in the UK. For this trial, 5,602 individuals scoring ≥ 8 on the AUDIT in the UK were recruited online between 2020-2022, with half being randomly assigned to the intervention (Drink Less), and half to the comparator (NHS alcohol advice webpage). Individuals were then followed up after one, three, and six months, with the primary outcome being self-reported weekly alcohol consumption at six months. The main findings were that the Drink Less app led to a small but statistically significant reduction in alcohol consumption in increasing and high-risk drinkers, compared to the comparator.

Research by Garnett et al. (2024), also examined the effectiveness of Drink Less, and found that engagement with the app, particularly self-reported adherence, and self-monitoring behaviour, were important mediators of the effectiveness of the app. Therefore, those behaviours play a key role in supporting alcohol reduction among users of the app.

Previous research that explored treatments for alcohol use disorder and their effects on sleep have largely been based on face-to-face interventions. However, recent studies have begun to examine these effects using digital interventions. For example, Griffith et al. (2024), explored digital sleep–alcohol intervention for young adults and highlighted sleep quality as a potential motivator for reducing alcohol consumption. Similarly, Fucito et al. (2025), demonstrated that digital and wearable-based interventions may improve both sleep and alcohol-related outcomes amongst young adults at risk of harmful drinking.

Nevertheless, research exploring sleep outcomes within digital alcohol interventions remains comparatively underexplored. Given the large amount of data derived from the Drink Less app it presents an opportunity to use this to explore further questions within the research field of digital interventions for alcohol.

Data derived from app users presents a unique opportunity to investigate what happens when individuals try to reduce their drinking using a digital intervention. Users of the Drink Less app are asked to report their daily alcohol consumption along with rating their sleep quality. This provides the opportunity to identify different trajectories of alcohol use among people who are trying to reduce their alcohol consumption with the help of a digital intervention, and to explore how

different trajectories of drinking over time are associated with changes in sleep quality.

As engagement with digital interventions typically declines over time (Eysenbach, 2005; Perski et al., 2017), the present study focuses on the first 30 days of app use. This research project aims to add to the literature on digital interventions for alcohol by identifying trajectories and examining the extent to which a reduction in drinking is associated with improvements in sleep quality among highly active users of the Drink Less app during the first 30 days of app use.

Research Questions:

1) What trajectories of drinking can be identified among highly active users of the Drink Less app over a 30-day period?
2) To what extent are the identified trajectories associated with differential improvements in self-reported sleep quality?

Method

Design:

The study was a secondary analysis of data gathered from users of the Drink Less app (Garnett et al., 2019), with up to six months of data available for each user. The study used longitudinal, observational data to identify trajectories of drinking using Latent Class Analysis. Following the identification of latent classes, Linear Mixed Models were used to examine how these latent classes of drinkers (based on their patterns of change in alcohol consumption) differed in terms of how their self-reported sleep, changed over time.

Ethics:

Ethical approval was sought from University College London's Research Ethics Committee (CEHP/2020/579) for data collection; participants who voluntarily downloaded the app from the Apple app store consented to having their anonymised data used for scientific research purposes. If they would like to opt out, they could do by selecting the "opt out" option in the "Help" section of the app.

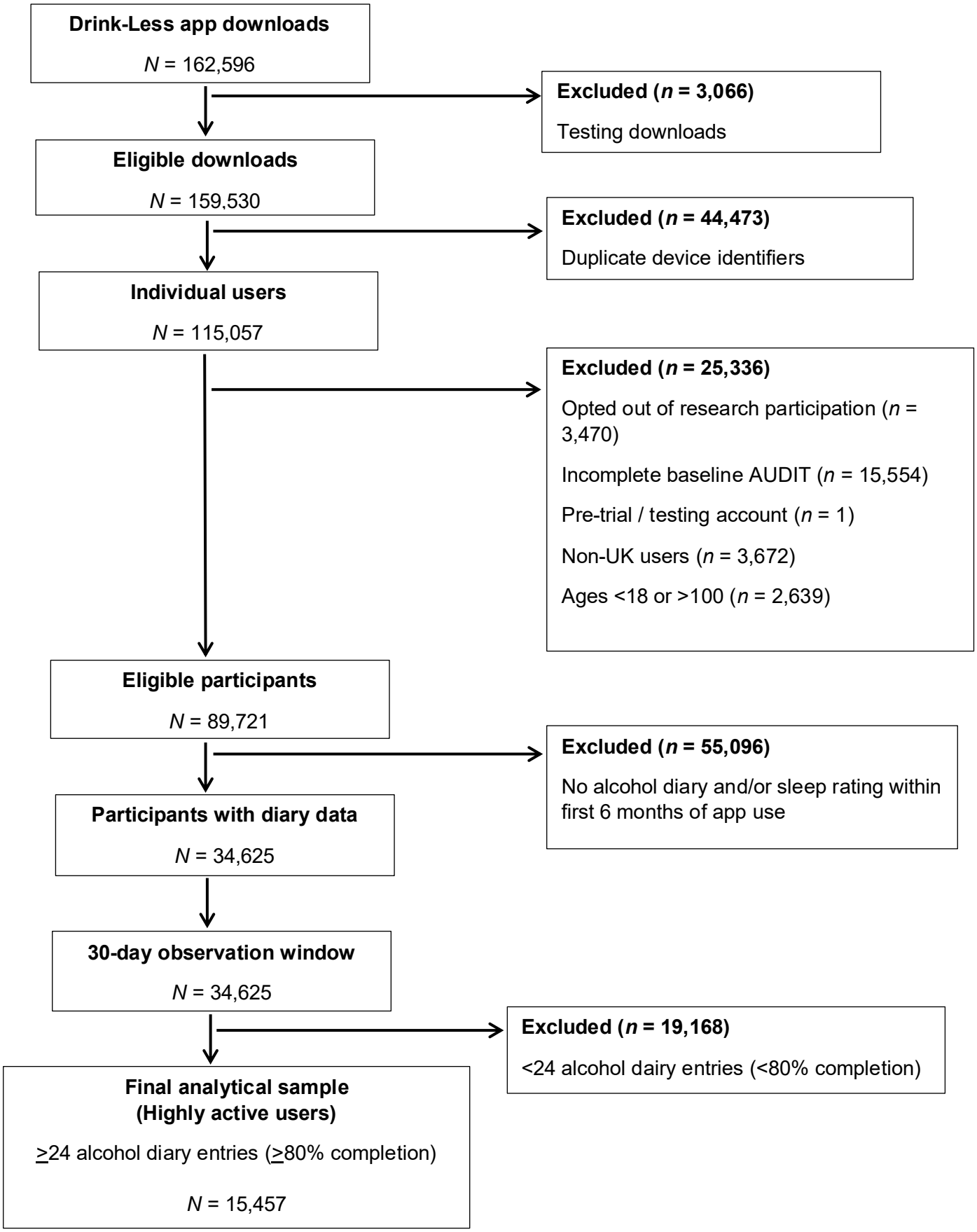
For the current study, ethical approval was sought from the University of Sheffield Research Ethics Committee for access to the anonymised data from participants who had consented to their data being used for research (30/11/23; Reference: 057957, Appendix B).

Participants:

Participants were users of the Drink Less app (Garnett et al., 2019). Data were extracted on the 9th April 2024 and included all app downloads between 18 May 2016 and 8 April 2024. Participant selection occurred in several stages to identify the final analytical sample and is summarised in Figure 1.

Figure 1

Flow diagram of participant selection



The initial dataset comprised of 162,596 app downloads. After excluding testing downloads, duplicate device identifiers, users who had not consented to research participation, users with incomplete baseline AUDIT data, users with ages <18 or >100, and users located outside the UK, 89,721 participants remained. Potentially eligible participants were required to have completed at least one alcohol diary and one sleep quality rating within the first six months of app use, resulting in a potentially eligible sample of 34,625.

Selection of the final analytical sample:

A common phenomenon of digital health interventions is declining user engagement over time (Eysenbach, 2005; Perski et al., 2017; Baumel et al., 2019; Meyerowitz-Katz et al., 2020). To identify an appropriate observation period, alcohol diary completion was examined across the six-month period for which data was available. As diary completion declined progressively over time, a 30-day observation window was selected because it retained approximately 50% diary completion (see Figure 2). Within this 30-day observation window a subgroup of highly active users was identified (see Figure 3) using the recommended ≥80% diary completion threshold (≥24 alcohol diary entries) for intensive longitudinal analyses (Jones et al., 2019; Williams et al., 2021; Wrzus & Neubauer, 2023). This resulted in a final analytical sample of 15,457 active users. See Table 1 for participant characteristics.

Figure 1

Mean number of daily alcohol diary entries completed per week, during the first six months of app use based on data from all participants with at least one valid alcohol diary during this period, N = 34,625

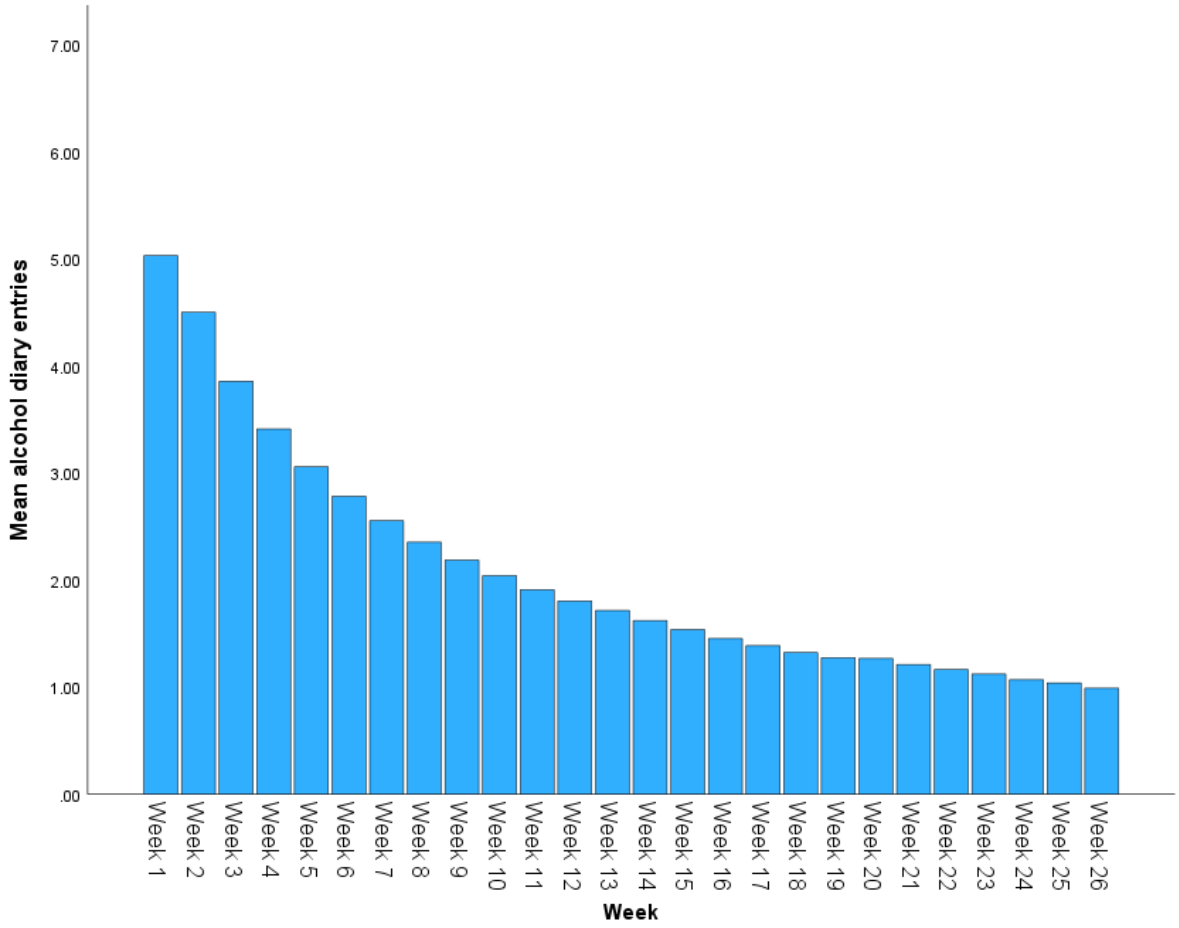


Figure 2

Distribution of the total number of alcohol diary entries completed during the first 30 days of app use based on data from all participants with at least one valid alcohol diary during the first six months, N = 34,625

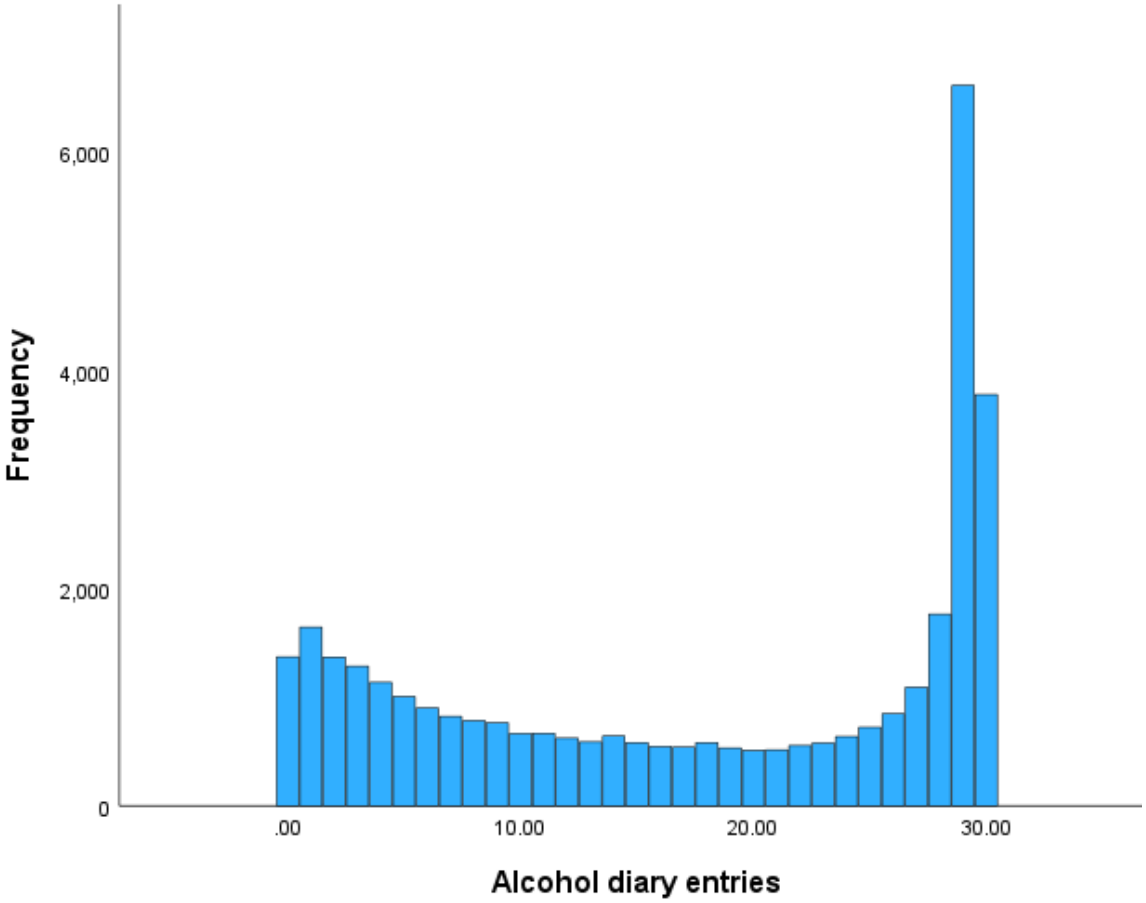


Table 1

Baseline characteristics and between-group differences among active (n = 15,457) and non-active users (n = 19,168), based on data from all participants with at least one valid alcohol diary during the first six months, N = 34,625

	Active Users	Non-active users	Test statistic	*p*	Effect size
Age, mean (SD)	47.44 (11.31)	47.02 (12.12)	$t(33{,}898.31) = -3.32$	<.001	$d = -.04$
Male, n (%)	53.8%	46.1%	$\chi^2(1) = 203.28$	<.001	$V = .08$
Baseline AUDIT, mean (SD)	15.09 (6.20)	16.69 (6.79)	$t(34{,}096.69) = 22.86$	<.001	$d = .25$
Goal to reduce drinking (%)	93.8%	94.8%	$\chi^2(1) = 15.79$	<.001	$V = .02$
Baseline alcohol consumption (units on day 1), mean (SD)	3.80 (6.92)	4.05 (6.91)	$t(29{,}215.65) = 3.05$	.002	$d = .04$
Baseline sleep quality, mean (SD)	5.56 (2.46)	5.42 (2.50)	$t(17{,}178.55) = -3.75$	<.001	$d = .06$

[1]

[1] Active users are the final included sample retained for all statistical analyses. Baseline alcohol consumption and sleep quality refer to the alcohol units and sleep quality ratings recorded on Day 1 of the 30-day observation period. *d* = Cohen's *d*; *V* = Cramér's *V*. AUDIT scores range from 0 to 40 with scores of 8 or higher indicative of “hazardous drinking. Sleep quality measure ranged from 0 (“badly”) to 10 (“like a baby”).

Active and non-active users differed significantly in age, gender, baseline audit score, goal to reduce drinking, baseline alcohol consumption and baseline sleep quality, although these differences had very small effect sizes,. Specifically, active users were slightly older than non-active users; however, the magnitude of this difference was negligible. A greater proportion of active users were male compared with non-active users, although the magnitude of this difference was small. Active users also had slightly lower baseline AUDIT scores than non-active users, with a small effect size. A slightly greater proportion of non-active users reported a goal to reduce their drinking, although the magnitude of this difference was negligible. Active users reported slightly lower baseline alcohol consumption than non-active users, although the magnitude of this difference was negligible. Finally, active users reported slightly higher baseline sleep quality than non-active users, although the effect was small. Only active users were retained for subsequent analyses.

Measures:

Alcohol Use Disorders Identification Test (AUDIT): The AUDIT (Saunders et al., 1993) is a 10-item self-report screening questionnaire developed by the World Health Organization to identify hazardous and harmful alcohol use (see Appendix A). The AUDIT assesses three domains: alcohol consumption (Items 1–3), symptoms of alcohol dependence (Items 4–6), and alcohol-related harm (Items 7–10). Each item is scored from 0 to 4, yielding a total score ranging from 0 to 40, with higher scores indicating greater alcohol-related risk. A total score of 8 or above is the recommended threshold for identifying hazardous or harmful drinking (Babor et al., 2001). The AUDIT has a median internal consistency reliability of Cronbach's α = .83, test–retest reliability coefficients ranging from .64 to .92, a median sensitivity of

.86, and a median specificity of .89 for identifying hazardous and harmful alcohol use (Reinert & Allen, 2007). The AUDIT was completed at baseline.

Alcohol Diary: Participants were prompted daily at 8am via app notifications to complete their self-monitoring diaries in which they recorded the number and type of alcoholic drinks consumed the previous day. These entries were converted into UK alcohol units to estimate daily alcohol consumption (see Appendix C). Daily diary and ecological momentary assessment (EMA) methods are commonly used within alcohol research due to their ability to reduce retrospective recall bias associated with longer-term self-report measures and have demonstrated acceptable validity for assessing day-to-day alcohol consumption (Stone & Shiffman, 2002; Monk et al., 2015). Repeated daily alcohol reporting using digital diary methodologies has also been shown to be a feasible and effective approach for monitoring alcohol consumption over time (Jones et al., 2018). Participants could also indicate if they had not consumed any alcohol on a given day, and these records were subsequently coded as zero alcohol units for that day. Daily alcohol diary data were used to identify drinking trajectories.

Sleep Diary: As part of the self-monitoring diaries, participants were also prompted to rate their sleep quality using the question "*How did you sleep?*". Responses were recorded on a 0–10 rating scale, anchored from "Badly" to "Like a baby", with higher scores indicating better subjective sleep quality (see Appendix D). The present study focused specifically on this daily sleep quality rating. Single-item measures are commonly used in EMA and digital health research because they minimise participant burden and facilitate repeated measurement in naturalistic settings (Diamantopoulos et al., 2012; Song et al., 2023). Although they are less comprehensive than multi-item questionnaires, single-item measures have

demonstrated acceptable validity for assessing subjective constructs in intensive longitudinal research (Diamantopoulos et al., 2012; Song et al., 2023). Daily sleep quality ratings were used to examine changes in subjective sleep quality.

Demographic and baseline characteristics: Upon registration, participants reported their year of birth and gender (male or female). Age was calculated by subtracting the reported year of birth from the year of app registration. Users also indicated whether their goal was to reduce their alcohol consumption (yes/no). These variables were used to describe the study sample and examine differences between active and non-active users.

Procedure:

Participants downloaded the Drink Less app voluntarily. Upon registration, participants provided informed consent for their anonymised data to be used for research purposes and completed baseline demographic questions and the AUDIT (Saunders et al., 1993). Participants also indicated whether their goal was to “reduce their drinking” or “just browsing”. They were also prompted to complete the AUDIT-C once per month. Participants received daily notifications at 8:00am prompting them to complete their self-monitoring diaries.

Although alcohol and sleep diary data were available, data relating to engagement with specific intervention modules were not available for analysis in the current study. Consequently, it was not possible to determine the extent to which participants differed in their use of the app’s intervention components.

Data screening and preparation:

Prior to analysis, data were screened and patterns of missing data were examined. Daily alcohol consumption values exceeding 40 units were capped at 40 units to reduce the influence of implausible outliers while retaining observations for analysis. Days without completed diary entries were treated as missing observations rather than assumed abstinent days. Preliminary inspection of the alcohol diary data (daily alcohol units consumed) showed that the data was positively skewed, therefore, a square root transformation was applied prior to analysis to improve the distribution of the data (see Appendix E).

Missing Data: Given the longitudinal nature of the Drink Less dataset, missing data were expected due to declining engagement with the app over time. Although analyses were restricted to highly active users for alcohol diaries (≥24 alcohol diary entries), missing observations remained for both alcohol and sleep diary data within the 30-day observation period. Little's Missing Completely at Random (MCAR) test (Little, 1988) and logistic regression analyses were conducted to examine the nature and predictors of missingness. The results of these analyses informed the working assumption regarding the missing-data mechanism.

Alcohol diaries: To examine the nature of missing data, a two-step approach was employed using Little's MCAR test (Little, 1988) and logistic regression analyses. Due to the large sample size, it was not computationally feasible to apply Little's MCAR test to every alcohol diary across the entire 30-day period. Instead, the Little's MCAR test was conducted using observations from five key milestone time points across the 30-day observation period (days 1, 8, 15, 22, and 30). These five time points were entered simultaneously into the MCAR analysis to determine

whether the hypothesis of MCAR could be rejected. Logistic regression analyses were then conducted to examine whether age, gender, baseline AUDIT score, and drinking goal predicted the number of missing alcohol diary entries across the 30-day observation period.

Sleep diaries: The same two-step approach was applied to examine the nature of missing sleep diary data. Little's MCAR (Little, 1988) tests were conducted using observations from the same five key milestone time points across the 30-day observation period (days 1, 8, 15, 22, and 30). These five time points were entered simultaneously into the MCAR analysis to determine whether the hypothesis of MCAR could be rejected. Logistic regression analyses were then conducted to examine whether age, gender, baseline AUDIT score, and drinking goal predicted the number of missing sleep diary entries across the 30-day observation period.

Linear Mixed Models were estimated using the Restricted Maximum Likelihood (REML) method, allowing all available sleep observations to contribute to the analysis without requiring complete outcome data. No minimum completion threshold was applied to the sleep diary data as REML allows participants with incomplete longitudinal observations to contribute all available data. Applying an additional completion threshold would have excluded participants with incomplete data, thereby reducing the sample size, and statistical power, while further increasing selection bias.

Data Analysis:

First, a latent class analysis (LCA) was conducted to identify distinct patterns of alcohol use over time, followed by linear mixed models (LMMs) to examine how

these classes were characterised by differential changes in self-reported sleep over 30-days. Each method is described in more detail below.

Latent class analysis: Drinking trajectories were modelled using a growth mixture model (GMM). GMM is a type of latent trajectory analysis which offers additional flexibility over other methods (e.g. growth-based trajectory models) by incorporating random effects within drinking trajectories to account for between-individual variation within subgroups (Nagin & Odgers, 2010). GMMs were fit using 100 random parameter starting points to reduce the likelihood of parameter estimates reaching a local maximum likelihood (Proust-Lima et al., 2017). Models were estimated using the Restricted Maximum Likelihood (REML) method which provides less biased estimates of variance components (Harville, 1977).

All models included random intercept and random slope parameters to account for between-person variation in both initial consumption and changes in consumption over time. The distribution of random effects was allowed to vary by drinking trajectory, as trajectory groups were expected to differ in their homogeneity. Baseline age, AUDIT score, gender, and motivation to reduce drinking were included as covariates within the models to improve the precision of trajectory estimation and support the handling of incomplete longitudinal data (Proust-Lima et al., 2017). Participants were assigned to a drinking trajectory based on their posterior probability for group membership, ensuring an empirically driven classification approach.

Models were tested for each of linear, quadratic and cubic trajectory shapes, and were fit iteratively, beginning with a single latent class. Additional latent classes were introduced where this improved model fit according to Akaike Information

Criteria (AIC) and Bayesian Information Criteria (BIC). Where model fit criteria were not in agreement, the Adjusted Lo-Mendell-Rubin likelihood ratio test (Lo et al., 2001) was used to decide between models. To ensure that identified drinking trajectories represented meaningful subgroups, models that contained a drinking trajectory comprising less than 5% of the total sample were not selected, regardless of model fit statistics (Nagin, 2005).

All latent class analyses were performed using R version 4.3.3. The lcmm package was used to fit the GMM (Proust-Lima et al., 2017).

Linear mixed models: Following the identification of drinking trajectories, linear mixed models were used to assess how the identified drinking classes were characterised by differential changes in self-reported sleep over time.

LMMs are particularly suited for longitudinal data with repeated measures, as they allow for the inclusion of all available data points without requiring complete data for every participant (Verbeke & Molenberghs, 2000). These models also account for the repeated measures structure of the data, with observations nested within individuals, by including random intercepts to capture individual variability.

Fixed effects included time, drinking class, and their interaction to test whether changes in self-reported sleep quality differed over time by drinking class, while adjusting for baseline age, AUDIT score, gender, and motivation to reduce drinking.

LMM analyses were conducted using SPSS version 29.0.2.0.

Post-hoc comparisons: Where significant differences between drinking trajectory groups were identified, Bonferroni-adjusted pairwise comparisons were conducted to examine differences in sleep quality at baseline. The “light drinking

throughout" trajectory was used as the reference group, and comparisons were made between this group and each of the other drinking trajectory groups.

Results

Data were analysed for the 30 day highly active users (*N* = 15,457) defined as participants who had at least 80% valid alcohol diary entries during the observation period. Differences in baseline characteristics between active and non-active users are presented in Table 1 above.

During the 30-day observation period, active users completed a mean of 28.43 alcohol diaries (SD = 1.61) and 10.48 sleep quality diaries (SD = 9.91) out of a possible 30 observations.

Missing Data

Alcohol diary entries: Little's MCAR test indicated that alcohol diary data were not missing completely at random, X^2 (229) = 429.29 $p < .01$. Regression analysis with all variables measured at baseline (age, gender, AUDIT score and the goal to reduce drinking) as independent variables and the number of valid alcohol diary entries over the 30-day observation period as the dependent variable was statistically significant (R^2) = 0.002, F(4, 15064) = 6.39, p < .01. The number of valid alcohol diaries was predicted by baseline AUDIT (standardised B = -.03, SE = .01, t(15060) = 4.01, p < .001; participants with higher audit scores had more missing data), participant age (B = -.02, SE = .00, t(15060) = 2.69, p < .01; older participants had more missing data) and gender (B = -.00, SE = .03, t(15060) = 2.69, p < .01; male participants had more missing data). The goal to reduce drinking was not a significant predictor (B = -.02, SE = .06, t(15060) =.40, p = .69).

Sleep diary entries: Similarly, Little's MCAR test indicated that the missing sleep diary data were not missing completely at random, X^2 (346) = 923.98 $p < .01$. Regression analysis with all baseline variables as independent variables and the number of valid sleep diary entries over the 30-day observation period as the dependent variable was statistically significant (R^2) = 0.02, F(4, 15064) = 67.12, p < .01. The number of valid sleep diaries was predicted by baseline AUDIT (standardised B = .10, SE = .04, t(15060) = 11.57, p < .01; participants with lower audit scores had more missing data), participant age (B = .05, SE = .01, t(15064) = 2.69, p < .01; younger participants had more missing data) and goal to reduce drinking (B = -.06, SE = .35, t(15064) = 7.25, p < .01; participants who were not trying to reduce their drinking had more missing data). Gender was not a significant predictor (B = .00, SE = .17, t(15064) = -.11, p = .92).

Under Rubin's missing data framework, the observed patterns of missingness combined with the fact that the analytic sample were missing no more than 20% of alcohol diaries supports the decision to assume a Missing at Random (MAR) mechanism for the primary analyses (Rubin, 1976; Little & Rubin, 2019). A missing Not at Random (MNAR) pattern can never be completely ruled out. However, all four baseline characteristics were included as covariates in all analyses, which permitted the models to condition on participant characteristics that were associated with missingness.

Latent Class Analysis

A linear model with four drinking trajectories was selected as the best fitting model based on the AIC and BIC values. Whilst the five-class linear model had better model fit, according to AIC and BIC, one class contained fewer than 5% of the

sample, therefore the four-class model was deemed most appropriate (see Table 2). The four classes (figure 3) characterised by their slope and intercept are as follows: Light drinking throughout (37.7%), moderate drinking, decreasing (7.8%), heavy drinking, decreasing (20.3%), and heaviest drinking, decreasing (34.1%). The light drinking throughout trajectory was designated the reference class as it was the largest class.

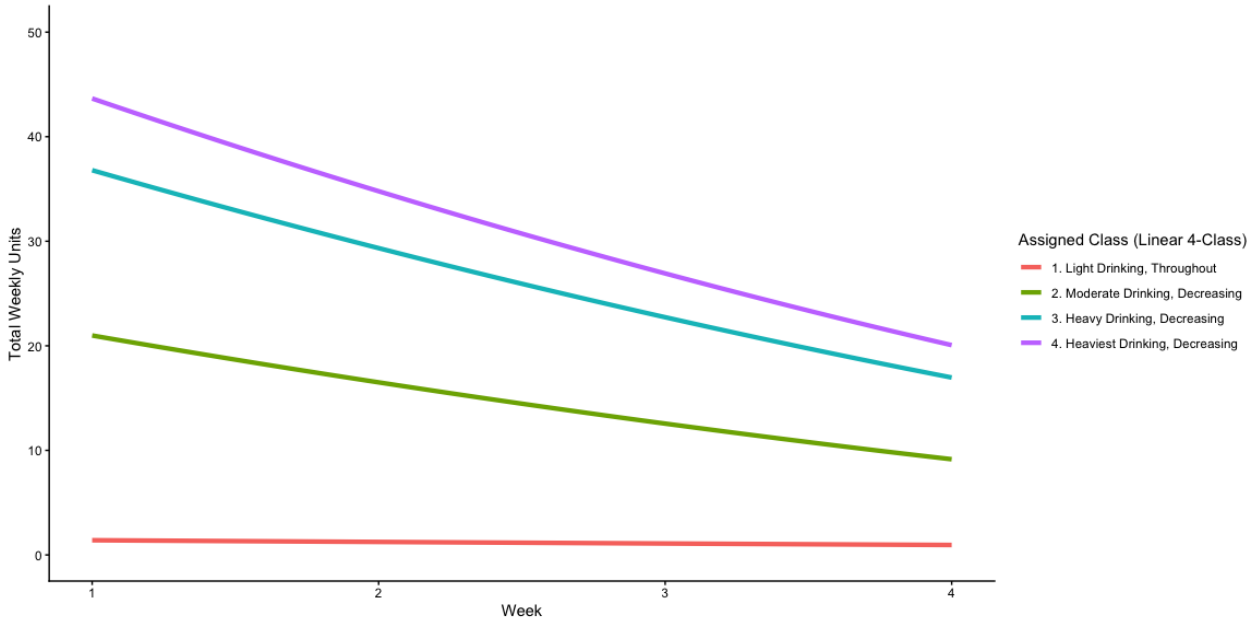
Table 2

Model fit criteria for growth mixture models performed on the active users (N = 15,475), for 30 days of app usage

Number of classes	Maximum Likelihood	AIC	BIC	Entropy	Class 1 size (%)	Class 2 size (%)	Class 3 size (%)	Class 4 size (%)	Class 5 size (%)
Linear shape									
1	-172,603	345,218	345,264	1	100	-	-	-	-
2	-171,833	343,689	343,773	0.63	55.9	44.1	-	-	-
3	-171,495	343,023	343,145	0.58	14.7	48.6	36.7	-	-
4	-171,227	342,496	342,656	0.57	37.7	7.8	20.3	34.1	-
5	-171,013	342,079	342,278	0.59	32.1	25.4	18.2	20.9	3.4
Quadratic shape									
1	-166,076	332,167	332,220	1	100	-	-	-	-
2				Did not converge					
3				Did not converge					
4				Did not converge					
5	-162,506	325,075	325,312	0.65	18.2	42.1	25.5	12.3	1.9
Cubic shape									
1	-164,243	328,503	328,564	1	100	-	-	-	-
2				Did not converge					
3				Did not converge					
4				Did not converge					
5	-160,141	320,355	320,630	0.67	25.4	18.2	32.1	21.9	2.4

Figure 3

Four class drinking trajectories for one month of app usage amongst the highly active users



Linear Mixed Models:

These models examined the effects of drinking class, time, and their interaction on self-reported sleep quality while adjusting for baseline age, AUDIT score, gender, and motivation to reduce drinking. Random intercepts and random slopes for time were included to account for individual variability. The model was estimated using the REML method which provides less biased estimates of variance components and, under the working MAR assumption allows participants with incomplete longitudinal observations to contribute all available data.

Sleep

Model fit statistics for the linear mixed model are presented in Table 3. The overall explanatory power of the model was small (R^2 = .014), indicating that the model accounted for approximately 1.4% of the variance in self-reported sleep quality. However, despite the small effect size, the model identified significant

differences in baseline sleep quality (day 1) across the drinking classes and rates of sleep improvement across the drinking classes.

Table 3

Model fit statistics for the linear mixed model predicting sleep quality for 30 days of app usage in the active users

-2 log likelihood	AIC	BIC	CAIC	ICC	Random intercept variance	Residual variance	R^2
689529.17	689535.17	689565.09	689568.09	.365	1.796	3.868	.014

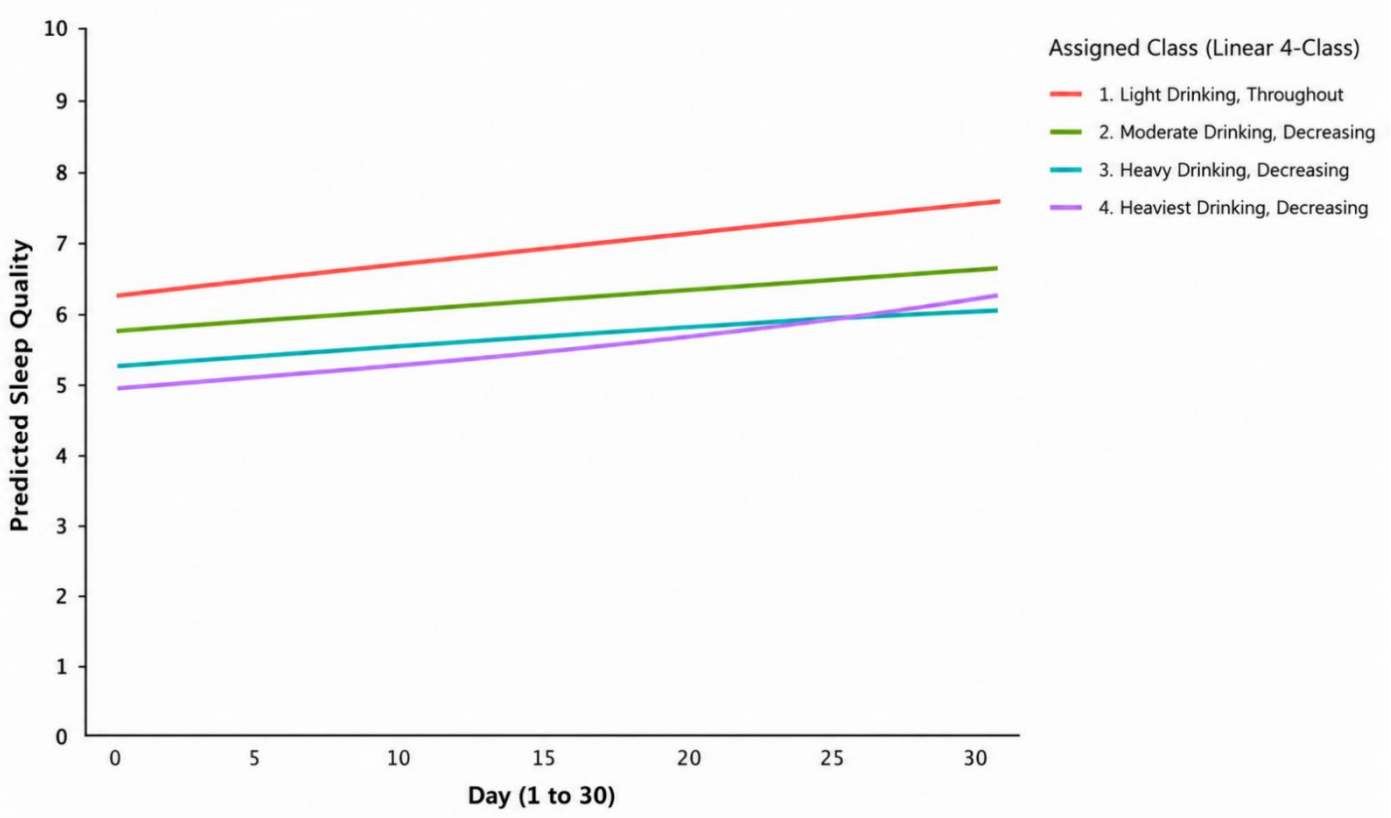
The linear mixed model revealed a significant main effect of time, $F(1, 158459) = 539.931$, $p < .001$, indicating that self-reported sleep quality changed significantly across the first 30 days of app use within the active users. A significant main effect of class was also found $F(3, 158459) = 7.299$, $p < .001$, indicating that sleep quality differed across the four drinking trajectory groups. The interaction between time and class was also statistically significant, $F(3, 158459) = 20.855$, $p < .001$ indicating that sleep quality change over time differed significantly between the four drinking classes. To aid interpretation of the significant interaction, fixed-effect slope estimates were examined to assess how the rate of improvement in sleep quality over the 30-day period differed between the four groups (see Figure 4). Although all classes showed significant improvements in sleep quality over time, the "light drinking throughout" group demonstrated the steepest improvement in sleep quality across the 30-day period ($\beta = 0.032$, $p < .001$). In comparison, the "heavy drinking, decreasing" ($\beta = -0.020$, $p < .001$), "moderate drinking, decreasing" ($\beta = -0.015$, $p < .001$), and "heaviest drinking, decreasing" ($\beta = -0.008$, $p = .018$) groups all

demonstrated significantly shallower rates of improvement relative to the “light drinking throughout” group.

Bonferroni-adjusted pairwise comparisons examining baseline differences between groups indicated that the “light drinking throughout” group reported significantly better sleep quality at day 1 compared to the “moderate drinking, decreasing” group (mean difference = 0.162, 95% CI [0.001, 0.324], p = .049), the “heavy drinking, decreasing” group (mean difference = 0.279, 95% CI [0.113, 0.446], p < .001), and the “heaviest drinking, decreasing” group (mean difference = 0.351, 95% CI [0.143, 0.558], p < .001).

Figure 4

Predicted trajectories of sleep quality among active users across the first 30 days of app use separated by drinking class



Discussion

The present study examined trajectories of alcohol consumption amongst highly active users of the Drink Less app and explored whether reductions in drinking were associated with self-reported sleep quality during the first 30 days of app use. The findings provide valuable insight into the relationship between alcohol reduction and sleep quality in the context of digital interventions.

Summary of main findings

Results revealed four distinct drinking trajectories amongst highly active users of the Drink Less app over a 30-day period: “light drinking throughout”, “moderate drinking decreasing”, “heavy drinking decreasing”, and “heaviest drinking, decreasing”. Across all groups, sleep quality significantly improved over time, however, the rate of improvement differed significantly between groups. The “light drinking throughout” group demonstrated the steepest improvement in sleep quality over the 30-day period, whereas the moderate and heavy drinking groups showed slower rates of sleep improvement relative to this group. Additionally, the “light drinking throughout” group reported significantly better sleep quality at baseline (day 1) compared to the other drinking trajectories.

In line with previous findings, the identification of four distinct trajectories demonstrates heterogeneity in drinking behaviour during attempts to reduce alcohol consumption (Witkiewitz et al., 2017; Gueorguieva et al., 2010). Rather than assuming that individuals attempting to reduce their alcohol consumption follow a uniform pattern, trajectory modelling captures the complexity of real-world behaviour change and allows for a more nuanced understanding of how patterns of alcohol consumption vary between individuals. The present findings therefore provide further

evidence that alcohol reduction behaviour is heterogeneous, although the extent to which these patterns reflect broader patterns of alcohol reduction should be interpreted cautiously given the highly selected sample.

App users in the “light drinking throughout” trajectory reported better sleep quality at baseline (day 1 of app use) compared to the other drinking groups. This finding aligns with previous literature which demonstrated the associations between heavier alcohol consumption and poor sleep quality (Brower, 2003; Chakravorty et al., 2016). Alcohol is known to disrupt normal sleep architecture, increase sleep fragmentation, and reduce restorative sleep despite initially promoting feelings of sedation (Roehrs & Roth, 2001). The relatively poor sleep at baseline observed among heavier drinkers may reflect the cumulative effects of heavier alcohol consumption on sleep prior to app use.

The sleep trajectories provide a more nuanced picture of the relationship between drinking patterns and sleep quality. Although all drinking groups showed significant improvements in sleep over time, the rate of improvement was highest in the “light drinking throughout” group relative to the other three groups. This is particularly noteworthy given that this group maintained relatively low levels of alcohol consumption throughout the observation period. One possible explanation for this finding is that regular self-monitoring of alcohol consumption and daily experiences through the Drink Less app may have increased users’ awareness of their drinking and sleep patterns and potentially encouraged broader changes in health-related behaviours or routines that influence sleep. Additionally, the Drink Less app includes self-monitoring and feedback as a core intervention component which has been identified as a potential mechanism through which the app influences behaviour change (Garnett et al., 2024). This may be particularly relevant

to the “light drinking throughout” group, who demonstrated significant improvements in sleep despite maintaining relatively low levels of alcohol consumption. However, this interpretation should be treated cautiously, as the present study did not assess engagement with specific app components, and the observational design of the study prevents conclusions about what contributed to these changes in sleep.

The trajectory of the “heaviest drinking, decreasing” group is also noteworthy as, despite starting from a lower baseline of sleep quality, they demonstrated a rate of improvement similar to that seen in the “light drinking throughout” group. Furthermore, visual inspection of Figure 4 suggests that sleep quality in the “heaviest drinking, decreasing” group at the end of the 30-day observation window was comparable to that of the “light drinking throughout” group at baseline. However, the statistically significant interaction and post-hoc tests established that the rate of improvement in the “heaviest drinking, decreasing” group was significantly lower than the rate of improvement in the “light drinking throughout” group.

The magnitude of these differences should be considered in the context of the explanatory power of the overall model. Although results found drinking trajectory to be significantly associated with change in sleep quality over time, likely reflecting the large sample size and therefore substantial statistical power to detect relatively small differences, the overall model only explained 1.4% of the variance in sleep quality. Thus, 98.6% of the variance in sleep quality over time was not explained by drinking trajectory, indicating that factors other than patterns of alcohol consumption were likely to account for differences in the sleep improvement. It is possible that sleep improvements may reflect characteristics of the group that were not captured by the trajectories themselves. For example, individuals in the “light drinking throughout” group may have been better able to maintain broader lifestyle factors associated with

good sleep quality such as more stable routines, lower levels of alcohol-related physiological disruption, or fewer co-occurring psychological difficulties that could negatively impact sleep. In contrast, individuals within the heavier drinking trajectories may have differed from the light drinking group and experienced residual withdrawal symptoms, stress, mood difficulties, and continued disrupted sleep patterns despite reductions in alcohol consumption. However, these explanations remain speculative because such factors were not directly assessed in the present study.

Taken together, the findings do not provide straightforward evidence of a dose-response relationship between reductions in alcohol consumption and improvements in sleep quality. Rather, sleep quality improved across all four drinking trajectories, with the “light drinking throughout” group demonstrating the steepest improvement, while the three heavier drinking groups showed relatively similar rates of change. The finding that the “heaviest drinking, decreasing” group demonstrated substantial improvement despite starting with poorer sleep further highlights the complexity of this relationship. These patterns suggest that changes in sleep during the first 30 days of app use may reflect a combination of alcohol-related and non-alcohol-related factors, rather than changes in alcohol consumption alone. However, given the small magnitude of the effects, large sample size, highly selected sample, and observational nature of the data, these findings should be interpreted cautiously and should not be taken as evidence of a causal relationship between alcohol reduction and improvements in sleep.

Strengths and Limitations

A key strength of the study was the use of a large, real-world dataset derived from highly active users of the Drink Less app. The large sample size provided strong statistical power and ecological validity, enabling the identification of naturally occurring patterns in drinking behaviour and sleep quality over time. The use of latent class analysis allowed for the identification of distinct drinking trajectories that may not have been detectable with a smaller sample. Subsequently, this provided a more nuanced understanding of how active users may differ in their drinking patterns during the first 30 days of app use.

Another strength was the careful assessment and handling of missing data. The use of Restricted Maximum Likelihood estimation allowed all available observations to contribute to model estimation under the working MAR assumption, rather than excluding users with incomplete data. This enabled a more accurate estimation of within-person change over time.

Despite these strengths, several limitations must also be acknowledged. The study relied entirely on self-reported measures for both alcohol consumption and sleep quality. Self-reported measures are susceptible to recall bias and social desirability effects that may have led to underreporting of alcohol consumption or misreporting of sleep quality. Also, sleep quality was only assessed using a single-item self-report rating scale rather than a validated sleep measure or objective sleep measures. Although this is common within digital interventions in order to reduce participant burden and attrition (Matthews et al., 2022; Stone & Shiffman, 2002), they do not comprehensively capture the complexity of sleep processes such as sleep duration, sleep latency, or sleep fragmentation. Nevertheless, research suggests that

single-item sleep quality measures can still demonstrate good validity, including strong correlations with established multidimensional sleep measures such as the Pittsburgh Sleep Quality Index (PSQI), and therefore can still provide a useful indication of subjective sleep quality within large-scale digital intervention studies (Snyder et al., 2018).

Additionally, the overall effect size of the linear mixed model was very small, with differences between alcohol trajectory groups accounting for only 1.4% of the variation in sleep quality over the 30-day observation window. Consequently, whilst the associations between drinking trajectories and sleep quality were statistically significant, the magnitude and clinical significance of these effects should be interpreted cautiously. The relatively small effect size also suggests that external factors not measured within the current study are likely to have contributed substantially to sleep quality during the observation period.

The study also focused exclusively on highly active users who completed alcohol diaries on at least 80% of days during the first month of app use. Whilst this strengthened the reliability of analyses, it resulted in a highly selective analytical sample. Active users differed from non-active users in age, gender, baseline AUDIT score, drinking goal, baseline alcohol consumption and baseline sleep quality, although the effect sizes for these differences were generally small. These differences suggest that the highly active users may have systematically differed from the less engaged users potentially resulting in a sample that is not completely representative of the wider population of Drink Less users. Furthermore, the available data only provided limited information regarding characteristics of the sample. Consequently, the extent to which the groups differed in other important psychological, behavioural, socioeconomic, or lifestyle characteristics cannot be

established, further limiting the generalisability of the findings beyond the highly selected analytical sample.

A further limitation concerns the mechanism of missing data which cannot be determined definitively from the observed data. The association between baseline AUDIT score, age, and gender, and the missingness of alcohol and sleep diaries is consistent with a missing at random (MAR) pattern (Rubin, 1976). These variables, which were available for all participants, were included as covariates in all analyses. Doing so supported the handling of missing values and thereby improved the precision of trajectory estimation (Proust-Lima et al., 2017). However, a missing not at random (MNAR) can never be completely ruled out. In this case, it is plausible that missing alcohol diary data could be related to (unobserved) alcohol consumption on those days. Similarly, the observed predictors of sleep diary missingness (baseline AUDIT, age, and goal to reduce drinking) do not exclude the possibility of dependence on unobserved sleep quality. As the unobserved alcohol consumption and sleep quality values cannot be assessed, MAR and MNAR cannot be empirically distinguished using the observed data alone. Thus, under Rubin's classical taxonomy (Rubin, 1976; Little & Rubin, 2019) MAR was adopted as the working assumption based on the available evidence and observed predictors of missingness, while recognising that MNAR could not be ruled out (Rubin, 1976; Little & Rubin, 2019). If the data were instead MNAR, with missingness related to unobserved alcohol consumption or sleep quality, this would have introduced bias into the estimated drinking trajectories and sleep outcomes. For example, if individuals with higher alcohol consumption or poorer sleep were more likely to have missing diary data, their levels of alcohol consumption or sleep difficulties may have been underestimated, potentially affecting trajectory classification and the observed

associations between drinking trajectory and sleep improvement. Consequently, the findings should be interpreted with caution.

Another limitation was the absence of data relating to engagement with specific intervention modules within the Drink Less app. Although alcohol diary and sleep diary data were available, it was not possible to determine which intervention components participants accessed or how frequently they engaged with them. Subsequently, these findings cannot determine whether particular behaviour change techniques or levels of engagement with specific app features were associated with greater improvements in alcohol consumption or sleep quality.

The observational nature of the study design limits causal inference as improvements in sleep quality cannot be attributed solely to alcohol reduction or app engagement. It remains unclear whether reductions in alcohol use directly contributed to improvements in sleep quality, whether improvements in sleep supported reductions in alcohol use, or whether both outcomes were influenced by external factors. Therefore, the observed associations should not be interpreted as evidence that reductions in alcohol consumption caused improvements in sleep quality.

It should also be noted that data were collected over a large time period which increases the influence of unmeasured contextual factors on data collection. For example, data collection overlapped with the COVID-19 pandemic which may have affected both alcohol consumption and sleep quality, limiting the extent to which observed changes in sleep quality can be attributed solely to alcohol reduction.

Finally, the relatively short 30-day period limits the ability to observe longer-term changes in both alcohol consumption and sleep quality. Previous literature

suggests that sleep recovery following reductions in alcohol use may continue over extended periods of abstinence or reduced drinking (Brower, 2001). Therefore, the current findings may primarily reflect early-stage changes in sleep quality during alcohol reduction rather than longer-term recovery processes. However, it is important to note that extending analyses over longer time periods would likely have resulted in greater missingness and a considerably smaller sample of active users, reducing statistical power and further limiting the representativeness of the sample in relation to the wider population of app users.

Clinical Implications

The findings of the current study have several important implications. The findings suggest that improvements in sleep quality may occur during the early stages of engagement with a digital alcohol-reduction intervention. However, the relatively small differences in rates of improvement between groups suggest that improvements in sleep may not be explained by changes in alcohol consumption alone. This highlights the importance of routinely assessing and addressing sleep difficulties during alcohol recovery. Sleep disturbances may therefore require direct intervention rather than assuming that sleep will automatically improve alongside reductions in drinking.

Additionally, digital alcohol interventions such as Drink Less may provide a valuable platform for integrating sleep-focused interventions or sleep psychoeducation alongside alcohol reduction strategies. Incorporating evidence-based sleep interventions such as CBT-I principles into digital alcohol interventions could potentially enhance broader wellbeing outcomes during recovery particularly, where sleep problems remain despite changes in alcohol consumption.

Future Directions

Future research should examine engagement with specific intervention modules within the app. Understanding which behaviour change techniques or app features are most strongly associated with improvements in alcohol consumption and sleep quality may help optimise the effectiveness of digital interventions and inform the development of more person-centred care.

Furthermore, based on findings from the current study and the systematic review, future research should explore whether integrating targeted sleep-focused interventions, such as CBT-I principles or sleep hygiene strategies into digital alcohol interventions leads to greater improvements in both sleep quality and alcohol-related outcomes. This could help optimise digital interventions and maximise their impact by tailoring approaches to meet individual needs.

Conclusion

The present study explored how reductions in alcohol consumption relate to changes in sleep quality among highly active users of the Drink Less app over the first 30-days of app use. Four distinct drinking trajectories were found and all groups showed significant improvements in sleep quality over time. The “light drinking throughout” group demonstrated the steepest improvement, but the rate of improvement was smaller in the moderate and heavy drinking groups. However, group differences explained only 1.4% of the variation in sleep quality over time. This suggests that improvements in sleep quality during early engagement with a digital alcohol-reduction intervention are unlikely to be attributable to changes in alcohol consumption alone. Overall, the findings highlight the importance of considering and assessing sleep quality alongside alcohol consumption within digital alcohol interventions.

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Appendices

Appendix A: Alcohol Use Disorders Identification Test (AUDIT)

Alcohol use disorders identification test (AUDIT)

AUDIT is a comprehensive 10 question alcohol harm screening tool. It was developed by the World Health Organisation (WHO) and modified for use in the UK and has been used in a variety of health and social care settings.

Questions	Scoring system					Your score
	0	1	2	3	4	
How often do you have a drink containing alcohol?	Never	Monthly or less	2 to 4 times per month	2 to 3 times per week	4 times or more per week	
How many units of alcohol do you drink on a typical day when you are drinking?	0 to 2	3 to 4	5 to 6	7 to 9	10 or more	
How often have you had 6 or more units if female, or 8 or more if male, on a single occasion in the last year?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily	
How often during the last year have you found that you were not able to stop drinking once you had started?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily	
How often during the last year have you failed to do what was normally expected from you because of your drinking?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily	
How often during the last year have you needed an alcoholic drink in the morning to get yourself going after a heavy drinking session?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily	
How often during the last year have you had a feeling of guilt or remorse after drinking?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily	
How often during the last year have you been unable to remember what happened the night before because you had been drinking?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily	
Have you or somebody else been injured as a result of your drinking?	No		Yes, but not in the last year		Yes, during the last year	
Has a relative or friend, doctor or other health worker been concerned about your drinking or suggested that you cut down?	No		Yes, but not in the last year		Yes, during the last year	

Total AUDIT score	

Scoring:

- 0 to 7 indicates low risk
- 8 to 15 indicates increasing risk
- 16 to 19 indicates higher risk,
- 20 or more indicates possible dependence

Giving feedback and advice

If the score is lower

If the score is 8 or above, give brief advice to reduce risk for alcohol harm. If the score is 20 or above, consider referral to specialist alcohol harm assessment.

Alcohol unit reference

Appendix B: Ethical approval from University of Sheffield

The University Of Sheffield.

Downloaded: 18/12/2023
Approved: 30/11/2023

Registration number:
Psychology
Programme: DclinPsy

Dear

PROJECT TITLE: To what extent is a reduction in drinking associated with improvements in functioning and health and is this association moderated by gender, age, and alcohol consumption at baseline
APPLICATION: Reference Number 057957

This letter confirms that you have signed a University Research Ethics Committee-approved self-declaration to confirm that your research will involve only existing research, clinical or other data that has been robustly anonymised. You have judged it to be unlikely that this project would cause offence to those who originally provided the data, should they become aware of it.

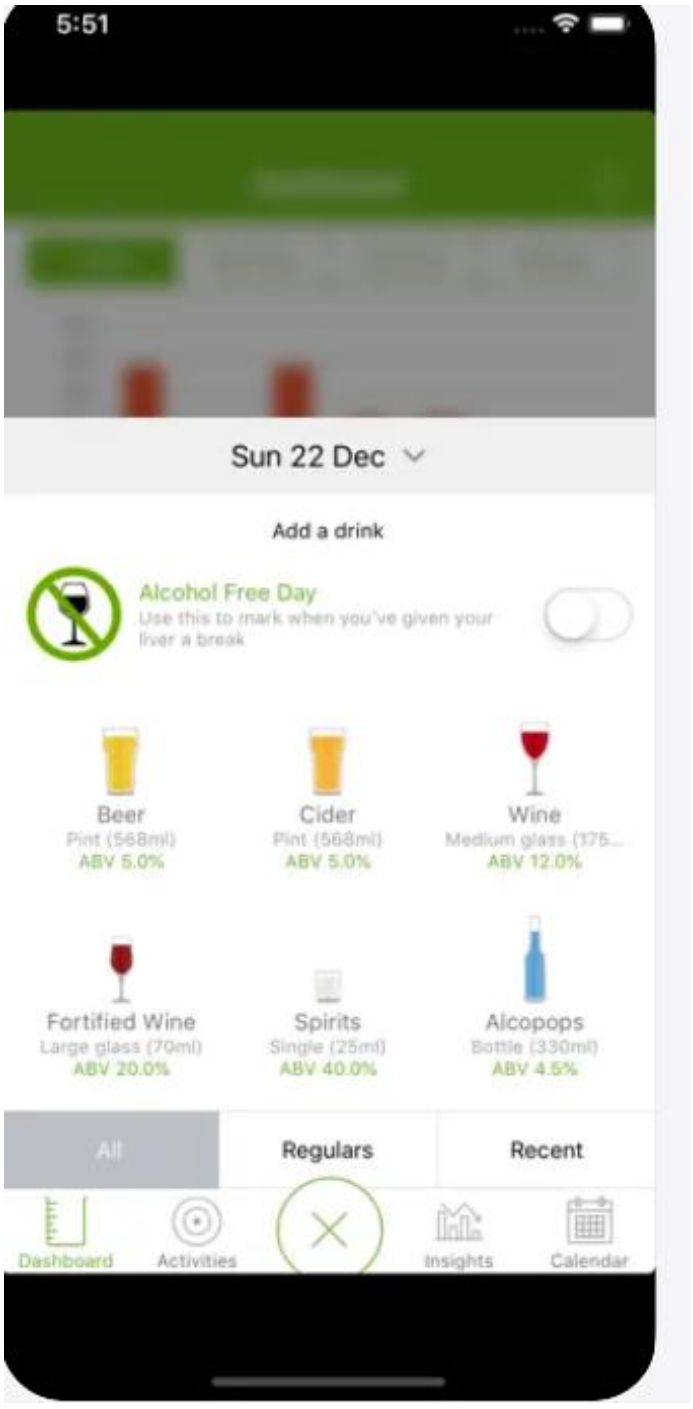
As such, on behalf of the University Research Ethics Committee, I can confirm that your project can go ahead on the basis of this self-declaration.

If during the course of the project you need to deviate significantly from the above-approved documentation please inform me since full ethical review may be required.

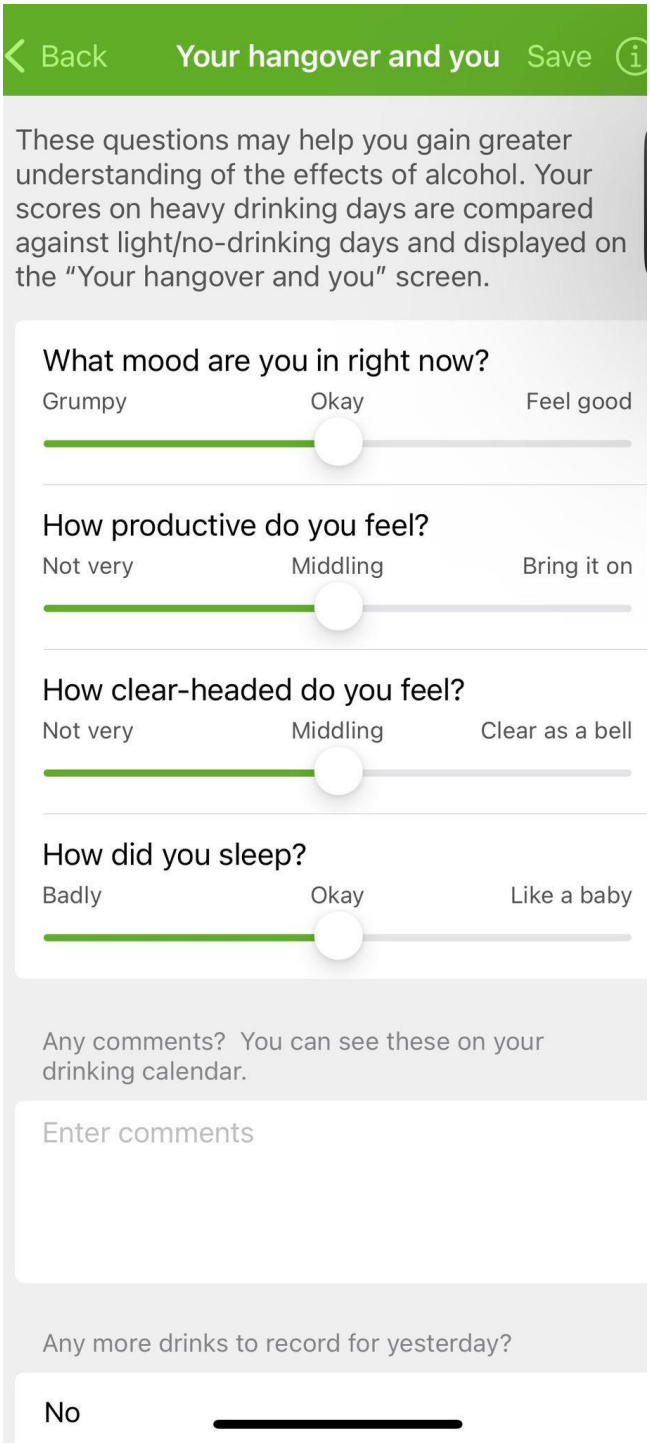
Yours sincerely

Department Of Psychology Research Ethics Committee
Departmental Ethics Administrator

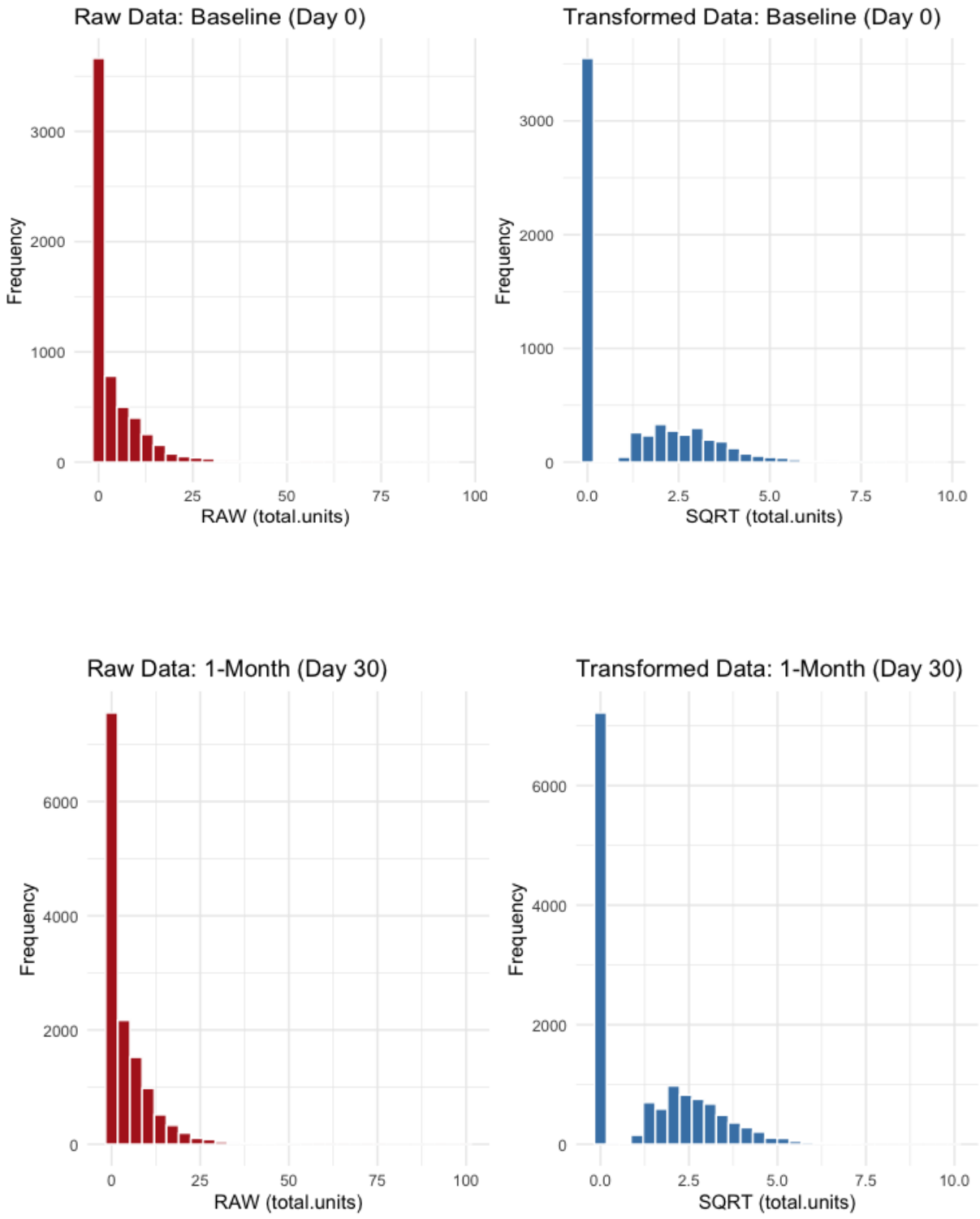
Appendix C: Screenshot demonstrating how users recorded alcohol consumption within the Drink Less app



Appendix D: Screenshot demonstrating how users recorded sleep within the Drink Less app



Appendix E: Alcohol consumption data transformations



Preliminary inspection of the daily alcohol consumption variables demonstrated substantial positive skew, with a small number of extremely high alcohol unit recordings producing a long right tail. Initial latent class mixed models using the raw alcohol consumption data repeatedly failed to converge and produced non-positive definite matrix errors. To improve distributional properties and variance stability, a square root transformation was applied to the longitudinal alcohol consumption variables prior to the latent class analysis to reduce skew and improve model stability.