IARC HANDBOOKS

ALCOHOL POLICIES

VOLUME 20B

IARC HANDBOOKS OF CANCER PREVENTION

International Agency for Research on Cancer
World Health Organization

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This publication represents the views and expert opinions of an IARC Working Group on the Evaluation of Cancer-Preventive Interventions, which met in Lyon, France, 7–11 October 2024

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IARC HANDBOOKS OF CANCER PREVENTION

International Agency for Research on Cancer

World Health Organization

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International Agency for Research on Cancer

The International Agency for Research on Cancer (IARC) was established in 1965 by the World Health Assembly, as an independently funded organization within the framework of the World Health Organization. The headquarters of the Agency are in Lyon, France.

The Agency has as its mission to reduce the cancer burden worldwide through promoting international collaboration in research. The Agency addresses this mission through conducting cancer research for cancer prevention in three main areas: describing the occurrence of cancer, identifying the causes of cancer, and evaluating preventive interventions and their implementation. Each of these areas is a vital contribution to the spectrum of cancer prevention.

The publications of the Agency contribute to the dissemination of authoritative information on different aspects of cancer research. Information about IARC publications, and how to order them, is available at https://publications.iarc.who.int/.

IARC Handbooks of Cancer Prevention

In 1969, the International Agency for Research on Cancer (IARC) initiated a programme on the evaluation of the carcinogenic risk of chemicals to humans involving the production of monographs of critical reviews and evaluations of individual chemicals.

The *IARC Handbooks of Cancer Prevention* complement the *IARC Monographs*' identifications of carcinogenic hazards. The objective of the programme is to coordinate and publish critical reviews of data on the cancer-preventive effects of primary or secondary interventions, and to evaluate these data in terms of cancer prevention with the help of international working groups of experts in prevention and related fields. The lists of evaluations are regularly updated and are available at https://handbooks.iarc.who.int/.

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NOTE TO THE READER

The *IARC Handbooks of Cancer Prevention* series was launched in 1995 to complement the *IARC Monographs*' evaluations of carcinogenic hazards. The *IARC Handbooks of Cancer Prevention* evaluate the published scientific evidence of cancer-preventive interventions.

Inclusion of an intervention in the *Handbooks* does not imply that it is cancer-preventive, only that the published data have been examined. Equally, the fact that an intervention has not yet been evaluated in a *Handbook* does not mean that it may not prevent cancer. Similarly, identification of organ sites with *sufficient evidence* or *limited evidence* that the intervention has a cancer-preventive activity in humans should not be viewed as precluding the possibility that an intervention may prevent cancer at other sites.

The evaluations of cancer-preventive interventions are made by international Working Groups of independent scientists and are qualitative in nature. No recommendation is given for regulation or legislation.

Anyone who is aware of published data that may alter the evaluation of cancer-preventive interventions is encouraged to make this information available to the *IARC Handbooks* programme, International Agency for Research on Cancer, 25 avenue Tony Garnier, CS 90627, 69366 Lyon CEDEX 07, France, or by email to ihb@iarc.who.int, in order that these data may be considered for re-evaluation by a future Working Group.

Although every effort is made to prepare the *Handbooks* as accurately as possible, mistakes may occur. Readers are requested to communicate any errors to the *IARC Handbooks* programme at ihb@iarc.who.int. Corrigenda are published online on the relevant webpage for the volume concerned (IARC Publications: https://publications.iarc.who.int/).

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[1] Working Group Members and Invited Specialists serve in their individual capacities as scientists and not as representatives of their government or any organization with which they are affiliated. Affiliations are provided for identification purposes only. Invited Specialists do not serve as Meeting Chair or Subgroup Chair, draft text that pertains to the description or interpretation of cancer data, or participate in the evaluations. Each participant was asked to declare potentially relevant research, employment, and financial interests that are current or that have occurred during the past 4 years. Minimal interests are not disclosed here and include stock valued at no more than US$ 1000 overall, grants that provide no more than 5% of the research budget of the expert's organization and that do not support the expert's research or position, and consulting or speaking on matters not before a court or government agency that does not exceed 2% of total professional time or compensation. All other non-publicly funded grants that support the expert's research or position and all consulting or speaking on behalf of an interested party on matters before a court or government agency are disclosed as potentially significant conflicts of interests.

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[4] Claire Wilkinson reports having provided expert opinion about the evidence of changes to trading hours to alcohol outlets and harms to HWL Ebsworth Lawyers.
[5] Norman Maldonado reports being employed by the World Bank's Health Tax Program supporting country offices on design and implementation of excise taxes on tobacco, alcohol, and sugar-sweetened beverages.
[6] Tim Stockwell reports that his unit benefited from research funding from Alko Inc., and having received honoraria from the Liquor and Cannabis Regulation Branch.
[7] Each Observer agreed to respect the Guidelines for Observers at *IARC Handbooks* meetings. The Observer did not serve as Meeting Chair or Subgroup Chair, draft or revise any part of the *Handbook*, or participate in the evaluations. They also agreed not to contact participants before or after the meeting, not to lobby them at any time, not to send them written materials, and not to offer them meals or other favours. IARC asked and reminded Working Group members to report any contact or attempt to influence that they may have encountered, either before or during the meeting.

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PREAMBLE – PRIMARY PREVENTION

The Preamble to the *IARC Handbooks of Cancer Prevention* describes the objectives and scope of the programme, general principles and procedures, and scientific review and evaluations. The *IARC Handbooks* embody the principles of scientific rigour, impartial evaluation, transparency, and consistency. The Preamble should be consulted when reading an *IARC Handbook* or a summary of an *IARC Handbook*'s evaluations. Separate Instructions for Authors describe the operational procedures for the preparation and publication of a volume of the *IARC Handbooks*.

A. GENERAL PRINCIPLES AND PROCEDURES

1. Background

Prevention of cancer is the mission of the International Agency for Research on Cancer (IARC). Cancer prevention is needed even more today than when IARC was established, in 1965, because the global burden of cancer is high and continues to increase, as a result of population growth and ageing and increases in cancer-causing exposures and behaviours, especially in low- and middle-income countries (Stewart & Kleihues, 2003; Boyle & Levin, 2008; Stewart & Wild, 2014).

Broadly defined, prevention is "actions aimed at eradicating, eliminating, or minimizing the impact of disease and disability, or if none of these is feasible, retarding the progress of disease and disability" (Porta, 2014). Cancer prevention encompasses primary, secondary, and tertiary prevention. Primary prevention consists of actions that can be taken to lower the risk of developing cancer. Secondary prevention entails methods that can find and ameliorate precancerous conditions or find cancers in the early stages, when they can be treated more successfully. Tertiary prevention is the application of measures aimed at reducing the impact of long-term disease and disability caused by cancer or its treatment.

The *IARC Handbooks of Cancer Prevention* provide critical reviews and evaluations of the scientific evidence on the preventive effects of primary or secondary cancer prevention measures. The evaluations of the *IARC Handbooks* are used by national and international health agencies to develop evidence-based interventions or recommendations for reducing cancer risk.

The *IARC Handbooks of Cancer Prevention* series was launched in 1995 by Dr Paul Kleihues, then Director of IARC, in recognition of the need for a series of publications that would critically review and evaluate the evidence on a wide range of cancer-preventive interventions. The first volume of the *IARC Handbooks* (IARC, 1997) reviewed the evidence on cancer-preventive

effects of non-steroidal anti-inflammatory drugs, specifically aspirin, sulindac, piroxicam, and indomethacin. *Handbooks* Volume 6 (IARC, 2002a) was the first that evaluated behavioural interventions (weight control and physical activity), and *Handbooks* Volume 7 (IARC, 2002b) was the first that evaluated cancer screening (breast cancer screening). *Handbooks* Volumes 11–14 (IARC, 2007, 2008, 2009, 2011) focused on tobacco control. After a 3-year hiatus, the *IARC Handbooks* series was relaunched in 2014 with the preparation of *Handbooks* Volume 15 (IARC, 2016), which re-evaluated breast cancer screening.

IARC's process for developing *Handbooks* engages international, expert scientific Working Groups in a transparent synthesis of different streams of evidence, which is then translated into an overall evaluation according to criteria that IARC has developed and refined (see Part A, Section 6). Scientific advances are periodically incorporated into the evaluation methodology, which must be sufficiently robust to encompass a wide variety of interventions, ranging from broad societal measures to individual behaviour and to chemoprevention.

This Preamble, first prepared as the *Handbooks* Working Procedures in 1995 and later adapted to the topics of cancer screening and tobacco control, is primarily a statement of the general principles and procedures used in developing a *Handbook*, to promote transparency and consistency across *Handbooks* evaluations. In addition, IARC provides Instructions for Authors to specify more detailed operating procedures.

2. Objectives, scope, and definitions

2.1 Objectives and scope

The scope of the *IARC Handbooks of Cancer Prevention* series is to contribute to reducing the incidence of or mortality from cancer worldwide. To this end, the *IARC Handbooks* programme prepares and publishes, in the form of volumes of *Handbooks*, critical scientific reviews and evaluations of the available evidence on the efficacy, effectiveness, and harms of a wide range of cancer-preventive interventions. The primary target audiences for the *Handbooks* are national and international agencies with responsibility for, or advocating for, public health. The *IARC Handbooks* are an important part of the body of information on which public health decisions for cancer prevention may be based. However, public health options to prevent cancer vary from one setting to another and from country to country, and relate to many factors, including socioeconomic conditions and national priorities. Therefore, no recommendations are given in the *Handbooks* with regard to regulations or legislation, which are the responsibility of individual governments or other international authorities. However, the *IARC Handbooks* may aid national and international authorities in devising programmes of health promotion and cancer prevention, understanding important benefits and harms, and considering cost–effectiveness evaluations.

The *IARC Handbooks* programme also does not make formal research recommendations. However, because *Handbooks* synthesize and integrate streams of evidence on cancer prevention, critical gaps in knowledge that merit research may be identified.

2.2 Definition of interventions for primary prevention

The current *IARC Handbook* addresses a specific intervention or class of interventions for **primary prevention**. Primary prevention "aims to reduce the incidence of disease by personal and communal efforts" (Porta, 2014). The term "intervention" in this *Handbook* refers to any action aimed at reducing the incidence of cancer in humans. Primary prevention interventions include increasing human exposure to known cancer-preventive agents, reducing human exposure to known cancer hazards, providing means to reduce the effects of exposure to cancer hazards, or otherwise intervening on human pathological states that cause cancer. In broad terms, such interventions include, for example, regulating exposure to carcinogens, administering chemopreventive pharmaceuticals or other agents, vaccinating against cancer-causing infections, modifying the environment (e.g. planting trees or constructing shade structures in areas of high ambient levels of solar ultraviolet radiation), or promoting personal or societal action to increase the prevalence of healthy lifestyles or behaviours or decrease the prevalence of unhealthy lifestyles or behaviours.

Primary preventive interventions can be applied across a continuum of:

(i) the general population (often circumscribed by age and sex);

(ii) subgroups with particular predisposing host characteristics, such as genetic susceptibility, precursor lesions, or particular diseases other than cancer, or with high exposure to environmental, occupational, or behavioural risk factors; and

(iii) people with a history of cancer who are at high risk of a further primary cancer.

Although the intent of the *IARC Handbooks* is to evaluate interventions, i.e. a dynamic comparison, there will be circumstances under which an evaluation of the association between exposure to an agent and cancer incidence, i.e. a static comparison, is appropriate. In principle, the approaches to scientific review of the relevant studies in this section will not differ between those entailing dynamic interventions and those entailing static exposures. Therefore, in this Preamble the term "intervention" applies to studies of both types, unless specifically stated otherwise.

2.3 Definitions of efficacy, effectiveness, and harms

Efficacy and effectiveness are two fundamental concepts underlying the evaluation of preventive interventions (Cochrane, 1972). Efficacy was defined by Porta (2008) as "the extent to which a specific intervention, procedure, regimen or service produces a beneficial result under ideal conditions … Ideally, the determination of efficacy is based on the results of a randomized controlled trial". Effectiveness was defined by Porta (2008) as "a measure of the extent to which a specific intervention, procedure, regimen or service, when deployed in the field in routine circumstances, does what it is intended to do for a specific population".

The distinction between efficacy and effectiveness of an intervention at the population level is an important one to make when evaluating preventive interventions. Efficacy is a necessary, but not sufficient, basis for recommending an intervention. Whereas efficacy of an intervention can be inferred if effectiveness is established, efficacy does not guarantee effectiveness because of the number of implementation steps, each with uncertainty, required to deliver an efficacious prevention intervention as an effective programme in a target population. Ideally, efficacy is established before a preventive intervention is implemented in a whole community or population, so as to determine whether a case for population-wide implementation can be made

on the basis of the balance of the benefits and harms and the financial costs of the intervention. However, it has not been unusual for preventive interventions to be implemented in the absence of evidence of efficacy. Should that occur, evaluation of effectiveness may be the only way to determine whether the case for the intervention is strong enough to justify its continuation or implementation elsewhere.

In addition to being shown to be efficacious or effective, preventive interventions must satisfy other requirements if they are to be considered for implementation in practice, including an acceptable balance of benefits and harms. In the present context, harm is defined as any impairment or increase in risk of impairment as a result of exposure to or participation in a preventive intervention. Harms include physical, psychological, social, and economic consequences of a preventive intervention. Adverse events in health care are a subset of harms. Evaluation of these potential harms is an important component of the summary of the evidence.

Other issues to be considered include the cost, cost–effectiveness, affordability, economic efficiency, health equity impact, feasibility, acceptability, relative value, and human rights impact of the intervention. Depending on the specific intervention, some of these issues may be of sufficiently high interest to be reviewed in the *IARC Handbook*.

3. Identification and selection of interventions and outcomes for review

3.1 Development of an analytical framework

As one of the first steps in the review and evaluation process of the *IARC Handbooks*, the IARC Secretariat, with the support of the Working Group, drafts an analytical framework. Such a framework depicts the relationships among the study population, intervention, comparator, and intermediate outcomes or changes in health status as relevant. The analytical framework includes both benefits and harms, and key contextual issues related to participation and implementation of the intervention and its impact on population health. The framework defines the intervention in its broadest context and specifies the aspects for which the *Handbook* will review and evaluate the evidence.

In this framework, IARC defines the intervention and the outcome to be evaluated, according to one of two scenarios:

Scenario 1: evaluation of the effect of a specified *intervention*, that is, an action that results in a change in a potentially preventive exposure, in producing a specified change in *cancer incidence*.

Scenario 2: a two-step evaluative framework from which, for scientific reasons, the level of evidence that an intervention prevents cancer is established by way of an intermediate outcome.

- In Step 1, the effect of a specified intervention on an intermediate outcome, such as exposure to a particular risk factor or preventive factor for cancer in humans, is evaluated (Jonas et al., 2018). Step 1 alone might be taken if it **has been established in authoritative sources** (e.g. the *IARC Monographs* programme) that a change in the intermediate outcome (decreasing exposure to a risk factor or increasing exposure to a preventive factor) reduces the risk of cancer in humans.
- In Step 2, the effect of the change in the intermediate outcome (decrease in exposure to the risk factor or increase in exposure to the preventive factor) on cancer incidence in humans is evaluated. Evaluation of data streams to support Step 2 alone might be done in preparation for a subsequent evaluation of data to support Step 1 if it **has not yet been established in authoritative sources** that a

change in the intermediate outcome reduces the risk of cancer in humans.

The analytical framework determines whether evidence is reviewed for Step 1 only, Step 2 only, or both Steps 1 and 2. A *Handbook* might, for example, include both Steps 1 and 2 when a systematic review and evaluation of Step 2 is necessary (e.g. is not yet available from other authoritative sources) and the number of studies to be reviewed for Steps 1 and 2 is manageable. Taking Steps 1 and 2 together is equivalent to Scenario 1 with inclusion of one or more intermediate outcomes in the evaluation scheme. The sections below provide additional details on the selection of the interventions and outcomes for review.

3.2 Selection of the interventions

For each new volume of the *Handbooks*, IARC selects one or more interventions for review by considering the availability of pertinent research studies, the need to evaluate an important development in cancer prevention, or the need to re-evaluate a previously evaluated intervention. IARC will also consider current public health priorities in specific geographical regions, for example the concerns of countries or regions with a high risk of specific cancer types (see Part A, Section 6, Step 1). IARC will also pay attention to topics that extend beyond those covered by other agencies.

Interventions not previously evaluated in the *IARC Handbooks* series are selected for evaluation, where the body of evidence is large enough to warrant evaluation, on the basis of one or both of the following criteria:

- The intervention is of putative preventive value, but its effects have not been established formally;
- The available evidence suggests that the intervention has the potential to significantly reduce the incidence of cancer, or to have a significant impact on an intermediate outcome or outcomes known or highly suspected to be linked to cancer (see Section 3.1; see also Part A, Section 6, Step 2).

In addition, an intervention previously evaluated in a *Handbook* may be re-evaluated if important new data become available about its effects or if its technology or implementation has changed enough for there to be substantial changes in its effects. Occasionally, a re-evaluation may be limited to one or several specific cancer sites or to specific aspects of the preventive intervention (e.g. reduction in excess body fatness) to which the new evidence predominantly relates. For re-evaluations, the full body of evidence relevant to the intervention of interest is considered, either by de novo review of all evidence or by accepting as accurate the evidence review of the previously published *Handbook* and undertaking a de novo review of evidence published since the previous review. Both approaches lead to an evaluation based on all relevant evidence (see Part A, Section 6, Steps 4 and 5). The choice of the approach is subject to the judgement of the Working Group.

3.3 Selection of the outcomes

In primary prevention of cancer, the outcome targeted by the preventive intervention or interventions is reduction in the incidence of cancer (Scenario 1; see Part A, Section 3.1).

As described above, an intermediate outcome may be chosen as the evaluation outcome for a *Handbook* when there is evidence that a change in the intermediate outcome (decreasing exposure to the risk factor or increasing exposure to the preventive factor) can lead to a reduction in the incidence of one or more types of cancer. An example of such a target is an increase in the smoking cessation rate, which is a commonly used outcome for studies designed to determine the preventive effects of new methods of reducing the incidence of tobacco-caused cancer

Table 1 Roles of participants at *IARC Handbooks* meetings

Category of participant	Role			
	Prepare text, tables, and analyses	Participate in discussions	Participate in evaluations	Eligible to serve as Meeting Chair or Subgroup Chair
Working Group members	✓	✓	✓	✓
Invited Specialists	✓[a]	✓		
Representatives of health agencies		✓[b]		
Observers		✓[b]		
IARC Secretariat	✓[c]	✓	✓[d]	

[a] Only for sections not directly relevant to the evaluation
[b] Only at times designated by the Meeting Chair and/or Subgroup Chair
[c] Only when needed or requested by the Meeting Chair and/or Subgroup Chair
[d] Only for supporting Working Group members and for clarifying or interpreting the Preamble

by way of reducing the prevalence of tobacco smoking. Other examples of changes in intermediate outcomes include a decrease in excess body fatness, a decrease in the levels of diesel engine emissions in urban environments, and an increase in the population coverage of human papillomavirus (HPV) vaccination.

Alternatively, a *Handbook* could, as a first step, evaluate the evidence that changing the intermediate outcome can lead to a reduction in the incidence of one or more types of cancer if such evidence is not already available from authoritative sources, followed by an evaluation of the effect of an intervention on the intermediate outcome (Scenario 2, Step 2 followed by Step 1; see Part A, Section 3.1). An example of such a scenario is evaluation of the evidence that reducing consumption of alcoholic beverages reduces incidence of alcohol-related cancer or precancer, followed by evaluation of the efficacy or effectiveness of a specific intervention in reducing the consumption of alcoholic beverages.

4. The Working Group and other meeting participants

Five categories of participants can be present at *IARC Handbooks* meetings (Table 1):

(i) *Working Group* members have ultimate responsibility for determining the final list of studies that contribute evidence to the evaluation, performing the scientific review of the evidence, and making the final, formal evaluation of the strength of evidence for the capacity of the screening interventions to reduce cancer incidence or cancer mortality. The Working Group is multidisciplinary and is organized into Subgroups of experts in the fields that the *Handbook* covers.

IARC selects the Working Group members on the basis of relevant expertise and an assessment of declared interests (see Part A, Section 5). Consideration is also given to diversity in scientific approaches, in stated positions on the strength of the evidence supporting the intervention, and in demographic characteristics. Working Group members generally have published research related to the interventions being reviewed or to the cancer types or intermediate outcomes that the interventions being reviewed are thought to prevent or affect; IARC uses literature searches to identify most experts. IARC also encourages public nominations through its Call for Experts. IARC's reliance on Working Group members with expertise on the subject matter or relevant methodologies is supported

by decades of experience documenting that there is value in specialized expertise and that the overwhelming majority of Working Group members are committed to the objective evaluation of scientific evidence and not to the narrow advancement of their own research results or a predetermined outcome (Wild & Cogliano, 2011). Working Group members are expected to serve the public health mission of IARC and to refrain from using inside information from the meeting or meeting drafts for financial gain until the full volume of the *Handbooks* is published (see also Part A, Section 7).

IARC selects, from among the Working Group members, individuals to serve as Meeting Chair and Subgroup Chairs. Subgroup Chairs have preferably served in previous Handbooks meetings as Working Group members or in similar review processes. At the opening of the meeting, the Working Group is asked to endorse the Meeting Chair selected by IARC or to propose an alternative. The Meeting Chair and Subgroup Chairs take a leading role at all stages of the review process (see Part A, Section 7) to promote open scientific discussions that involve all Working Group members in accordance with committee procedures and to ensure adherence to the processes described in this Preamble.

(ii) *Invited Specialists* are experts with critical knowledge and experience on the interventions being reviewed, the cancer types that the interventions being reviewed are thought to prevent, or relevant methodologies, but who have a declared conflict of interest that warrants exclusion from developing or influencing the evaluations. The Invited Specialists do not draft any section of the *Handbook* that pertains to the description or interpretation of the data on which the evaluation is based, or participate in the evaluations. Invited Specialists are invited in limited numbers, when necessary, to assist the Working Group by contributing their unique knowledge and experience to the discussions.

(iii) *Representatives of national and international health agencies* may attend because their agencies are interested in the subject of the *Handbook*. The Representatives of national and international health agencies do not draft any section of the *Handbook* or participate in the evaluations. Representatives can participate in discussions at times designated by the Meeting Chair or a Subgroup Chair. Relevant World Health Organization (WHO) staff members attend as members of the *IARC Secretariat* (see below).

(iv) *Observers* with relevant scientific credentials are admitted in limited numbers. Attention is given to the balance of Observers from entities with differing perspectives on the interventions under review. Observers are invited only to observe the meeting, do not draft any section of the *Handbook* or participate in the evaluations, must agree to respect the Guidelines for Observers at *IARC Handbooks* meetings (IARC, 2018), and must not attempt to influence the outcomes of the meeting. Observers may speak at Working Group or Subgroup sessions at the discretion of the Chair.

(v) The *IARC Secretariat* consists of scientists who are designated by IARC or WHO and who have relevant expertise. The IARC Secretariat coordinates and facilitates all aspects of the review and evaluation process and ensures adherence to the processes described in this Preamble throughout the development of the scientific reviews and evaluations (see Part A, Sections 5 and 6). The IARC Secretariat announces and organizes the meeting, identifies and invites the Working Group members, and assesses the declared interests of all meeting participants

in accordance with WHO requirements (see Part A, Section 5). The IARC Secretariat supports the activities of the Working Group (see Part A, Section 7) by performing systematic literature searches, performing title and abstract screening, organizing conference calls to coordinate the development of drafts and to discuss cross-cutting issues, and reviewing drafts before and during the meeting. Members of the IARC Secretariat serve as meeting rapporteurs, assist the Meeting Chair and Subgroup Chairs in facilitating all discussions, and may draft text or tables or assist a Subgroup in the conduct of additional analyses when designated by the Meeting Chair or a Subgroup Chair. After the meeting, the IARC Secretariat reviews the drafts for factual accuracy of research results cited. The participation of the IARC Secretariat in the evaluations is restricted to clarifying or interpreting the Preamble.

All meeting participants are listed, with their principal affiliations, in the front matter of the published volume of the *Handbooks*. Pertinent interests, if any, are listed in a footnote to the participant's name. Working Group members and Invited Specialists serve as individual scientists and not as representatives of any organization, government, or industry (Cogliano et al., 2004).

The roles of the participants are summarized in Table 1.

5. Development of a volume of the *IARC Handbooks*

Each volume of the *Handbooks* is developed by an ad hoc, specifically convened Working Group of international experts. Approximately 1 year before the meeting of a Working Group, a preliminary list of interventions to be reviewed (see Part A, Section 3), together with a Call for Data and a Call for Experts, is announced on the *Handbooks* programme website (https://handbooks.iarc.who.int/).

The IARC Secretariat selects potential Working Group members based on the criteria described in Part A, Section 4. Before a meeting invitation is extended, each potential participant, including the IARC Secretariat, completes the WHO Declaration of Interests form to report financial interests, employment and consulting (including remuneration for serving as an expert witness), individual and institutional research support, and non-financial interests, such as public statements and positions related to the subject of the meeting. IARC assesses the declared interests to determine whether there is a conflict that warrants any limitation on participation (see Table 1).

Approximately 2 months before a meeting, IARC publishes on the *Handbooks* programme website the names and principal affiliations of all participants and discloses any pertinent and significant conflicts of interest, for transparency and to provide an opportunity for undeclared conflicts of interest to be brought to IARC's attention. It is not acceptable for Observers or third parties to contact other participants before a meeting or to lobby them at any time. Meeting participants are asked to report all such contacts to IARC (Cogliano et al., 2005).

The Working Group meets at IARC to discuss and finalize the scientific review and to develop summaries and evaluations. At the opening of the meeting, all meeting participants update their Declarations of Interests forms, which are then reviewed for conflicts of interest by IARC. Declared interests related to the subject of the meeting are disclosed to the meeting participants during the meeting and in the published volume of the *Handbooks* (Cogliano et al., 2004).

The objectives of the meeting are twofold: peer review of the drafts and consensus on the evaluations. During the first part of the meeting, Working Group members work in Subgroups to

Table 2 Public engagement during the development of a volume of the *IARC Handbooks*

Approximate time frame	Milestones
~1 year before a *Handbooks* meeting	IARC posts on the *Handbooks* programme website: Preliminary List of Interventions to be reviewed Call for Data and Call for Experts open Requests for Observer Status open WHO Declarations of Interests form
~8 months before a *Handbooks* meeting	Call for Experts closes
~4 months before a *Handbooks* meeting	Requests for Observer Status close
~2 months before a *Handbooks* meeting	IARC publishes the names, principal affiliations, and declared conflicts of interest of all meeting participants, and a statement discouraging contact of Working Group members by outside parties
~1 month before a *Handbooks* meeting	Call for Data closes
***Handbooks* meeting**	
~2–4 months after a *Handbooks* meeting	IARC publishes a summary of evaluations and key supporting evidence as a scientific article in a high-impact journal or on the *Handbooks* programme website
~9–12 months after a *Handbooks* meeting	IARC Secretariat publishes the verified and edited master copy of the plenary drafts as a *Handbooks* volume

review the pre-meeting drafts, develop a joint Subgroup draft, and draft Subgroup summaries. During the last part of the meeting, the Working Group meets in plenary sessions to review the Subgroup drafts and summaries and to develop the consensus evaluations. As a result, the entire volume is the joint product of the Working Group and there are no individually authored sections. After the meeting, the master copy is verified by the IARC Secretariat (see Part A, Section 4(v)), edited, and prepared for publication. The aim is to publish the volume of the *Handbooks* within approximately 12 months of the Working Group meeting. The IARC Secretariat prepares a summary of the outcome for publication in a scientific journal or on the *Handbooks* programme website soon after the meeting.

The time frame and milestones for public engagement during the development of a volume of the *IARC Handbooks* are summarized in Table 2.

6. Overview of the scientific review and evaluation process

Principles of systematic review are applied to the identification, screening, synthesis, and evaluation of the evidence (as described in Part B, Sections 2–6 and detailed in the Instructions for Authors). For each volume of the *Handbooks*, the information on the conduct of the literature searches, including search terms and the inclusion and exclusion criteria that were used for each relevant stream of evidence, is recorded.

The Working Group considers all relevant studies, including pertinent reports and reviews on: use of the intervention targeted directly to cancer or to a relevant intermediate outcome or outcomes; all experimental and observational studies in humans (including systematic reviews and meta-analyses) of the putative effect of the intervention or interventions on cancer incidence or a relevant intermediate outcome, and any related harms; all relevant experimental studies in animals; and all relevant mechanistic studies.

In general, only studies that have been published or accepted for publication in the openly available scientific literature are reviewed. Materials that are publicly available and whose content is final may be reviewed if there is sufficient information to enable peer evaluation of the quality of the methods and results of the studies (see Step 1, below). Such material may include reports from government agencies, dissertations for higher degrees, and other apparently reputable scientific sources. Systematic Internet searches for potentially relevant "grey literature" are not usually done. The reliance on published and publicly available studies promotes transparency and protects against citation of information that, although purportedly final, may change before it is published.

The steps of the review process are as follows:

Step 1. Identification of the review question: After the intervention (or interventions) and outcome (or outcomes) to be reviewed have been specified, the IARC Secretariat, in consultation with the Working Group, drafts the review question (or questions) in PICO form (population, intervention/exposure, comparator, and outcome) as required to determine the inclusion and exclusion criteria for the studies. An analytical framework is developed to assist in identifying and formulating the review questions, and encompasses the inclusion of studies in humans, studies in experimental animals, and mechanistic studies when relevant, with the aim of making as large a contribution as possible to the global prevention of cancer.

Step 2. Comprehensive and transparent identification of the relevant information: The IARC Secretariat specifies search terms for the key PICO components of each question and identifies relevant studies through initial comprehensive literature searches in authoritative biomedical databases (e.g. PubMed). The literature searches are designed in consultation with a librarian and other technical experts. The scope and specifications of the searches may be modified, and the searches rerun, depending on the amount, relevance, and perceived completeness of the articles they identify. The IARC Secretariat may also identify relevant studies from reference lists of past *Handbooks*, retrieved articles, or authoritative reviews, and through the Call for Data (see Table 2). The Working Group provides input and advice to the IARC Secretariat to refine the search strategies, and identifies additional articles through other searches and personal expert knowledge.

For certain types of interventions (e.g. administration of regulated pharmaceuticals), IARC also gives relevant regulatory authorities, and parties regulated by such authorities, an opportunity to make pertinent unpublished studies publicly available by the date specified in the Call for Data. Consideration of such studies by the Working Group is dependent on the public availability of sufficient information to enable an independent peer evaluation of: (i) completeness of reporting of pertinent data; (ii) study quality; and (iii) study results.

Step 3. Screening, selection, and organization of the studies: The IARC Secretariat screens the retrieved articles by reviewing the title and abstract against the inclusion and exclusion criteria agreed upon by the Working Group and technical experts in the review process. Potentially relevant studies are then made available to Working Group members for full-text screening and inclusion in or exclusion from the evidence base using agreed criteria specific to this task.

Step 4. Extraction of information from included studies, including characteristics relevant to study quality: Working Group members, working individually as members of defined Subgroups before the *Handbooks* meeting, review and succinctly describe pertinent characteristics and results of included studies as detailed in Part B, Sections 2–4. Study design and results are tabulated systematically in a standard format. This step may be iterative with Step 5.

Step 5. Assessment of study quality: Also before the *Handbooks* meeting, Working Group members evaluate the quality and informativeness of each study they included based on the considerations (e.g. design, conduct, analysis, and reporting of results) described in Part B, Sections 2–4. Evaluation of study quality can be done either narratively or by use of a risk of bias assessment tool when a relevant one is available and can add value to the process. Interpretations of the results, and the strengths and limitations of each study, are clearly outlined in square brackets as part of the description of that study (see Part B).

Step 6. Peer review: Several months before the meeting, the pre-meeting drafts produced from Steps 4 and 5 are peer-reviewed by other members of the Working Group (usually within the same Subgroup). The IARC Secretariat also reviews the drafts for completeness, consistency between drafts, and adherence to the *Handbooks* Instructions for Authors. The peer-review comments are sent to the Working Group members, who produce a revised pre-meeting draft. The revised drafts are reviewed and revised in Subgroup sessions during the *Handbooks* meeting.

Step 7. Synthesis of results and quality of the studies: The results and quality of the included studies are synthesized by the Working Group to provide a summary of the evidence and its quality for each outcome. This synthesis can be narrative or quantitative (for details, see the Instructions for Authors), and the quality synthesis may include use of an overall quality of evidence assessment tool, such as GRADE (Siemieniuk & Guyatt, 2019).

Meta-analyses of large bodies of evidence may be performed by the Working Group and/or by the IARC Secretariat before the meeting if such meta-analyses would assist in evidence synthesis and evaluation. For more information on the conduct and use of such meta-analyses, see Part B, Section 2.1d.

Step 8. Interpretation of study results and evaluation of strength of evidence: The whole Working Group reviews the study descriptions and the summaries of the body of evidence for each outcome or end-point, discusses the overall strengths and limitations of the evidence in each stream of data, and evaluates the strength of evidence for a preventive effect on cancer or an intermediate outcome in each stream using transparent methods, which may include the use of established specific tools. The preventive effect is described in terms given in Part B, Sections 6a–c for each stream of evidence. The Working Group then integrates the strength-of-evidence conclusions from all streams of evidence (see Part B, Section 6d) and develops the rationale for its overall consensus evaluation of the cancer-preventive effect of the intervention (see Part B, Sections 6d–e).

7. Responsibilities of the Working Group

The Working Group is responsible for the final list of studies included in the evaluation and the review and evaluation of the evidence for a *Handbook*, as described above. The IARC Secretariat supports these activities (see Part A, Section 4). To ensure that the process is rigorous, independent, and free from individual conflicts of interest, Working Group members must accept the following responsibilities:

(i) Before the meeting, Working Group members:

- help in developing the analytical framework;
- ascertain that all appropriate studies have been identified and selected;
- assess the methods and quality of each included study;
- prepare pre-meeting drafts that present an accurate quantitative and/or textual

synthesis of the body of evidence, with key elements of study design and results and notable strengths and limitations;
- participate in conference calls organized by the IARC Secretariat to coordinate the development of pre-meeting drafts and to discuss cross-cutting issues; and
- review and provide comments on pre-meeting drafts prepared by other members of their Subgroup or of the Working Group.

(ii) At the meeting, Working Group members work in Subgroups to:

- critically review, discuss, and revise the pre-meeting drafts and adopt the revised versions as consensus Subgroup drafts; and
- develop and propose an evaluation of the strength of the evidence summarized in the consensus Subgroup drafts (see Part B, Section 5), using the *IARC Handbooks* criteria (see Part B, Section 6a–c).

(iii) At the meeting, Working Group members work in plenary sessions to:

- present their Subgroup drafts for scientific review by and discussion with the other Working Group members, and subsequent revisions, as needed;
- participate in review and discussion of other Subgroup drafts and in their adoption as a consensus Working Group draft;
- participate in review and discussion of the summaries and evaluations of the strength of the evidence developed in Subgroups (see Part B, Sections 6a–c), and contribute to their revision, as needed, and their adoption by consensus of the full Working Group; and
- contribute to the discussion of and adoption by consensus of an overall evaluation proposed by the Meeting Chair using the guidance provided in Part B, Section 6d.

The Working Group strives to achieve consensus evaluations. Consensus reflects broad agreement among the Working Group members, but not necessarily unanimity. If unanimity has not been reached when the interpretations of the evidence by all Working Group members have been expressed and debated, the judgement of the majority of the Working Group members is taken as the consensus. When consensus is reached in this way, the Meeting Chair may poll Working Group members to determine and record the diversity of scientific opinion on the overall evaluation.

Only the final product of the plenary sessions represents the views and expert opinions of the Working Group. The *Handbook* is the joint product of the Working Group and represents an extensive and thorough peer review of the body of evidence (review of individual studies, synthesis, and evaluation) by a multidisciplinary group of experts. Initial pre-meeting drafts and subsequent revisions are temporarily archived but are not released, because they would give an incomplete and possibly misleading impression of the consensus developed by the Working Group over its complete deliberation.

B. SCIENTIFIC REVIEW AND EVALUATION

This part of the Preamble discusses the types of evidence that are considered and summarized in each section of a *Handbook*, followed by the scientific criteria that guide the evaluations. In addition, a section of General Remarks at the front of the volume discusses the reasons the interventions were scheduled for evaluation and any key issues encountered during the meeting.

1. Intervention and outcome characterization

An intervention for primary cancer prevention has been defined in this Preamble to be any action aimed at reducing the incidence of cancer in humans (Part A, Section 2). Given this definition, the efficacy or effectiveness of an intervention would be most directly approached by research that examines whether the delivery of the intervention results in a measurable change in a cancer-related exposure that leads to a reduction in the incidence of cancer. However, such research is often lacking, and therefore the possibility of cancer-preventive effects has often been inferred from static associations of cancer incidence with prevalence of exposure to cancer-causing agents or cancer-preventive agents. For example, all measures that are now taken to minimize environmental exposure to asbestos (e.g. regulation of removal of asbestos from buildings or demolition of buildings known to contain asbestos) are based on the very strong evidence that people who have had identifiable exposure to asbestos have a higher incidence of cancer than people who have not had such exposure. Similarly, the evaluation of *Handbooks* Volume 16 that there "is *sufficient evidence* in humans for a cancer-preventive effect of absence of excess body fatness" is almost exclusively based on the substantial body of evidence that cancer incidence is lower in people without excess body fatness than it is in people with excess body fatness; this is a static comparison, not a dynamic comparison as the term "intervention" implies.

1.1 Intervention characterization

This section provides informative background on the intervention and the factors that mediate it. It also summarizes the prevalence and level of the intervention across geographical areas and across the life-course. Methods used to assess exposure to the intervention in key experimental and observational epidemiological studies are described and evaluated. This section also reports on validated biomarkers of internal exposure, metabolites, or other intermediate outcomes that are routinely used for exposure assessment. Concepts of absorption, distribution, metabolism, and excretion, where relevant, are considered in the section on mechanistic evidence (see Part B, Section 4b).

(a) Identification of the intervention

The intervention being evaluated is unambiguously identified. The information provided will vary widely depending on the type of intervention but should be sufficient to enable the implementation of an intervention in practice with reasonable confidence that its outcomes in populations would be similar to those of the intervention from which the bulk of the evidence evaluated in the *Handbook* originated.

Many interventions are multifaceted and comprise complex sets of actions. Interventions determined by personal behaviour or circumstances may result from, be influenced by, or be correlated with a diverse range of behavioural and environmental factors, such as smoking, alcohol consumption, diet, sleep and physical activity patterns, remoteness of residence, and socioeconomic circumstances. The description of such interventions should include their variability across human populations and environments, and their known relationships with other health-determining factors.

(b) Global occurrence and use

Geographical patterns and time trends in occurrence are summarized. A concise overview of quantitative information about sources, prevalence, and levels of individual and population interventions, whether purposive or incidental, is provided. Representative data from formal environmental or behavioural monitoring or surveillance data, research studies, government reports and websites, online databases, and other

citable, publicly available sources are tabulated. Data from low- and middle-income countries are sought and included to the extent that is feasible; information gaps for key regions are noted.

If available, data are reported by region and by other relevant characteristics, such as sex, age, socioeconomic status, and other variables considered relevant by the Working Group.

(c) Regulations and guidelines

Regulations or guidelines that have been established for the intervention (e.g. permissible levels of fortification in food, national dietary guidelines) are described and may be tabulated if they are informative for the interpretation of current or historical levels of the intervention. Information on applicable populations, the basis for regulation, and the timing of regulation may be noted.

(d) Intervention assessment in key epidemiological studies

Epidemiological studies reviewed in the context of the *IARC Handbooks* programme evaluate cancer prevention interventions (or effects on intermediate outcomes) by comparing outcomes across groups differently exposed to changes in a putative cancer-preventing intervention. Therefore, the type and the quality of intervention assessment methods used are key considerations when interpreting study findings. This section summarizes and critically reviews the intervention assessment methods used in both experimental and observational epidemiological studies that contribute data relevant to the *Handbooks* evaluation.

All interventions have two principal dimensions: (i) dose (sometimes defined as concentration or intensity), and (ii) time considerations, including duration (time from first to last exposure), pattern or frequency (whether continuous or intermittent), and windows of susceptibility. This section considers how each of the key epidemiological studies characterizes these dimensions. Interpretation of information for chemical, biological, or physical interventions may also be informed by consideration of mechanistic evidence on absorption, distribution, metabolism, and excretion (e.g. as described in Part B, Section 4b).

In experimental epidemiological studies, the investigators determine, usually by way of randomization, who will and who will not be assigned to the intervention; however, in practice the assignment is not always adhered to. Therefore, a critical assessment of such studies requires careful evaluation using appropriate guidelines or assessment frameworks (e.g. fidelity to intervention implementation and extent of non-adherence to intervention).

Intervention intensity and timing in observational epidemiological studies can be characterized by using environmental monitoring data, records from workplaces or other sources, and subject or proxy reports collected by way of questionnaires or interviews. Both objective and subjective data sources are used, individually or in combination, to assign levels or values of an intervention metric to members of the study population.

Key epidemiological studies with interventions on cancer or intermediate outcomes are identified, and the intervention assessment approach and its strengths and limitations are summarized in text and tables. The Working Group identifies concerns about intervention assessment methods and their impacts on the overall quality of each study reviewed. The Working Group notes the studies where the information provided to characterize the intervention properly, the adherence to the intended intervention in each arm of experimental studies, or the assessment of the intervention in observational studies is inadequate. The Working Group further discusses the likely direction of bias due to non-adherence or to error in intervention assessment in studies where adequate information is available.

1.2 Outcome characterization

(a) Evaluation of cancer outcomes

The cancers are defined and described in terms of their International Classification of Diseases for Oncology (ICD-O) (IARC, 2019) or International Classification of Diseases (ICD) categories, with other relevant morphological or molecular characteristics where relevant.

Benign neoplasms, pre-neoplastic lesions, malignant precursors, and other end-points closely related to cancer may also be reviewed when they relate to the intervention reviewed and are known to predict the primary cancer outcome. These studies can strengthen evidence from studies of cancer itself. For example, the results of controlled trials of sun protection measures in preventing development of cutaneous melanocytic naevi (which are strong risk factors for development of later cutaneous melanoma) in children provide support for the efficacy of sun protection measures in preventing cutaneous melanoma in adults (Thun et al., 2018).

(b) Evaluation of intermediate outcomes

Potentially relevant intermediate outcomes vary widely across human biology, pathology, and behaviour. (Intermediate outcomes that are biomarkers of early biological effects, which are not topics evaluated in *IARC Handbooks*, are described in Part B, Section 4.) All intermediate outcomes are described as precisely as possible, using an applicable international standard classification (e.g. ICD classification). When, as with some behavioural or physiological risk factors, they can be defined or measured in a range of ways, the definitions that are acceptable for the evaluation are clearly defined and acceptable standards for measurement stated.

When an intermediate outcome is the outcome being evaluated, the evidence base establishing that the intermediate outcome has an established causal or preventive association with cancer incidence is briefly summarized.

In what follows, the term "cancer incidence" refers to the **outcome of a *Handbooks evaluation***, that is, to the incidence of **cancer** or of an **intermediate outcome**, as defined in the analytical framework.

2. Studies of cancer prevention in humans

This section includes all pertinent experimental and observational studies in humans that include cancer or a specified intermediate outcome (if it is the topic of the *Handbook*) as a study outcome. As noted above, only observational studies in which changes in the exposure (i.e. intervention) in relation to the outcome have been analysed will be considered, unless specifically stated otherwise. Among many others, these studies also encompass studies with biomarkers as intervention metrics (Alexandrov et al., 2016). As mentioned above, studies that assess biomarkers of early biological effects are reviewed in Part B, Section 4.

This section includes specification and assessment of beneficial effects, as well as potential harms.

2.1 Assessment of beneficial effects

(a) Types of studies considered

Several types of epidemiological study designs contribute to the evaluation of cancer prevention in humans (Table 3). These studies include experimental studies and different types of observational studies (i.e. cohort, case–control, and ecological). In addition to these types of studies, innovations in epidemiology enable other designs that may be considered in *Handbooks* evaluations.

Table 3 Types of epidemiological studies that contribute to the evaluation of cancer prevention

Experimental studies	
	• High level of investigator control over assignment to the intervention and non-intervention group • Ideally random assignment, either of individuals or of groups, to the intervention and non-intervention group • Provides evidence for the efficacy or effectiveness of a preventive intervention • Includes a range of quasi-experimental designs in which there is lack of random assignment to the intervention and non-intervention; quasi-experimental studies are often at high risk of bias
Observational (non-experimental) studies	
Cohort	• In a prospective cohort study, information on the intervention and non-intervention is collected from individuals who are then followed up over time to assess subsequent outcomes. Further intervention information may be collected at intervals during follow-up. • In a retrospective cohort study, information on intervention and subsequent outcomes in a defined group of individuals, which was usually recorded for purposes other than research, is accessed after the outcomes have occurred. • Nested within these studies, case–control and case–cohort studies provide efficiency and an opportunity to collect additional intervention information.
Case–control	• In a case–control study, individuals newly diagnosed with the outcome in a defined population and a sample of "control" individuals without the outcome from the same source population and time period are enrolled, and their intervention histories are compared. • Intervention information collected from cases and controls must refer to time before disease onset to reasonably infer a temporal association.
Mendelian randomization	• Mendelian randomization studies are cohort or case–control studies in which an intervention is inferred using appropriate genomic surrogate(s) (Yarmolinsky et al., 2018). • These studies are considered to be less prone to bias than other observational studies because the genomic variants from which intervention is inferred are randomly allocated at conception.
Ecological	• The association between an intervention and an outcome is examined not in individual people but in units of population defined geographically and/or temporally. Uncontrolled confounding is a major issue for ecological studies. • Results from ecological studies can support a hypothesis about an intervention–outcome association or, when taken together with results of case–control and cohort studies, support judgements on causal associations. • Results may be persuasive when population-wide implementation of an intervention leads to changes in cancer incidence or mortality: (a) in several populations, and there is no similar trend in similar populations not, or much less, subject to the intervention (e.g. Hakama, 1983); or (b) in a single population, by use of time series analysis when longitudinal data on both the intervention and the outcome are available (e.g. Bernal et al., 2017).

(b) Identification of eligible studies in humans

Relevant studies in humans are identified using principles of systematic review as described in Part A and further detailed in the Instructions for Authors provided to each Working Group. Eligible studies include all studies in humans of the association of a putative cancer-preventive intervention with the occurrence of cancer, or a specified intermediate outcome if it is a topic of the *Handbook*. Multiple publications on the same study population are identified so that the number of independent studies is accurately represented. Multiple publications may result, for example, from successive follow-ups of a single trial population or cohort, from analyses focused on different aspects of an intervention–outcome association, or from inclusion of overlapping populations. In these situations, the most recent or most informative report is usually reviewed first, with recourse to the other reports if important information (e.g. methodological

detail) is not included in the most recent or most informative report.

(c) Study quality and informativeness

Epidemiological studies are susceptible to several different sources of error. Study quality is assessed as part of the structured expert review process undertaken by the Working Group. A key aspect of quality assessment is consideration of the possible roles of chance and bias in the interpretation of epidemiological studies.

Chance, also called "random variation", can produce misleading study results. This variability in study results is strongly influenced by the sample size: smaller studies are more likely than larger studies to have effect estimates that are imprecise and, therefore, are more likely to be misleading. Confidence intervals around a study's point estimate of effect are routinely used to indicate the range of values of the estimate that could be produced by chance. Both experimental and observational epidemiological studies are prone to effects of chance, and experimental studies are arguably more prone, because of their smaller sample sizes, associated with the greater cost of conducting such studies.

Bias is the effect of factors in study design, conduct, or reporting that lead an association to erroneously appear stronger than, weaker than, or opposite in direction to the association that really exists between an intervention and an outcome. Biases that require consideration are varied and can be broadly categorized as selection bias, information bias, and confounding bias (Rothman et al., 2008). Selection bias in an epidemiological study can occur when the inclusion of participants from the eligible population or their follow-up in the study is influenced by their intervention status or their outcome (usually disease occurrence). Under these conditions, the measure of association found or not found in the study may not accurately reflect the association or lack thereof that might otherwise have been found in the eligible population (Hernán et al., 2004). Information bias results from inaccuracy in intervention or outcome measurement. Both can cause an association between hypothesized cause and effect to appear stronger or weaker than it really is. Confounding arises when a third factor is associated with both the intervention and the outcome and, because of this, influences the apparent association between them (Rothman et al., 2008). An association between the intervention and another factor that is associated with an increase or a decrease in the incidence of or mortality from the disease can lead to a spurious association or the absence of a real association of the intervention with the outcome. When either of these occurs, confounding is present.

In principle, experimental studies are less prone to each of these sources of bias, because selection for intervention or non-intervention is determined by the investigator (usually by random allocation) and not by the study participants or their characteristics. However, bias may still arise as a result of lack of concealment, non-random allocation, lack of blinding, post-randomization exclusions, non-acceptance of or non-adherence by the study participants to the intervention condition of the study arm to which they are randomized, or study loss to follow-up. One potential shortcoming of randomized studies is their potentially limited external validity (relevance) and consequently limited generalizability to non-studied populations.

In assessing the quality of the studies, the Working Group considers the following aspects:

- **Study description:** Clarity in describing the study design, implementation, and conduct, and the completeness of reporting of all other key information about the study and its results.
- **Study population:** Whether the study population was appropriate for evaluating the association between the intervention and the outcome. Whether the study was designed and conducted in a manner that would

minimize selection bias and other forms of bias. The designated outcomes in the study population must have been identified in a way that was independent of the intervention of interest, and the intervention must have been assessed in a way that was not related to outcome status. In these respects, completeness of recruitment into the study from the population of interest (which is less of an issue for experimental efficacy studies than for effectiveness studies and observational studies) and completeness of follow-up for the outcome (see below) are very important.

- **Outcome measurement:** The appropriateness of the outcome measure (incidence of cancer, mortality from cancer, or an intermediate outcome, as defined in Part B, Section 1.2) for the intervention and the cancer type under consideration, the outcome ascertainment methodology, and the extent to which outcome misclassification may have led to bias in the measure or measures of association.
- **Intervention measurement:** This includes: (i) the adequacy (including the validity and the reliability) of the methods used to assess the intervention in observational studies, and adherence to the intervention condition in experimental studies, and (ii) the likelihood (and direction) of bias in the measure or measures of association because of intervention measurement error or misclassification in observational studies and non-adherence to the intervention condition in experimental studies (see Part B, Section 1.1. Of particular relevance is an assessment of the error associated with the measurement of change over time in several study designs, including prospective longitudinal studies (e.g. change in body weight estimated from contemporary recall of past body weight and self-reported or measured current body weight at recruitment into a cohort study).
- **Assessment of potential confounding:** The extent to which the authors took into account in the study design and analysis potentially confounding variables (including co-exposures, as described in Part B, Section 1d) that could influence the occurrence of the outcome and may be related to the intervention of interest. Important sources of potential confounding by such variables should, where possible, have been addressed in the study design, such as by randomization, matching, or restriction, or in the analysis by statistical adjustment. In some instances, where direct information on confounders is unavailable, use of indirect methods to evaluate the potential impact of confounding on intervention–outcome associations is appropriate (e.g. Axelson & Steenland, 1988; Richardson et al., 2014).
- **Other potential sources of bias:** Each epidemiological study is unique in its study population, its design, its data collection, and, consequently, its potential biases. For example, repeated assessments of exposure to the intervention over time can be influenced by the occurrence of the outcome and thus bias the result and sometimes lead to "reverse causation". All possible sources of bias are considered for their possible impact on the results, including the possibility of reporting bias (selective reporting of some results).
- **Statistical methodology:** The studies are evaluated for the adequacy of the statistical analysis methods used and their ability to obtain unbiased estimates of intervention–outcome associations, confidence intervals, and test statistics for the significance of measures of association. Appropriateness of methods used to address confounding, including adjusting for matching when necessary and avoiding treatment of probable mediating variables as confounders, is considered. For example, the use of directed

acyclic graphs can inform about whether confounding and selection biases have been specified correctly (Hernán et al., 2004). Detailed analyses of cancer risks in relation to summary measures of intervention, such as cumulative exposure to the intervention, or temporal variables, such as age at first intervention or time since first intervention, are reviewed and summarized when available.

For the sake of economy and simplicity, this Preamble refers to the **list of possible sources of error** with the phrase "**chance, bias, and confounding**", but it should be recognized that this phrase encompasses a comprehensive set of concerns pertaining to study quality. These elements of study quality do not constitute and should not be used as a formal checklist of indicators of study quality. Rather, the assessment by the Working Group is reported in a narrative way, in the form of comments in square brackets. **The judgement of the experts is critical** in determining **how much weight to assign to different issues** when considering how all these potential sources of error should be integrated and how to rate the potential for error related to each. However, it is important that the process undertaken, including the weight given to various studies, be **replicable** and be described in a way that is **transparent** to readers.

- **Study informativeness:** The informativeness of a study is its ability to show a true preventive effect, if one exists, between the intervention and the outcome in a relevant population, and not to show an effect if one does not exist. Key determinants of informativeness include having a study population of sufficient size to obtain precise estimates of effect, sufficient elapsed time from intervention to measurement of outcome for an effect, if present, to be observable, presence of at least moderate heterogeneity of exposure to the intervention (intensity, frequency, and/or duration) in the study population, and biologically relevant definitions of the intervention.

(d) Meta-analyses and pooled analyses

Independent epidemiological studies of the same intervention with a comparatively weak effect or small sample size may produce inconclusive results that are difficult to summarize. Combined analyses of data from multiple studies may increase the precision of estimates. There are two types of combined analysis: (i) meta-analysis, which involves combining summary statistics, such as relative risks from individual studies; and (ii) pooled analysis, which involves a pooled analysis of the raw data from the individual studies (Greenland & O'Rourke, 2008). There are also "umbrella reviews", systematic reviews of multiple meta-analyses, which may be evaluated by the Working Group.

The strengths of combined analyses are increased precision due to increased sample size and, in the case of pooled studies, the opportunity to better control for potential confounders and to explore interactions and modifying effects that may help to explain heterogeneity between studies. A disadvantage of combined analyses is the possible lack of comparability of results from various studies, because of differences in specification of the intervention or the outcome, population characteristics, subject recruitment, data collection procedures, methods of measurement, and effects of unmeasured covariates, which may differ among studies. These differences in study methods and quality can influence the results of both pooled analyses and meta-analyses.

Meta-analyses considered by the Working Group may include high-quality published meta-analyses, updates of such meta-analyses, and new meta-analyses. When published meta-analyses are considered by the Working Group, they should comply with basic quality standards for meta-analyses and their underlying systematic reviews (e.g. AMSTAR, 2017):

their risk of bias is carefully evaluated, including the completeness of the studies included, the methods used to identify and the criteria used to select eligible studies, and the accuracy of the data extracted from the individual studies.

Subject to the judgement of the IARC Secretariat and in consultation with the Working Group, the updating of meta-analyses or the conduct of ad hoc meta-analyses may be performed by the Working Group and/or by the IARC Secretariat during preparation for a *Handbooks* meeting, when there are sufficient studies of an intervention–outcome association to aid the Working Group's assessment of the association. When results from both experimental and observational studies are available, any combined analyses should be conducted separately for experimental and observational studies, with consideration given to separate combined analyses of cohort and case–control studies, because of their different propensities to bias. The results of such ad hoc meta-analyses, which are specified in the text of the *Handbook* by presentation in square brackets, may come from the addition of the results of more recent studies to those of published meta-analyses or from de novo meta-analyses. Additional details on the conduct of such ad hoc meta-analyses are provided in the Instructions for Authors.

Irrespective of the source of the information for the meta-analyses and pooled analyses, the criteria for information quality applied are the same as those applied to individual studies. The sources of heterogeneity among the studies contributing to them are carefully considered and the possibility of publication bias evaluated.

(e) Considerations in assessing the body of epidemiological evidence

The ability of the body of epidemiological evidence to inform the Working Group about the cancer-preventive effect of an intervention is related to both the quantity and the quality of the evidence. There is no formulaic answer to the question of how many cancer prevention studies in humans are needed from which to draw inferences about preventive effect, although more than a single study in a single population will almost always be needed.

After the quality of individual epidemiological studies of cancer or of an intermediate outcome has been assessed and the informativeness of the various studies on the association between the intervention and cancer or an intermediate outcome has been evaluated, the body of evidence is assessed and a consensus scientific judgement is made about the strength of the evidence that the intervention under review prevents cancer in humans. In making its judgement, the Working Group considers several aspects of the body of evidence (e.g. Hill, 1965; Rothman et al., 2008; Vandenbroucke et al., 2016).

A strong association (e.g. a large relative risk or a relative risk that is well below 1.0) is more likely to be causal than a weak association, because it is harder for confounding or other biases to create a false strong association. However, it is recognized that estimates of effect of small magnitude do not imply lack of causality and may have a substantial impact on public health if the outcome is common or if the intervention is highly feasible. Estimates of effects of small magnitude can also contribute useful information if the magnitude of the effect correlates with the level of intervention in populations that are differently exposed.

Associations that are consistently observed in several studies of the same design, in studies that use different epidemiological approaches, or under different circumstances of intervention are more likely to indicate preventive efficacy or effectiveness than are isolated observations from single studies. If there are inconsistent results among investigations, possible reasons for such inconsistencies are sought – such as differences in time since initiation of the intervention (latency), intervention levels (e.g. dosage), or assessment

methods – and their implications for the overall findings are assessed.

Results of studies that are judged to be of high quality and highly informative are given more weight than those of studies that are judged to be methodologically less sound or less informative.

Temporality of the association is also an essential consideration, that is, the intervention must precede the outcome. The likelihood of reverse causation (i.e. the outcome prompts the intervention) is greater in observational studies of interventions, which often entail self-reported behaviour change, than in studies of static exposures.

An observation that cancer incidence decreases with increasing exposure to a putative preventive intervention is considered to be an indication of a preventive effect, although the absence of a graded response is not necessarily evidence against a causal relationship, and there are several reasons why the shape of the intervention–outcome association may be non-monotonic (e.g. Stayner et al., 2003).

Confidence in a causal interpretation of the evidence from studies in humans is enhanced if it is coherent with physiological and biological knowledge, including information about target organ exposure to the intervention, characteristics of tumour subtypes, and evidence of biological mechanisms by which the intervention could exert a cancer-preventive effect (see Part B, Section 4b).

The Working Group considers whether or not there are subpopulations with increased susceptibility to the cancer-preventive effects of the intervention. For example, studies that identify inter-individual differences in cancer susceptibility to the intervention on the basis of sociodemographic characteristics (e.g. age, sex, race, ethnicity), other behavioural factors (e.g. smoking or alcohol consumption), genetic polymorphisms, or age at first intervention (e.g. childhood interventions) may contribute to the identification of cancer-preventive interventions in humans. Such studies may be particularly informative if genetic polymorphisms are found to be modifiers of the intervention–outcome relationship, because evaluation of polymorphisms may increase the ability to detect an effect in susceptible subpopulations. Identifying susceptible subpopulations can also improve the specificity of targeting interventions.

2.2 Harms of the intervention

Potential harms to individuals that are linked to the intervention under review are also reviewed. Evidence of harm may come from any type of epidemiological study and may also be reported separately from evidence on the potential beneficial effects of the intervention. Although the *IARC Handbooks* do not formally evaluate the harms associated with an intervention in the way that is done for the benefits, the review of the evidence of harms aims to be as complete, rigorous, and informative as it is for the evidence of beneficial effects.

There are three broad categories of possible harms associated with interventions: (i) biological harm (e.g. toxicity of a chemopreventive agent), (ii) physical harm (e.g. injury associated with increased physical activity), and (iii) psychosocial harm (e.g. community-based interventions and social marketing campaigns specifically targeting obesity; Walls et al., 2011). Evidence of occurrence of biological, physical, and psychosocial harm (including emerging harms identified using qualitative methods in intervention studies) is reviewed and described, and the potential impacts of the harm are discussed.

Known financial harms or opportunity costs (Walls et al., 2011), which can apply at the individual level (e.g. higher cost of healthy foods, impacts of increases in tobacco taxes on smokers of lower socioeconomic status, membership of a weight-loss plan) or the community level (e.g. community-based interventions and campaigns), may be noted.

2.3 Balance of benefits and harms

Ideally, the benefits and harms of primary prevention interventions are expressed in similar terms, such as quality-adjusted life years (QALYs) gained (benefits) or lost (harms) per 1000 individuals of the target population. After identification of all published estimates of the balance of benefits and harms based on the same combination or combinations of intervention and outcome, the Working Group selects those based on the highest-quality evaluative studies of the intervention, critically assesses each, and summarizes the results, in narrative or tabular format as appropriate. The results do not contribute to the overall evaluation of each intervention, but they may be highlighted in the rationale after the evaluation and can be used to aid decisions about implementation of and participation in the relevant primary preventive interventions.

2.4 Cost–effectiveness

For a primary preventive intervention that can deliver a beneficial outcome, cost–effectiveness is usually expressed as the estimated financial cost of implementing the intervention per unit of benefit it delivers, which is most often measured in terms of QALYs gained. The ratio of costs to benefits (i.e. level of cost–effectiveness) needed to implement a health service programme varies from country to country, depending principally on the wealth of the country and on who pays (e.g. the government or individual citizens). Although most primary preventive interventions come at a net cost to health services, some can deliver a gain in QALYs and a reduction in health service cost (Vos et al., 2010). Although assessments of cost–effectiveness that account for all costs (e.g. that are not restricted to health service costs) are less frequently done, it is important to note that their perspective may differ markedly from one based on health service costs only.

Taking a similar approach to that taken for the balance of benefits and harms described above, the Working Group identifies published reports of well-conducted cost–effectiveness analyses based on the highest-quality evaluative studies of the primary preventive intervention, critically assesses each, and summarizes the results, in narrative or tabular format as appropriate. The results do not contribute to the overall evaluation of each intervention, but they may be highlighted in the rationale after the evaluation and can be used by governments and health services to aid decisions about implementation of the intervention for which there is sufficient evidence of a preventive effect. In addition, it is important to note that when the intervention is targeted towards a risk factor for cancer that is also a risk factor for other chronic diseases, any estimate of cost–effectiveness that is based solely on cancer is of limited use for policy purposes.

3. Studies of cancer prevention in experimental animals

(a) Types of study considered

Animal models are an important component of research on cancer prevention. Models are available that enable the evaluation of the effects of interventions on the development or progression of cancer in most major organ sites. Animal models for cancer include: (i) carcinogen-induced (e.g. chemical, physical, or infectious/biological); (ii) genetically engineered; (iii) transplantable systems (e.g. xenograft, organoid); and (iv) spontaneously developing tumours. Most cancer-preventive interventions investigated can be categorized at the biological level as those that: (i) prevent molecules from reaching or reacting with critical target sites; (ii) reduce the sensitivity of target tissues to carcinogens; or (iii) interrupt the evolution of the neoplastic process. There is increasing interest in the use of combinations of interventions as a means

of increasing efficacy and minimizing toxicity; animal models are useful in evaluating such combinations. The development of optimal strategies for intervention in humans can be facilitated by the use of animal models that mimic the neoplastic process in humans. The questions posed below (modified from Lewis et al., 2017) may assist in determining the relevance of individual studies in experimental animals to the evaluation of cancer-preventive effects in humans:

- Are the timing, route, level, and frequency of exposure comparable with those in humans, after accounting for relevant species differences?
- Is the cancer that is induced (i.e. by a biological, physical, or chemical agent, or genetic manipulation) relevant to the cancer in humans?
- Is the time at which the outcome is assessed relevant and justified?
- Does the study explore only mechanisms or pathways of cancer development?
- Is the outcome measure cancer incidence or progression rather than surrogate measures of tumour activity, such as tumour size or number of tumours?
- Do the outcome measures mimic those being evaluated in humans? More specifically, does the tumour mimic the human disease in terms of the organs or tissues affected, and at the histopathological or genetic level? Does the progression of the disease mimic the cancer in humans?

Relevant studies of cancer in experimental animals are identified using principles of systematic review as described in Part A and further detailed in the Instructions for Authors provided to each Working Group. Consideration is given to all available long-term (i.e. lifetime or near-lifetime) studies of cancer in experimental animals with the intervention under review and, when appropriate, related interventions (see Part A, Section 7). After a thorough evaluation of the pertinent study features (see Part B, Section 3b), studies judged to be irrelevant or inadequate according to the criteria determined in consultation with the Working Group may be excluded. Guidelines for conducting and reporting studies in experimental animals have been published (e.g. OECD, 2018; Percie du Sert et al., 2018).

(b) Study evaluation

Important considerations for assessing study quality include: (i) whether the intervention under review was clearly characterized; (ii) whether the intervention exposure or dose was characterized and monitored adequately; (iii) whether the control animals, exposure doses, duration of dosing, timing and frequency of dosing, duration of observation, and route of exposure to the intervention were appropriate; (iv) whether appropriate experimental animal species and strains were evaluated, including appropriate sex and age; (v) whether there were adequate numbers of animals per group; (vi) whether animals were allocated randomly to groups; (vii) whether all experimental conditions, with the exception of the tested intervention, were identical between the groups; (viii) whether the histopathology review was adequate; and (ix) whether the data were analysed correctly and reported according to well-accepted standards (e.g. Percie du Sert et al., 2018).

Specific factors to be considered in interpreting the results of cancer prevention experiments include: (i) the timing of the intervention over the course of the animals' lifespan; (ii) the timing and duration of administration of the intervention in relation to any carcinogen administration; (iii) dose–response effects; (iv) the site specificity of the anticipated cancer-preventive outcome; (v) the spectrum and relevance of the preventive outcome, from pre-neoplastic lesions to invasive cancers; (vi) the incidence, latency, and magnitude of the outcome, and the multiplicity

of the relevant neoplasms and/or other lesions; and (vii) the number and structural diversity of experimental or environmental exposures, and carcinogenic mechanisms underpinning the animals' baseline risk of the cancer to which the intervention was targeted. In addition, because administration of an intervention may result in prevention of tumours at one site but unintended consequences at other sites, it is important that multiple organs are examined in animal experiments.

Because certain factors, including diet, food or water consumption, infection, and stress, may modulate cancer risk, consideration should be given to the potential for interaction between these factors and the intervention being studied.

(c) Statistical considerations

The statistical methods used should be clearly stated and should be the generally accepted techniques refined for this purpose (Peto et al., 1980; Gart et al., 1986; Portier & Bailer, 1989; Bieler & Williams, 1993). An appropriate unit of analysis should be used (e.g. cage or individual animal in feed studies). The statistical methods should reflect the outcomes of the study (e.g. tumour incidence or multiplicity, or overall survival of the animals). For outcomes other than survival, the potential influence of different overall survival time between exposed and unexposed animals should be considered.

4. Mechanistic evidence and other relevant biological data

For a rational implementation of cancer-preventive measures, it is important not only to assess preventive end-points but also to understand the mechanisms by which the intervention exerts its cancer-preventive action. Mechanistic studies derived from human research and complemented by experimental models support cancer prevention research in humans by providing critical insight into the biological processes that can mediate the relationship between an intervention and a cancer outcome. Studies of mechanisms provide evidence for biological plausibility, inform causality, and can identify biomarkers relevant to the carcinogenic process. The study of mechanistic biomarkers can provide insights into human heterogeneity in response to carcinogens according to age, sex, genetic background, and other variables that are important to the application of cancer-preventive interventions in human populations. This array of possible contributions by mechanistic studies means that outcomes and end-points will vary widely depending on the types of intervention and the specific types of cancer examined in each *Handbook*.

Mechanistic studies and data are identified, screened, and evaluated for quality and human relevance using principles of systematic review, as described in Part A and further elaborated in the Instructions for Authors provided to each Working Group, and as detailed below.

(a) Types of studies considered

This section focuses primarily on studies in humans, including intervention trials and longitudinal studies with cancer-relevant biomarkers that may serve as exposure or intermediate end-points. Data from relevant experimental models may also be incorporated, especially when data from studies in humans are limited or are not practical to obtain.

(b) Evidence of cancer prevention

Possible mechanisms of action of interventions aiming at cancer prevention may include, but are not limited to: (i) altering the absorption, distribution, metabolism, and excretion of a known cancer-promoting or cancer-preventive agent; (ii) reducing endogenous DNA damage (e.g. by decreasing the oxidative stress and DNA–protein cross-links) or activating DNA repair or modulating epigenetic mechanisms;

(iii) altering host physiology, such as the endocrine environment (e.g. by modulation of exogenous ligands, including hormones) or the microbiome; (iv) affecting cell biology to reduce a cell's susceptibility to transformation, initiation, and progression of tumorigenesis (e.g. by regulating cell differentiation, proliferation, migration, invasion, and cell death through apoptosis and senescence); and (v) modifying the tumour microenvironment, including the inflammatory and immune responses. Inter-individual variations in these responses or outcomes associated with host factors such as age, sex, race/ethnicity, and genetic heterogeneity (e.g. metabolic polymorphisms) are also considered.

In the case of potentially chemopreventive agents, studies of absorption, distribution, metabolism, and excretion in humans and other mammalian species are summarized. The metabolic fate of the intervention agent is described, noting the metabolites that have been identified and their reactivity. A metabolic schema may indicate the relevant metabolic pathways and products, and whether supporting evidence is derived from studies in humans, in experimental animal systems, or in in vitro models. When available, physiologically based pharmacokinetic models and their parameter values are included.

(c) Harms of the preventive intervention

Any intervention that has putative beneficial effects must be assessed for potential harms. Toxic and other potentially harmful effects of a cancer-preventive intervention that are observed in studies in humans or studies in experimental animals and that might predict harmful effects in humans are reviewed, and the relevant evidence about them is summarized.

(d) Study quality and evidence synthesis

The Working Group summarizes the studies, with an emphasis on characterizing consistencies or differences in results within and across studies of varying experimental designs and model systems. Based on considerations of the quality of the studies (e.g. design, methods and reporting of results, as described in Part B, Section 3b) and relevance to humans, the Working Group may give greater weight to some included studies.

Evaluation of the results of studies in humans includes consideration of study quality, as discussed in Part B, Section 2. For observational and other studies of mechanisms of cancer prevention in humans, the quality of the study design, the intervention exposure assessment, and the accuracy (validity and precision) of the biomarker measurement are considered, as are other important factors, including those described for the evaluation of studies of cancer prevention in humans (Vermeulen et al., 2018). Specific guidelines to assess the quality of molecular biomarker and genetic studies are given in STROBE-ME (Gallo et al., 2011) and STREGA (Little et al., 2009), respectively.

In addition to studies in humans, mechanistic insights may be complemented by studies in experimental systems, including animal models (Le Magnen et al., 2016) and in vitro studies. Important considerations for in vitro studies include the ability of the system to recapitulate the carcinogenic process that occurs in humans and to model the exposure of the intervention as would be experienced in vivo (Lewis et al., 2017; Gordon et al., 2018).

The synthesis is focused on the evidence that is most informative for the overall evaluation. Evidence from several streams of mechanistic data, especially those from studies in humans, can strengthen mechanistic conclusions.

5. Summary of data reported

(a) Intervention characterization

The nature of the intervention and its characteristics, common use, and implementation in different settings, including geographical patterns and time trends, are summarized as

appropriate depending on the intervention under review. Intervention assessment methods used in key epidemiological studies reviewed by the Working Group, their strengths, and their limitations are also summarized.

(b) Cancer prevention in humans

Results of epidemiological studies pertinent to an evaluation of the cancer-preventive effects of the interventions and their harms in humans are summarized. The overall strengths and limitations of the epidemiological evidence are highlighted to indicate how the evaluation was reached. The target organ(s) or tissue(s) in which a decrease in cancer occurrence was observed are identified. Intervention–outcome associations and other quantitative data may be summarized when available. When the available epidemiological studies pertain to a mixed intervention (e.g. fruits and vegetables), the Working Group may seek to identify the specific agent or group of agents most likely to be responsible for any cancer-preventive effect. The evaluation is focused as narrowly as is appropriate or as the available data permit. Summaries of the evidence on the balance of benefits and harms and on cost–effectiveness are also provided.

(c) Cancer prevention in experimental animals

Results pertinent to an evaluation of a cancer-preventive effect in animals are summarized to indicate how the evaluation was reached. For each animal species and study design, it is stated whether or not changes in overall survival or tumour incidence, latency, severity, or multiplicity were observed, and the tumour sites are indicated. Dose–response patterns are also summarized. Possible harms of the intervention are noted.

(d) Mechanistic and other relevant data

Results pertinent to mechanisms of cancer prevention are summarized. The summary encompasses the informative studies on cancer-preventive mechanisms with adequate evidence for evaluation, and on any other aspects of sufficient importance to affect the overall evaluation. High-quality studies in humans, when available, are prioritized. In addition, supporting findings from experimental animal models or in vitro systems are summarized, especially when data from studies in humans are limited.

6. Evaluation and rationale

Evaluation of the evidence is guided by an analytical framework that depicts the relationships among the population, intervention, comparator, and outcomes (including both benefits and harms), and key contextual issues related to adherence to and implementation of the intervention and its impact on population health. The analytical framework may articulate both direct pathways (the intervention has a direct effect on cancer outcomes) and indirect pathways (the intervention has an effect on an intermediate outcome that has an established causal or preventive association with cancer incidence).

Consensus evaluations of the strength of the evidence of cancer-preventive effects of the intervention in humans, in experimental animals, and in mechanistic studies are made using transparent criteria and defined descriptive terms (see below). The Working Group then develops a consensus overall evaluation of the strength of the evidence that the intervention under review prevents cancer and assigns the intervention to one of four categories (see below).

When the Working Group has reviewed multiple, closely related interventions (e.g. different forms of an intervention on the same presumed cause of cancer), they may be grouped together for the purpose of a unified evaluation

of the strength of the evidence that they prevent cancer.

The framework for these evaluations, described below, may not encompass all factors relevant to a particular evaluation of preventive effect. After considering all relevant scientific findings, the Working Group may, exceptionally, assign the intervention to a different category from the one that a strict application of the framework would indicate, while providing a clear rationale for the overall evaluation reached.

When there are substantial differences of scientific interpretation among the Working Group members, the overall evaluation will be based on the consensus of the Working Group. A summary of the alternative interpretations may be provided, together with their scientific rationale and an indication of the degree of support for each.

The evaluation categories refer to the strength of the evidence that an intervention can prevent cancer in humans. Consideration may be given to how strongly or weakly the intervention can prevent cancer. In addition, actual and potential harms of the proposed intervention are addressed qualitatively and quantitatively, as the evidence base permits.

In what follows, the term "cancer prevention" refers to the **outcome of a *Handbooks* evaluation**, that is, to a **cancer outcome** or an **intermediate outcome**, as defined in the analytical framework. Thus, the wording of these evaluations is the same when an intermediate outcome, not cancer itself, is the outcome studied. As noted above, evaluation of an intermediate outcome is performed only when the intermediate outcome has an established causal or preventive association with cancer incidence.

(a) Cancer prevention in humans

Cancer-preventive effects in humans are evaluated on the basis of the principles outlined in Part B, Section 2. The evidence relevant to cancer prevention in humans is classified into one of the following categories:

Sufficient evidence of cancer prevention in humans: A causal preventive association between the intervention and cancer in humans has been established. That is, a cancer-preventive association has been observed consistently in the body of evidence (including several high-quality studies) and chance, bias, and confounding as causes of this association were ruled out with reasonable confidence.

Limited evidence of cancer prevention in humans: A causal preventive association between the intervention and cancer in humans is plausible. That is, a cancer-preventive association has been observed in the body of evidence, but chance, bias, or confounding as causes of this association could not be ruled out with reasonable confidence.

Inadequate evidence of cancer prevention in humans: The current body of evidence does not enable a conclusion to be drawn about the presence or absence of a preventive association between the intervention and cancer in humans. Common situations that lead to a determination of *inadequate evidence of cancer prevention in humans* include: (a) no data are available in humans; (b) there are studies available in humans, but of poor quality or informativeness; and (c) there are studies available in humans of sufficient quality, but their results are inconsistent or otherwise do not enable a conclusion to be drawn.

Evidence suggesting lack of cancer prevention in humans: There are several high-quality studies covering, through direct or indirect pathways, the full range of levels of the intervention that humans are known to encounter that are mutually consistent in not showing a preventive association between the intervention and the studied cancers at any observed level of intervention. The results from these studies alone or in combination had narrow confidence intervals with their upper bounds above or close to the

null value (e.g. a relative risk of 1.0). Similarly, bias and confounding as possible causes of this null result were ruled out with reasonable confidence, and the studies were considered informative. A conclusion of *evidence suggesting lack of cancer prevention in humans* is limited to the cancer sites, populations, life stages, conditions and levels of intervention, and length of observation covered by the pertinent studies. The target organ(s) or tissue(s) where evidence suggesting of lack of cancer prevention was observed in humans are identified.

(b) Cancer prevention in experimental animals

Cancer-preventive effects in experimental animals are evaluated on the basis of the principles outlined in Part B, Section 3. The evidence relevant to cancer prevention in experimental animals is classified into one of the following categories:

Sufficient evidence of cancer prevention in experimental animals: A preventive association has been established between the intervention and increased cancer-related survival, decreased incidence, increased latency, and/or decreased multiplicity of malignant neoplasms or of an appropriate combination of benign and malignant neoplasms in several independent, high-quality studies and model systems.

Limited evidence of cancer prevention in experimental animals: The data suggest a preventive association between the intervention and cancer in experimental animals. That is, an association has been observed but the data are limited for making a definitive evaluation because: (a) the evidence of a cancer-preventive association is based on only a few high-quality studies; (b) the intervention decreases incidence, increases latency, and/or decreases multiplicity only of benign neoplasms; or (c) there are unresolved questions about the adequacy of the design, conduct, or interpretation of the available studies.

Inadequate evidence of cancer prevention in experimental animals: The studies cannot be interpreted as showing the presence or absence of a preventive association between the intervention and cancer in experimental animals because of major qualitative or quantitative limitations of the data available, or no data are available on cancer in experimental animals.

Evidence suggesting lack of cancer prevention in experimental animals: Evidence from high-quality studies in several experimental models shows that, within the limits of the tests used (e.g. tumour site, age at intervention, conditions and levels of intervention tested), the intervention has no preventive association with cancer in experimental animals.

(c) Mechanistic evidence

Mechanistic studies are evaluated on the basis of the principles outlined in Part B, Section 4. The mechanistic evidence is classified into one of the following categories:

Strong mechanistic evidence: There are a substantial number of high-quality studies in humans that consistently link the intervention to a mechanistic pathway by which it could prevent cancer.

Limited mechanistic evidence: The evidence from mechanistic data in humans is suggestive of a cancer-preventive effect of the intervention, but (a) there are a limited number of high-quality studies, or (b) the studies cover a narrow range of experiments or relevant end-points, or (c) there are some inconsistencies in studies of similar design, or (d) there is unexplained incoherence across studies of different end-points, or (e) the available data are limited to studies in experimental model systems.

Inadequate mechanistic evidence: The evidence from mechanistic data in both humans and experimental model systems is lacking, or the data are inconsistent in linking the intervention to any mechanistic pathway by which it could prevent cancer.

(d) Overall evaluation

Finally, the body of evidence is considered as a whole. Overall evaluation of the intervention is a matter of scientific judgement that reflects the strength of the evidence derived from the studies reviewed. The levels of evidence from studies in humans, mechanistic data, and studies in experimental animals are weighed into the overall evaluation, and statements are made about cancer prevention in humans with the wording of one of the standard categories as described below.

One of the two overall evaluation scenarios (see Part A, Section 3.1) will apply, depending on the nature of the evidence that has been reviewed (Table 4; see also Part A). If, for logistic reasons, evidence for Step 1 and Step 2 of Scenario 2 has been reviewed at two separate *Handbooks* meetings, no overall evaluation will be made for Step 2 alone.

None of these evaluations quantify the fraction of the burden of a particular cancer that a specific intervention would prevent; thus, some interventions may prevent a small fraction of the cancer, some may prevent a larger fraction, and these fractions may vary across populations, for example as a function of the prevalence of the relevant risk factors.

Overall evaluation categories

(i) The intervention *is established to prevent cancer in humans* (Group A)

This category is used for interventions for which there is *sufficient evidence* of cancer prevention in humans, either directly (Scenario 1) or in two steps (Scenario 2): from the intervention to the intermediate outcome (Step 1) and from the intermediate outcome to cancer (Step 2).

The organ sites on which the evidence in humans is based are stated here. A statement is also made of what the Working Group considers to be the magnitudes of the benefits and the harms of the intervention, in as nearly comparable terms as possible, for people adhering to the intervention as commonly implemented in practice, and whether or not the benefits outweigh the harms.

(ii) The intervention *probably prevents cancer in humans* (Group B1)

In Scenario 1, this category is used for interventions for which there is *limited evidence* of cancer prevention in humans and either *strong mechanistic evidence* in humans or *sufficient evidence* in experimental animals with all the criteria for the relevance to humans being met (see Part B, Section 3a).

In Scenario 2, this category is used for interventions for which there is *sufficient evidence* in humans that the intervention has a cancer-preventive effect on the intermediate outcome (Step 1), *limited evidence* that the intermediate outcome has a cancer-preventive effect in humans (Step 2), and either *sufficient evidence* in experimental animals with all the criteria for the relevance to humans being met or *strong mechanistic evidence* in humans (see Part B, Section 3a). Alternatively, this category is used when there is *limited evidence* in humans that the intervention has a cancer-preventive effect in the intermediate outcome (Step 1) and *sufficient evidence* that the intermediate outcome has a cancer-preventive effect in humans (Step 2).

(iii) The intervention *possibly prevents cancer in humans* (Group B2)

In Scenario 1, this category is used for interventions for which there is *limited evidence* of cancer prevention in humans, *less than strong evidence* from mechanistic data, and *less than sufficient evidence* of cancer prevention in experimental animals.

In Scenario 2, this category is used when (i) there is *sufficient evidence* in humans that the intervention has a cancer-preventive effect on the intermediate outcome (Step 1), and *limited evidence* in humans and *less than sufficient evidence* in experimental animals or *less than strong evidence* from mechanistic data that the intermediate outcome has a cancer-preventive

Table 4 Summary of the strength of the evidence in each evidence stream contributing to the overall evaluation

Scenario 1: Direct evidence that the intervention prevents cancer			
Strength of the evidence that the intervention prevents cancer in humans	**Strength of the evidence from mechanistic studies that the intervention prevents cancer**	**Strength of the evidence that the intervention prevents cancer in experimental animals**	**Overall evaluation**
Sufficient	–	–	Group A
Limited	*Strong*	–	Group B1
Limited	–	*Sufficient*	Group B1
Limited	*Less than strong*	*Less than sufficient*	Group B2
Inadequate	–	–	Group C
Evidence suggesting lack of cancer prevention	–	*Evidence suggesting lack of cancer prevention*	Group D

Scenario 2: Evidence that the intervention prevents cancer by way of an intermediate outcome (risk factor or preventive factor)			
Step 1	**Step 2**[a]		**Overall evaluation**[a]
Strength of the evidence that the intervention decreases exposure to the risk factor or increases exposure to the preventive factor in humans	**Strength of the evidence that decreasing exposure to the risk factor or increasing exposure to the preventive factor prevents cancer in humans**	**Strength of the evidence that decreasing exposure to the risk factor or increasing exposure to the preventive factor prevents cancer in experimental animals or mechanistic studies**[b]	
Sufficient	*Sufficient*[c]	–	Group A
Sufficient	*Limited*	*Sufficient*	Group B1
Sufficient	*Limited*	*Less than sufficient*	Group B2
Limited	*Sufficient*	–	Group B1
Limited	*Limited*	–	Group B2
Inadequate	–	–	Group C
–	*Evidence suggesting lack of cancer prevention*	*Evidence suggesting lack of cancer prevention*	Group D
Evidence suggesting lack of cancer prevention	–	–	Group D

[a] This overall evaluation applies only when evidence from both Step 1 and Step 2 is available. When a *Handbook* evaluates only Step 2, no overall evaluation is made.

[b] Evidence in experimental animals and mechanistic data is considered to be *sufficient* when there is *strong evidence* from mechanistic data (mechanistic studies in humans) or *sufficient evidence* in experimental animals.

[c] The evidence in this category may be considered to be *sufficient* when it is based on observational studies of change in cancer incidence associated with self-reported or observed (by way of time-separated repeated measures) change in the level of a risk factor or preventive factor (e.g. smoking cessation; increase in consumption of fruits and vegetables), OR, exceptionally, studies of variation in cancer incidence with the level of a risk factor or preventive factor measured at one time point.

effect; OR (ii) there is *limited evidence* in humans that the intervention has a cancer-preventive effect on the intermediate outcome (Step 1), and *limited evidence* in humans that the intermediate outcome has a cancer-preventive effect, and any evidence category in experimental animals and mechanistic data.

When the evidence is classified in Group B1 or Group B2, the evaluation is followed by a description of harms, actual and potential.

(iv) The intervention *is not classifiable as to its capacity to prevent cancer in humans* (Group C)

In both Scenario 1 and Scenario 2, this category is used for interventions for which there is *inadequate evidence* in humans, irrespective of the level of evidence from mechanistic data and studies in experimental animals. Interventions that do not fall into any other category are also placed in this category.

(v) The intervention *probably does not prevent cancer in humans* (Group D)

In Scenario 1, this category is used for interventions for which there is *evidence suggesting lack of cancer prevention* both in humans and in experimental animals. In Scenario 2, this category is used when there is *evidence suggesting lack of cancer prevention* both in humans and in experimental animals for the intermediate outcome to cancer, irrespective of the level of evidence for the intervention to the intermediate outcome; or there is *evidence suggesting lack of cancer prevention* for the intervention to the intermediate outcome, irrespective of the level of evidence for the intermediate outcome to cancer.

(e) Rationale

The reasoning that the Working Group used to reach its evaluation is summarized so that the basis for the evaluation offered is transparent. It includes concise statements of the principal line or lines of argument that emerged in the deliberations of the Working Group, the conclusions of the Working Group on the strength of the evidence for each stream, an indication of the body of evidence that was pivotal to these conclusions, and an explanation of the reasoning of the Working Group in making evaluations.

In the rationale, the Working Group may draw attention to the fact that actions on the evaluations should be taken in the light of country- or setting-specific circumstances that influence the public health priority, feasibility, and acceptability of programmes based on the interventions evaluated.

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GENERAL REMARKS

The *IARC Monographs* programme classified alcoholic beverages as carcinogenic to humans (Group 1) on the basis of *sufficient evidence* of causality for cancers of the oral cavity, pharynx, larynx, oesophagus (squamous cell carcinoma), liver (hepatocellular carcinoma), colorectum, and female breast in humans (IARC, 1988, 2010, 2012). In 2020, an estimated 741 300 new cancer cases, or 4.1% of all new cancer cases globally, were attributable to alcohol consumption (Rumgay et al., 2021). Overall, the health consequences of alcoholic beverage consumption are substantial. In 2019, alcohol consumption was responsible for an estimated 115.9 million disability-adjusted life years lost, or 4.6% of all disability-adjusted life years lost globally, across 31 communicable and noncommunicable disease categories and injury types (WHO, 2024a).

In response to these harms, in 2010 the World Health Organization (WHO) adopted the Global Strategy to Reduce the Harmful Use of Alcohol, which outlines 10 key policy areas (WHO, 2010). More recently, the Global Alcohol Action Plan 2022–2030 (WHO, 2024b) was adopted to promote stronger implementation of cost-effective, high-impact interventions. These interventions include the five priority actions to reduce alcohol-related harm described in the SAFER technical package (WHO, 2023). *IARC Handbooks* Volume 20 complements the global strategy by exploring the potential effects of alcohol policy interventions on cancer incidence.

Rationale for *IARC Handbooks* Volumes 20A and 20B

There is no empirical evidence directly linking population-level alcohol policy interventions with cancer incidence. The associations of alcohol policy interventions with cancer mortality were assessed in three studies (Jiang et al., 2019; Alattas et al., 2020; Díaz et al., 2024). In a study in Australia, liquor licence liberalization in the 1960s was associated with higher total cancer mortality rates among men and, to a lesser extent, among women; the lowering of the minimum legal drinking age in the 1970s was not associated with total cancer mortality rates among either men or women, and the introduction of random breath testing programmes after 1976 was associated with lower total cancer mortality rates among men and among women (Jiang et al., 2019). In a study in the USA, a 10% increase in the restrictiveness of alcohol policies (as measured by a validated alcohol policy restrictiveness scale) was associated with an 8.5% decrease in risk of death from six cancer types, although the inclusion of prostate cancer – a cancer type that is not causally linked to alcohol – and the exclusion of colorectal and

oesophageal cancers weakens these findings (Alattas et al., 2020). A global study of 169 countries also found an inverse association between alcohol policy preparedness (as measured by an index of the existence of alcohol-related public policies) and cancer mortality rates (Díaz et al., 2024). Importantly, these studies assessed associations with cancer mortality, not incidence, and the associations may be confounded by favourable trends in screening and improvements in treatments, limiting causal inference.

Following a request by and in collaboration with the WHO Regional Office for Europe, the *IARC Handbooks of Cancer Prevention* programme developed a two-part volume evaluating the evidence that alcohol policy interventions reduce alcohol-related cancer risk. The analytical framework for the review and evaluation of such evidence follows Scenario 2, as described in the Preamble to the *IARC Handbooks* for Primary Prevention (IARC, 2019). Scenario 2 involves a two-step process. In Step 1, the effect of a specified intervention on the change in an intermediate outcome (e.g. exposure to a risk factor) is evaluated. In Step 2, the effect of the change in the intermediate outcome (e.g. decrease in exposure to the risk factor) on cancer incidence in humans is evaluated (Fig. 1A). The Preamble further stipulates that if Step 2 has not yet been established from authoritative sources, then Step 2 should be conducted first.

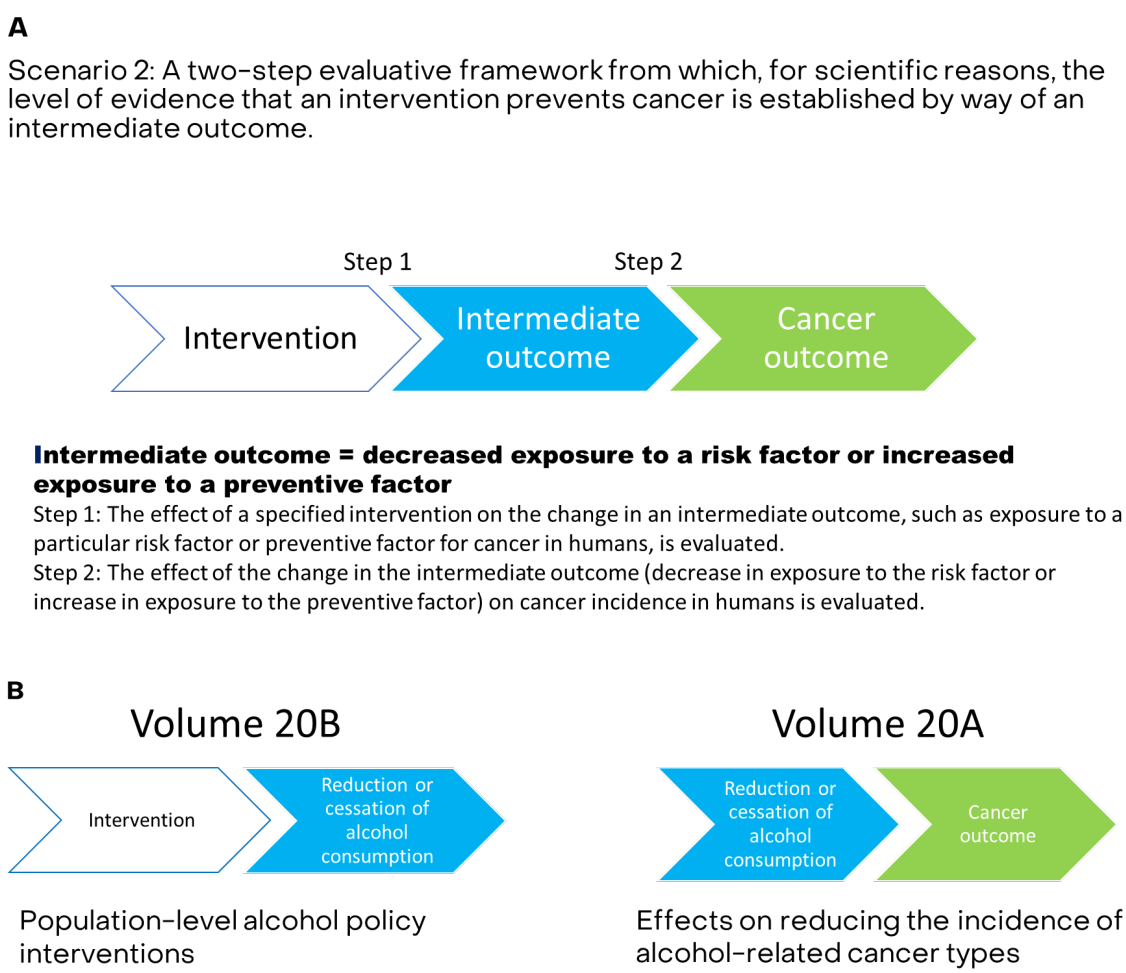
Content of *IARC Handbooks* Volumes 20A and 20B

For *IARC Handbooks* Volume 20A, a Working Group of 15 scientific experts reviewed and evaluated the epidemiological and mechanistic evidence that reduction or cessation of alcoholic beverage consumption reduces the risk of each of the seven established alcohol-related cancer types (Step 2, Fig. 1B) (Gapstur et al., 2023; IARC, 2024). The Working Group concluded that there is *sufficient evidence* that, compared with continuing consumption, reduction or cessation of alcoholic beverage consumption reduces the risk of oral cavity cancer and oesophageal cancer. For laryngeal cancer, colorectal cancer, and female breast cancer, the evidence that reduction or cessation of alcoholic beverage consumption reduces risk was *limited*, and for pharyngeal cancer and liver cancer the evidence was *inadequate*. In addition, on the basis of *strong evidence* for three biological mechanisms, there is *sufficient evidence* from mechanistic studies that cessation of alcohol consumption reduces alcohol-related carcinogenesis.

On the basis of these evaluations, Step 1 of Scenario 2 was warranted. In *IARC Handbooks* Volume 20B, a Working Group of 20 scientific experts reviewed and evaluated the evidence on the effects of selected population-level alcohol policy interventions on reducing alcoholic beverage consumption (Step 1, Fig. 1B). The interventions evaluated were selected because their implementation aims to reduce total alcohol consumption at the national or subnational level, which has been shown to effectively reduce alcohol-attributable harms (Babor et al., 2023). Consistent with the scope of the *IARC Handbooks* and as explained in the Preamble, the Working Group did not quantify the individual or synergistic effects of the interventions on alcohol consumption.

In this volume, the evidence on the effectiveness of health care-based interventions to reduce alcohol consumption is summarized but not evaluated, because such interventions target individuals and their potential population-level effects on consumption are not usually measured. WHO has noted that the "provision of health service responses to substance use disorders is one of the key pillars of societal efforts to reduce the health and social burden of psychoactive substance use" (WHO, 2024a). Specifically for cancer, the Working Group identified only

Fig. 1 (A) The *IARC Handbooks* analytical framework for review of the evidence for primary prevention; (B) *IARC Handbooks* Volumes 20A and 20B



one study in which the associations of alcohol rehabilitation or abstinence with the risk of alcohol-related cancers were assessed (Schwarzinger et al., 2024). In that study, based on data from the French National Hospital Discharge database, the incidence of each of the seven alcohol-related cancer types, separately and combined, was lower for people who underwent rehabilitation or had a history of abstinence compared with those with alcohol dependence without rehabilitation or a history of abstinence. These results suggest the potential importance of health care-based interventions in reducing cancer incidence.

Evaluation of alcohol policy interventions – a convergence of many disciplines

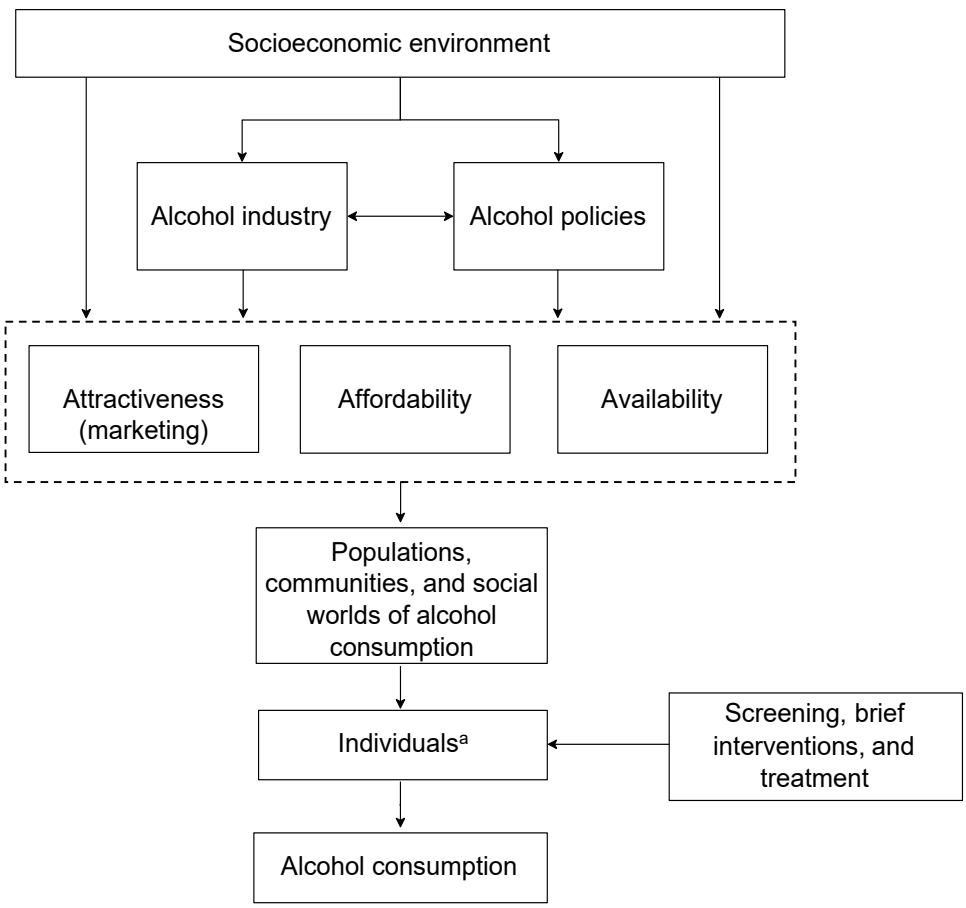
This volume of the *IARC Handbooks* is the result of collaboration among experts representing a broad range of disciplines, including

public policy, law, economics, behavioural health, communication, and public health. Some of the terminology used by these disciplines varies. For example, a study that compares differences in exposures or other factors among large groups (or populations) of people using aggregate-level data may be referred to as an ecological study in epidemiology and as a macro-level study in some other disciplines. To effectively discuss and interpret the body of evidence reviewed in this *IARC Handbooks* volume, the Working Group addressed the variation in terminology by finding common vocabulary where possible (e.g. using common terms for different types of study designs) and defining specific terms in the text or in the Glossary.

Socioeconomic context

Scientific evaluation of the evidence on the effects of population-level alcohol policy interventions on alcoholic beverage consumption requires an understanding of the influences of the socioeconomic environment on individual behaviours and, ultimately, health outcomes. In the conceptual model shown in Fig. 2, the

Fig. 2 Conceptual model of influences on alcohol consumption



[a] Individuals also includes individual-level factors that influence alcohol consumption (e.g. age, genetics).
Prepared by the Working Group.

socioeconomic environment encompasses the non-medical factors and conditions in which people are born, grow up, work, live, and age. These determinants include a wide set of forces and systems, i.e. political systems, economic policies and systems, development agendas, social norms and policies, and commercial determinants (i.e. private sector activities and the environment in which commerce takes place), that directly or indirectly affect the health of the population. Indeed, civil society, governments, and the alcohol industry each play a part in influencing alcohol consumption. Furthermore, the alcohol industry is very innovative, takes advantage of rapidly changing conditions, and drives change through product development and marketing at a faster pace than policy-makers can typically act or respond. As discussed in detail in the Final Considerations (Section 9) of this volume, some of the evidence that was reviewed and evaluated is based on research that predates the current, and rapidly evolving, era of algorithmically targeted, data-driven alcohol marketing and online retail sales and home delivery.

Industry-funded research

As discussed by Babor et al. (2023), "The alcohol industry comprises a nexus of actors who control the essential commercial processes of production, marketing, and supply. Their practices determine, in large part, trends in consumption and harm, and collectively they exercise considerable influence in the policy arena." Because of this influence, research or researchers that have received support from alcohol industry interests should be appropriately disclosed in the studies. These disclosures have not been consistently reported, which can contribute to policy-maker confusion, because the research base can sometimes look more mixed in its conclusions than when industry-funded studies are clearly identified (for further discussion on this topic, see Golder and McCambridge, 2021; Bartlett and McCambridge, 2024). Where the information was available, the Working Group indicated support of a study or researchers by alcohol industry interests.

Selection of the studies

As described in the analytical framework for *IARC Handbooks* Volumes 20A and 20B (Fig. 1A and 1B), the intermediate outcome is narrowly focused on a reduction in (or cessation of) alcoholic beverage consumption. A comprehensive assessment of the evidence that alcohol policy interventions reduce alcohol-related harms or increase life expectancy is beyond the scope of this volume. However, for a limited number of alcohol policy interventions that were evaluated, there was a paucity of studies of their effects on changes in the directly assessed alcoholic beverage consumption outcomes. To address this evidence gap, the Working Group reviewed and evaluated studies of changes in selected proxy health outcomes (e.g. liver cirrhosis mortality; Dills and Miron, 2004) if an association between total alcohol consumption and the proxy outcome was previously established.

Similarly, based on the Preamble, the interventions to be evaluated should be of a restrictive nature, with the aim to reduce alcoholic beverage consumption. However, for some interventions there was a paucity of such studies. To address this evidence gap, the Working Group also included studies that assessed the effects of permissive alcohol policy interventions (e.g. lowering the minimum legal purchase or drinking age) on change in alcohol consumption or its proxy outcomes. This is consistent with liberalization of some alcohol policies that has occurred in many countries in recent decades.

In both cases, such evidence, although somewhat indirect, remains informative for understanding the effects of alcohol policy interventions on alcoholic beverage consumption in the population. The specific eligibility criteria for the studies reviewed are discussed in more detail in each section of this volume.

In reviewing the literature, the Working Group noted that the terms used to characterize drinking status (e.g. lifetime abstinence, recent abstinence, current drinking), the amount of consumption (e.g. light, moderate, heavy), and the amount of pure alcohol (i.e. ethanol) in a single alcoholic drink have been defined differently among different studies, in different settings, and between men and women, and have changed over time. In a report from the WHO Regional Office for Europe, drinking status was categorized as abstinence over the lifetime, abstinence in the previous 12 months (i.e. former drinking), and current consumption. The amount of alcohol consumed was categorized as moderate (≤ 2 drinks per day), risky (3–6 drinks per day), and heavy (> 6 drinks per day), where a single drink contains 10 g of pure alcohol (WHO Regional Office for Europe, 2020). However, because of the variability among studies reviewed, these terms are specifically defined in each section and, when appropriate, for each study.

Global support for alcohol policies

This volume reflects a focused evaluation of the evidence on the effects of alcohol policy interventions on alcoholic beverage consumption. Political decision-makers and regulators are often more interested in reductions in alcohol-related harms. Therefore, it is critically important to acknowledge that the selected population-level interventions evaluated may have health and social effects that are measured by outcomes not assessed in this volume, including, for example, beneficial effects on reducing traffic injury and increasing life expectancy (Babor et al., 2023; Rehm et al., 2023). It is also important to acknowledge that some alcohol policy interventions also may have unintended adverse consequences (e.g. increases in organized crime) (Babor et al., 2023), and assessment of the evidence on these outcomes was outside of the scope of this volume. Nonetheless, the evaluations formulated by the Working Group are part of a broader set of evidence, including the cost–effectiveness of interventions, good practices, government experience, and ethical considerations.

Despite considerable efforts to support the development and implementation of national alcohol policies, the percentage of countries that reported having a written national alcohol policy increased only modestly, from 43% in 2008 to 56% in 2019, and there is an "urgent need for action" (WHO, 2024a). The conclusions of this *IARC Handbooks* volume have the potential to support policy-makers, public health professionals, and community advocates in developing and implementing evidence-based alcohol policies, in alignment with WHO strategies and recommendations as articulated in the Global Alcohol Action Plan 2022–2030 (WHO, 2024b) and the SAFER technical package (WHO, 2023).

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LIST OF ABBREVIATIONS

AA	Alcoholics Anonymous
ABC	Alcoholic Beverage Control
ACPI	Alcohol Control Policy Index
APC	total adult alcohol per capita consumption
ARIMA	autoregressive integrated moving average
AUD	alcohol use disorder
AUDIT	Alcohol Use Disorders Identification Test
AUDIT-C	AUDIT-Consumption
CI	confidence interval
CSR	corporate social responsibility
FDA	United States Food and Drug Administration
FMCG	fast-moving consumer goods
GISAH	Global Information System on Alcohol and Health
GP	Green Paper on Alcohol Policy
GST	goods and services tax
HED	heavy episodic drinking
IACPI	International Alcohol Control Policy Index
IARC	International Agency for Research on Cancer
MD	mean difference
MUP	minimum unit price
NMA	network meta-analysis
OECD	Organisation for Economic Co-operation and Development
OR	odds ratio
RAPI	Restrictiveness of Alcohol Policy Index
RCT	randomized controlled trial
RR	relative risk
SBI	screening and brief intervention
SMD	standardized mean difference
TSF	12-step facilitation
VAT	value-added tax
WHO	World Health Organization
XR-NTX	extended-release injectable naltrexone

GLOSSARY

Alcohol use disorder (AUD)	A medical condition characterized by an impaired ability to stop or control alcohol consumption despite adverse social, occupational, or health consequences. The World Health Organization (WHO) International Classification of Diseases, 11th revision (ICD-11) states that "disorders due to use of alcohol" (referred to herein as alcohol use disorders or AUD) are characterized by the pattern and consequences of alcohol use and include the two diagnostic categories of "alcohol dependence" and "harmful pattern of use of alcohol", among others. This term encompasses the conditions that some people refer to as alcohol abuse, alcohol dependence, alcohol addiction, and alcoholism.
Alcohol Use Disorders Identification Test (AUDIT)	A screening tool developed by the World Health Organization (WHO) and used by practitioners to identify individuals with hazardous and harmful patterns of alcohol consumption who would benefit from reducing or ceasing consumption.
Autocorrelation	A measure of the relationship between a variable's current value and its previous values.
Broadcast media	An advertising technique that uses media such as radio and television that reach target audiences using airwaves as the transmission medium.
Cirrhosis	A consequence of chronic liver inflammation that is followed by diffuse hepatic fibrosis, in which the normal hepatic architecture is replaced by regenerative hepatic nodules, which eventually leads to liver failure.
Collinearity	A statistical term that refers to a high correlation between at least two predictor variables (or independent variables) in a regression model. Collinearity can be a concern when interpreting the effect estimates of the predictor variables.
Corporate social responsibility (CSR)	The idea that a business has a responsibility to the society that exists around it. As a business strategy, it consists of philanthropic activities and public relations campaigns designed to improve the public image of a company or to influence political decisions in its favour.
Disorders due to use of alcohol	See "Alcohol use disorder (AUD)".
Endogeneity	A statistical term that refers to a situation where the effect of an independent variable (i.e. an alcohol policy intervention) on a dependent variable (i.e. alcoholic beverage consumption) cannot be causally interpreted because the independent variable is correlated with unexplained variation in the dependent variable.
Government alcohol monopoly	Exists when responsibility for all or part of the alcohol market (i.e. production, sales, wholesale, distribution, exportation, and importation of alcoholic beverages) is allocated to a national or subnational governmental department or authority.
Off-premises outlet	A place that is licensed to sell alcohol in containers to customers who purchase it and then take it somewhere else for consumption.
On-premises outlet	A place that is licensed to sell and serve alcoholic beverages to allow customers to consume alcohol within a specifically designated area where servers can monitor consumption.

Paid media or paid advertising	Any marketing effort that involves paying to display promotional content across social media, search engines, or websites.
Price elasticity of demand	The responsiveness of individuals' demand for alcoholic beverages to changes in specific parameters, including price and income (also referred to as price elasticity).
Recorded alcohol	Alcoholic beverages consumed according to the official statistics at the country level based on production, import, export, and sales or taxation data and intended for consumption.
SAFER	Acronym that stands for the five alcohol policy interventions recommended by the World Health Organization (WHO), which are based on accumulated evidence of their impact on population health and their cost–effectiveness: **S**trengthen restrictions on alcohol availability; **A**dvance and enforce drink-driving countermeasures; **F**acilitate access to screening, brief interventions, and treatment; **E**nforce bans or comprehensive restrictions on alcohol advertising, sponsorship, and promotion; and **R**aise prices on alcohol through excise taxes and pricing policies.
Shared media	A type of marketing where companies create and share content with their customers and followers on social media. This type of marketing is different from paid or owned media because it relies heavily on engagement and interaction with customers.
Social determinants of health	Non-medical factors that influence health outcomes, including economic stability, education, social and community context, health-care access, and the built environment.
Surrogate alcohol	Non-beverage alcohol that is not officially intended for human consumption.
Target marketing	A marketing strategy that breaks a market into segments and then concentrates marketing efforts on one segment or a few key segments consisting of the customers whose needs and desires most closely match a product or service offering.
Total alcohol per capita consumption (APC)	The total amount of alcohol consumed per person (individuals aged ≥ 15 years) over a calendar year, in litres of pure alcohol. The estimate is the sum of the 3-year average of the per capita (≥ 15 years) recorded alcohol consumption and of the per capita (≥ 15 years) unrecorded alcohol consumption for a calendar year, adjusted for tourist consumption. In *IARC Handbooks* Volume 20B, total alcohol consumption specifically refers to consumption of all three major types of alcoholic beverages (i.e. beer, wine, and spirits).
Tourist [alcohol] consumption	Consumption by tourists visiting the country and consumption by inhabitants visiting other countries. Positive figures denote alcohol consumption of outbound tourists being greater than alcohol consumption by inbound tourists, and negative numbers the opposite. Tourist consumption is based on United Nations tourist statistics.
Unrecorded alcohol	Alcohol products that are not taxed and are outside the official system of government control, such as home or informally produced (legal or illegal) alcohol, smuggled alcohol, surrogate alcohol, or alcohol products obtained through cross-border shopping (i.e. recorded in a different jurisdiction).
WHO African Region	Algeria, Angola, Benin, Botswana, Burkina Faso, Burundi, Cabo Verde, Cameroon, the Central African Republic, Chad, the Comoros, the Congo, Côte d'Ivoire, Democratic Republic of the Congo, Equatorial Guinea, Eritrea, Eswatini, Ethiopia, Gabon, The Gambia, Ghana, Guinea, Guinea-Bissau, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritania, Mauritius, Mozambique, Namibia, the Niger, Nigeria, Rwanda, Sao Tome and Principe, Senegal, Seychelles, Sierra Leone, South Africa, South Sudan, Togo, Uganda, the United Republic of Tanzania, Zambia, Zimbabwe.
WHO Eastern Mediterranean Region	Afghanistan, Bahrain, Djibouti, Egypt, Iran (Islamic Republic of), Iraq, Jordan, Kuwait, Lebanon, Libya, Morocco, Oman, Pakistan, Qatar, Saudi Arabia, Somalia, the Sudan, the Syrian Arab Republic, Tunisia, the United Arab Emirates, West Bank and Gaza Strip, Yemen.
WHO European Region	Albania, Andorra, Armenia, Austria, Azerbaijan, Belarus, Belgium, Bosnia and Herzegovina, Bulgaria, Croatia, Cyprus, Czechia, Denmark, Estonia, Finland, France, Georgia, Germany, Greece, Hungary, Iceland, Ireland, Israel, Italy, Kazakhstan, Kyrgyzstan, Latvia, Lithuania, Luxembourg, Malta, Monaco, Montenegro, the Netherlands, North Macedonia, Norway, Poland, Portugal, the Republic of Moldova, Romania, the Russian Federation, San Marino, Serbia, Slovakia, Slovenia, Spain, Sweden, Switzerland, Tajikistan, Türkiye, Turkmenistan, Ukraine, United Kingdom, Uzbekistan.

WHO Global Information System on Alcohol and Health (GISAH) database	The Global Information System on Alcohol and Health (GISAH) is a comprehensive database maintained by the World Health Organization (WHO). It serves as a key resource for monitoring the global status of alcohol consumption, alcohol-related harm, and the effectiveness of alcohol control policies. The database provides data that help researchers, policy-makers, and public health officials track and compare population-level estimates of alcohol consumption among different regions of the world and assess the health impacts of alcohol consumption and the implementation and effectiveness of alcohol policies worldwide.
WHO Region of the Americas	Antigua and Barbuda, Argentina, the Bahamas, Barbados, Belize, Bolivia (Plurinational State of), Brazil, Canada, Chile, Colombia, Costa Rica, Cuba, Dominica, the Dominican Republic, Ecuador, El Salvador, Grenada, Guatemala, Guyana, Haiti, Honduras, Jamaica, Mexico, Nicaragua, Panama, Paraguay, Peru, Saint Kitts and Nevis, Saint Lucia, Saint Vincent and the Grenadines, Suriname, Trinidad and Tobago, Uruguay, the USA, Venezuela (Bolivarian Republic of).
WHO South-East Asia Region	Bangladesh, Bhutan, the Democratic People's Republic of Korea, India, Indonesia, Maldives, Myanmar, Nepal, Sri Lanka, Thailand, Timor-Leste.
WHO Western Pacific Region	American Samoa (USA), Australia, Brunei Darussalam, Cambodia, China, the Cook Islands, Fiji, French Polynesia (France), Guam (USA), Hong Kong Special Administrative Region (China), Japan, Kiribati, the Lao People's Democratic Republic, Macao Special Administrative Region (China), Malaysia, the Marshall Islands, Micronesia (Federated States of), Mongolia, Nauru, New Caledonia (France), New Zealand, Niue, Northern Mariana Islands (USA), Palau, Papua New Guinea, the Philippines, the Pitcairn Islands (United Kingdom), the Republic of Korea, Samoa, Singapore, Solomon Islands, Taiwan (China), Tokelau, Tonga, Tuvalu, Vanuatu, Viet Nam, Wallis and Futuna (France).

1. OVERVIEW OF *IARC HANDBOOKS* VOLUME 20B

1.1 WHO strategy on alcohol policy

The World Health Organization (WHO), in collaboration with other institutions and contributors from Member States, has published a series of systematic analyses and status reports on the global epidemiology of alcoholic beverage consumption, alcohol-related harms, and government actions and recommended policies to reduce those harms.

Based on increasing evidence on the effects of alcohol consumption on disease risk reported in the first Global Burden of Disease studies (Murray and Lopez, 1996; Ezzati et al., 2002; Rehm et al., 2004), in 2005 the World Health Assembly adopted resolution WHA58.26 (WHO, 2005) to strengthen efforts to implement alcohol policies to reduce the health and social burdens caused by alcohol. In 2010, the World Health Assembly endorsed the Global Strategy to Reduce the Harmful Use of Alcohol (resolution WHA63.13) (WHO, 2010), which was developed through extensive consultations with Member States, intergovernmental organizations, and non-state actors. The main purpose of the global strategy was to support and complement public health policies in Member States.

The 2010 global strategy includes policy options and interventions that are grouped into 10 recommended target areas (Box 1.1) (WHO, 2010). Despite the significant value of the strategy to influence Member States with respect to implementing alcohol policy (WHO Regional Office for Europe, 2024), alcohol is the only psychoactive substance without a legally binding international treaty (Babor et al., 2023).

The 2010 global strategy did not specify a quantitative target for the reduction of alcoholic beverage consumption (WHO, 2010). The first global alcohol reduction target was established in the WHO Global Action Plan for the Prevention and Control of Noncommunicable Diseases, which included cancer as one of four disease groups and targeted a 10% reduction in the harmful use of alcohol by 2025, relative to 2010 (WHO, 2011, 2013). However, by 2021, only limited global progress on the reduction of alcohol consumption had been achieved. Therefore, in 2022 the Seventy-fifth World Health Assembly adopted the Global Alcohol Action Plan 2022–2030 (WHO, 2024a), which proposes detailed actions to achieve progress in the 10 recommended target areas of the 2010 global strategy. These proposed actions were specified for all stakeholders, including governments, nongovernmental organizations, academia, and economic operators. Targets also were set to align with the 2015 Sustainable Development Goals Target 3.5 and Indicator 3.5.2 (UN DESA, 2024). One of these global targets is a 20% reduction in the harmful use of alcohol by 2030, also relative to 2010 (WHO, 2024a). An indicator for

Box 1.1 Recommended target areas for policy options and interventions

1. Leadership, awareness, and commitment
2. Health services' response
3. Community action
4. Drink-driving policies and countermeasures
5. Availability of alcohol
6. Marketing of alcoholic beverages
7. Pricing policies
8. Reducing the negative consequences of drinking and alcohol intoxication
9. Reducing the public health impact of illicit alcohol and informally produced alcohol
10. Monitoring and surveillance

Source: WHO (2010).

measuring this target is the total adult alcohol per capita consumption (APC), which is the total amount of alcohol consumed per adult (individuals aged ≥ 15 years) over a calendar year, in litres of pure alcohol; it is calculated as the sum of the 3-year average of the per capita (≥ 15 years) recorded alcohol consumption and the estimated 3-year average of the per capita (≥ 15 years) unrecorded alcohol consumption for a calendar year, adjusted for tourist consumption (see the Glossary and Rehm et al., 2020).

A second global target stipulates that by 2030, 70% of Member States will have introduced, enacted, or maintained the implementation of high-impact alcohol policy options and interventions (WHO, 2024a), including three "best buys" for reducing alcohol-related harm (WHO, 2017a), drink-driving regulations and laws, and health system interventions including screening, brief interventions, and treatment. The three "best buy" alcohol policy interventions to reduce alcohol-related harm were identified in 2017 within the WHO strategy for noncommunicable diseases (WHO, 2017a) and were recently reconfirmed as the most cost-effective interventions (WHO, 2023b, 2024b). The "best buy" alcohol policy interventions are "1. Increase excise taxes on alcoholic beverages; 2. Enact and enforce bans or comprehensive restrictions on exposure to alcohol advertising (across multiple types of media); 3. Enact and enforce restrictions on the physical availability of retailed alcohol (via reduced hours of sale)" (WHO, 2023b). As part of the definition of "best buys", these interventions must be relatively easy to implement in lesser-resourced countries. Since 2010, most countries have reported no progress in implementing the "best buys", and although a larger percentage of countries reported progress in 2019 than in 2015, the rate of change is insufficient to meet global targets, signalling the urgent need for action (WHO, 2024c).

The Global Alcohol Action Plan 2022–2030 has an overlapping list of recommended high-impact interventions (WHO, 2024a), and SAFER is an initiative to facilitate implementing them (WHO, 2023c). In addition to the three "best buys", the SAFER initiative also includes two individual-level high-impact interventions: Advance and enforce drink-driving countermeasures, and Facilitate access to screening, brief interventions, and treatment (WHO, 2023c). WHO regional offices have published their own strategies and action plans, which are aligned with the global activities and highlight additional targets.

1.2 Selection of alcohol policies for review and evaluation

WHO has used the term "alcohol policy" to refer to "the set of measures in a jurisdiction or society aimed at minimizing the health and social harms from alcohol consumption. These measures may be in any governmental or societal sector and may include measures which are not directly aimed at alcohol consumption; for instance, the promotion of alternatives to drinking, where such a measure has the aim of minimizing alcohol-related harms. A national alcohol policy will be made up of a set of individual policies, strategies, and implementing actions. There are also a variety of other policies which impinge on alcohol-related problems, increasing or reducing them, but which are neither normally described as alcohol policies nor normally included within an overall alcohol policy, since the policies are not adopted or implemented with the minimization of alcohol problems as a primary aim" (WHO, 2007).

For Volume 20B of the *IARC Handbooks*, the Working Group reviewed and evaluated the strength of evidence on the potential for alcohol policy interventions related to (i) taxation and pricing policies (see Section 2.2), (ii) policies to limit physical availability (see Section 3.2), (iii) alcohol marketing bans (see Section 4.3.2), and (iv) government alcohol monopolies and other coordinated multiple alcohol policy interventions (see Sections 5.2 and 5.3) to reduce alcohol consumption. These interventions were selected because their implementation aims to reduce alcohol consumption at the population (i.e. national or subnational) level, meaning that all people who can potentially consume alcohol or whose consumption may be a harm to others (Babor et al., 2023) in a given jurisdiction are affected (Keyes and Galea, 2016). These policies also have other goals, such as raising revenue through taxes (WHO, 2023a) or reducing violence or harm to others besides the individual who consumes alcohol. Notably, evidence from studies of some selected availability, marketing, and multiple alcohol policy interventions was summarized but not evaluated.

The evidence on the effects of health care-based interventions (i.e. screening and brief interventions, and psychosocial and pharmacotherapy interventions) to reduce alcohol consumption also was summarized but not evaluated (see Section 6), because these interventions are aimed at individuals and their possible population-level effects on alcohol consumption are not usually measured. In addition, there are national agencies that evaluate the evidence and ensure that only pharmacological agents that have been shown to reduce alcohol consumption-related outcomes in randomized clinical trials are approved for intervention (e.g. EMA, 2010).

1.3 Primary outcomes

The primary outcome considered in this volume of the *IARC Handbooks* is average change (or difference) in total alcohol consumption – defined here as the sum of at least the three major alcoholic beverage types (i.e. spirits, wine, and beer) – over time based on both population-level (also known as aggregate-level) and individual-level or household-level measures. If outcome data on total alcohol consumption were not available, then studies with data on consumption of all major beverage types separately were included; studies with outcome data on consumption of only one or two major beverage types were not included.

The population-level measures of alcohol consumption were based on country-specific estimates of APC (Poznyak et al., 2013) or, if APC data were not available, on recorded consumption only. In some studies, total alcohol consumption was estimated using empirical

data from alcohol-attributable outcomes, sales data only, or other proxy outcomes.

The individual-level measures of alcohol consumption were based primarily on self-reported data from, for example, quantity–frequency or alcohol use questionnaires; the household-level measures of alcohol consumption were based primarily on off-premises purchasing records and used to estimate either individual-level or household-level alcohol consumption. The outcomes in many studies were the amount of total alcohol consumed (e.g. grams of ethanol per day) or the prevalence of individuals who consume alcohol (which are each associated with APC; WHO, 2018). If these outcomes were not available, then studies of the frequency of alcohol consumption or the prevalence of heavy episodic drinking were included; in studies of psychosocial and pharmacological interventions, additional outcomes also were included. Age at initiation of alcohol consumption was eligible as an outcome, but no studies were identified.

In studies where the outcome in a regression model was expressed as the logarithm of the amount of alcohol consumed, the Working Group calculated the estimated percentage change in consumption due to the intervention using the method described in Pardoe (2021).

Exceptionally, for some alcohol policy interventions where few studies on alcohol consumption outcomes were available, studies using proxy outcomes for alcohol consumption (e.g. liver cirrhosis mortality) were included if an association with alcohol consumption was previously established in the existing body of literature.

1.4 General study eligibility criteria and types of studies included

Only studies with empirical evidence on the potential effects of alcohol policy interventions on average changes (or differences) in alcohol consumption over time were eligible for inclusion in the reviews leading to an evaluation. The several types of studies that contributed to the body of evidence are described in Table 1.1. Meta-analyses of such studies also were eligible for inclusion. Of note, cross-sectional studies that assessed consumption at only a single time point were not eligible for inclusion in the reviews leading to an evaluation because they are the least informative for assessing the potential for an alcohol policy intervention to reduce alcohol consumption.

Generally, when multiple reports (i.e. publications) of the same alcohol policy intervention in relation to the same outcome in the same (or nearly the same) study population were identified, the most recent or most informative report was reviewed first, and related reports were included if they provided additional methodological detail or results from sub-analyses.

1.5 General methodological considerations

Several general methodological issues must be carefully considered when reviewing and evaluating studies of alcohol policy interventions in relation to alcohol consumption. Described below are the general methodological considerations that apply to all alcohol policy interventions evaluated; methodological considerations that are specific to a type of alcohol policy intervention are described in detail in each section.

1.5.1 Confounding

Confounding can occur in all types of studies of alcohol policy interventions in relation to alcohol consumption if the groups being compared (e.g. countries with and without an alcohol policy intervention) have different distributions of other variables that are related to both the intervention and the outcome under study. In addition to sociodemographic characteristics of the study population(s), potential confounding

Table 1.1 Types of studies eligible for inclusion in the reviews leading to an evaluation on the effects of alcohol policy interventions on the consumption of alcoholic beverages

Study type	Description Strengths Potential weaknesses	Comparisons			
		Average change in consumption over time in the intervention and control groups[a]	Post-intervention consumption in the intervention and control groups[b]	Average consumption over time in the intervention and control groups[c]	Pre-intervention and post-intervention consumption (no control group)[d]
Individual-level and household-level data					
Randomized controlled trial	**Description:** Individuals or other units of analysis (e.g. communities) are randomized to an alcohol policy intervention or control group; alcohol consumption data are collected before and after (and possibly during) the intervention. **Strengths:** (1) includes a control group; (2) intervention is well defined; (3) minimizes selection bias; (4) randomization ensures that, on average, confounding factors are distributed equally between groups; (5) for the primary outcome, statistical chance is minimized because sample size is established during the design of the study **Potential weaknesses:** (1) often of short duration, and feasibility of assessing long-term outcomes may be limited; (2) cross-contamination of the intervention; (3) dropouts; (4) strict inclusion and exclusion criteria and study setting can limit generalizability; (5) may be underpowered for subgroup analyses or other outcome analyses; (6) potential bias due to self-reported measures of alcohol consumption (including those specific to repeated assessment)	✓	✓	N/A	N/A
Non-randomized controlled trial	**Description:** Individuals or other units of analysis (e.g. communities) are allocated, but not randomly, to an alcohol policy intervention or control group; alcohol consumption data are collected before and after (and possibly during) the intervention. **Strengths:** (1) includes a control group; (2) intervention and control are well defined; (3) for the primary outcome, statistical chance is minimized because sample size is established during the design of the study; (4) longer observation periods are possible **Potential weaknesses:** (1) cross-contamination of the intervention; (2) dropouts; (3) may be underpowered for subgroup analyses or other outcome analyses; (4) potential bias due to self-reported measures of alcohol consumption (including those specific to repeated assessment)	✓	✓	N/A	N/A

Table 1.1 (continued)

Study type	Description Strengths Potential weaknesses	Comparisons			
		Average change in consumption over time in the intervention and control groups[a]	Post-intervention consumption in the intervention and control groups[b]	Average consumption over time in the intervention and control groups[c]	Pre-intervention and post-intervention consumption (no control group)[d]
Prospective (or longitudinal) cohort; panel series	**Description:** Individuals are enrolled in a cohort, are followed up over time, and alcohol consumption is reported before and after the alcohol policy intervention. The investigator does not control the timing of the intervention. The individuals may be nested within a unit of analysis, such as schools or households. **Strengths:** (1) if the cohort includes individuals from locations with and without an intervention, can compare change in consumption in those exposed to the intervention versus those unexposed to the intervention; (2) can assess different alcohol policy interventions or different levels of an intervention at different time points; (3) appropriate for stationary, closed populations **Potential weaknesses:** (1) if all individuals in the cohort reside in one location with a known intervention date, only the pre-intervention to post-intervention change in consumption can be assessed because there is no control; (2) information about confounding variables may not be available; (3) potential attrition or contaminations if individuals move; (4) potential bias due to self-reported measures of alcohol consumption (including those specific to repeated assessment) in individual-level studies	✓	✓	✓	✓
Repeated cross-sectional survey (serial cross-sectionals)	**Description:** Different samples of individuals or households (e.g. adolescents in grades 9–12) report their alcohol consumption at different points over time (e.g. biennially). The investigator does not control the timing of the alcohol policy intervention. The individuals may be nested within a unit of analysis, such as grades, schools, households, or communities. **Strengths:** (1) convenient and cost-effective compared with most other individual-level analyses; (2) can assess different alcohol policy interventions or different levels of a policy intervention at different time points; (3) can target specific populations **Potential weaknesses:** (1) difficult to infer causality; (2) confounding variables may not be available; (3) potential bias due to self-reported measures of alcohol consumption (including those specific to repeated assessment) in individual-level studies	✓	✓	✓	✓

Table 1.1 (continued)

Study type	Description Strengths Potential weaknesses	Comparisons			
		Average change in consumption over time in the intervention and control groups[a]	Post-intervention consumption in the intervention and control groups[b]	Average consumption over time in the intervention and control groups[c]	Pre-intervention and post-intervention consumption (no control group)[d]
Single-time-point cross-sectional with alcohol consumption reported at the time of the survey and recalled before the intervention[e]	**Description:** Individuals are enrolled in a study after the alcohol policy intervention. They report their alcohol consumption at the time of the survey (after the alcohol policy intervention) and recall their consumption before the intervention. The investigator does not control the timing of the alcohol policy intervention. **Strengths:** (1) convenient and cost-effective compared with most individual-level analyses; (2) can assess different alcohol policy interventions or different levels of an intervention; (3) can target specific populations; (4) if the study population includes individuals from locations with and without known intervention dates, can compare change in consumption in those exposed to the intervention versus those unexposed to the intervention **Potential weaknesses:** (1) difficult to infer causality; (2) information about confounding variables may not be available; (3) all biases related to self-report, plus specifically susceptible to survival bias because individuals with high alcohol consumption may be less likely to survive until the time of the study; (4) potential bias due to self-reported measures of alcohol consumption (including those specific to repeated assessment) in individual-level studies	✓	✓	✓	✓
Population-level data					
Non-randomized community trial	**Description:** Communities or other units of analysis (e.g. counties) are allocated, but not randomly, to an alcohol policy intervention or control group; alcohol consumption data are collected before and after (and possibly during) the intervention. **Strengths:** (1) includes a control group; (2) intervention and control are well defined; (3) for the primary outcome, statistical chance is minimized because sample size is established during the design of the study; (4) longer observation periods are possible **Potential weaknesses:** (1) cross-contamination of the intervention; (2) information about confounding variables may not be available; (3) population-level measures of alcohol consumption cannot be assigned to subgroups (e.g. men, women, different age groups) without information from survey data	✓	✓	N/A	N/A

Table 1.1 (continued)

Study type	Description Strengths Potential weaknesses	Comparisons			
		Average change in consumption over time in the intervention and control groups[a]	Post-intervention consumption in the intervention and control groups[b]	Average consumption over time in the intervention and control groups[c]	Pre-intervention and post-intervention consumption (no control group)[d]
Time series, interrupted time series, and panel	**Description:** *Time series* refers to the collection of population-level data (e.g. alcohol per capita consumption) repeatedly over many time points (e.g. annually, monthly) to establish a trend. In an *interrupted time-series* study, the time-series data are used to establish a trend that is "interrupted" by an alcohol policy intervention at a known time point. *Panel data* refers to time-series data collected from many units (e.g. countries, states, provinces). **Potential weaknesses:** (1) if the time-series data are based only on one location with a known intervention date, only the pre-intervention to post-intervention change in consumption can be assessed because there is no control location; (2) information about confounding variables may not be available; (3) endogeneity; (4) population-level measures of alcohol consumption cannot be assigned to subgroups (e.g. men, women, different age groups) without information from survey data	✓	✓	✓	✓

N/A, not applicable.

[a] Comparison is the average change in alcohol consumption over time between the intervention group and a control group (i.e. a difference-in-differences).

[b] Comparison is the difference in post-intervention alcohol consumption between the intervention group and the control group. If the data are available, the researcher may control for pre-intervention consumption.

[c] Comparison is the average difference over time in consumption between the intervention group and the control group. This is when the specific dates of the intervention are unknown.

[d] Comparison is the difference between pre-intervention and post-intervention alcohol consumption in the intervention group (i.e. there is no control group).

[e] This study design is included only when few or no other studies address a specific alcohol policy intervention.

factors include, for example, the enactment of other alcohol policy interventions, temporal changes in other sociopolitical factors (global, regional, national, or subnational), and the price or affordability of alcohol.

1.5.2 Considerations in studies with individual-level or household-level data

There are two important issues that can bias estimates of the average amounts of alcohol consumption in the population when measuring consumption at the individual level. First, many studies with individual-level or household-level data are based on non-probability samples of the population, and the information obtained may not be representative of the general population (Kruskal and Mosteller, 1979). Second, the alcohol consumption behaviour of respondents often differs from that of non-respondents (Rehm et al., 2021). It has been estimated that only between 20% and 70% of total alcohol consumption is accounted for in survey responses when compared with recorded consumption or with APC; this is referred to as the coverage rate (Midanik, 1982; Rehm et al., 2007). For these reasons, the average amount or prevalence of alcohol consumption in the population cannot be inferred from most population-level surveys. More importantly, coverage rates can vary widely over time within a country and among different countries, even when standardized survey questions are used within regions with similar economic characteristics, such as the European Union (Kilian et al., 2020). Other factors can also affect the validity and reliability of alcohol consumption measured by self-report, including recall bias (Poznyak et al., 2013), the difficulty for respondents to convert their alcohol consumption over longer periods into standard drinks (NIAAA, 2003), and the tendency to underestimate amounts of alcohol consumption (i.e. social desirability bias; Davis et al., 2010).

If sampling, non-response, or reporting of alcohol consumption differ systematically based on the presence (or absence) of an alcohol policy intervention (e.g. a lower proportion of the drinking population responds to surveys in countries that recently introduced an alcohol policy intervention compared with countries without such an intervention), then the differences in consumption between intervention and control groups (or countries) could be biased.

Studies that assessed the potential effects of an alcohol policy intervention on alcohol consumption in a single population by comparing consumption between one survey collected before and one survey collected after implementation of the intervention, without a control population, may be prone to additional biases (Cook and Campbell, 1986; Shadish et al., 2002). For example, maturation or natural changes of respondents or sensitization due to repeated measures of consumption in the same study population over time could bias the effect estimates. In addition, if the selection of study participants was based on extreme amounts of alcohol consumption during a specific time period, then regression to the mean may also be a concern.

1.5.3 Considerations in studies with population-level data

Considerations related to the inclusion of appropriate control groups or jurisdictions also are important for studies with population-level data. Interrupted time-series analyses (Beard et al., 2019; Jiang et al., 2022) that use control groups from other regions or countries are less prone to bias compared with interrupted time-series analyses in just one region or country without controls (Shadish et al., 2002; Rehm et al., 2023).

In studies of population-level measures of alcohol consumption, it is not possible to assess the effect of alcohol policy interventions

on average changes (or differences) in alcohol consumption over time in specific subpopulations. For example, it is not possible to assess whether a reduction in APC is due to a proportional reduction in alcohol consumption by all groups in a population (Raninen and Livingston, 2020) or to a reduction in consumption only in a selected group of individuals (e.g. among individuals who consume moderate amounts of alcohol while individuals with heavy alcohol consumption patterns did not change their consumption).

Many studies based on population-level measures of alcohol consumption did not use APC data but instead relied on recorded alcohol data only. Therefore, alcohol consumption may be underestimated. This issue may be particularly concerning in jurisdictions with a high proportion of unrecorded alcohol consumption (Rehm et al., 2016; see Section 1.6). The potential bias from not including tourist consumption is likely to be minimal, except in small countries where the tourists considerably outnumber the population.

Endogeneity should be, but often is not, assessed in studies of alcohol policy interventions based on population-level data. Endogeneity refers to the correlation between the independent variable and unexplained variation (i.e. error) in the dependent variable outcome. An important type of endogeneity occurs when the outcome (e.g. APC) is a predictor of an alcohol policy intervention. For example, if a country is experiencing high or increasing alcohol-related harms, there may be stronger willingness to enact alcohol policy interventions, compared with periods when alcohol-related harm is low or decreasing. In epidemiology, this is referred to as reverse causation. Few of the studies reviewed (e.g. Saffer and Dave, 2002) assessed endogeneity and, if it was detected, accounted for it in the statistical analysis.

1.5.4 Enactment, implementation, and enforcement

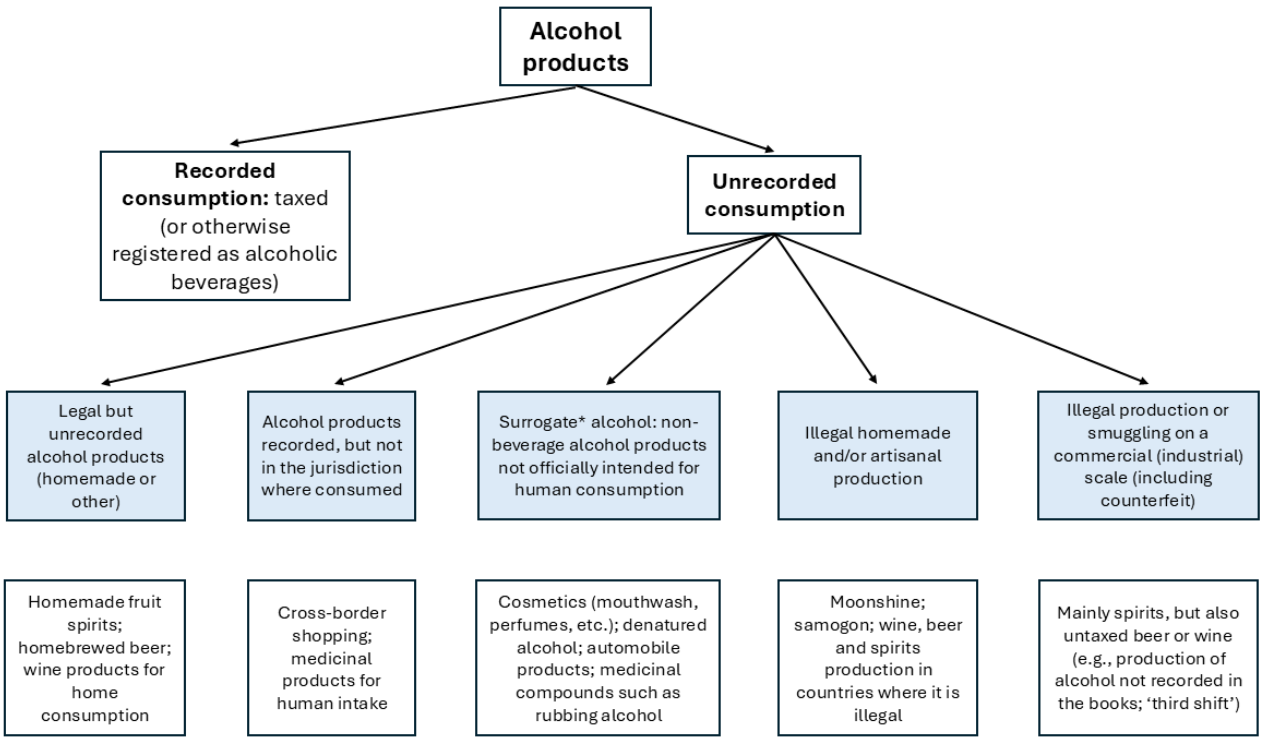
The dates of enactment (i.e. passage or ratification) and implementation (i.e. introduction) of alcohol policies are often different, and the sequence of these events can vary. In addition, the timing of the implementation of an intervention can vary considerably, ranging from immediately to several years after enactment of the policy. For example, in Estonia the national alcohol plan was intensively discussed in 2012, the first implementation of key policies was in 2013, and the official publication of the plan was ratified in 2014 (WHO Regional Office for Europe, 2024). Inclusion of the period between enactment of the policy and implementation of a policy intervention may affect the association with alcohol consumption. When the information was available, these dates are described.

The Working Group recognized that enforcement is another important consideration. The enforcement strategies and degree of enforcement may vary among different policies and jurisdictions. Alcohol policies will be most effective if they are adequately enforced. However, for the alcohol policy interventions assessed, there were few or no empirical studies that considered the potential effects of enforcement.

1.6 Unrecorded alcohol consumption

Unrecorded alcohol is defined as alcohol that is not officially registered in statistics for sales, production, trade, or taxation in the country where it is consumed (Rehm et al., 2022; WHO, 2022), and it can be classified into five categories (WHO, 2022; Fig. 1.1). Unrecorded alcohol consumption can be a substantial proportion of a jurisdiction's total alcohol consumption. The proportion of APC that is due to unrecorded alcohol consumption is 21.8% globally, but it

Fig. 1.1 Categories of unrecorded alcohol (first row, categories; second row, examples)



* Surrogate alcohol may be intended for human consumption, but intentionally not declared as such in order to evade taxes. For this form of surrogate alcohol, the term pseudo-surrogate is sometimes used.
Reproduced from Rehm et al. (2022), copyright 2022, with permission from Elsevier; adapted from Lachenmeier et al. (2021); adapted from Rehm et al. (2014), John Wiley & Sons.

ranges from 10.7% to 37.0% among WHO regions (WHO, 2024c).

Methods have been established for measuring unrecorded alcohol consumption (Razvodovsky, 2010; Rehm and Poznyak, 2015), and for the component of illicit alcohol the methods have also been discussed in the WHO technical manual (WHO, 2023a). These methods are based on data collected in population surveys (e.g. the WHO STEPS survey; WHO, 2017b), expert assessment (e.g. the nominal group technique; Probst et al., 2018; Rehm and Poznyak, 2015), and indirect methods (Razvodovsky, 2010); econometric modelling is used for extrapolation for countries with no estimates from any method. However, there are several sources of measurement error that may lead to inaccurate estimates of unrecorded alcohol consumption.

Rehm et al. (2022) identified the following determinants of unrecorded alcohol consumption: the current level of unrecorded consumption and the price of unrecorded products, the availability of unrecorded products for the most vulnerable populations (e.g. individuals with heavy alcohol consumption, low income, or low education level), implementation of the government's countermeasures against unrecorded consumption, the presence of large-scale producers of unrecorded alcohol, and loopholes in the legislation that leave room for tax evasion.

Taxes may potentially affect unrecorded alcohol consumption through prices (WHO, 2023a). When well-designed taxation policies lead to an increase in taxes, prices of recorded alcoholic beverage products increase and recorded consumption decreases. Producers and

sellers of unrecorded alcohol have the incentive to keep their products less expensive than recorded products; this can be achieved by keeping the price the same or increasing the price proportionately less than the price increase in recorded products. As a result, prices of both recorded and unrecorded products increase because of the tax increase. However, unrecorded products may become comparatively less expensive, which creates a potential incentive for consumers to substitute recorded products with the more affordable unrecorded products.

Despite the logic described above, the empirical evidence on the effects of alcohol policy interventions in relation to unrecorded alcohol consumption is inconsistent. For example, tax increases in Thailand led to increased unrecorded alcohol consumption (Chaiyasong et al., 2011), whereas tax increases in Lithuania had no effect on unrecorded consumption (Štelemėkas et al., 2023). These inconsistencies may be due to measurement error in the estimates of unrecorded alcohol consumption (for an overview, see Rehm et al., 2022), lack of data on unrecorded consumption in wide parts of a jurisdiction, variations in the presence or type of specific policy interventions targeting unrecorded consumption, or cultural differences in the history of producing unrecorded alcohol.

In a systematic umbrella review, Guindon et al. (2022) compared the effects of taxes and prices on alcohol consumption in low- and middle-income countries and in high-income countries based, in part, on evidence that unrecorded consumption is higher in low- and middle-income countries (Probst et al., 2018). The body of evidence suggests that price responsiveness in low- and middle-income countries was similar to that in high-income countries.

One category of unrecorded alcohol is alcohol products obtained through cross-border shopping (see Fig. 1.1), i.e. alcoholic beverages purchased in another jurisdiction and brought by a consumer for personal use into their own jurisdiction under "travellers' allowances" rules for the importation of alcoholic beverages (Rehm et al., 2022). Cross-border shopping may occur between countries, between regions within a country, or between jurisdictions within customs unions (e.g. the European Union) that allow free movement of people and goods but have different alcohol policies.

Whether consumers engage in cross-border shopping for alcohol, and by doing so reduce the effect of a restrictive alcohol policy, depends on a range of factors, including a price difference, opportunity costs associated with cross-border shopping, or availability differences (Rehm et al., 2022; Babor et al., 2023). Cross-border shopping is of concern for assessing the effects of alcohol policy interventions that affect price or availability in one jurisdiction, and is often monitored for this reason (e.g. Patterson et al., 2022). For example, population-level recorded alcohol sales data in a jurisdiction that implements strong availability restrictions may overestimate a decrease (or average difference) in alcohol sales due to an increase in cross-border shopping in a neighboring jurisdiction with a lower restriction.

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2. TAXATION AND PRICING POLICIES

2.1 General concepts and considerations

2.1.1 Key concepts and definitions

The World Health Organization (WHO) Global Alcohol Action Plan 2022–2030 encourages Member States to implement "high-impact and effective strategies and interventions, supported by legislative measures" that include alcohol policy interventions to address "the affordability of alcoholic beverages, by appropriate taxation and pricing policies" (WHO, 2024a). Governments, at the national or subnational level depending on the division of legislative power in the country, have primarily imposed taxes on alcoholic beverages for the purposes of raising revenue. However, alcohol taxation and pricing policies have also been implemented for public health purposes to reduce alcohol-related harms.

From an economic perspective, applying taxation and pricing policies to alcoholic beverages can reduce or correct for negative internalities and externalities associated with alcoholic beverage consumption. "Negative internalities arise when individuals do not fully consider or account for the cost of their current actions – for example, the decision to consume large amounts of alcohol – on their future selves", whereas negative externalities are the harms and "costs that are not borne by the consumer or producer of the product but by others in society or society at large" (WHO, 2023a).

There is substantial and consistent evidence that when alcoholic beverages become less affordable, alcohol consumption is reduced (WHO, 2023a). The affordability of alcoholic beverages is mainly a function of their prices and average income level, and affordability can be understood as the proportion of income needed to purchase a given amount of alcohol (WHO, 2023a). In addition to taxation and pricing policies, other economic and behavioural factors, as well as other public policies aiming to regulate alcohol, influence the prices of alcoholic beverages (WHO, 2023a). Alcoholic beverages may become more affordable as a result of increases in average income levels (particularly in low- and middle-income countries); a lack of regular adjustments to specific alcohol excise tax rates for inflation and average income growth; government actions such as the implementation of subsidies, tax incentives, loans, and grants; pricing strategies; and alcohol industry actions (WHO, 2023a).

Price elasticity of demand (hereafter referred to as price elasticity) "refers to the responsiveness of individuals' demand for alcoholic beverages to changes in specific parameters, including price and income" (WHO, 2023a). Of particular interest for changes in alcohol taxes is the

concept of own-price elasticity, which measures the proportionate change in the demand for a product in response to a proportionate change in the price of the product (WHO, 2023a). The link between tax and price (i.e. pass-through) and how this influences alcoholic beverage consumption is discussed in Section 2.2.1(c).

2.1.2 Types of taxation and pricing policies

(a) Taxation

Taxes can be defined as "compulsory unrequited payments to the general government or to a supranational authority" (OECD, 2023). These mandatory payments are made by taxpayers (e.g. individuals, corporations), and "benefits provided by government to taxpayers are not normally in proportion to their payments" (OECD, 2023). A tax is different from a fee or a charge, which is tied to a specific service or activity. There are three main types of alcohol taxes: general taxes, excise taxes, and customs taxes (WHO, 2023a) (Fig. 2.1).

(i) General taxes

General taxes include sales tax, value-added tax (VAT), and goods and services tax (GST). They all apply to the sale of goods and services. A sales tax is usually applied only once, at the final point of sale, and is based on the retail price. VAT and GST may operate in the same way as a sales tax, but in some systems they are imposed on each sale in the process of moving a product from the point of manufacture to the final sale. A general tax is usually applied to all goods or services available, although there may be tiered tax rates with products being clustered into different tiers for the purpose of imposing different tax rates. There may also be exceptions made for certain products, such as the exemption of taxes on fresh fruits and vegetables to encourage the consumption of healthy food.

(ii) Excise taxes

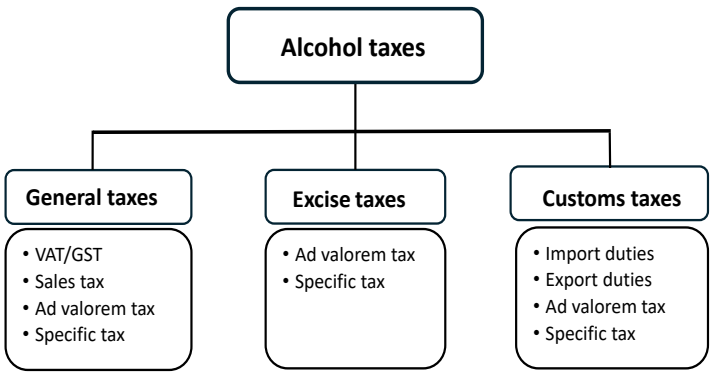
Excise taxes (also called excise duties) are imposed on the production, sale, or consumption of specified goods, such as energy, tobacco, sugar-sweetened beverages, or alcoholic beverages. With respect to alcoholic beverages, there are two main types of excise taxes, which may be applied independently or in conjunction with each other: ad valorem taxes and specific taxes. Ad valorem taxes on alcoholic beverages are based on the value of the product and are often applied on the retail price of the product. Specific taxes (also called ad quantum taxes) on alcoholic beverages are based on either the ethanol content in the product or the overall product volume.

Excise taxes can be designed to target specific alcoholic beverages (WHO, 2023a) and may include tiers with different levels of tax applying to different categories of alcoholic beverages, such as tiers based on the beverage type (Angus et al., 2019) or the ethanol content. A minimum excise tax (also called a tax floor) can be set to ensure "that the cheapest alcoholic beverages retain a certain level of tax, while also limiting industry's ability to manipulate the price of these alcoholic beverages downwards" (WHO, 2023a).

(iii) Customs taxes

Customs taxes (also called customs duties) are imposed on goods at the border as they are imported into or exported out of a country. In addition to generating revenue for a country, customs duties may be applied for reasons such as protecting national industries or regulating trade between countries. Customs duties can vary significantly depending on the type of goods, the country of origin, and the trade agreements in place between countries. There has been a move towards reducing the duties on alcoholic beverages to very low or zero rates (Zeigler, 2009).

Fig. 2.1 Types of alcohol taxes



GST, goods and services tax; VAT, value-added tax.
Created by the Working Group.

(iv) Other forms of taxes

Other forms of taxes on alcoholic beverages may exist, and these vary from country to country. For example, in France, in addition to consumption taxes, a contribution called the Social Security contribution applies to beverages with an ethanol content exceeding 18% by volume (République française, 2024).

(b) Minimum pricing policies

Recently, minimum pricing (floor pricing) policies have been implemented or considered in an increasing number of jurisdictions (WHO Regional Office for Europe, 2022). Minimum pricing "sets a fixed price level below which a specific volume of a finished product cannot be sold" to consumers; minimum unit pricing "is more specific and sets a level below which a fixed volume of alcohol (such as a standard drink) cannot be sold" (WHO Regional Office for Europe, 2022). Both minimum pricing and minimum unit pricing might be set for some or all alcoholic beverages. Some countries have implemented both a minimum price and a minimum unit price (MUP) policy (WHO Regional Office for Europe, 2022). One of the advantages of using ethanol content as the reference point for the minimum price is that it does not encourage consumers to substitute lower-strength alcoholic beverages with higher-strength beverages. Minimum pricing policies can also be established by banning below-cost sales of alcoholic beverages and can be implemented in combination with increased taxes on alcoholic beverages (WHO Regional Office for Europe, 2022) to ensure that retailers do not subvert the purpose of an increase in taxes by selling alcoholic beverages at a below-cost price. A key difference between a minimum pricing policy and a taxation policy is that the income from a minimum price typically goes to the retailer (with the exception of countries with government alcohol monopolies), whereas the income from a tax goes to the government as revenue.

(c) Restrictions on discounting and price promotions

Discounts reduce the price of alcoholic beverages and can increase bulk purchases. Price discounts used by the alcohol industry take many forms (Puac-Polanco et al., 2020), including

happy hours, off-premises sales discounts, and free gifts with a purchase. During happy hours, alcoholic beverages in bars or other on-premises locations are sold at a reduced price or offered as two-for-one deals or multiple servings for the price of one. Examples of off-premises sales discounts are a 20% discount off the regular price, "buy one, get one free" discounts, and discounts that apply to bulk purchases or repeat purchases, such as through loyalty schemes. Offering free gifts (e.g. food or clothing) with the purchase of alcoholic beverages, although not technically a discount on the alcoholic beverage, in effect reduces the price of a basket of goods that includes alcoholic beverages.

Policies that ban or restrict the sale of alcoholic beverages on a discounted basis can be implemented on their own or in conjunction with taxation and/or minimum pricing policies to maintain their intended effects and ensure that they are not counteracted by price promotions.

(d) Restrictions on subsidies

A subsidy is the "transfer of resources from a government to a domestic entity without an equivalent contribution in return" (Van Heuvelen, 2023). The complete removal of or partial reduction in the subsidies that a government extends to the alcohol industry may also affect the prices of alcoholic beverages. Although there is debate about what constitutes a subsidy, it can be in the form of "direct grants to domestic companies, tax incentives, or favourable terms for financing" (Van Heuvelen, 2023). The availability of subsidies enables alcoholic beverages to be sold at a lower price, because the subsidies reduce some of the costs involved in production.

Restrictions on subsidies that a government extends to the alcohol industry can also affect the prices of alcoholic beverages. These types of restrictions can include eliminating tax breaks for alcohol producers, price supports, and industry-funded education programmes.

(e) Global variation in type of alcohol taxation and pricing policies

Globally, many countries apply one type of excise tax or a combination of different types of excise taxes on alcoholic beverages. As of July 2022, 148 WHO Member States applied national-level excise taxes to at least one type of alcoholic beverage; Member States that did not apply an excise tax on alcoholic beverages tended to have a comprehensive ban on alcohol sales (WHO, 2023b). However, in recent decades, alcoholic beverages have become more affordable (WHO, 2023a), as a result of factors such as increases in incomes, decreases in costs of alcohol production, and lack of adjustment of taxes for inflation. In 2019, only 44% of countries that applied an excise tax on beer adjusted that tax for inflation (WHO, 2024b). Governments may also tax different alcoholic beverages differently to support or incentivize local alcohol industries. For example, wine-producing regions tax wine at a lower rate than other types of alcoholic beverages, and wine is exempt from excise tax in at least 22 countries, most of which are in the WHO European Region (WHO Regional Office for Europe, 2020; WHO, 2023b).

Far fewer jurisdictions have implemented minimum pricing policies. A 2022 report from the WHO Regional Office for Europe identified 22 jurisdictions across 13 sovereign states that have minimum prices for alcohol (WHO Regional Office for Europe, 2022).

The Working Group did not identify any information on global variation in policies related to restrictions on discounting, promotions, or subsidies.

2.1.3 Study eligibility and methodological considerations

In Section 2.2, the evidence on the effects of alcohol taxation, minimum pricing, and bans on discounting policies on alcoholic beverage consumption is reviewed and evaluated. The general outcomes, study eligibility criteria, methodological considerations, and other issues that apply to all policies evaluated are described in Sections 1.3–1.6. In addition, for Section 2.2, studies were considered for inclusion if taxation or pricing policies were applied to at least one of the three major types of alcoholic beverages (i.e. spirits, wine, and beer). Studies on the effects of a single alcohol taxation or pricing policy or on the effects of multiple policies of the same type of taxation or pricing policy (e.g. an increase in the excise tax on beer and the removal of the tax exemption for small beer breweries) implemented during the same period or within the period of repeated measurement (in the case of repeated cross-sectional analysis) were also eligible for inclusion. However, studies on the effects of a combination of different types of alcohol policies (e.g. changes in taxation and minimum pricing or cross-border allowances) were excluded.

For studies on the effects of alcohol taxation policies, only studies that controlled for income (e.g. real income, household income, median household income, disposable income, income level, or per capita income) or employment status were included, to ensure that the observed effect of taxes on alcohol consumption reflected the true net effect by accounting for the affordability pathway. Although controlling for income was not an inclusion criterion for studies on the effects of pricing policies, the studies in which the models controlled for income or its proxies were influential in the evaluations.

It is critically important to consider that a change in tax is passed through to consumers via changes in the prices of alcoholic beverages; this pass-through can affect alcohol consumption (Fig. 2.2). Evidence that clearly establishes the pathway from tax to price and from price to consumption is briefly summarized in Section 2.2.1(c) and is used as supportive evidence in the evaluation of the evidence on the effects of alcohol taxation policies on alcohol consumption.

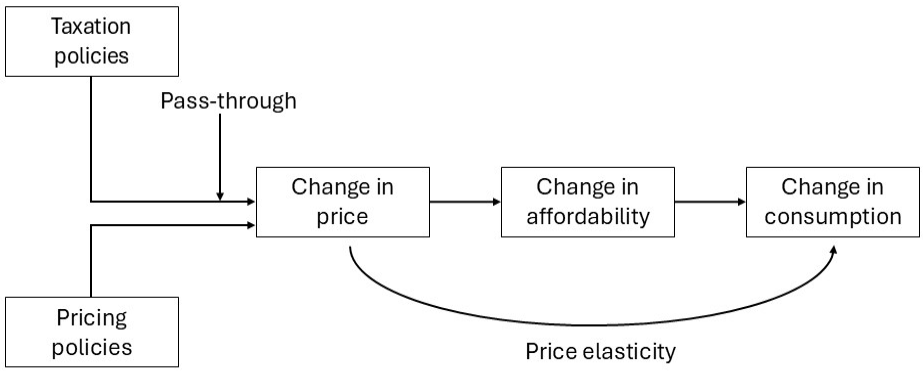
Several other methodological issues were considered when assessing the evidence on the effects of all alcohol taxation and pricing policies on alcoholic beverage consumption. First, as discussed in Section 1.5.1, confounding by temporal changes in sociopolitical factors, such as specific economic cycles and inflation, must be considered when assessing the evidence.

Second, the Working Group acknowledges the potential overlap among all of the studies with individual-level or household-level data and some of the studies with population-level data on the effects of minimum pricing policies, and among all of the studies on the effects of bans on discounting policies that assessed the same alcohol policies in Scotland, United Kingdom. However, because of the variation in study populations, control (or comparison) populations, data sets, periods after implementation of the policy, and statistical methods of analysis, the studies were considered to be independent studies for the evaluation. For example, among some studies that assessed the same policy intervention, the geographical controls were another jurisdiction within the United Kingdom.

Third, because the effect of alcohol taxes and prices on alcohol consumption within a jurisdiction may be influenced by cross-border shopping (see Section 1.6), the studies that included control variables for the distance to the control jurisdictions for reducing confounding by cross-border shopping were particularly informative.

Fourth, in some studies, the outcome was based only on, for example, off-premises sales of alcoholic beverages or excluded some major off-premises discount alcohol retail chains and therefore may underestimate the overall

Fig. 2.2 Causal pathway between alcohol taxation and pricing policies and alcoholic beverage consumption



Policies that increase the price of alcohol make alcoholic beverages less affordable, which in turn leads to reductions in alcoholic beverage consumption. Pass-through is the degree to which taxes are passed through to prices for consumers. Price elasticity describes the degree to which the demand for alcoholic beverages changes when prices change.
Created by the Working Group.

effect of the policy intervention on alcoholic beverage consumption because of exclusion of on-premises alcohol sales, online retail sales, or the major discount alcohol retail chains. In addition, because many of the studies relied on alcohol sales data as the outcome, it is important to include control variables for seasonal and other time-related changes in alcohol sales (e.g. COVID-19-related restrictions) in the model.

Fifth, an important strength of some studies with individual-level data is the inclusion of appropriate weighting procedures if the study population was based on non-representative sampling (e.g. quota sampling in market research data).

Finally, an important strength of several studies is that the analyses were stratified by categories of, for example, sociodemographic or behavioural factors (i.e. income or alcohol consumption status) before implementation of the alcohol taxation or pricing policies. These studies were particularly informative because they provided information about the potential heterogeneity of effects in the study population.

2.2 Effects of taxation and pricing policies on alcoholic beverage consumption

2.2.1 Taxation

(a) Studies with population-level data

The effects of alcoholic beverage taxes on alcoholic beverage consumption were assessed in four studies with population-level data: two panel regression studies, in 14 countries in the Organisation for Economic Co-operation and Development (OECD) (Saffer, 1989) and in the USA (Subbaraman et al., 2020), and two interrupted time-series studies, in Sweden (Ponicki et al., 1997) and in the USA (Esser et al., 2016) (Table 2.1).

Saffer (1989) used annual population-level data from 1970 through 1983 to assess the associations of a US$ 1 higher national tax (typically an excise tax) for each major type of alcoholic beverage with alcohol consumption (litres of ethanol per capita per year). In the most fully adjusted model, which includes tax variables for each type of alcoholic beverage and control variables for real income and other factors, a

Table 2.1 Effects of alcohol taxes on alcoholic beverage consumption – studies with population-level data

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison group (if applicable)	Outcome Effect estimates	Covariates	Comments
Saffer (1989) 14 OECD countries: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Ireland, Luxembourg, the Netherlands, Norway, Sweden, United Kingdom, USA Adult population aged ≥ 15 years 1970–1983	Panel regression Brewers Association of Canada International Survey of Alcohol Beverage Control Policies Fixed-effects panel data regression analysis	National tax (typically an excise tax) per litre of pure ethanol for beer, wine, and spirits	Difference in natural log of alcohol consumption per capita per year (litres of ethanol) per US$ 1 higher tax Spirits tax: $\beta = -0.008$ ($t = 4.64$) Wine tax: $\beta = 0.003$ ($t = 0.67$) Beer tax: $\beta = -0.014$ ($t = 2.68$)	Real income, general mortality rate, spirits advertising ban, country, year	Neither *P* values nor CIs for effect estimates were reported; it was reported that both the beer tax and the spirits tax "significantly" lowered consumption, whereas the wine tax had "no significant effect"
Ponicki et al. (1997) Sweden Adult population aged ≥ 15 years January 1984–October 1994	Interrupted time series Systembolaget alcohol sales and price data, which include alcoholic beverages with an ethanol content > 3.5% Ordinary least-squares regression analysis (for overall results); seemingly unrelated regression analysis (for quality quintile results)	Implementation of a revenue-neutral tax policy on 1 July 1992 based on ethanol content, with rates increasing as ethanol concentration increased No control or comparison group	Proportional change in log of alcohol sales per capita per month (litres of ethanol) after the tax increase compared with the overall trends in alcohol sales Spirits: $\beta = 0.010$ ($P > 0.05$) Wine: $\beta = -0.038$ ($P > 0.05$) Beer: $\beta = -0.073$ ($P < 0.05$) Proportional change in log of alcohol sales per capita per month (litres of ethanol) after the tax increase compared with the pre-1992 trends in alcohol sales **Beverage-specific quality quintiles:** **Spirits:** 1: $\beta = -0.108$ ($P < 0.01$) 2: $\beta = -0.091$ ($P < 0.01$) 3: $\beta = -0.232$ ($P < 0.01$) 4: $\beta = 0.185$ ($P < 0.01$) 5: $\beta = 0.262$ ($P < 0.01$)	Mean real income per active earner, unemployment rate, month of the year, time and quadratic time, number of Fridays and weekdays in each month	Elasticities of total alcohol consumption were not estimated The quality quintiles represent the lowest to highest perceived quality classes categorized based on brand prices for each major beverage type The price per litre of ethanol increased more rapidly than the ethanol content per volume, resulting in higher taxes per unit of ethanol for beverages with higher ethanol concentrations

Table 2.1 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison group (if applicable)	Outcome Effect estimates	Covariates	Comments
Ponicki et al. (1997) (cont.)			**Wine:** 1: $\beta = -0.374$ ($P < 0.01$) 2: $\beta = -0.351$ ($P < 0.01$) 3: $\beta = -0.159$ ($P < 0.01$) 4: $\beta = 0.080$ ($P > 0.05$) 5: $\beta = 0.508$ ($P < 0.01$) **Beer:** 1: $\beta = -0.339$ ($P < 0.01$) 2: $\beta = -0.303$ ($P < 0.01$) 3: $\beta = -0.097$ ($P > 0.05$) 4: $\beta = -0.166$ ($P < 0.01$) 5: $\beta = -0.205$ ($P < 0.01$)		
Esser et al. (2016) Maryland, USA Adult population aged ≥ 15 years 2010–2012	Interrupted time series Monthly county-level aggregate alcohol sales data Multilevel mixed-effects multiple linear regression model	An increase of 3 percentage points (from 6% to 9%) in sales tax on all alcoholic beverages on 1 July 2011 No control or comparison group	Percentage change (95% CI) in total alcohol sales per 100 adults per month (gallons of ethanol) in the 18 months after the tax increase relative to expected sales estimated from trends before the tax increase –3.78% (–4.82% to –2.74%)	County characteristics (population density, annual county per capita income, unemployment rate), seasonal variation, national unemployment rates	Elasticities of total alcohol consumption were not estimated In sensitivity analyses in which the national unemployment rate was replaced with GDP in the model, the percentage change in total alcohol sales after vs before the tax increase was –6.50% (95% CI, –7.48% to –5.52%)

Table 2.1 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison group (if applicable)	Outcome Effect estimates	Covariates	Comments
Subbaraman et al. (2020) USA Adult population aged ≥ 15 years 2000–2013	Panel data Beverage Information Group Beer, Wine, and Liquor Handbooks; the National Alcohol Beverage Control Association database; the Alcohol Epidemiologic Data System; and producer-reported percentage of alcohol by volume Log–log panel data regression model	State-level specific excise and ad valorem excise beer and spirits taxes and tax changes from 2000 to 2013	Price elasticity (95% CI) on per capita total alcohol consumption (total volume) Beer tax: $\beta = -0.09$ (−0.16 to −0.01) Spirits tax: $\beta = -0.03$ (−0.10 to 0.04)	Year, state-level beer tax, spirits tax, government-controlled spirits sales, state-level outlet density, sales tax, unemployment, poverty, median household income, percentage of population non-Hispanic African American, percentage of population Hispanic, percentage of population aged ≥ 15 years, with fixed effects for state and year and clustering of standard errors by state	**Price elasticity (95% CI) on beer consumption:** Beer tax: $\beta = -0.19$ (−0.27 to −0.10) Spirits tax: $\beta = 0.06$ (−0.02 to 0.13) **Price elasticity (95% CI) on wine consumption:** Beer tax: $\beta = 0.11$ (−0.07 to 0.28) Spirits tax: $\beta = -0.18$ (−0.46 to 0.10) **Price elasticity (95% CI) on spirits consumption:** Beer tax: $\beta = -0.008$ (−0.13 to 0.11) Spirits tax: $\beta = -0.12$ (−0.24 to −0.002)

CI, confidence interval; GDP, gross domestic product; OECD, Organisation for Economic Co-operation and Development.

higher beer tax and a higher spirits tax were each associated with "significantly" lower alcohol consumption ([–1.4%] for the beer tax, [–0.8%] for the spirits tax), whereas a higher wine tax had "no significant effect" on alcohol consumption ([0.3%]). [The strengths of this study are that the tax and income for each country were converted to United States dollars and normalized using purchasing power parity across countries, and that beer, wine, and spirits taxes were included in the same model to estimate their independent associations. The limitations of the study are that only national alcohol taxes were assessed, without accounting for subnational taxes or VAT or sales taxes, and that it is unclear whether the alcohol consumption outcome is based on recorded alcohol consumption data only.]

On 1 July 1992, the Swedish government alcohol retail monopoly introduced a revenue-neutral reformulation of the tax rates according to the ethanol content per unit volume of the alcoholic beverages sold. Using monthly alcohol sales and price data from January 1984–October 1994, Ponicki et al. (1997) assessed the effect of the tax change on alcoholic beverage-specific prices and sales (litres of ethanol per capita per month) for beer, wine, and spirits, overall and by beverage-specific quintiles of quality classes categorized on the basis of their prices (i.e. lower-priced items were categorized as lower quality). The proportions by which beverage-specific prices and sales after 1 July 1992 were above or below their overall trend for overall prices or sales, or above or below their pre-1992 trend for quality quintiles of beverage-specific alcohol sales were estimated. The tax adjustment on ethanol content decreased the overall average price of spirits and wine and in higher quality classes, whereas the lower quality classes of spirits and wine became more expensive. The tax adjustment had no effect on overall beer price and in most quality classes of beer, with a small beer price increase in the highest quality classes. The tax adjustment was not associated with a change in overall spirits or wine sales ([1.0%]; $P > 0.05$ for spirits, and [–3.7%]; $P > 0.05$ for wine), but there was a [7.0%] ($P < 0.05$) decrease in overall beer sales. However, spirits and wine sales decreased in the three lowest quality classes and increased in the two highest quality classes, whereas beer sales decreased for all quality classes. [The strength of this study is the assessment of the effect of the change in tax on the change in price and then on the change in sales of each quality class of each type of alcoholic beverage. The limitations of the study are the assessment of a revenue-neutral reformulation of the tax structure according to the ethanol content of each alcoholic beverage and not the tax increase itself; that the outcome was change in sales for each alcoholic beverage type and not change in total alcohol sales; that the outcomes were price and sales data from an alcohol retail monopoly, which may not shift the price among the different alcoholic beverage-specific classes in response to market demand; and that it is unclear whether the alcohol sales outcome is based on off-premises alcohol sales data only.]

On 1 July 2011, the sales tax on all types of alcoholic beverages was increased by 3 percentage points, from 6% to 9%, in Maryland, USA. Esser et al. (2016) used county-level aggregate alcohol sales data from 1 January 2010 to 31 December 2012 to assess the effects of this tax increase on the percentage change in total alcohol sales (gallons of ethanol per 100 adults per month) during the 18 months after the tax increase relative to expected sales estimated from trends before the tax increase. In the model that adjusted for county characteristics (population density, average income, and unemployment rate), seasonality, and national unemployment rates, total alcohol sales were lower (–3.78%; 95% confidence interval [CI], –4.82% to –2.74%) during the 18 months after the tax increase, compared with the expected sales estimated from trends in the 18 months before the tax increase. In sensitivity analyses, after adjustment for county characteristics, seasonality, and annual gross domestic

product (GDP), the reduction in total alcohol sales was greater (–6.50%; 95% CI, –7.48% to –5.52%). [The strengths of this study are the inclusion of income and other factors as control variables, and the sensitivity analyses with different covariates. The limitations of the study are the lack of geographical control or comparison jurisdiction, the potential for cross-border shopping in other states near Maryland, and that the alcohol sales outcome is based on recorded alcohol sales data only.]

In the USA, Subbaraman et al. (2020) estimated the price elasticity of state-level specific excise taxes and ad valorem excise taxes combined for beer and for spirits on alcohol consumption (total volume per capita) of total alcohol, beer, wine, and spirits using 2000–2013 aggregated state-level data. A higher beer tax and a higher spirits tax were each associated with lower total alcohol consumption, with estimates of price elasticity of –0.09 (95% CI, –0.16 to –0.01) for the beer tax and –0.03 (95% CI, –0.10 to 0.04) for the spirits tax. In other words, a 1% increase in the beer tax was associated with a 0.09% reduction in total alcohol consumption. Similarly, a higher beer tax was associated with lower beer consumption (price elasticity, –0.19; 95% CI, –0.27 to –0.10) and a higher spirits tax was associated with lower spirits consumption (price elasticity, –0.12; 95% CI, –0.24 to –0.002). Neither the beer tax nor the spirits tax was statistically significantly associated with wine consumption. [The strengths of this study are the inclusion of income and control variables for other factors in the model. The limitations of the study are the lack of control for differences in tax or market structures across different states, which could potentially affect prices independently of the tax, and that it is unclear whether the alcohol sales outcome is based on recorded alcohol sales data only.]

(b) Studies with individual-level or household- level data

The effects of alcoholic beverage taxes on alcoholic beverage consumption also were assessed in four studies with individual-level or household-level data: two repeated cross-sectional studies (An and Sturm, 2011; Subbaraman et al., 2020) and one longitudinal panel study in the USA (Saffer et al., 2022) and one repeated cross-sectional study in Australia (Alexeev and Weatherburn, 2021) (Table 2.2).

An and Sturm (2011) used repeated cross-sectional waves of Behavioral Risk Factor Surveillance System survey data collected in 1984–2009 in the USA to assess the effect of the sum of state-level and federal alcohol excise taxes per gallon of beer on the change in alcohol consumption (the number of standard drinks consumed per month among individuals who consume alcohol) in the total population and stratified by race/ethnicity. In the preferred models for the total population, a 1% increase in excise tax was associated with a "statistically significant" 0.46% reduction in the number of standard drinks consumed per month. Similarly, a 1% increase in excise tax was associated with a "statistically significant" reduction in alcohol consumption in each race/ethnicity stratum (range, –0.80% to –0.35%). [The strength of this study is the large sample. The limitations of the study are the use of beer taxes as a proxy for all alcoholic beverage taxes, the lack of data on the prices of alcoholic beverages, the small variation in alcohol taxes over time across states, that federal taxes varied only once during the study period (in 1991), and that the response rates to the survey were not reported.]

In the USA, Subbaraman et al. (2020) estimated the price elasticity of state-level specific excise taxes and ad valorem excise taxes combined for beer and for spirits and general alcohol sales tax on total alcohol consumption (total volume of alcoholic beverages consumed in the past

Table 2.2 Effects of alcohol taxes on alcoholic beverage consumption – studies with individual-level and household-level data

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
An and Sturm (2011) USA 1 939 550 individuals (mean age, 44 years) 1984–2009	Repeated cross-sectional study 26 waves of the Behavioral Risk Factor Surveillance System survey 2-part regression: (1) probit and (2) ordinary least squares	Sum of federal and state-specific beer excise tax (as a proxy for the average tax for all types of alcoholic beverages)	Change in natural log of alcohol consumed per month (number of standard drinks) among individuals who consume alcohol Total population: $\beta = -0.46$ ($t = -17.45$) Non-Hispanic White: $\beta = -0.48$ ($t = -18.62$) African American: $\beta = -0.35$ ($t = -5.26$) Hispanic: $\beta = -0.35$ ($t = -4.97$) Asian or Pacific Islander: $\beta = -0.43$ ($t = -3.58$) Native American: $\beta = -0.75$ ($t = -3.51$) Other race or multirace: $\beta = -0.80$ ($t = -3.57$)	Sex, age and age squared, race/ethnicity, annual household income, education, employment status, marital status	Response rates not reported 1 standard drink = 14 g of ethanol "Both the dependent variable (i.e. alcohol consumption) and the treatment variable (i.e. tax) are in logarithmic form so that the coefficient of tax can be interpreted as elasticity" Neither *P* values nor CIs for effect estimates were reported; however, it was reported that the tax effects on alcohol consumption among individuals who consume alcohol are "all statistically significant"
Subbaraman et al. (2020) USA 18 072 adults aged ≥ 18 years who consume alcohol 2000–2015	Repeated cross-sectional study United States National Alcohol Survey for 2000, 2005, 2010, and 2015 Survey-weighted regressions (log–log models for continuous outcomes and logistic models for dichotomous outcomes) with no fixed effects for state	State-level specific excise and ad valorem excise beer and spirits taxes from 2003 to 2013	Price elasticity (95% CI) on total alcohol consumption (total volume of alcoholic beverages consumed over the past 12 months) **Beer tax:** White women: $\beta = -0.01$ (−0.22 to 0.19) White men: $\beta = 0.04$ (−0.19 to 0.26) African American women: $\beta = -0.5$ (−0.93 to −0.06) African American men: $\beta = 0.27$ (−0.37 to 0.91) Hispanic women: $\beta = -0.25$ (−0.84 to 0.35) Hispanic men: $\beta = 0.12$ (−0.49 to 0.72)	Year and state-level: beer tax, spirits tax, government control of spirits sales, sales tax; respondent zip code-level: off-premises beer outlets (logged), off-premises spirits outlets (logged), on-premises beer outlets (logged), on-premises spirits outlets (logged); respondent individual-level: age, marital status, education, employment, income; fixed effects for wet or dry region and year	Response rates not reported

Table 2.2 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Subbaraman et al. (2020) (cont.)			**Spirits tax:** White women: β = 0.04 (−0.25 to 0.33) White men: β = 0.02 (−0.33 to 0.36) African American women: β = 0.48 (−0.56 to 1.52) African American men: β = −0.51 (−1.6 to 0.59) Hispanic women: β = −1.04 (−1.56 to −0.51) Hispanic men: β = −0.92 (−1.65 to −0.19) **Sales tax:** White women: β = −0.02 (−0.14 to 0.09) White men: β = −0.06 (−0.18 to 0.06) African American women: β = −0.1 (−0.46 to 0.26) African American men: β = 0.01 (−0.46 to 0.48) Hispanic women: β = −0.09 (−0.41 to 0.23) Hispanic men: β = 0.11 (−0.14 to 0.36)		

Table 2.2 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Alexeev and Weatherburn (2021) Australia 223 979 adults aged ≥ 15 years (fully adjusted DiD model) 2002–2018	Time series 16 waves of the Household, Income and Labour Dynamics in Australia survey DiD using ordinary least-squares regression	70% increase in excise tax on ready-to-drink alcoholic beverages during 2008–2009 Age is used to define the control and treatment groups	Difference in the annual change in the log number of standard drinks consumed per day between adults aged 15–69 years and adults aged ≥ 70 years from 2009 to 2018 $\beta = -0.0889$ ($P < 0.001$)	Individual smoking status, household income, interaction variable for the state of New South Wales, linear cohort effects	Response rates not reported "Because the dependent variable is effectively log-transformed and 2009 is the omitted category, the estimated coefficients can be interpreted as the average annual percentage change in drinking relative to 2009" Compared with adults aged ≥ 70 years, there was a slightly greater reduction in alcohol consumption among adults aged 25–69 years than among adults aged 15–24 years
Saffer et al. (2022) USA (including Washington, DC; excluding 2 states without Nielsen Homescan data and 5 other states with a tax increase between 2008 and 2012) 22 262 households with an adult aged ≥ 25 years September 2007–August 2009 (pre-tax-increase period) to September 2009–August 2011 (tax-increase period)	Panel data Nielsen Homescan alcohol purchasing data Standard DiD	Increases in excise taxes in Illinois in 2009 per gallon of beer (21%), wine (90%), and spirits (90%) Comparison group: households in other states	Difference in the change in log total alcohol purchases per capita per month (ounces of ethanol) between Illinois and control states from the pre-tax-increase period to the tax-increase period **Consumption category:** Heavy: $\beta = -0.0662$ ($P < 0.05$) Other: $\beta = -0.0437$ ($P < 0.10$) **Income category:** Low: $\beta = 0.0233$ ($P \geq 0.10$) Other: $\beta = -0.0510$ ($P < 0.05$)	None reported	Response and attrition rates not reported A total of 985 828 household–month observations were aggregated into 48 observations for the intervention group (i.e. Illinois) and 48 observations for the comparison group The "other" alcohol consumption category is the combination of moderate and light alcohol consumption

CI, confidence interval; DiD, difference-in-difference.

12 months) using data from the 2000, 2005, 2010, and 2015 United States National Alcohol Survey. In analyses stratified by race/ethnicity and sex (a total of six strata), a 1% increase in the beer tax was associated with a 0.5% ($P < 0.05$) reduction in alcohol consumption among African American women; in the other five race/ethnicity and sex strata, the estimates of price elasticities for the beer tax ranged from −0.25% to 0.27% ($P > 0.05$ for each group). A 1% increase in the spirits tax was associated with a 1.04% ($P < 0.05$) reduction in alcohol consumption among Hispanic women and a 0.92% ($P < 0.05$) reduction in alcohol consumption among Hispanic men; in the other four race/ethnicity and sex strata, the estimates of price elasticity for the spirits tax ranged from −0.51% to 0.48% ($P > 0.05$ for each group). Within the race/ethnicity and sex strata, the estimates of price elasticity for the state-level general sales tax on total alcohol consumption ranged from −0.02% to 0.11% ($P > 0.05$ for each group). [The strength of this study is the inclusion of individuals from many different states for different years of observation. The limitations of the study are that the results were provided for the change in alcohol consumption by race/ethnicity and sex strata but not the overall change in alcohol consumption in the entire study population, that there was a lack of data on actual alcohol prices, and that the response rates for the telephone survey were not reported.]

During 2008 and 2009, the tax on ready-to-drink alcoholic beverages increased by 70% in Australia as part of a national strategy to reduce binge drinking. Alexeev and Weatherburn (2021) used 16 waves of annual (i.e. 2002–2018, excluding 2009) repeated cross-sectional data from the Household, Income and Labour Dynamics in Australia (HILDA) survey to assess the effect of the increase in the tax on ready-to-drink alcoholic beverages on the difference in the annual change in alcohol consumption (number of standard drinks consumed per day) between adults aged 15–69 years and adults aged ≥ 70 years from 2009 to 2018. In the most fully adjusted difference-in-difference model, there was a decrease (–8.9%; $P < 0.001$) in alcohol consumption among adults aged 15–69 years compared with adults aged ≥ 70 years. [The strengths of this study are the large individual-level data set, the inclusion of control variables for household income and other individual characteristics in the model, and the statistical approach using difference-in-difference models. The limitations of the study are that results are provided for the difference in the change in alcohol consumption between age groups but not the overall change in alcohol consumption in the entire study population, and that the response rates to the survey (overall or by age group) were not reported.]

On 1 September 2009, there was a large increase in alcohol excise tax (21% for beer, 90% for wine, and 90% for spirits) in Illinois, USA, which was plausibly exogenous to the state's level of alcohol consumption (Saffer et al., 2022). Saffer et al. (2022) used Nielsen Homescan household purchasing data to assess the effects of the increases in excise taxes in Illinois on the difference in the change in total alcohol purchases (ounces of ethanol per capita per month) between Illinois and states without a tax increase from the pre-tax-increase period (September 2007–August 2009) to the tax-increase period (September 2009–August 2011). In analyses stratified by alcohol consumption category, the increase in taxes resulted in decreases in total alcohol purchases in both the heavy consumption category ([–6.4%]; $P < 0.05$) and the other consumption category ([–4.3%]; $P < 0.10$). In analyses stratified by income, the increase in taxes resulted in a decrease in total alcohol purchases in the other income category ([–5.0%]; $P < 0.05$) but not in the low-income category ([2.4%]; $P \geq 0.10$). [The strengths of this study are the consistency of results among consumption strata and the analytical approach for assessing the difference-in-difference in total

alcohol purchases using geographical controls. The limitations of the study are the aggregation of household data, the small sample size, the lack of information on individual-level control variables in the model, that the response and attrition rates were not reported, and that the alcohol purchases outcome is based on off-premises alcohol purchases data only.]

(c) Pathways from taxation policies to price and consumption

One of the basic laws of economics defines the demand for a product as being negatively related to the retail price of the product if everything else is kept constant. This means that an increase in price leads to a decrease in the demand for the product (Chaloupka et al., 2002). Taxes, as well as production, advertising, distribution, and other costs, affect prices, and increasing taxes can increase price. The passing of taxes to prices is measured with the pass-through rate (WHO, 2023a). Taxes can be passed through to prices proportionally (entirely shifted, rate = 1), less than proportionally (undershifted, rate < 1), or more than proportionally (overshifted, rate > 1). The actual pass-through rate can depend on the market structure (i.e. more competitive markets imply rapid and entirely shifted taxes, whereas more concentrated markets imply overshifted taxes), the structure of the taxes (e.g. ad valorem excise taxes give producers more room to undershift taxes in certain products and overshift them in others), the availability of close product substitutes, and other parameters (Poterba, 1996; Xu and Chaloupka, 2011; WHO, 2023a). Fig. 2.2 illustrates the pathway from alcohol taxation policies to price and consumption.

A narrative review of the effect sizes of pass-through is beyond the scope of this volume. Evidence from selected studies on the pass-through rates of alcohol taxes to alcohol prices (Ally et al., 2014; Shang et al., 2020; Gehrsitz et al., 2021; Wilson et al., 2021) demonstrates substantial pass-through rates of alcohol taxes to prices (Table 2.3). The Working Group did not identify any studies in which alcohol taxes were not passed through to prices.

The effect of changes in prices of alcoholic beverages on alcohol consumption (i.e. price elasticity of demand) has been widely studied. Babor et al. (2023) summarized the price elasticity of demand for total alcohol and/or specific major types of alcoholic beverages estimated from eight meta-analyses; among the five meta-analyses that assessed total alcohol, the mean price elasticity ranged from −0.50 to −0.64 (3 studies) and the median price elasticity ranged from −0.50 to −0.77 (2 studies). In one of the meta-analyses, Gallet (2007) found that the long-run estimates were usually more elastic (−0.82) than the short-run estimates (−0.52), which indicates that individuals who consume alcoholic beverages tend to reduce their consumption more in the long term than in the short term after an increase in price. In a large umbrella review of 30 systematic reviews, there was evidence suggesting that the own-price elasticity for total alcohol is similar between high-income countries and low- and middle-income countries (Guindon et al., 2022). Own-price elasticity has also been shown to differ by population subgroup. For example, individuals who consume larger amounts of alcohol tend to be more inelastic (Guindon et al., 2022); in other words, they reduce their consumption by less than average after a price increase. In addition, younger individuals are less responsive to price than older individuals (Gallet, 2007).

2.2.2 Minimum pricing

(a) Studies with population-level data

The effects of alcohol minimum pricing policies on population-level measures of alcohol consumption were assessed in studies in Scotland, United Kingdom (Robinson et al., 2021; Giles et al., 2022, 2024), Saskatchewan, Canada (Stockwell et al., 2012), and the Northern

Table 2.3 Selected studies on pass-through rates of alcohol taxes to alcoholic beverage prices

Reference Location Study period	Study type Data source Method of analysis	Tax policy intervention	Pass-through[a]	Comments
Ally et al. (2014) United Kingdom March 2008–August 2011	Time series Product-level price data from 4 supermarkets representing ~50% of all off-premises alcohol sales; AC Nielsen volume of off-premises sales in England and Wales across the price distribution of each type of alcoholic beverage Quantile regression	4 episodes of excise tax-only changes, 2 episodes of VAT-only changes, and 1 simultaneous change in both tax and VAT The pass-through outcomes are based on all tax episodes	**Beer:** Tax increases were overshifted to beer prices for products sold above the 15th percentile of the price of beer and undershifted to beer prices for products sold up to the 15th percentile of the price of beer, with the largest undershifting for the lowest 5th percentile of products (pass-through rate, 0.85; 95% CI, 0.79 to 0.92) **Spirits:** Tax increases were overshifted to spirits prices for products sold above the 15th percentile of the price of spirits and undershifted to spirits prices for products sold up to the 15th percentile of the price of spirits, with the largest undershifting for the lowest 5th percentile of products (pass-through rate, 0.86; 95% CI, 0.83 to 0.89) **Wine:** Tax increases were overshifted to wine prices for products sold above the 5th percentile of the price of wine and undershifted to wine prices for products sold up to the 5th percentile of the price of wine ([pass-through rate, 0.9; 95% CI, 0.8 to 1.1])[b] **Cider/RTD:** Tax increases were overshifted to cider/RTD prices for products sold above the 5th percentile of the price of cider/RTD and undershifted to cider/RTD prices for products sold up to the 5th percentile of cider/RTD prices ([pass-through rate, 0.89; 95% CI, 0.82 to 0.95])[b]	Beer: 38% of sales were at prices in the lowest 5th percentile of the price of beer distribution Spirits: 16% of sales were at prices in the lowest 5th percentile of the price of spirits distribution Wine: 9.5% of sales were at prices in the lowest 5th percentile of the price of wine distribution Cider/RTD: 28% of sales were at prices in the lowest 5th percentile of the price of cider/RTD distribution
Shang et al. (2020) 64 cities in 27 OECD countries 2003–2016	Time series Worldwide Cost of Living Survey, biennial data on alcoholic beverage prices from supermarkets and mid-priced stores in major cities Fixed-effects, dynamic, or first-difference regression models	Tax rates on 1 January for 2003, 2005, 2007, 2009, 2012, and 2014 (OECD tax database) The pass-through outcomes are for the specific tax increase for each type of alcoholic beverage and are pooled from fixed-effects, dynamic, and first-difference models that examine different price levels (high, low, and average prices for beer and wine, and high and low prices for spirits)	**Beer:** Beer tax increases were non-significantly overshifted to beer prices (pass-through rate, 1.24; 95% CI, 0.67 to 1.81) **Wine:** Wine tax increases were overshifted to wine prices (pass-through rate, 2.4; 95% CI, 1.70 to 3.11) **Spirits:** Spirits tax increases were overshifted to cognac prices (pass-through rate, 1.71; 95% CI, 1.07 to 2.35), were overshifted to Cointreau liqueur prices (pass-through rate, 1.98; 95% CI, 1.21 to 2.76), and were non-significantly overshifted to Scotch whisky prices (pass-through rate, 1.14; 95% CI, 0.52 to 1.75); in contrast, spirits tax increases were undershifted to gin prices (pass-through rate, 0.85; 95% CI, 0.34 to 1.35)	

Table 2.3 (continued)

Reference Location Study period	Study type Data source Method of analysis	Tax policy intervention	Pass-through[a]	Comments
Gehrsitz et al. (2021) USA September 2007–August 2011	Time series NielsenIQ weekly retail scanner point-of-sale data representing ~50% of national grocery and drug store sales volume; after exclusions, the included products represented half of the total alcohol sales among included stores DiD ordinary least-squares models	The increase in excise tax was from US$ 4.50 to US$ 8.55 per gallon for spirits, from US$ 0.73 to US$ 1.39 per gallon for wine, and 4 cents per gallon for beer, in Illinois on 1 September 2009 The pass-through rates are the net proportional increase in price due to the tax increase in Illinois relative to other states without tax increases for each alcoholic beverage type	**Beer:** Beer tax increases (pass-through rate, NR) **Spirits:** Spirits tax increases were overshifted to spirits prices (pass-through rate, "about 1.5"; $P < 0.001$); the pass-through rate was similar across all deciles of spirits prices **Wine:** Wine tax increases were overshifted to wine prices (pass-through rate, "about 1.3"; $P < 0.001$); the pass-through rate was > 1 for deciles of wine prices up to the 70th percentile but not for the 3 higher deciles	
Wilson et al. (2021) United Kingdom On-premises retailers in England (~2000) 2007–2017	Quarterly on-premises sales data from CGA On Premise Measurement Service Quantile regression analysis	11 excise tax changes and 3 sales tax changes from 2007 to 2017 The pass-through outcomes are for the specific tax increase for each type of alcoholic beverage and are shown for 11 quantiles of the baseline price	**Beer:** Beer tax increases were overshifted to beer prices for products sold above the 95th percentile of the price of beer ([pass-through rate, 1.18; 95% CI, 1.06 to 1.24]) [b], and undershifted for all quantiles up to the 85th percentile of the price of beer **Spirits:** Spirits tax increases were overshifted to spirits prices for products sold above the 95th percentile of the price of spirits ([pass-through rate, 1.27; 95% CI 1.24 to 1.31])[b], and undershifted for all quantiles up to the 75th percentile of the price of spirits **Wine:** Wine tax increases were overshifted to wine prices for products sold above the 95th percentile of the price of wine ([pass-through rate, 1.12; 95% CI 1.08 to 1.16])[b], and undershifted for all quantiles up to the 85th percentile of the price of wine	

CI, confidence interval; DiD, difference-in-difference; NR, not reported; OECD, Organisation for Economic Co-operation and Development; RTD, ready-to-drink alcoholic beverages; VAT, value-added tax.

[a] Overshifted taxes occur when the retail price of a product increases by proportionally more than the amount of the tax increase (i.e. rate > 1). Undershifted taxes occur when the retail price of a product increases by proportionally less than the amount of the tax increase (i.e. rate < 1). Entirely shifted taxes occur when the retail price of a product increases by the same proportional amount as the tax increase (rate = 1).

[b] Pass-through rate and 95% CI approximated from the graphs published in the reference.

Territory, Australia (Taylor et al., 2021; O'Brien et al., 2022) (Table 2.4).

(i) Scotland, United Kingdom

The Alcohol (Minimum Pricing) (Scotland) Act 2012 paved the way for the Alcohol (Minimum Price per Unit) (Scotland) Order 2018, which introduced a MUP of 6.25 pence per gram of pure ethanol in Scotland on 1 May 2018. The Public Health (Minimum Price for Alcohol) (Wales) Act 2018 led to the implementation of the same policy in Wales on 2 March 2020. The MUP policy prohibited retailers from selling alcohol below the MUP threshold and led to an average price increase of 10% for off-premises sales of alcoholic beverages in Scotland, which was heavily skewed towards the cheapest alcoholic beverages on the market (Ferguson et al., 2022).

In the most recent report on the effect of the MUP in Scotland on population-level measures of alcohol consumption, Giles et al. (2024) used both off-premises (from NielsenIQ) and on-premises (from CGA Strategy) retail alcohol sales data to estimate the change in total, off-premises, and on-premises alcohol sales (litres of ethanol per adult per week) from January 2013 to April 2021. In a model that controlled for trends in alcohol sales in England and Wales combined and other factors, 3 years after implementation of the MUP, there was a reduction in total alcohol sales in Scotland (–3.0%; 95% CI, –4.2% to –1.8%), which was largely due to a reduction in off-premises alcohol sales (–3.6%) and not on-premises alcohol sales (0.0%). In an earlier report (Robinson et al., 2021), the reduction in off-premises alcohol sales (litres of ethanol per adult per year) during the 12 months after implementation of the MUP was similar (–3.5%; 95% CI, –4.9% to –2.2%) to the reduction 3 years after implementation of the MUP. To test the robustness of the data sources, the change in off-premises alcohol sales from January 2017 through April 2021 was compared between two data sources (Giles et al., 2024); the percentage change in alcohol sales was slightly greater using the NielsenIQ data (–4.2%; 95% CI, –6.0% to –2.4%) than using the IRI data (–2.8%; 95% CI, –4.6% to –1.0%). [The strengths of these analyses are the geographical control of trends in alcohol sales in England and Wales combined during the same period, the different analyses for different periods after implementation of the MUP, and the inclusion of household disposable income, seasonal trends, and other control variables in the model. The limitation of these analyses is that the NielsenIQ and IRI data both exclude purchases in two discount retail chains and some online sales.]

(ii) Saskatchewan, Canada

On 1 April 2010, the alcohol minimum pricing policy changed in Saskatchewan, which is a province with a government monopoly on alcohol distribution and, at that time, a partial government monopoly on alcohol sales in liquor stores; the changes included an increase in minimum prices for alcoholic beverages already covered by a minimum pricing policy, as well as the inclusion of additional types of alcoholic beverages that were previously not covered (Stockwell et al, 2012). The effects of the change in the minimum pricing policy on alcohol consumption (litres of ethanol per capita per year) from April 2008–March 2010 to April 2010–March 2012 were assessed by Stockwell et al. (2012). A 1% increase in minimum price led to a −0.84% (95% CI, –1.16% to −0.52%) decrease in alcohol consumption. [The strengths of this study are the inclusion of control variables for mean price per standard drink, household income, and period in the model. The limitation of the study is the lack of a geographical control.]

(iii) Northern Territory, Australia

On 22 August 2018, the Northern Territory Legislative Assembly passed a bill that set the MUP for alcohol at Aus$ 1.30 per standard drink (10 g of ethanol); it was applied to all off-premises,

Table 2.4 Effects of alcohol minimum pricing policies on alcoholic beverage consumption – studies with population-level data

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Scotland, United Kingdom					
Giles et al. (2024), Robinson et al. (2021) Scotland, England, Wales (United Kingdom) Adult population aged ≥ 16 years January 2013 to April 2021 (Giles et al., 2024), January 2013 to April 2019 (Robinson et al., 2021)	Interrupted time series NielsenIQ and IRI off-premises and CGA Strategy on-premises alcohol sales data Seasonal ARIMA	Implementation of a minimum price of 50 pence per United Kingdom unit (6.25 pence per gram of ethanol) for the sale of alcohol in Scotland on 1 May 2018 England and Wales (control countries)	Percentage change (95% CI) in alcohol sales per adult per week (litres of ethanol) **From January 2013–April 2018 to May 2018–April 2021 (on-premises was truncated at 2020)** (Giles et al., 2024) Total: –3.0% (–4.2% to –1.8%) NielsenIQ off-premises: –3.6% (–4.8% to –2.5%) CGA on-premises: 0.0% (–0.2% to 0.1%) **From January 2017–April 2018 to May 2018–April 2021** (Giles et al., 2024) NielsenIQ off-premises: –4.2% (–6.0% to –2.4%) IRI off-premises: –2.8% (–4.6% to –1.0%) Percentage change (95% CI) in alcohol sales per adult per year (litres of ethanol) from January 2013–April 2018 to May 2018–April 2019 (Robinson et al., 2021) NielsenIQ off-premises: –3.5% (–4.9% to –2.2%)	2013–2021 (or 2013–2020) models: trends in alcohol sales in England and Wales combined, trends in household disposable income, underlying seasonal and secular trends, introduction of COVID-19-related restrictions and MUP in Wales; models for trends in off-premises and on-premises sales also were mutually adjusted 2017–2021 off-premises model: including same covariates 2013–2019 off-premises models: trends in off-premises alcohol sales in England and Wales combined, trends in household disposable income, on-premises alcohol sales	The percentage change from January 2013–April 2018 to May 2018–April 2021 represents a 1.1% decrease in alcohol sales in Scotland and a 2.4% increase in alcohol sales in England and Wales in uncontrolled models In earlier analyses, the reduction in off-premises alcohol sales through April 2019 was consistent between the NielsenIQ (–6.6%) and IRI (–4.0%) data sets (Giles et al., 2022)

Table 2.4 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Saskatchewan, Canada					
Stockwell et al. (2012) Saskatchewan, Canada Adults aged ≥ 15 years April 2008–March 2010 (pre-MUP period) to April 2010–March 2012 (MUP period)	Interrupted time series SLGA on-premises and off-premises retail alcohol sales data ARIMA	Minimum alcohol retail prices and prices for bar and restaurant owners were increased for beer, wine, and spirits other than brandy and cognac and were newly introduced for higher-strength coolers, brandy, and pre-mixed cocktails in Saskatchewan on 1 April 2010 No control or comparison	Percentage change (95% CI) in alcohol consumption per capita per year (litres of ethanol) per 1% increase in minimum price from the pre-MUP period to the MUP period −0.84% (−1.16% to −0.52%)	Mean price per standard drink, household income, period	The SLGA has a government monopoly on alcohol distribution and a partial government monopoly on the sale of alcohol in liquor stores The effect estimate is a price elasticity [The authors state that there was no change in alcohol consumption during the study period in a neighbouring province (Alberta), but no data were shown]
Northern Territory, Australia					
Taylor et al. (2021) Northern Territory, Australia Adult population aged ≥ 15 years Q1 2013–Q3 2018 (pre-MUP period) to Q4 2018–Q3 2019 (MUP period)	Interrupted time series Quarterly wholesale alcohol supply data Interrupted time-series method of analysis not specified	Implementation of a MUP of Aus$ 1.30 per standard drink (10 g of ethanol) in the Northern Territory on 1 October 2018 No control or comparison	Change (95% CI) in alcohol consumption per capita per quarter (litres of ethanol) from the pre-MUP period to the MUP period Northern Territory: $\beta = -0.15$ (−0.22 to −0.09) Darwin and Palmerston combined: $\beta = -0.06$ (−0.019 to 0.08 [sic])	Time, number of people on the BDR at the end of each quarter, season	Short period after implementation of the MUP Effects for Darwin and Palmerston combined were reported separately because other alcohol policy interventions were introduced elsewhere in the Northern Territory during nearly the same time period as the MUP [The Working Group noticed that the lower value of the CI for the Darwin and Palmerston combined effect is implausible]

Table 2.4 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
O'Brien et al. (2022) Australia Total population August 2018–February 2020	Interrupted time series Alcohol consumption estimated based on concentration of ethyl sulfate in urban and rural wastewater samples (n = 2816 before implementation, n = 2101 after implementation of the MUP) Linear mixed model	Same intervention as above in the Northern Territory Other Australian states and territories (control)	Change (99% CI) in alcohol consumption per 1000 people per day (number of standard drinks) from August 2018 to: **Northern Territory:** October 2018: −1231 (−1633 to −830) October 2019: −520 (−851 to −189) February 2020: −283 (−681 to 116) **Queensland:** October 2018: −310 (−550 to −114) **All other states and territories:** October 2018: "no significant drop"	State or territory, capital or regional area	1 standard drink = 10 g of ethanol

ARIMA, autoregressive integrated moving average; BDR, Banned Drinker Register; CI, confidence interval; MUP, minimum unit price; Q, quarter; SLGA, Saskatchewan Liquor and Gaming Authority.

on-premises, and online retail sales of alcoholic beverages and was implemented on 1 October 2018. Two interrupted time-series studies assessed the effects of this policy intervention on population-level measures of alcohol consumption (Taylor et al., 2021; O'Brien et al., 2022).

Taylor et al. (2021) used wholesale alcohol supply volume (litres of ethanol) data from quarter 1 of 2013 to quarter 3 of 2019 to estimate the change in alcohol consumption (litres of ethanol per capita per quarter) in the Northern Territory. Results were also reported separately for the Northern Territory cities Darwin and Palmerston combined, because these cities were less likely to have been affected by other alcohol policy interventions implemented elsewhere in the Northern Territory during nearly the same time period. From quarter 1 of 2013–quarter 3 of 2018 to quarter 4 of 2018–quarter 3 of 2019, there was a reduction in alcohol consumption in the Northern Territory (−0.15 litres of ethanol per capita per quarter; 95% CI, −0.22 to −0.09) but not in Darwin and Palmerston combined (−0.06 litres of ethanol per capita per quarter; 95% CI, −0.019 to 0.08 [sic]). [The strength of this study is the good-quality sales data and the inclusion of a control variable for seasonality. The limitations of the study are the lack of a geographical control, the short period after implementation of the MUP (1 year), the lack of control for income or for other alcohol policy interventions implemented in the Northern Territory just before implementation of the MUP, and that the alcohol consumption outcome was based on wholesale alcohol supply data only. The Working Group noticed that the lower value of the confidence interval for the Darwin and Palmerston combined effect is implausible.]

O'Brien et al. (2022) used data from wastewater treatment plants in all states and territories in Australia to estimate alcohol consumption (number of standard drinks per 1000 people per day) from the concentrations of ethyl sulfate (an alcohol-specific metabolite) in the wastewater. From August to October 2018, there was a 38.8% reduction in alcohol consumption (−1231 standard drinks per 1000 people per day; 99% CI, −1633 to −830) in the Northern Territory. The decrease in consumption was attenuated by October 2019 (−520 standard drinks per 1000 people per day; 99% CI, −851 to −189) and February 2020 (−283 standard drinks per 1000 people per day; 99% CI, −681 to 116). In most other states and territories in Australia, the decreases in alcohol consumption from August to October 2018 were not statistically significant except in Queensland, where there was a modest decrease in alcohol consumption (−310 standard drinks per 1000 people per day; 99% CI, −550 to −114). [The strengths of this study are the novel data source, which is not affected by self-report biases or retail sales data coverage, and the large number of wastewater samples before and after the intervention. The limitation of the study is the lack of control for income or for other alcohol policy interventions implemented in the Northern Territory just before implementation of the MUP.]

(b) Studies with individual-level or household-level data

The effects of the Alcohol (Minimum Pricing) (Scotland) Act 2012 (described in Section 2.2.2(a)(i)) on alcoholic beverage consumption or sales, relative to other geographical controls in the United Kingdom, in the first few months up to 3 years after the 1 May 2018 implementation of the MUP were assessed in six informative studies with individual-level or household-level data (O'Donnell et al., 2019; Anderson et al., 2021; Griffith et al., 2022; Holmes et al., 2022; Rehm et al., 2022; Stevely et al., 2023) (Table 2.5). One of the studies also assessed the effects of the MUP in Wales (Anderson et al., 2021).

Three longitudinal panel studies used Kantar fast-moving consumer goods (FMCG) purchase panel data, which are derived from scanning

Table 2.5 Effects of alcohol minimum pricing policies on alcoholic beverage consumption – studies with individual-level and household-level data

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison group (if applicable)	Outcome Effect estimates	Covariates	Comments
O'Donnell et al. (2019) Scotland, England (United Kingdom) Adult households (Scotland: n = 5325; England: n = 54 807) January 2015–April 2018 (pre-MUP period) to May–December 2018 (MUP period)	Longitudinal panel Kantar FMCG purchase panel data of off-premises sales (barcode scanning of purchases at home) Linear regression	Implementation of a minimum price of 50 pence per United Kingdom unit (6.25 pence per gram of ethanol) for the sale of alcohol in Scotland on 1 May 2018 England (control)	Change (95% CI) in off-premises alcohol purchases per adult per household per week (grams of ethanol) from the pre-MUP period to the MUP period β = –9.50 (–13.89 to –5.11)	Time, country, interaction of time and country	Quota sampling; response and attrition rates not reported; short period after implementation of the MUP The reduction in alcohol purchases was only in the highest quintile of the purchasing distribution and was greater in the 3 lowest quintiles than in the 2 highest quintiles of the income distribution
Anderson et al. (2021) Scotland, Wales, Northern England, Western England (United Kingdom) Adult households (35 242 total) Scotland: January 2015–April 2018 (pre-MUP period) to May–December 2018 (immediate MUP period) and January–12 July 2020 (mid-term MUP period); Wales: January–February 2020 (pre-MUP period) to March–12 July 2020 (MUP period)	Longitudinal panel Kantar FMCG purchase panel data of off-premises sales (barcode scanning of purchases at home) Wales: immediate effects were between January–February 2020 and March–12 July 2020 ARIMA	Same intervention as above in Scotland, and in Wales on 2 March 2020 Northern England (control for Scotland) Western England (control for Wales)	Difference (95% CI) in the change in off-premises alcohol purchases per adult per household per day (grams of ethanol on the days when alcohol was purchased) between Scotland and Northern England and between Wales and Western England from the pre-MUP period to the MUP period Scotland, immediate: β = –7.57 (–7.88 to –7.26) Scotland, mid-term: β =–7.06 (–7.47 to –6.66) Wales, immediate: β = –7.05 (–7.64 to –6.46)	Difference between Scotland and Northern England for age of main household shopper, income per adult household member, proportion of households with head of household manual worker or non-working No covariates for estimates of immediate effects in Wales	Quota sampling; response and attrition rates not reported; unexplained gap in data after implementation of the MUP in Scotland; short period after implementation of the MUP in Wales Additional control for COVID-19-related restrictions showed similar mid-term effects of MUP in Scotland (–6.82; 95% CI, –7.40 to –6.25) and similar immediate effects of MUP in Wales (–7.17; 95% CI, –8.09 to –6.25)

Table 2.5 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison group (if applicable)	Outcome Effect estimates	Covariates	Comments
Griffith et al. (2022) Scotland, England (United Kingdom) 32 468 households Adult households (Scotland: *n* = 2972; England: *n* = 29 496) May 2016–April 2018 (pre-MUP period) to May 2018–January 2020 (MUP period)	Longitudinal panel Kantar FMCG purchase panel data of off-premises sales (barcode scanning of purchases at home) DiD	Same intervention as above in Scotland England (control)	Difference in the percentage change in the off-premises alcohol purchases per adult per week (number of units) between Scotland and England from the pre-MUP period to the MUP period 11.2% (*P*, NR) Difference in the percentage change in the probability of purchasing off-premises alcohol between Scotland and England from the pre-MUP period to the MUP period 3.0% (*P*, NR)	Household fixed effects (including equivalized income), time effects, major holidays	Quota sampling; response and attrition rates not reported 1 unit = 8 g of ethanol The number of alcohol units purchased per adult per week decreased by 9.2% in the 70th to 80th percentile, by 3.6% in the 80th to 90th percentile, by 10.4% in the 90th to 95th percentile, and by 14.8% in the > 95th percentile of the long-run purchasing distribution but not in lower percentile strata
Rehm et al. (2022) Scotland, England (United Kingdom) 53 347 women, 53 143 men aged ≥ 18 years January 2015–April 2018 (pre-MUP period) to May–December 2018 (MUP period)	Interrupted time series On- and off-premises alcohol consumption from Kantar Alcovision alcohol consumption survey and retrospective 1-week diary data, which include self-report of occasions of alcohol consumption Generalized linear regression	Same intervention as above in Scotland England (control)	Difference (95% CI) in the change in alcohol consumption per adult per week (grams of ethanol) between Scotland and England from the pre-MUP period to the MUP period All: β = −5.94 (−10.60 to −1.29) Men: β = −3.30 (−10.25 to 3.64) Women: β = −8.59 (−14.32 to −2.85)	Seasonality, consumption level	Quota sampling; response and attrition rates not reported; short period after implementation of the MUP Sensitivity analyses comparing Scotland with Northern England showed similar results overall (−5.88 g of ethanol per adult per week), for women (−7.49 g of ethanol per adult per week), and for men (−4.26 g of ethanol per adult per week) Stratified analyses showed reductions in consumption among adults aged ≥ 65 years but not younger adults

Table 2.5 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison group (if applicable)	Outcome Effect estimates	Covariates	Comments
Holmes et al. (2022) Scotland, Northern England (United Kingdom) Adults aged ≥ 18 years assessed as "probably alcohol-dependent" by a service provider (Scotland: wave 1, *n* = 170; wave 2, *n* = 190; wave 3, *n* = 123; Northern England: wave 1, *n* = 85; wave 2, *n* = 86; wave 3, *n* = 52) November 2017–March 2020	Repeated cross-sectional survey Researcher-administered structured interviews during wave 1 (November 2017–April 2018, pre-MUP period), wave 2 (August 2018–February 2019, early-MUP period), and wave 3 (November 2019–March 2020, late-MUP period) Linear regression DiD	Same intervention as above in Scotland Northern England (control)	Difference in the change in alcohol consumption per adult during the week before entering treatment (number of units) between Scotland and Northern England From the pre-MUP period to the early-MUP period: NR (*P* = 0.42) From the pre-MUP period to the late-MUP period: NR (*P* = 0.95)		The only study that focuses on MUP effects among individuals dependent on alcohol; small sample size 1 unit = 8 g of ethanol Wave-specific mean consumption values were reported in descriptive tables, and parameter estimates from regression models were not reported
Stevely et al. (2023) Scotland, Northern England (United Kingdom) 110 361 adults aged ≥ 18 years (Scotland: *n* = 38 674; Northern England: *n* = 71 687) who reported consuming alcohol at least once per year January 2009–April 2018 (pre-MUP period) to May 2018–February 2020 (MUP period)	Repeated cross-sectional survey On- and off-premises alcohol consumption from Kantar Alcovision alcohol consumption survey and retrospective 1-week diary data, which include self-report of occasions of alcohol consumption Seasonal ARIMA	Same intervention as above in Scotland Northern England (control)	Change (absolute percentage points; 95% CI) in the prevalence of harmful, hazardous, or moderate alcohol consumption from the pre-MUP period to the MUP period Harmful: +0.6 (–1.1 to 2.3) Hazardous: –3.5 (–5.4 to –1.7) Moderate: +1.4 (–1.1 to 3.8)	Beverage preference, alcohol consumption patterns, changes in welfare policy, alcohol consumption patterns in Northern England	Quota sampling; response rates not reported Harmful: > 400 g of ethanol per week in men or > 280 g of ethanol per week in women; hazardous: 112–400 g of ethanol per week in men or 112–280 g of ethanol per week in women; moderate: < 112 g of ethanol per week

ARIMA, autoregressive integrated moving average; CI, confidence interval; DiD, difference-in-difference; FMCG, fast-moving consumer goods; MUP, minimum unit price; NR, not reported; SE, standard error.

the barcodes of all household purchases from selected retailers, to assess off-premises alcohol purchases, but these studies reported results for different periods after implementation of the MUP and for different groups within the study populations (O'Donnell et al., 2019; Anderson et al., 2021; Griffith et al., 2022). Two repeated cross-sectional studies used Kantar Alcovision survey data, which include a retrospective 1-week alcohol consumption diary, to assess alcohol consumption (Rehm et al., 2022; Stevely et al., 2023). The sixth study was a repeated cross-sectional study that used data from interviews with individuals who were recruited from specialist treatment providers and were determined to be "probably alcohol-dependent" (Holmes et al., 2022). The effects of the MUP policy on alcohol consumption were assessed using difference-in-difference (Griffith et al., 2022; Holmes et al., 2022) or other (O'Donnell et al., 2019; Anderson et al., 2021; Rehm et al., 2022; Stevely et al., 2023) methods of statistical analysis.

In an early longitudinal panel study, O'Donnell et al. (2019) assessed the change in off-premises purchases of alcoholic beverages (grams of ethanol per adult per household per week) in Scotland from January 2015–April 2018 to May–December 2018. In a model that controlled for change in alcohol purchases in England, in Scotland there was a reduction in weekly off-premises alcohol purchases after implementation of the MUP (–9.50 g of ethanol per adult per household per week; 95% CI, –13.89 to –5.11). However, in stratified analyses, the reduction in off-premises alcohol purchases was only in the highest quintile of the purchasing distribution (before implementation of the MUP) and was greater in the three lowest quintiles than in the two highest quintiles of the income distribution. [The strengths of this study are the large sample, stratified analyses by prior alcohol consumption and household income, and the inclusion of geographical controls. The limitations of the study are the short period after implementation of the MUP, the risk of bias due to quota sampling, that the response and attrition rates were not reported, and that the alcohol purchases outcome was based on off-premises alcohol purchases data only.]

In a second longitudinal panel study, Griffith et al. (2022) used the same data to assess the effects of the MUP in Scotland on the difference in the percentage change in off-premises alcohol purchases (number of units per adult per week) between Scotland and England from May 2016–April 2018 to May 2018–January 2020, overall and by strata of the long-run (i.e. number of alcohol units purchased per adult per week from May 2016 through April 2017) household alcohol purchase distribution. Compared with the change in England, the off-premises alcohol purchases decreased by 11.2% in Scotland, and the probability of purchasing off-premises alcohol in a given week decreased by 3.0%. However, in stratified analyses, there was no difference in the change in alcohol purchases among strata below the 70th percentile of the long-run household alcohol purchase distribution, whereas there was a reduction that increased across higher strata of the distribution, with the largest reduction in the 95th percentile of the distribution (–14.8%; $P < 0.05$). [The strengths of this study are the large sample, the longer period after implementation of the MUP (20 months), stratified analyses by the alcohol purchase distribution and proximity to the Scotland–England border, the inclusion of geographical controls, and control for household fixed effects, including equivalized household income, in the model. The limitations of the study are the risk of bias due to quota sampling, that the response and attrition rates were not reported, and that the alcohol purchases outcome was based on off-premises alcohol purchases data only.]

In the third longitudinal panel study, Anderson et al. (2021) estimated the difference in the change in off-premises alcohol purchases (grams of ethanol per adult per household per day on

days when alcohol was purchased) between Scotland and Northern England from January 2015–April 2018 to May–December 2018 (immediate effects) and from January 2015–April 2018 to January–12 July 2020 (mid-term effects). After implementation of the MUP, there was an immediate reduction in off-premises alcohol purchases in Scotland (–7.57 g of ethanol per adult per household per day; 95% CI, –7.88 to –7.26), which was maintained up to mid-2020. In this study, immediate effects (i.e. from January–February 2020 to March–12 July 2020) of the MUP in Wales on off-premises alcohol purchases in Wales compared with Western England were also assessed; there was an immediate reduction in off-premises alcohol purchases (–7.05 g of ethanol per adult per household per day; 95% CI, –7.64 to –6.46). [The strengths of this study are the large sample, the inclusion of geographical controls, the inclusion of household income and other control variables in the model, and that in the sensitivity analysis, after controlling for COVID-19-related restrictions, similar mid-term effects after implementation of the MUP were observed for Scotland and similar immediate effects after implementation of the MUP were observed for Wales. The limitations of the study are the short period after implementation of the MUP for Wales, an unexplained year-long break in the data series after implementation of the MUP for Scotland, the risk of bias due to quota sampling, that the response and attrition rates were not reported, and that the alcohol purchases outcome was based on off-premises alcohol purchases data only.]

Rehm et al. (2022) used the Kantar Alcovision repeated cross-sectional survey and retrospective 1-week diary data from 2015 through 2018 to assess the difference in the change in alcohol consumption (grams of ethanol per adult per week) between Scotland and England from January 2015–April 2018 to May–December 2018. Among individuals who reported any alcohol consumption during the week before reporting, there was a reduction in alcohol consumption in Scotland compared with England (–5.94 g of ethanol per adult per week; 95% CI, –10.60 to –1.29). The reduction in alcohol consumption was greater among women than among men (–8.59 g vs –3.30 g of ethanol per adult per week). In sensitivity analyses comparing Scotland with Northern England, the reductions in alcohol consumption were similar overall (–5.88 g of ethanol per adult per week) and for women and men (–7.49 g vs –4.26 g of ethanol per adult per week). In other stratified analyses, the reductions in alcohol consumption also were observed among men aged ≥ 65 years and among women aged ≥ 65 years ($P < 0.05$) but not in any strata of younger men or younger women. [The strengths of this study are the large sample, the stratified analyses by sex and age, the inclusion of geographical controls, the sensitivity analysis based on a subgroup of the geographical controls, and the inclusion of control variables for consumption level. The limitations of the study are the short period after implementation of the MUP, the risk of bias due to online quota sampling, that the response rates were not reported, and that there were no control variables for income in the models.]

Stevely et al. (2023) also used the Kantar Alcovision repeated cross-sectional survey and retrospective 1-week diary data to assess changes in the prevalence of harmful, hazardous, and moderate alcohol consumption from January 2009–April 2018 to May 2018–February 2020 in Scotland. In a model that controlled for alcohol consumption patterns in Northern England, there was a reduction in the prevalence of hazardous alcohol consumption (–3.5 percentage points; 95% CI, –5.4 to –1.7) but not harmful alcohol consumption (0.6 percentage points; 95% CI, –1.1 to 2.3) or moderate alcohol consumption (1.4 percentage points; 95% CI, –1.1 to 3.8) in Scotland. [The strengths of this study are the large sample, the stratified analysis by consumption level, the

inclusion of geographical controls, and the inclusion of control variables for beverage preferences, patterns of alcohol consumption, and changes in welfare policy. The limitations of the study are the risk of bias due to online quota sampling and that the response rates were not reported.]

Holmes et al. (2022) used data from three cross-sectional waves of survey data collection to assess the difference in the change in alcohol consumption (number of units of alcohol consumed per adult during the week before entering treatment) between Scotland and Northern England from November 2017–April 2018 (pre-MUP period) to August 2018–February 2019 (early-MUP period) and November 2019–March 2020 (late-MUP period). There were no differences in the changes in alcohol consumption from the pre-MUP period to the early-MUP period ($P = 0.42$) or from the pre-MUP period to the late-MUP period ($P = 0.95$). [The strengths of this study are the inclusion of two (early and late) MUP periods, the inclusion of geographical controls, and the unique focus on individuals who were dependent on alcohol and accessing specialist services. The limitations of the study are the small sample; that there were not control variables for sex, age, or income in the model; and that the effect estimates from the regression model were not reported.]

2.2.3 Bans on discounting

The effects of bans on alcohol discounting on alcoholic beverage consumption were assessed in one study with population-level data (Robinson et al., 2014) and two studies that used the same household-level alcohol purchases data set but different subsamples of the study population (Nakamura et al., 2014; Bokhari et al., 2023) (Table 2.6). All three studies assessed the same Scotland, United Kingdom, nationwide ban on off-premises multibuy discounts for alcoholic beverages, in which each product becomes cheaper as a function of how much of the product is purchased (e.g. buy one, get the second 50% off; buy three, get the fourth free), that was implemented in October 2011, as part of the Alcohol etc. (Scotland) Act 2010 (Scottish Parliament, 2010). There were no restrictions on temporary price discounts on individual items (e.g. 20% off a bottle of wine), and there were no other major concurrent alcohol policy interventions in England or Wales during the same time period.

Robinson et al. (2014) used Nielsen weekly retail alcohol sales data to assess the percentage change in off-premises alcohol sales (litres of ethanol per adult per week) in Scotland from 10 January 2009–September 2011 to October 2011–29 September 2012. In a model that controlled for the off-premises alcohol sales in England and Wales combined, there was a reduction of 1.7% (95% CI, −3.1% to −0.3%) in the off-premises alcohol sales per adult in Scotland. [The strengths of this study are the inclusion of geographical controls and the inclusion of control variables for changes in household disposable income, alcohol prices, and on-premises alcohol sales. The limitations of the study are the short period after implementation of the ban and that the alcohol sales outcome excludes data from two discount retail chains, which sold 5% of the total off-premises alcohol market in Great Britain in 2012, and is based on off-premises alcohol sales data only.]

Nakamura et al. (2014) used the Kantar FMCG purchase panel data to assess the difference in the percentage change in off-premises alcohol purchases (units of ethanol per adult per household per quarter) between Scotland and England and Wales combined from a pre-ban period (January–September 2011) to a ban period (October 2011–June 2012). There was no difference between Scotland and England and Wales combined in the change in off-premises alcohol purchases overall (−0.62%; $P > 0.05$) or when stratified by socioeconomic groups or pre-ban household purchasing volume. [The strengths of this study are the large sample, the inclusion of geographical controls, and the stratified analyses

Table 2.6 Effects of bans on alcohol discounting on alcoholic beverage consumption – studies with population-level data and household-level data

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison group (if applicable)	Outcome Effect estimates	Covariates	Comments
Population-level data					
Robinson et al. (2014) Scotland, England, Wales (United Kingdom) Adults aged ≥ 16 years 10 January 2009–September 2011 (pre-ban period) to October 2011–29 September 2012 (ban period)	Interrupted time series Nielson off-premises weekly retail alcohol sales data Seasonal ARIMA	Scotland ban on multibuy discounts of off-premises alcohol in October 2011 England and Wales combined (control)	Percentage change (95% CI) in the off-premises alcohol sales per adult per week (litres of ethanol) from the pre-ban period to the ban period −1.7% (−3.1% to −0.3%)	Change in household disposable income per adult, mean all-alcohol sales prices, on-premises alcohol sales, and off-premises alcohol sales in England and Wales combined	Short period after implementation of the ban The alcohol sales data exclude 2 discount retail chains, which sold 5% of the total off-premises alcohol market in Great Britain in 2012
Household-level data					
Nakamura et al. (2014) Scotland, England, Wales (United Kingdom) Adult households (18 305 total) January–September 2011 (pre-ban period) to October 2011–June 2012 (ban period)	Longitudinal panel Kantar FMCG purchase panel data of off-premises sales (barcode scanning of purchases at home) DiD least-squares regression	Scotland ban on multibuy discounts of off-premises alcohol in October 2011 England and Wales combined (control)	Difference (95% CI) in the change in log off-premises alcohol purchases per adult per household per quarter (units of ethanol) between Scotland and England and Wales combined from the pre-ban period to the ban period β = −0.006 (−0.059 to 0.046)	Household composition, time	Quota sampling; short period after implementation of the ban To calculate the units, the volume (mL) × ethanol by volume for each of 4 beverage types (beer and cider, wine, spirits, and flavoured alcoholic beverages) were summed Stratified analyses showed no differential effects by socioeconomic group or by pre-ban household purchasing volume

Table 2.6 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison group (if applicable)	Outcome Effect estimates	Covariates	Comments
Bokhari et al. (2023) Scotland, England, Wales (United Kingdom) Adult households (8376 total) 2nd week of January 2011–31 July 2012; 21 March–2 October 2011 (pre-ban period) to 3 October 2011–31 July 2012 (ban period)	Longitudinal panel Kantar FMCG purchase panel data of off-premises sales (barcode scanning of purchases at home) DiD regression	Scotland ban on multibuy discounts of off-premises alcohol in October 2011 England and Wales combined (control)	Difference in the change in log off-premises alcohol purchases per adult per household per week (units of ethanol) between Scotland and England and Wales combined from the pre-ban period to the ban period $\beta = 0.086$ ($P < 0.01$)	Household characteristics and type, pre-ban purchase volume, ethanol price changes, discount types, product attributes	Quota sampling; short period after implementation of the ban 1 unit = 8 g of ethanol Included only households that were continuously enrolled throughout the study period, were farther than 35 km from the Scotland–England border, and purchased the equivalent of ≥ 2 pints [≥ 1.136 L] of beer per adult per month

ARIMA, autoregressive integrated moving average; CI, confidence interval; DiD, difference-in-difference; FMCG, fast-moving consumer goods.

by socioeconomic group and pre-ban purchasing volume. The limitations of the study are the risk of bias due to quota sampling, the short period after implementation of the ban, that the response and attrition rates were not reported, and that the alcohol purchases outcome is based on off-premises alcohol purchases data only.]

Bokhari et al. (2023) also used the Kantar FMCG purchase panel data to assess the difference in the change in off-premises alcohol purchases (units of ethanol per adult per household per week) between Scotland and England and Wales combined during similar pre-ban and ban periods. However, the study population was restricted to households that were continuously enrolled throughout the pre-ban and ban periods, were farther than 35 km from the Scotland–England border, and purchased the equivalent of at least 2 pints [at least 1.136 L] of beer per adult per month. There was a greater increase ([8.98%]; $P < 0.01$) in off-premises alcohol purchases in Scotland compared with England and Wales. [The strengths of this study are the large sample, the inclusion of geographical controls, and the inclusion of control variables for pre-ban purchase volume, concurrent ethanol price changes, and product characteristics. The limitations of the study are the risk of bias due to quota sampling, the risk of bias due to exclusion of households that were not continuously enrolled for the whole period or purchased the equivalent of less than 2 pints (1.136 L) of beer per adult per month, the short period after implementation of the ban, and that the alcohol purchases outcome is based on off-premises alcohol purchases data only.]

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3. POLICIES TO LIMIT PHYSICAL AVAILABILITY

3.1 General concepts and considerations

3.1.1 Key concepts

A substantial literature exists describing and assessing the effects of changes in the availability of alcoholic beverages in different jurisdictions, mostly in middle-income and high-income countries. In general, the research indicates that more readily available alcoholic beverages increase alcohol-related harms (Babor et al., 2023). To reduce alcohol-related harms, the World Health Organization (WHO) Global Alcohol Action Plan 2022–2030 encourages Member States to implement "high-impact and effective strategies and interventions, supported by legislative measures" that include alcohol policy interventions to address "the availability of alcohol, by enacting and enforcing restrictions on spatial and temporal availability of alcoholic beverages" (WHO, 2024a).

National and subnational governments have the ability to control all or parts of the alcoholic beverage distribution chain (Babor et al., 2023). These parts, which are typically separate and have different stakeholders (e.g. companies or individuals), include alcohol production or importation, packaging (e.g. bottles or cans), wholesaling to retailers, and the distribution and retail sale of alcoholic beverages to individual customers. Alternatively, the parts of the distribution chain may be compressed, such that the production, wholesale storage, and retail sale of alcoholic beverages all occur at the same place and a producer sells directly to customers, either legally (e.g. cellar door or brewery sales) or illegally (e.g. sales of home-brewed alcohol).

An important aspect of the distribution chain that is specific to alcoholic beverages is the distinction between the retail sale of a product to be consumed elsewhere (off-premises) (e.g. stores selling primarily alcoholic beverages, general grocery stores, and convenience stores) and the sale and service of alcoholic beverages, usually by the drink, to individuals who consume alcohol on the premises of the seller (on-premises) (e.g. bars, restaurants, and nightclubs).

Government agencies have four main options for establishing systems to regulate the production, the distribution, and the retail sale, both off-premises and on-premises, of alcoholic beverages. The first option is to not exercise controls beyond general controls of consumables (i.e. let market forces alone determine the operations of the distribution chain). The second option is to license nongovernmental parties to do business (i.e. privatize) in each part of the distribution chain. The third option is for the government itself to run at least part of the chain (i.e. monopolize it) (see Section 5.2). The fourth option is for the government to establish and enforce

total bans (i.e. prohibitions) on all parts of the distribution chain, and in some jurisdictions on alcohol consumption, or to establish and enforce partial bans, which would affect only parts of the distribution chain. In many jurisdictions, combinations of options are implemented, for different types of alcoholic beverage products, different types of alcohol outlets, or different points in the distribution systems and retail systems for alcohol. However, most countries or subnational jurisdictions have fully privatized systems (Babor et al., 2023).

3.1.2 Types of alcohol policy interventions to limit physical availability

The 2010 WHO Global Strategy to Reduce the Harmful Use of Alcohol describes several alcohol policy interventions to limit the physical availability of alcoholic beverages (WHO, 2010). Interventions to limit the physical availability of alcoholic beverages include: total bans and partial bans on alcohol sales; regulations on the number, density, and location of on-premises and off-premises alcohol outlets; regulations on the days and hours of alcoholic beverage sales; regulations on the minimum legal purchase and drinking age; regulations on the quantities and types of alcoholic beverages sold; and, most recently, regulations on online alcohol retail sales and delivery services (Fig. 3.1).

(a) Total and partial bans on alcohol sales

Legislation that bans – or prohibits – the production, distribution, sale, and consumption of a commodity is the most extreme measure that a government can impose to regulate that commodity and is rare. Bans on alcohol sales can be permanent, as exemplified in some countries where alcohol consumption is prohibited on religious grounds and in countries influenced by the temperance movement in the early 20th century (e.g. the USA). Such bans can be temporary, as exemplified in countries where bans on alcohol sales were included among a range of interventions to reduce the risk of transmission of COVID-19 (Reuter et al., 2020; Anderson et al., 2022).

The review and evaluation of the evidence on the effects of alcohol bans on alcohol consumption (Section 3.2.4) focuses on studies of total bans, which are defined here as bans that prohibit both off-premises and on-premises alcohol sales. Notably, in studies where the total bans on alcohol sales coincided with bans on the production, transportation, or consumption of alcohol, the studies assessed the joint effect of these bans rather than the specific effect of the total ban on alcohol sales. Studies of the effects of partial bans, which are defined here as bans that prohibit either on-premises or off-premises alcohol sales for all types of alcoholic beverages, or that did not separate effects for periods of total and partial bans on alcohol sales, were summarized.

(b) Regulations on alcohol outlet density and location

Regulations on the density of on-premises and off-premises alcohol outlets limit the physical availability of alcoholic beverages. This can be achieved through licensing or zoning processes. In fully privatized alcohol distribution systems with a licensing system, alcohol licences can be highly profitable and therefore in demand. The value of these licences will depend partly on the amount of competition permitted. The more restrictions on the number of permitted licences for either on-premises or off-premises alcohol sales, the more valuable an individual licence will be. Regulators and policy-makers in different jurisdictions have made both gradual and sudden adjustments to the number of permitted licences for either on-premises or off-premises alcohol outlets. The most extreme forms of such a restriction include bans, either on specific types of alcohol outlets or on all types of on-premises or off-premises alcohol outlets.

Fig. 3.1 Major components of alcohol availability that can be regulated by policy interventions

Created by the Working Group.

Alcohol outlet density can be measured by the number of on-premises or off-premises alcohol outlets in a jurisdiction or by the number of alcohol outlets per capita or per unit area (e.g. square kilometre) in a jurisdiction. Changes in these measures occur naturally as a result of population increases in an area and increasing demand for local alcohol sales. Alcohol policy interventions that affect alcohol outlet density can result in sudden changes in alcohol availability.

Local regulations on where alcohol outlets may be located are often imposed by municipal authorities. For example, such regulations may stipulate minimum distances for the site of an alcohol outlet from schools, places of worship, and workplaces, or at sporting events.

(c) Regulations on days and hours of alcohol sales

In the past 50 years, most of the policy changes that affected alcoholic beverage availability resulted in an increase in the number of days per week or hours per day or time of day when alcohol can be sold (Babor et al., 2023). Most of the changes to permit an extra day of alcohol

sales, usually a Sunday, occurred in either North America or one of the Nordic countries (Sherk et al., 2018). In only a small number of instances have alcohol policy interventions introduced a restriction to the number of days per week or hours per day when alcohol can be sold at either on-premises or off-premises alcohol outlets, or at both types of outlets (Sanchez-Ramirez and Voaklander, 2018).

For both on-premises and off-premises alcohol outlets, changes to the days and hours of alcohol sales in a jurisdiction are important alcohol policy interventions. In most jurisdictions, off-premises alcohol outlets have more restricted hours of alcohol sales than on-premises alcohol outlets do, and nightclubs tend to be open much longer than other alcohol outlets. Bar and nightclub hours of alcohol sales may be restricted as a measure to prevent violence (Wilkinson et al., 2016) or expanded in response to pressure from retail lobby groups (e.g. Rossow and Norström, 2012).

Policies regulating the days or hours of alcohol sales contribute to reducing injuries, alcohol-related hospitalizations or emergency department visits, and violence (Popova et al., 2009; Wilkinson et al., 2016; Sanchez-Ramirez and Voaklander, 2018; Nepal et al., 2020).

(d) Regulations on minimum legal purchase and drinking age

National and subnational jurisdictions can regulate the age at which an individual may purchase alcoholic beverages (purchase age); possess or consume alcohol (drinking age), particularly in public settings; or be present at some places (e.g. pubs and nightclubs) where on-premises alcohol consumption is permitted (WHO, 2024b). In some countries, combinations of these regulations have been implemented. For example, in some European countries, individuals who are 1 year or 2 years younger than the minimum legal drinking age may be allowed to purchase beverages with low alcohol content such as beer and wine but not beverages with high alcohol content such as distilled spirits (Kamalow and Siedler, 2019). Other jurisdictions permit young people to be present in on-premises outlets and to consume alcohol at a younger age than the minimum legal purchase age (UK Government, 2024).

The penalties imposed for violating the minimum legal purchase age or drinking age regulations vary globally. Typically, minimum legal purchase age regulations are enforced by penalizing vendors or adults who purchase alcohol and then supply it to individuals who are younger than the minimum legal purchase age; in some jurisdictions, fines may also be imposed on an individual who is younger than the minimum legal purchase age and who purchases alcohol (Babor et al., 2023). In addition, many countries also define an age at which it is a criminal offence to provide alcohol to a young person or a child (e.g. age < 5 years in the United Kingdom) (UK Government, 2024).

(e) Restrictions on quantities and types of alcoholic beverages sold

Point-of-sale restrictions limit the quantity of alcoholic beverages that a person may purchase. For example, to help prevent aggressive and violent behaviour, alcoholic beverage purchases can be limited at large sporting and entertainment events (Lenk et al., 2010; Lyne and Galloway, 2012). In Australia, local restrictions have sometimes been implemented to ration the number of beers or 4 L casks of wine that one customer can purchase (d'Abbs and Hewlett, 2023).

Some jurisdictions also place restrictions on the sale of higher-risk alcoholic beverages that have been linked with poisonings or violent incidents. For example, in Australia only beer with < 3.8% ethanol by volume can be served at cricket matches (Mundy, 2017). Historically, some countries have imposed complete prohibitions on one type of alcoholic beverage sold but

not on others (e.g. beer was prohibited in Iceland until 1989; Ólafsdóttir, 2012).

(f) Regulations on online alcohol retail sales and home delivery

Online retail sales of alcoholic beverages by the use of websites, apps, and digital platforms, and the integration of online retail sales with delivery services – through which alcoholic beverage products, including individual drinks, may be delivered from on-premises and off-premises outlets to consumers' homes – are blurring the distinction between off-premises and on-premises alcohol sales and increasing access to alcoholic beverages (WHO, 2024b). As part of larger shifts towards online retail sales in general, growth in the online sales and home delivery of alcoholic beverage products has been driven by both established and emerging retailers investing in digital technologies to coordinate the sale and delivery of alcohol (Williams and Schmidt, 2014). During the COVID-19 pandemic, the enactment of emergency provisions expanded the online retail sales and home delivery of alcoholic beverages, often with the intention of making such provisions temporary to support businesses, but in many jurisdictions these temporary provisions were not rescinded or were made permanent (Colbert et al., 2021; Lemp et al., 2024).

Online retail sales and home delivery of alcoholic beverages greatly increase the convenience of access to alcoholic beverages (WHO, 2024b). Furthermore, they potentially make it easier to circumvent existing regulations on alcohol outlet density and days and hours of alcohol sales that focus on physical alcohol outlets. They also make it more difficult to enforce laws on responsible service of alcohol, such as regulations against service to individuals who are younger than the minimum legal purchase age and individuals who are intoxicated. Research suggests that compliance with existing age verification policies (an aspect of responsible service of alcohol) is poor (Colbert et al., 2021). For example, a recent study of enforcement of the minimum legal purchase age for online sales of alcohol in two cities in Australia found that > 20% of deliveries to young adults (ages 18–24 years) were made without an age verification (Coomber et al., 2024).

The effects of the online retail sales and home delivery of alcohol on alcoholic beverage consumption are not fully understood. Associations of online retail sales and home delivery with alcohol consumption were assessed in three cross-sectional studies that were based on convenience samples of participants (Mojica-Perez et al., 2019; Huckle et al., 2021; Colbert et al., 2023). Among 528 adults in Australia who reported using an online alcohol delivery service in the past month, the median number of standard drinks purchased in their most recent order ranged from 20.8 when purchased from specialized fast delivery services to 92.2 when purchased from wine clubs (Mojica-Perez et al., 2019). In a study of 1158 adults in Australia who had used online alcohol delivery services in the past 3 months, 20.1% reported using an alcohol home delivery service to extend a home drinking session, among whom there was a 6.3-fold higher odds of "hazardous or harmful" alcohol consumption (defined as a score of ≥ 8 on the Alcohol Use Disorders Identification Test [AUDIT] questionnaire) compared with those who reported not using a delivery service to extend a home drinking session (Colbert et al., 2023). In a large study, which was conducted in April–May 2020 during COVID-19 pandemic restrictions, 2173 adults in New Zealand responded to a survey on Facebook (Huckle et al., 2021). Compared with respondents who had not purchased any alcohol during the pandemic restrictions, the odds ratio for consuming ≥ 6 drinks (a drink was defined as 15 mL of ethanol) on a typical occasion in the past week was 1.75 (95% confidence interval

[CI], 1.36 to 2.27) among respondents who had purchased alcohol online and 1.47 (95% CI, 1.00 to 2.19) among respondents who had purchased alcohol from supermarkets. Overall, these results suggest that increasing alcohol availability through the online retail sales and home delivery of alcohol has the potential to result in increased alcohol consumption.

The rapid expansion of online retail sales and home delivery of alcoholic beverages in some jurisdictions has led to alcohol policy interventions to restrict these activities. However, it is unclear whether these interventions reduce alcohol consumption or prevent increased alcohol consumption; no studies that assessed this association were identified by the Working Group.

(g) Global variation in types of alcohol availability policies

Globally, there is considerable variability in both the types of regulatory systems and alcohol policy interventions that affect alcohol availability. [The WHO Global Survey on Alcohol and Health in 2019 (the most recent year for which data are available) included data from 154 countries, of which 10 countries indicated that alcohol sales were totally banned (WHO, 2024c).] In some of these 10 countries, the total ban on alcohol sales has been lifted (Roeloffs, 2024). Some subnational jurisdictions (e.g. municipalities, states, or provinces) also implement local bans on alcohol sales (Chakrabarti et al., 2024).

[Table 3.1 shows the percentage of countries in each WHO region, and globally, that in 2019 reported regulating alcohol outlet density and the days and hours of alcohol sales, both by alcohol outlet types (i.e. on-premises and off-premises) and by major type of alcoholic beverage (WHO, 2024c). The prevalence of all types of regulations varies among the different WHO regions, with the highest prevalence in the WHO Eastern Mediterranean Region and the WHO Western Pacific Region. Globally, only between 22% and 27% of countries reported regulations on outlet density, whereas most countries reported regulations on hours of alcohol sales (range, 52% to 61%, depending on outlet type and alcoholic beverage type), and fewer countries reported regulations on days of sales (range, 31% to 44%, depending on outlet type and alcoholic beverage type).]

Most WHO Member States have regulations on minimum legal purchase and/or drinking age (WHO, 2024b). [Recent data from 2019 show that the minimum legal purchase age for off-premises alcohol purchase ranged from 16 years to 21 years for each of the major alcoholic beverage types, and approximately two thirds of countries had a minimum legal purchase age of 18 years (Fig. 3.2); the distribution was similar for minimum legal purchase age for on-premises alcohol purchase (WHO, 2024c).] Since 2012, 14 countries either established or raised the minimum legal purchase age for beer for both on-premises and off-premises alcohol purchase (7 countries to age 18 years, 1 country to age 20 years, and 6 countries to age 21 years); 5 additional countries either established or raised the minimum legal purchase age for beer for either on-premises or off-premises alcohol purchase (WHO, 2024b).

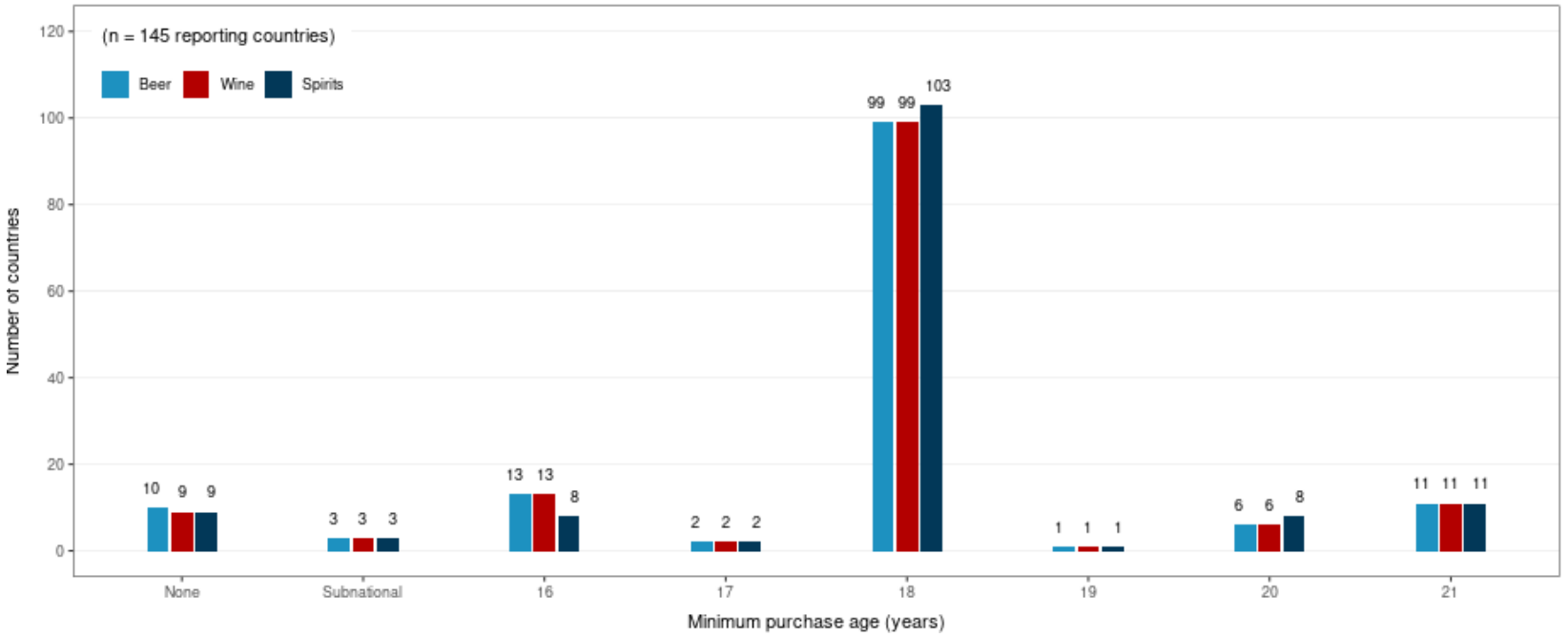
Alcohol policies on remote alcohol sales and delivery are most common in the WHO European Region (48.8% for on-premises orders and 51.2% for off-premises orders for at least one type of alcoholic beverage) and least common in the WHO Region of the Americas (8.0% for on-premises and 8.3% for off-premises) and the WHO South-East Asia Region (0.0% for on-premises and off-premises) (WHO, 2024b). A systematic review of policies governing the online sales and home delivery of alcohol in six English-speaking high-income countries showed that 72 of 77 jurisdictions permitted the online sales and home delivery of alcohol as of November 2020 (Colbert et al., 2021); only seven jurisdictions required age verification at the time

Table 3.1 Percentage of reporting countries with regulations on the density of on-premises and off-premises alcohol outlets and on the days and hours of alcohol sales, by major alcoholic beverage types for each WHO region and globally, in 2019 (*n* = 139)

Regulation type	WHO region	On-premises			Off-premises		
		Beer	Wine	Spirits	Beer	Wine	Spirits
Outlet density	Africa	24%	24%	24%	18%	17%	17%
	Americas	45%	45%	45%	52%	52%	39%
	Eastern Mediterranean	67%	67%	70%	70%	44%	67%
	Europe	9%	9%	9%	9%	11%	11%
	South-East Asia	38%	38%	38%	38%	43%	43%
	Western Pacific	18%	24%	24%	24%	12%	12%
	World	26%	27%	27%	25%	25%	22%
Days of sales	Africa	13%	17%	14%	15%	15%	15%
	Americas	53%	57%	53%	55%	58%	42%
	Eastern Mediterranean	70%	78%	70%	70%	64%	78%
	Europe	13%	13%	13%	19%	21%	21%
	South-East Asia	63%	63%	63%	63%	71%	71%
	Western Pacific	47%	47%	47%	47%	29%	29%
	World	32%	34%	33%	33%	34%	31%
Hours of sales	Africa	50%	48%	48%	38%	34%	34%
	Americas	75%	75%	75%	72%	72%	69%
	Eastern Mediterranean	70%	78%	70%	70%	55%	78%
	Europe	46%	46%	51%	53%	53%	53%
	South-East Asia	88%	88%	88%	88%	86%	86%
	Western Pacific	59%	59%	59%	59%	41%	41%
	World	59%	58%	61%	54%	52%	53%

WHO, World Health Organization.
Created by the Working Group based on WHO (2024c).

Fig. 3.2 Number of countries by minimum legal purchase age for off-premises purchase of beer, wine, and spirits in 2019



Created by the Working Group based on survey data collected for WHO (2024c).

of purchase, but nearly all (n = 71) required age verification at delivery.

3.1.3 Study eligibility and methodological considerations

In Section 3.2, the Working Group reviewed and evaluated the evidence on restrictive and permissive alcohol availability policy interventions that regulate alcohol outlet density, days and hours of alcohol sales, minimum legal purchase and drinking age, and total bans on alcohol sales in relation to alcoholic beverage consumption. The Working Group summarized, but did not evaluate, (i) studies of partial bans on alcohol sales, because they all assessed temporary restrictions on alcohol sales during the COVID-19 pandemic and the results of these studies are not generalizable; (ii) studies of policy interventions that regulate alcohol outlet locations, because the few available studies assessed policies regulating different types of locations; and (iii) studies of multiple alcohol availability policy interventions, because the few available studies assessed very different types of interventions.

The general outcomes, study eligibility criteria, methodological considerations, and other issues are described in Sections 1.3–1.6. For alcohol availability policy interventions, the Working Group applied additional study inclusion or exclusion criteria on the interventions or outcomes studied or the study populations included.

For alcohol availability policy interventions that regulate alcohol outlet density and for total bans on alcohol sales, there was a paucity of studies in which alcohol consumption was the outcome. Therefore, for those interventions, the Working Group also included studies of the effects of those interventions on proxy outcomes for alcohol consumption if an association between alcohol consumption and the proxy outcome was previously established. The proxy outcomes included were alcohol intoxication, alcohol poisoning, liver cirrhosis, and alcohol-related injury, which are among the more than 40 categories of the International Classification of Diseases, 11th revision (ICD-11) that are 100% attributable to alcohol consumption (WHO, 2021, 2024b). Other proxy outcomes included were aggression and violence, because they are strongly associated with alcohol consumption (Rehm et al., 2017; Malta et al., 2024; WHO, 2024b) and the prevalence of interpersonal violence due to others' alcohol consumption is high (Kilian et al., 2024a). For example, in some studies, 70–80% of homicides occurred when the perpetrator had consumed alcohol (Norström, 1998; Room and Rossow, 2001). Finally, some studies assessed the effects of policy interventions on alcohol-related hospital admissions or unnatural deaths, which may have included some of the proxy outcomes described above and other health conditions or suicide. For total bans on alcohol sales, given the paucity of studies, the Working Group also included studies in which temporal patterns were inferred from a questionnaire administered at a single time point with current and recalled alcohol consumption.

There were a large number of studies that assessed the effects of restrictive or permissive alcohol policy interventions on minimum legal purchase or drinking age on alcohol consumption. Therefore, for this alcohol availability policy intervention only, the Working Group excluded panel analyses in which the study period did not clearly include a change in the policy intervention. Two meta-analyses on the effects of restrictions on days of alcohol sales on alcohol consumption that were identified by the Working Group were excluded because some of the studies included in the meta-analysis did not meet the inclusion criteria defined for this volume.

The Working Group also excluded studies that focused only on special populations (e.g. pregnant women) and studies on the effects of

interventions that target patrons who are intoxicated, specific types of alcoholic beverages that constitute a very small proportion of the alcohol market (e.g. alcopops), the volume of packaged alcohol, and alcohol purchase quantities, because these policies were unlikely to have meaningful effects on alcohol consumption in the general population. The Working Group also excluded studies of COVID-19 pandemic restrictions that did not specifically investigate the effects of total or partial ban periods (Scarmozzino and Visioli, 2020; Mapanga et al., 2023), because the change in alcohol consumption from before to during or after the ban period could not be determined.

Several methodological issues were considered when assessing the evidence on the effects of alcohol availability policy interventions on alcohol consumption or proxy outcomes for alcohol consumption. First, restrictions on alcohol availability may be less effective in the presence of cross-border shopping due to more permissive regulations in neighbouring jurisdictions, and the Working Group noted studies in which potential bias due to cross-border shopping was investigated. Second, although most of the studies of regulations on alcohol outlet density were based on permissive policy interventions, the few studies that assessed restrictive policy interventions were informative. Third, during the COVID-19 pandemic, stockpiling of alcoholic beverages may have occurred before the implementation of temporary bans on alcohol sales. In studies with population-level data, the effect estimates for a temporary ban on alcohol sales in relation to alcohol consumption may be biased due to stockpiling. The direction of this bias would vary depending on whether the ban period assessed included the sales of stockpiled alcohol (Robinson et al., 2013). In studies with individual-level or household-level data, the association between the ban and self-reported alcohol consumption will depend on stockpiling.

3.2 Effects of alcohol policy interventions to limit physical availability on alcoholic beverage consumption

3.2.1 Regulations on alcohol outlet density or location

The effects of alcohol policy interventions that restricted alcohol outlet density on proxy outcomes for alcohol consumption were assessed in two studies with population-level data (Zhang et al., 2015; de Vocht et al., 2016). The effects of permissive alcohol outlet density policy interventions on alcoholic beverage consumption or on proxy outcomes were assessed in three studies with population-level data (Wagenaar and Langley, 1995; Stockwell et al., 2009; Tabb et al., 2016) and one study with individual-level data (Gohari et al., 2021) (Table 3.2). All six studies were based on data from high-income countries (Canada, New Zealand, the United Kingdom, and the USA).

(a) Alcohol outlet density and alcohol consumption

Wagenaar and Langley (1995) assessed the effect of a policy change that permitted grocery stores in New Zealand to sell table wine starting in April 1990 on the percentage change in total alcohol sales (litres of ethanol per quarter) and beer, wine, and distilled spirits sales from the pre-policy change period (Q3 1983–Q1 1990) to the policy period (Q2 1990–Q2 1993). In a model that controlled for unemployment rate, there was no change in total alcohol sales (–1.6%; 95% CI, –7.3% to 4.6%). However, there was an increase in wine sales (15.6%; 95% CI, 6.7% to 25.2%) but not in beer sales (–4.1%; 95% CI, –8.9% to 1.0%) or spirits sales (–7.9%; 95% CI, –19.8% to 5.7%). [The strength of this study is the use of autoregressive integrated moving average (ARIMA) models to account for underlying trends, autocorrelation, and seasonality. The limitations of

Table 3.2 Effects of alcohol outlet density regulations on alcoholic beverage consumption or proxy outcomes

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Alcohol consumption outcomes					
Wagenaar and Langley (1995) New Zealand Total population Q3 1983–Q2 1993	Interrupted time series Data on quarterly sales of beer, wine, and distilled spirits from the New Zealand Department of Statistics ARIMA	Grocery stores permitted to sell table wine from April 1990 as established by the Sale of Liquor Act of 1989 No control or comparison area	Percentage change in total alcohol sales per quarter (litres of ethanol) from Q3 1983–Q1 1990 to Q2 1990–Q2 1993 –1.6% (95% CI, –7.3% to 4.6%)	Unemployment rate	From Q3 1983–Q1 1990 to Q2 1990–Q2 1993, there was an increase in wine sales (15.6%; 95% CI, 6.7% to 25.2%) but not in beer sales (–4.1%; 95% CI, –8.9% to 1.0%) or spirits sales (–7.9%; 95% CI, –19.8% to 5.7%) Fluctuations in alcohol prices and drink-driving policies in the early 1990s may have confounded the observed association
Stockwell et al. (2009, 2011) British Columbia, Canada; rest of Canada Adult population aged ≥ 15 years Fiscal years, April 1998–March 1999 to April 2007–March 2008	Interrupted time series and panel Alcohol per capita consumption in British Columbia and the rest of Canada for fiscal years 1998/1999 to 2007/2008; alcohol sales data from the Ministry of Public Safety and the British Columbia Liquor Distribution Branch for fiscal years 2003/2004 to 2007/2008 Descriptive analysis, linear regression, and multilevel regression	The number of private off-premises alcohol outlets in British Columbia was permitted to increase gradually from 1988 until 2000, when a moratorium on new off-premises alcohol outlet licences was put in place, which was lifted in 2002 Comparison region: the rest of Canada for alcohol consumption, and the other Canadian provinces for outlet density	Fiscal year (from April to March) / Alcohol consumption per capita per fiscal year (litres of ethanol) / Number of off-premises alcohol outlets per 10 000 adults **British Columbia:** 1998/1999: 7.74 / 2.37 1999/2000: 7.68 / 2.37 2000/2001: 7.55 / 2.37 2001/2002: 7.58 / 2.37 2002/2003: 7.65 / 2.67 2003/2004: 7.76 / 3.01 2004/2005: 8.32 / 3.30 2005/2006: 8.46 / 3.47 2006/2007: 8.68 / 3.58 2007/2008: 8.87 / 3.62		[The values for alcohol consumption and number of alcohol outlets were provided by the authors] The increase in the total number of off-premises alcohol outlets per 10 000 adults from fiscal year 2001/2002 to fiscal year 2007/2008 was due mainly to the increase in the number of private off-premises alcohol outlets The number of on-premises alcohol outlets per 10 000 adults decreased slightly from fiscal year 2001/2002 to fiscal year 2007/2008 in British Columbia (from 22.46 to 21.61) and in the other Canadian provinces (from 24.68 to 24.09)

Table 3.2 (continued)

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates			Covariates	Comments
Stockwell et al. (2009, 2011) (cont.)			**Comparison region:**				For fiscal years 2003/2004 through 2007/2008, the difference in the natural logarithm of the monthly alcohol-related mortality rate (per 10 000 adults) per 1-unit increase in the number of alcohol outlets per 10 000 adults was 0.10 (95% CI, −0.03 to 0.23) for off-premises outlets and 0.08 (95% CI, 0.02 to 0.15) for on-premises outlets (Stockwell et al., 2011)
			1998/1999	7.19	6.79		
			1999/2000	7.33	7.40		
			2000/2001	7.49	7.25		
			2001/2002	7.48	7.16		
			2002/2003	7.59	7.05		
			2003/2004	7.58	6.97		
			2004/2005	7.89	6.91		
			2005/2006	8.09	6.78		
			2006/2007	8.20	6.67		
			2007/2008	8.24	6.54		
			Difference in alcohol consumption per capita per year (litres of ethanol) per additional off-premises alcohol outlet per 10 000 adults between British Columbia and the rest of Canada from 1998/1999 to 2007/2008 [β = 0.57 (95% CI, 0.09 to 1.04)]			Annual alcohol per capita consumption in the rest of Canada and year (linear and quadratic terms)	
			Change in the natural logarithm of alcohol sales per capita per month (litres of ethanol) per additional alcohol outlet per 10 000 adults between British Columbia and the rest of Canada from 2003/2004 to 2007/2008 (Stockwell et al., 2009) On-premises outlets: β = 0.17 (95% CI, 0.16 to 0.19) Government or private off-premises outlets: β = 0.06 (95% CI, 0.01 to 0.11)			Density of types of alcohol outlets, percentage of private off-premises alcohol outlets, percentage of adult population aged 20–29 years, percentage of males, proportion of low-income families, population density, and temporal and spatial effects	

Table 3.2 (continued)

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Gohari et al. (2021) Ontario, Canada 2179 students in grade 9 from 56 secondary schools School years 2013–2014, 2014–2015, 2015–2016, and 2016–2017	Prospective cohort Self-reported longitudinal data on the frequency of alcohol consumption and binge drinking among students in grade 9 in 56 secondary schools across Ontario; 4 survey waves Pre–post analysis	Alcohol sales were permitted in 450 grocery stores in Ontario in December 2015 Intervention group: 652 students from 13 schools with at least 1 grocery store selling alcohol within 10 km of the school after the policy intervention Comparison: 1527 students from 43 schools with no grocery store selling alcohol within 10 km of the school	Ratio of the 1-year probability of transitioning from alcohol abstention after the introduction of the policy to the 1-year probability of transitioning before the introduction of the policy **Transition to periodic consumption:** Intervention group: 0.93 Comparison group: 0.95 **Transition to low-risk consumption:** Intervention group: 0.77 Comparison group: 0.98 **Transition to high-risk regular consumption:** Intervention group: 1.71 Comparison group: 0.50		Categories of alcohol consumption were defined as: abstention, never consumed alcohol or did not consume alcohol in past year; periodic consumption, monthly consumption and no binge drinking (≥ 5 alcoholic beverages on 1 occasion); low-risk consumption, some steady monthly consumption but limited binge drinking; and high-risk regular consumption, 1–3 times per week and binge drinking 2–4 times per month Results for a second comparison group in Alberta, Canada, are not shown here because of the small sample size in that comparison group

Table 3.2 (continued)

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Proxy outcomes for alcohol consumption					
Zhang et al. (2015) USA Total population 1997–2007	Interrupted time series Data for violent crimes (i.e. homicide, rape, robbery, and aggravated assault) were obtained from the Atlanta Police Department; the annual violent crime exposure index for each of 354 census blocks was the sum of the inverse distances between a census block centroid and the nearest 7 violent crime events during a specific year Descriptive analysis	Establishing and enforcing restrictions on retail sales of alcohol in the Buckhead neighbourhood of Atlanta Comparison: 2 sociodemographically similar neighbourhoods (Midtown and Downtown)	Percentage change in the annual violent crime exposure index from 1997–2002 to 2003–2007 Buckhead: −17.5% Midtown: −8.4% Downtown: −9.8%		[The authors defined the pre-intervention period (1997–2002) and the post-intervention period (2003–2007)] The alcohol outlet exposure index decreased in Buckhead (–3.2%) but increased in Midtown (12.1%) and Downtown (12.4%) in 2003–2007 compared with 1997–2002 The policy intervention also included restrictions on hours of alcohol sales and enforcement of laws prohibiting alcohol sales to minors, which may have also contributed to the change in the violent crime exposure index

Table 3.2 (continued)

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
de Vocht et al. (2016) England, United Kingdom Total population 2007–2015	Panel Alcohol licensing data for England from the Home Office Alcohol and Late Night Refreshment Licensing data for the years 2007–2008 and 2011–2012 and quarterly age-standardized alcohol-related hospital episode (excluding accident and emergency department visits) rates across 319 of the 326 local authority areas in England from April 2009 to June 2015 Multilevel regression	A 2005 revision extended the 2003 Licensing Act to give local authorities power to address the cumulative impact of alcohol sales, and the 2011 Police Reform and Social Responsibility Act strengthened the ability of local authorities to control new alcohol outlets The level of intensity of policy implementation and enforcement was based on whether the local authority used cumulative impact areas and whether any licences for new premises were declined during each year because applicants for a new licence had not demonstrated how they would avoid threatening the licensing objectives; the score was used to define no activity and tertiles of intensity (low, medium, high) Comparison: areas with no activity were the reference category	Difference in the change in the natural logarithm of the alcohol-related hospital admissions per 100 000 people per 10 years (age-standardized number of admissions) compared with areas with no activity Low intensity of implementation: $\beta = -0.006$ (SE = 0.055; *P*, NR) Medium intensity of implementation: $\beta = -0.065$ (SE = 0.058; $P > 0.05$) High intensity of implementation: $\beta = -0.229$ (SE = 0.067; $P < 0.05$) $P = 0.006$ for variation by level of intensity	Individual local authority area time trends, calendar year quarter, baseline (2007–2008) population size, deprivation, and alcohol-related crime rates to control for non-random implementation of policies; interactions with time for level of implementation and all covariates	Alcohol-related hospital admissions included the proxy outcomes defined in Section 3.1.3 (liver cirrhosis, injuries, violence, and alcohol poisoning or intoxication) and other health conditions (e.g. malignant neoplasms of the oesophagus and hypertensive heart disease) The estimated annual decrease in alcohol-related hospital admissions for local areas with no activity was 0.6%; the change in alcohol-related hospital admission rates in the areas with high intensity of implementation equated to an additional decrease of 5% in 2015 compared with the expected rate in the absence of activity The sensitivity analysis in which the 2007–2008 status was studied in relation to alcohol-related hospital admissions in 2009–2015 showed comparable results, and the sensitivity analyses using validated 2009–2013 data only also showed similar results but with smaller effect sizes ($\beta \geq -0.138$)

Table 3.2 (continued)

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
de Vocht et al. (2016) (cont.)					The β coefficient is for a 10-year period, because the time axis was rescaled for the regression model
Tabb et al. (2016) Seattle, Washington, USA Total population 2010–2013	Panel Annual violence data from the Seattle Police Department for 567 census blocks in Seattle and alcohol outlet data from the Washington State Liquor and Cannabis Board Hierarchical spatio-temporal disease mapping modelling	In 2012, Initiative 1183 was passed in Washington State, privatizing the wholesale distribution and retail sale of alcoholic beverages at off-premises alcohol outlets (i.e. retailers with an area of ≥ 10 000 square feet, generally grocery stores and warehouse clubs) and on-premises alcohol outlets (i.e. restaurants, bars, taverns, and nightclubs) No control or comparison area	RR (95% CI) of assaults (number per year per census block) for each additional off-premises alcohol outlet Aggravated assaults: 1.077 (1.033 to 1.122) Non-aggravated assaults: 1.061 (1.022 to 1.101) RR (95% CI) of assaults (number per year per census block) for each additional on-premises alcohol outlet Aggravated assaults: 1.046 (1.033 to 1.059) Non-aggravated assaults: 1.050 (1.038 to 1.063)	Census block characteristics (percentage of the population aged 15–29 years; percentage of households with an annual income < US$ 15 000; percentage of vacant units, a density measure for public transportation stops; location in the downtown Seattle area; percentage of Black, female-headed households; a racial and ethnic diversity index; proportion of commercial land use and risky retailers, which may increase violence; and number of alcohol outlets of the other type) and interactions between the spatial and temporal effects	The policy intervention was implemented in a government monopoly setting, and as part of the privatization law, taxes and fees equalling 10% of the wholesale price and 17% of the retail price were introduced; see the study by Barnett et al. (2020) in Section 5.2.2 During the study period, the number of alcohol outlets in Seattle increased from < 560 to 635 for off-premises outlets and from < 1600 to 1760 for on-premises outlets

ARIMA, autoregressive integrated moving average; CI, confidence interval; NR, not reported; Q, quarter; RR, relative risk; SE, standard error.

the study are the lack of a geographical control, that the fluctuations in alcohol prices during the study period and numerous efforts to reduce alcohol-impaired driving in the early 1990s were not accounted for in the statistical analysis, and that alcohol sales were based on recorded alcohol sales data only.]

Stockwell et al. (2011) assessed the effect of the partial privatization of the government alcohol retail monopoly in British Columbia, Canada, in 2002 on the change in alcohol consumption (litres of ethanol per capita per year) from fiscal year April 1998/March 1999 to fiscal year April 2007/March 2008 compared with the change in alcohol consumption in the rest of Canada. The 2000 moratorium on new off-premises alcohol outlet licences was lifted in 2002, which resulted in a rapid increase in off-premises alcohol outlet density, from 2.37 per 10 000 adults in fiscal year 2001/2002 to 3.62 per 10 000 adults in fiscal year 2007/2008. In contrast, in the other Canadian provinces specifically there was a decreasing trend in off-premises alcohol outlet density, from 7.16 per 10 000 adults in fiscal year 2001/2002 to 6.54 per 10 000 adults in fiscal year 2007/2008. In British Columbia, there was a decreasing trend in alcohol consumption at the end of the 1990s until the onset of the partial privatization, after which alcohol consumption increased, from 7.58 L of ethanol in fiscal year 2001/2002 to 8.87 L of ethanol in fiscal year 2007/2008. This change in alcohol consumption in British Columbia was greater than the change in the rest of Canada, where alcohol consumption gradually increased, from 7.48 L of ethanol in fiscal year 2001/2002 to 8.24 L of ethanol in fiscal year 2007/2008. A linear regression analysis showed that in British Columbia the average difference in alcohol consumption per addi-t-ional off-premises alcohol outlet per 10 000 adults was [0.57 L] of ethanol (95% CI, [0.09 L to 1.04 L]). In an earlier multilevel analysis of data from 89 local health regions in British Columbia during 2003/2004 to 2007/2008 (Stockwell et al., 2009), each additional alcohol outlet per 10 000 adults was associated with higher alcohol sales (litres of ethanol per capita per month) for both on-premises outlets [19.1%; 95% CI, 17.5% to 20.7%] and off-premises outlets [6.16%; 95% CI, 1.33% to 11.2%]. [The strengths of this study are the inclusion of data for comparison regions in Canada as geographical controls, the adjustment for year, and, in the models for the period from 2003/2004 to 2007/2008, the adjustment for income and other potential confounders. The limitation of the study is that the multilevel analysis only included data for the period after the policy intervention.]

Gohari et al. (2021) assessed the effects of the 2015 policy permitting the sale of alcohol in 450 grocery stores in Ontario, Canada, on alcohol consumption among students who were in grade 9 (age not reported) at the time of the first survey. Self-reported alcohol consumption data were collected over two school years before the introduction of the policy (2013–2014 and 2014–2015) and two school years after the introduction of the policy (2015–2016 and 2016–2017). The ratios of the 1-year probabilities of transitioning from alcohol abstention to high-risk regular consumption, periodic consumption, or low-risk consumption after the introduction of the policy to before the introduction of the policy were assessed for students from schools with at least one grocery store selling alcohol within 10 km of the school (intervention group) and for students from schools with no grocery store selling alcohol within 10 km of the school (comparison group). Compared with before the introduction of the policy, after the introduction of the policy the 1-year probability of transitioning from alcohol abstention to high-risk regular consumption was 71% higher in the intervention group and 50% lower in the comparison group, the probability of transitioning from alcohol abstention to periodic consumption was 7% lower in the intervention group and 5% lower in the comparison group, and the probability of

transitioning from alcohol abstention to low-risk consumption was 23% lower in the intervention group and 2% lower in the comparison group. [The strengths of this study are the inclusion of comparison schools and the comparison of 1-year probabilities of transitions before with 1-year probabilities of transitions after the policy change. The limitations of the study are that no statistical test was performed and that there was no information on community-based or school-based interventions, which may have reduced the effect of the policy intervention on alcohol consumption.]

(b) Alcohol outlet density and proxy outcomes for alcohol consumption

Zhang et al. (2015) assessed the effects of the establishment and enforcement of alcohol sales restrictions on the change in on-premises alcohol outlet density (measured as an annual alcohol outlet exposure index) and the change in violent crime (measured as an annual violent crime exposure index) in a high-density cluster of alcohol outlets (a cluster zone) in Buckhead, a neighbourhood in Atlanta, Georgia, USA, compared with two other cluster zones in Atlanta (Midtown and Downtown). From 1997–2002 to 2003–2007, on-premises alcohol outlet density decreased by 3.2% in Buckhead and increased by 12.1% in Midtown and by 12.4% in Downtown. Over the same period, violent crime decreased by 17.5% in Buckhead, by 8.4% in Midtown, and by 9.8% in Downtown. [The strengths of this study are the inclusion of two geographical control areas in Atlanta and the use of geospatially defined measures of an exposure index for both alcohol outlet density and violent crime. The limitations of the study are that the results were descriptive; that factors other than alcohol outlet density, such as reduction in opening hours and changes in law enforcement, also may have contributed to the decrease in violent crime; that violent crime was probably underreported; that there was no information on off-premises alcohol outlet density; and that the analysis included only proxy outcomes for alcohol consumption.]

In England, United Kingdom, legislative changes in 2003, 2005, and 2011 strengthened the ability of local area authorities to control new alcohol licensing policies. Local authorities can designate cumulative impact zones (CIZs) to control new alcohol outlets in areas where the cumulative stress caused by existing overprovision of alcohol outlets threatens the licensing objectives, and applicants for a new alcohol licence must demonstrate how they will avoid threatening the licensing objectives. de Vocht et al. (2016) assessed the effect of a licensing intensity score (categorized as 1 = no activity, 2 = low intensity, 3 = medium intensity, and 4 = high intensity based on the annual local area implementation of CIZs and the intensity of enforcement of licensing scrutiny aggregated across 2007–2015) on the annual percentage change in alcohol-related hospital admission rates during 2009–2015. There was an annual decrease of 0.6% in alcohol-related hospital admissions in areas with no activity and additional annual decreases for areas with a low ([–0.06%]), medium (–0.6%), and high ([–2.3%]) intensity of enforcement of licensing scrutiny (P = 0.006 for variation by level of intensity). [The strengths of this study are the inclusion of licensing and alcohol-related hospital admissions data for 319 areas, the inclusion of areas with no activity as geographical controls, and the use of hierarchical growth models that accounted for temporal autocorrelation and other potential confounders. The limitations of the study are that the areas with higher intensity of enforcement of licensing scrutiny may have been more proactive in adopting other alcohol policies for which no information was available, and that the analysis included only proxy outcomes for alcohol consumption.]

In Washington State, USA, Initiative 1183 was passed in 2012, which privatized the wholesale distribution and retail sale of alcoholic beverages at on-premises alcohol outlets

and off-premises alcohol outlets with an area of ≥ 10 000 square feet. Tabb et al. (2016) assessed the effect of the increase in alcohol outlet density in Seattle, Washington, on the relative risk of aggravated and non-aggravated assaults (number per year per census block) using data from 2010–2013. Each additional off-premises alcohol outlet in a given census block was associated with an increase in aggravated assaults (7.7%; 95% CI, 3.3% to 12.2%) and non-aggravated assaults (6.1%; 95% CI, 2.2% to 10.1%). Similarly, each additional on-premises outlet also was associated with an increase in aggravated assaults (4.6%; 95% CI, 3.3% to 5.9%) and non-aggravated assaults (5.0%; 95% CI, 3.8% to 6.3%). [The strengths of this study are the inclusion of alcohol outlet density and assault data for 567 census blocks and the use of hierarchical spatio-temporal disease mapping models that accounted for spatial correlation and other potential confounders. The limitations of the study are the lack of a geographical control, that the study estimated the average effect of alcohol outlet density on assaults per year that occurred for all 4 years during the study period and not the change in the number of assaults from the 2 years before to the 2 years after the policy intervention, that factors correlated with the increase in alcohol outlet density may have also contributed to the increase in the number of assaults, and that the analysis included only proxy outcomes for alcohol consumption.]

(c) Location of alcohol outlets

The effects of alcohol policy interventions that restricted the location (which varied among the studies) of alcohol outlets on alcohol consumption were assessed in three studies in Europe (the Netherlands, Spain, and the United Kingdom) with population-level data (Knibbe et al., 2014; Matrai et al., 2014; Plant et al., 2014) (Table 3.3).

The effects of restrictions on the location of alcohol outlets on the percentage change in recorded alcohol consumption (litres of ethanol per capita per year) were assessed in three country-specific interrupted time-series analyses conducted within the European Alcohol Measures for Public Health Research Alliance (AMPHORA) project (Allamani et al., 2014) and covering the period 1961–2008 (Knibbe et al., 2014; Matrai et al., 2014; Plant et al., 2014).

Knibbe et al. (2014) estimated that alcohol consumption in the Netherlands decreased by 5% after the 2001 ban on alcohol sales in petrol stations ($P < 0.10$). Matrai et al. (2014) estimated that alcohol consumption in Spain decreased after the 1989 policy prohibiting alcohol sales in public education centres (–12%; 90% CI, –18% to –7%) and the 1990 policy prohibiting alcohol possession and sales at sporting events (–7%; 90% CI, –13% to –1%). Plant et al. (2014) (see also Section 3.2.2) estimated that the 2001 policy banning the sale or consumption of alcohol and other drugs in National Health Service workplaces in the United Kingdom was associated with an increase of 7% (90% CI, 3% to 11%) in alcohol consumption. [The strength of these studies is the inclusion of 45–48 years of annual alcohol consumption data covering multiple years before and after the policy interventions. The limitations of the studies are that the interventions were not fully described; the lack of a geographical control; that other alcohol policy interventions that occurred during the study period, often at about the same time as the intervention, are not accounted for in the statistical analysis; and that alcohol consumption was based on recorded alcohol consumption data only.]

3.2.2 Regulations on days or hours of alcohol sales

(a) Days of alcohol sales

The effects of permissive alcohol policy interventions that regulate the number of days of alcohol sales on alcoholic beverage consumption were assessed in two studies with population-level

Table 3.3 Effects of restrictions on the location of alcohol outlets on alcoholic beverage consumption

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Knibbe et al. (2014) The Netherlands Adult population aged ≥ 15 years 1961–2008	Interrupted time series Recorded alcohol consumption data from WHO GISAH (WHO, 2024c) Linear regression	2001 law banning alcohol sales in petrol stations No control or comparison area	Percentage change in recorded alcohol consumption per capita per year (litres of ethanol) from 1961–2000 to 2001–2008 –5% ($P < 0.10$)	Income, price of beer and spirits, proportion of men aged > 65 years, mother's average age at childbirth, and time trend	The intervention was not fully described *P* is based on the 90% CI shown in a figure; the estimated CI was not provided
Matrai et al. (2014) Spain Adult population aged ≥ 15 years 1962–2006	Interrupted time series Recorded alcohol consumption data from WHO GISAH (WHO, 2024c) Linear regression	Ban on alcohol sales in public education centres in 1989 together with a ban on alcohol possession and sales at sporting events in 1990 No control or comparison area	Percentage change in recorded alcohol consumption per capita per year (litres of ethanol) Public education centre ban, from 1962–1988 to 1989–2006: –12% (90% CI, –18% to –7%) Sporting events ban, from 1962–1989 to 1990–2006: –7% (90% CI, –13% to –1%)	Income, price of beer and spirits, proportion of men aged > 65 years, urbanization, and time trend	The interventions were not fully described The models did not account for the effect of changes in the minimum purchase age law, advertising ban, and BAC limit policy that occurred during the study period
Plant et al. (2014) United Kingdom Adult population aged ≥ 15 years 1961–2006	Interrupted time series Recorded alcohol consumption data from WHO GISAH (WHO, 2024c) Linear regression	Ban on the sale or consumption of alcohol and other drugs in National Health Service workplaces in 2001 No control or comparison area	Percentage change in recorded alcohol consumption per capita per year (litres of ethanol) from 1961–2000 to 2001–2006 7% (90% CI, 3% to 11%)	Income, price of beer, proportion of men aged > 65 years, mother's average age at childbirth, and time trend	The interventions were not fully described

BAC, blood alcohol concentration; CI, confidence interval; GISAH, Global Information System on Alcohol and Health; WHO, World Health Organization.

data (Norström and Skog, 2005; Yörük, 2014) and two studies with individual-level data (Carpenter and Eisenberg, 2009; Subbaraman et al., 2023). The effects of alcohol policy interventions that restrict days of alcohol sales on alcohol consumption were assessed in two other studies with individual-level data (Nelson, 2008; Kilian et al., 2024b) (Table 3.4). All six studies were based on data from high-income countries (Canada, Sweden, and the USA).

(i) Studies with population-level data

Norström and Skog (2005) conducted a two-phase trial to assess the effects of a policy intervention permitting Saturday off-premises alcohol sales on the change in off-premises total alcohol sales (litres of ethanol per capita per month). For phase I, in February 2000 Saturday off-premises alcohol sales were permitted in six Swedish counties (experimental) but not in seven other counties that did not share a border with the experimental counties (control). For phase II, in July 2001 Saturday off-premises alcohol sales were extended to all of Sweden. In a model that controlled for the change in alcohol sales in the control counties, in the six intervention counties there was a 3.7% (95% CI, [2.6% to 4.7%]) increase in alcohol sales from January 1995–January 2000 to February 2000–June 2001. In a second model that further controlled for the change in alcohol sales in the intervention counties during phase I, in all of Sweden there was a 3.6% (95% CI, [2.5% to 4.6%]) increase in alcohol sales from January 1995–January 2000 to July 2001–July 2002. [The strengths of this study are the inclusion of counties without the policy intervention as geographical controls and the use of ARIMA models to account for underlying trends, autocorrelation, and seasonality. The limitations of the study are that the effect of the policy change was assessed only in the first 13 months after the policy intervention was extended to all of Sweden, and that the outcome was for off-premises alcohol sales only.]

Yörük (2014) assessed the effects of Sunday off-premises alcohol sales on the difference in the change in alcohol consumption (gallons of ethanol per capita per year) between 5 states in the USA that implemented a policy intervention to permit Sunday off-premises alcohol sales in 1995–2004 and 12 states that retained laws banning off-premises alcohol sales. The policy intervention was associated with a 2.8% ($P = 0.05$) increase in alcohol consumption. [The strengths of this study are the assessment of policy interventions in five states over 10 years, the inclusion of states without the policy intervention as geographical controls, and the adjustment for income and other control variables in the models. The limitation of the study is that it is unclear whether cross-border shopping may have led to an overestimation of the observed association.]

(ii) Studies with individual-level data

In a study with individual-level data of permissive policy interventions on days of alcohol sales, Carpenter and Eisenberg (2009) assessed the effects of permitting Sunday off-premises alcohol sales in Ontario, Canada, Among participants who consumed alcohol in the week before the survey, after controlling for alcohol consumption in the other provinces, the policy intervention led to an increase of 0.028 ($P > 0.10$) in the number of drinks consumed during the previous week, but there was no effect on the probability of consuming alcohol in the previous week. [The strengths of this study are the inclusion of all provinces without the policy intervention as geographical controls and the adjustment for survey wave and other control variables in the models. The limitation of the study is that only individuals who reported alcohol consumption in the past week were included in the analysis for the amount of alcohol consumed.]

In the other study with individual-level data of permissive policy interventions on days of

Table 3.4 Effects of regulations on days of alcohol sales on alcoholic beverage consumption – studies with population-level and individual-level data

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Studies with population-level data					
Norström and Skog (2005) Sweden Adult population aged ≥ 15 years January 1995–July 2002	Non-randomized community trial Alcohol sales data from Systembolaget, the Swedish government alcohol retail monopoly ARIMA	Permitting Saturday off-premises alcohol sales from February 2000 in 6 counties (phase I) and from July 2001 in the whole country (phase II) Comparison: 7 counties in the middle and southern parts of Sweden where Saturday sales of alcohol were not permitted, separated from the experimental areas by buffer areas	Change in the natural logarithm of off-premises alcohol sales per capita per month (litres of ethanol) in the experimental counties **Phase I: from January 1995–January 2000 to February 2000–June 2001** $\beta = 0.036$ (SE = 0.005; $P < 0.001$) **Phase II: from January 1995–January 2000 to July 2001–July 2002** $\beta = 0.035$ (SE = 0.005; $P < 0.001$)	Phase I: alcohol sales in control areas in January 1995–January 2000; seasonality Phase II: alcohol sales in control areas in January 1995–January 2000 and July 2001–July 2002; seasonality	7 off-premises alcohol outlets located close to the Norwegian border were excluded from the analysis because a substantial fraction of the alcohol sold in these outlets was to Norwegians
Yörük (2014) USA Adult population aged ≥ 14 years 1990–2007	Panel Alcohol per capita consumption data from the National Institute on Alcohol Abuse and Alcoholism Linear regression (DiD)	Permitting Sunday off-premises alcohol sales in 5 states (New Mexico, 1995; Delaware, Massachusetts, and Pennsylvania, 2003; Rhode Island, 2004) Comparison: 12 control states that retained laws banning Sunday off-premises alcohol sales	Difference in the change in the natural logarithm of alcohol consumption per capita per year (gallons of ethanol) between states that permitted Sunday off-premises alcohol sales and states that did not from before to after the policy change $\beta = 0.028$ ($P = 0.05$)	State, year, state–year interaction, and state-level variables: income per capita; beer tax; education level; median age; percentage unemployed, female, Hispanic, and Black; and 0.08 BAC law	

Table 3.4 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Studies with individual-level data					
Nelson (2008) USA ~70 000 participants aged ≥ 12 years each year Survey period: 1999–2003	Repeated cross-sectional survey Alcohol consumption data from the National Survey on Drug Use and Health to obtain yearly estimates for 45 states Linear probability models	Statewide bans on Sunday off-premises alcohol sales	Difference in the yearly 30-day prevalence of alcohol consumption (percentage points) between states with a ban on Sunday off-premises alcohol sales and states with no ban Ages 12–17 years: −1.02 ($P < 0.05$) Ages 18–25 years: −1.84 ($P < 0.05$) Ages ≥ 26 years: −2.16 ($P < 0.05$)	Excise taxes, outlet density, retail monopoly, 0.08 BAC law, income per capita, poverty rate, percentage of elderly people, educational attainment, unemployment rate, racial and ethnic descriptors, and attendance at selected professional sporting events in each state	Banning Sunday off-premises alcohol sales was also associated with a lower yearly prevalence of binge drinking (−0.500 percentage points for ages 12–17 years, −0.699 percentage points for ages 18–25 years, and −0.463 percentage points for ages ≥ 26 years; $P < 0.05$ for all estimates) [Nelson has at times, but not in this paper, disclosed support from alcohol industry interests (Bartlett and McCambridge, 2024)]
Carpenter and Eisenberg (2009) Canada 95 970 adults aged ≥ 20 years 3 survey periods: 1994/1995, 1996/1997, and 1998/1999	Repeated cross-sectional survey Alcohol consumption data from National Population Health Surveys, a representative survey of the Canadian population Negative binomial and probit regression	Sunday off-premises alcohol sales permitted in Ontario in December 1997 Comparison: all other Canadian provinces that did not change the Sunday alcohol sales policy	Difference in the change in the probability of consuming alcohol during the previous week between Ontario and other provinces that did not change their policy from the pre-intervention surveys to the 1998/1999 survey 0.001 (SE = 0.006; $P > 0.10$) Difference in the change in alcohol consumption during the previous week (number of drinks) between Ontario and other provinces that did not change their policy from the pre-intervention surveys to the 1998/1999 survey among individuals who consumed alcohol in the previous week 0.028 (SE = 0.060; $P > 0.10$)	Age, sex, education level, marital status, health status, presence of children in the household, employment, survey wave, provincial unemployment rate, and province	The study found within-week substitution effects, particularly from drinking on Saturdays to drinking on Sundays A drink was defined as a bottle of beer, a glass of wine, or 1.5 ounces [44.4 mL] of distilled spirits There were off-premises government monopolies in Canada during the study period

Table 3.4 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Subbaraman et al. (2023) USA 13 555 women aged 18–44 years Survey years: approximately every 5 years from 1984 to 2020	Repeated cross-sectional survey Alcohol consumption data from National Alcohol Surveys, a representative survey of the United States population Negative binomial regression	The presence of state-level restrictive and permissive Sunday off-premises "liquor" sales in the year of the survey	Ratio of the number of alcoholic beverages consumed in the past 12 months when Sunday off-premises "liquor" sales were allowed to the number of alcoholic beverages consumed in the past 12 months when Sunday off-premises "liquor" sales were not allowed 1.20 (95% CI, 1.01 to 1.42)	Age, race/ethnicity, marital status, education level, employment, survey month, year, state and state-level alcohol policies (government control of retail liquor sales, heavy beer at gas stations, heavy beer at grocery stores, "liquor" at grocery stores, BAC driving limits), poverty, unemployment, and alcohol consumption level	The term "liquor" was not defined Sunday sales were also associated with a greater number of days consuming ≥ 5 and ≥ 8 alcoholic beverages in the past 12 months [Kerr disclosed previous financial support from the alcohol industry]

Table 3.4 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Kilian et al. (2024b) USA 6 989 274 adults aged ≥ 18 years Annual surveys: 2000–2019	Repeated cross-sectional survey Alcohol consumption data from 20 waves of the Behavioral Risk Factor Surveillance System, which collects data from a representative sample of non-institutionalized residents of the USA Logistic and linear regression	Statewide bans on Sunday off-premises alcohol sales, which in some cases allowed beer sales or local options authorizing local authorities to permit Sunday sales despite a statewide ban Among the 20 states that had a ban in 2000, 12 lifted it by 2019; 30 states had no ban	OR (95% CI) of any alcohol consumption within the past 30 days for adults in states with a ban compared with states with no ban **Men:** Low education: 1.11 (1.10 to 1.13) Medium education: 1.08 (1.07 to 1.11) High education: 0.99 (0.97 to 1.01) **Women:** Low education: 0.99 (0.97 to 1.00) Medium education: 1.01 (0.99 to 1.02) High education: 0.98 (0.97 to 0.99) Difference (95% CI) in the logarithm of alcohol consumption per day (grams of ethanol) between adults in states with a ban and states with no ban among individuals who consumed alcohol in the past month **Men:** Low education: −0.01 (−0.02 to 0.00) Medium education: −0.01 (−0.03 to 0.00) High education: −0.04 (−0.05 to −0.03) **Women:** Low education: −0.05 (−0.06 to −0.04) Medium education: −0.07 (−0.08 to −0.05) High education: −0.09 (−0.10 to −0.08)	Individual-level (age, race and ethnicity, marital status) and state-level covariates (state, control state, drinking culture, unemployment rate); interaction with education	Effect estimates were only reported for low education (high school diploma or less), medium education (some college but no bachelor's degree), and high education (bachelor's degree or more) separately [The results for the groups with low and medium education were provided by the authors] [Kerr disclosed previous financial support from the alcohol industry]

ARIMA, autoregressive integrated moving average; BAC, blood alcohol concentration; CI, confidence interval; DiD, difference in difference; OR, odds ratio; SE, standard error.

alcohol sales, Subbaraman et al. (2023) assessed whether state-level Sunday off-premises "liquor" sales in the USA are associated with alcohol consumption among women aged 18–44 years using data from the National Alcohol Surveys for 1984 through 2020. Regressions were adjusted for individual-level and state-level controls and clustering by state and included fixed effects for survey month and year. Compared with not allowing Sunday off-premises "liquor" sales at the state level, allowing these sales was associated with the consumption of 1.20 (95% CI, 1.01 to 1.42) times as many drinks in the past 12 months. [Kerr disclosed previous financial support from the alcohol industry. The strength of this study is the adjustment for alcohol policies and other potential confounders at the individual and state levels in the models. The limitations of the study are that only women aged 18–44 years were included, and that the intervention is unclear because the term "liquor" was not clearly defined.]

In the first study with individual-level data of restrictive policy interventions on days of alcohol sales, Nelson (2008) estimated the effects of state-level Sunday off-premises alcohol sales bans on the yearly 30-day prevalence of alcohol consumption in the USA using repeated cross-sectional annual National Survey on Drug Use and Health data from 1999 through 2003, which included self-reported alcohol consumption among individuals aged ≥ 12 years). Compared with states without a ban on Sunday off-premises alcohol sales, there was a lower prevalence of alcohol consumption among states with a ban in the age strata 12–17 years (–1.02 percentage points), 18–25 years (–1.84 percentage points), and ≥ 26 years (–2.16 percentage points) ($P < 0.05$ for all age strata). [The strength of this study is the adjustment for other alcohol policies, income, and other control variables in the models. The limitation of the study is that Nelson has at times, but not in this paper, disclosed support from alcohol industry interests (Bartlett and McCambridge, 2024).]

In the other study with individual-level data of restrictive policy interventions on days of alcohol sales, Kilian et al. (2024b) assessed the effect of bans on Sunday off-premises alcohol sales on the prevalence of alcohol consumption in the USA using repeated cross-sectional Behavioral Risk Factor Surveillance System data from 2000 through 2019. Among the 20 states that had such a ban in 2000, 12 lifted it by 2019; 30 states had no ban. In the group with high education level, Sunday off-premises alcohol sales bans were associated with a lower prevalence of alcohol consumption (odds ratio [OR], 0.99; 95%, CI 0.97 to 1.01 among men; OR, 0.98; 95% CI, 0.97 to 0.99 among women) and a lower average daily amount of alcohol consumed among individuals who consumed alcohol in the past month ([–3.92%; 95% CI, –4.88% to –2.96%] among men; [–8.61%; 95% CI, –9.53% to –7.69%] among women) compared with no bans. In the groups with low and medium education levels, Sunday off-premises alcohol sales bans were associated with a higher prevalence of alcohol consumption among men (OR, 1.11; 95% CI, 1.10 to 1.13 for low education; OR, 1.08; 95% CI, 1.07 to 1.11 for medium education), but not among women (OR, 0.99; 95% CI, 0.97 to 1.00 for low education; OR, 1.01; 95% CI, 0.99 to 1.02 for medium education), and a lower average daily amount of alcohol consumed among men ([–0.96%; 95% CI, –2.31% to 0.41%] for low education; [–1.12%; 95% CI, –2.55% to 0.34%] for medium education) and women ([–4.84%; 95% CI, –5.99% to –3.66%] for low education; [–6.36%; 95% CI, –7.47% to –5.24%] for medium education) who consumed alcohol in the past month compared with no ban. [Kerr disclosed previous financial support from the alcohol industry. The strengths of this study are the inclusion of alcohol consumption data for ~7 million individuals, the assessment of policy interventions in 12 states over 20 years, the inclusion of the states without the policy

intervention as geographical controls, and the control for covariates at state and individual levels. The limitations of the study are that the effect of the policy intervention was estimated separately for different education levels among women and among men, but not among men or among women overall.]

(b) Hours of alcohol sales

The effects of permissive alcohol policy interventions that regulate hours of alcohol sales on alcoholic beverage consumption were assessed in one study with population-level data in the United Kingdom (Plant et al., 2014). The effects of restrictive alcohol policy interventions that regulate hours of alcohol sales on alcohol consumption were assessed in two other studies with population-level data in Australia (Dunt et al., 2024) and in the three Baltic countries and Poland (Rehm et al., 2024), and in one study with individual-level data in Australia (White et al., 2018) (Table 3.5).

(i) Studies with population-level data

Plant et al. (2014) (see also Section 3.2.1) assessed the effect of the 2002 policy intervention permitting an extra hour of alcohol sales in restaurants in the United Kingdom on recorded alcohol consumption (litres of ethanol per capita per year). The policy was associated with an increase of 9% (90% CI, 5% to 13%) in alcohol consumption. [The strengths and limitations of this study are described in Section 3.2.1.]

Dunt et al. (2024) assessed the effects of a restrictive policy intervention that introduced an 18:00 closing time or early closing of on-premises alcohol outlets in four states in Australia in 1916 for periods of 21–52 years on alcohol consumption (litres of ethanol per capita per year) using data from 1901 through 2006. Alcohol consumption in Australia was lower during 1916–1955, when early closing restrictions were in place for most of the Australian population, compared with alcohol consumption during the rest of the study period (–2.9 L; 95% CI, –3.4 L to –2.4 L) and compared with alcohol consumption in 1901–1915, before the restrictions were in place (–1.2 L; 95% CI, –2.3 L to –0.1 L). [The strengths of this study are the inclusion of > 100 years of annual alcohol consumption data covering multiple years before and after the policy intervention and the use of ARIMA models to account for underlying trends and autocorrelation. The limitations of the study are the lack of a geographical control and that other changes during the study period may have confounded the observed associations.]

Rehm et al. (2024) estimated the effects of a reduction of ≥ 20% in weekly hours of off-premises alcohol sales on the change in total adult alcohol per capita consumption (APC) in Estonia, Latvia, Lithuania, and Poland. During the study period (2001 through 2020), all three Baltic countries introduced restrictions on hours of alcohol sales, whereas in Poland there was no decrease in the hours of off-premises alcohol sales. The implementation of a reduction of ≥ 20% in weekly hours of off-premises alcohol sales was associated with a 1-year reduction in APC (–0.33 L; 95% CI, –1.06 L to 0.41 L). [The strength of this study is the inclusion of 20 years of annual alcohol consumption data from four geographically neighbouring countries with time variation in the introduction of restrictions to hours of alcohol sales. The limitations of the study are the few control variables included in the model and the assessment of the effects of the policy interventions on a 1-year change in alcohol consumption only.]

(ii) Studies with individual-level data

White et al. (2018) assessed the association between a stringency score for restricting hours of on-premises and off-premises alcohol sales and the prevalence of alcohol consumption in the past month among adolescents aged 12–17 years using four waves of representative school-based surveys conducted every 3 years

Table 3.5 Effects of regulations on hours of alcohol sales on alcoholic beverage consumption – studies with population-level and individual-level data

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Studies with population-level data					
Plant et al. (2014) United Kingdom Adult population aged ≥ 15 years 1961–2006	Interrupted time series Recorded alcohol consumption from WHO GISAH (WHO, 2024c) Linear regression	Policy in 2002 allowing restaurants an extra hour of opening without having to apply for a supper hour certificate No control or comparison area	Percentage change in recorded alcohol consumption per capita per year (litres of ethanol) from 1961–2001 to 2002–2006 9% (90% CI, 5% to 13%)	Income, price of beer, proportion of men aged > 65 years, mother's average age at childbirth, and time trend	The statistical analysis did not account for other alcohol policy interventions that were implemented at about the same time
Dunt et al. (2024) Australia Adult population aged ≥ 15 years 1901–2006	Panel National annual alcohol per capita consumption (based on alcohol production and imports and excluding exports) from the Australian Bureau of Statistics and its predecessors ARIMA	Restrictive policy intervention implementing an 18:00 closing time or early closing of on-premises alcohol outlets in 1916 in 4 of the 6 states in Australia, which lasted until 1937 (Tasmania), 1955 (New South Wales), 1966 (Victoria), or 1967 (South Australia) No control or comparison area	Difference in alcohol consumption per capita per year (litres of ethanol) Between years with restrictions (1916–1955) and years with no restrictions (1901–1915 and 1955–2006): –2.9 (95% CI, –3.4 to –2.4) Between years with restrictions (1916–1955) and years before restrictions (1901–1915): –1.2 (95% CI, –2.3 to –0.1)	Autocorrelation, war, Great Depression	Increase in number of alcohol licences and shift from mainly on-premises to off-premises consumption may confound the observed associations Before the policy intervention, on-premises alcohol outlets closed at 23:00 or 23:30
Rehm et al. (2024) Estonia, Latvia, Lithuania, and Poland Adult population aged ≥ 15 years 2001–2020	Time series Alcohol consumption data from WHO estimates of APC (recorded and unrecorded) Linear regression	Policy interventions reducing the weekly hours of off-premises alcohol sales by ≥ 20% in Latvia (2002), Estonia (2008), and Lithuania (2009 and 2018) (no change in Poland) Comparison: not applicable	1-year change in APC associated with the implementation of ≥ 20% reduction in weekly hours of off-premises alcohol sales β = –0.33 (95% CI, –1.06 to 0.41)	Year, alcohol tax increases, and country	

Table 3.5 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Studies with individual-level data					
White et al. (2018) Australia 45 245 students aged 12–17 years Survey years: 2002, 2005, 2008, and 2011	Repeated cross-sectional survey 4 waves of school-based Australian Secondary Students' Alcohol and Drug surveys completed by students residing in metropolitan areas of 4 states in Australia; schools and students within schools were randomly sampled Multilevel logistic regression	Stringency scores for hours-of-sale policies for each state and year (range, 0–100) were computed based on the extent of policies that regulate off-premises, on-premises, and general outlets' hours of sale (during the week and on Sundays) and whether 24-hour sales were permitted	OR of alcohol consumption in the past month for a 10% higher stringency score for hours-of-sale policies in the year before the survey 0.80 (95% CI, 0.69 to 0.94)	Clustering of students at the school level and state, past-year youth access and supply policies stringency score, drink-driving policies stringency score and outlet density, television advertising in past 3 months, alcohol-related newspaper coverage, alcohol consumer price index, adult alcohol consumption rates, sex, age, socioeconomic status, students' self-reported academic ability, language spoken at home, smoking in the past month, and timing of survey (year and month)	The stringency scores for the policies did not include a measure of enforcement During the study period, states implemented restrictive or permissive policy changes

APC, total adult alcohol per capita consumption; ARIMA, autoregressive integrated moving average; CI, confidence interval; GISAH, Global Information System on Alcohol and Health; OR, odds ratio; WHO, World Health Organization.

from 2002 to 2011 in metropolitan areas of four states in Australia. State-specific stringency scores for hours-of-sale policies were computed for each year based on the policies that regulated the hours of alcohol sales during the week and on Sundays and whether 24-hour alcohol sales were permitted. The state stringency scores for hours-of-sale policies increased by 14% ($P < 0.001$) over the study period. A 10% increase in the stringency score was inversely associated with the prevalence of alcohol consumption (OR, 0.80; 95% CI, 0.69 to 0.94). [The strengths of this study are the large sample size and the adjustment for other alcohol policies, alcohol price, and other control variables in the model. The limitation of the study is that only individuals aged 12–17 years residing in metropolitan areas were included.]

3.2.3 Regulations on minimum legal purchase or drinking age

The effects of minimum legal purchase or drinking age alcohol policy interventions on alcoholic beverage consumption were assessed in 14 studies. Most of the studies were conducted in the USA or Canada, where there were frequent changes in minimum purchase and drinking age laws throughout the 1970s and 1980s. Among the studies with population-level data, the effects of permissive interventions were assessed in two studies in Canada (Smart and Schmidt, 1975; Xie et al., 2000) and one study in the USA (Smart, 1977), the effects of one restrictive policy and one permissive policy were assessed in another study in the USA (Wagenaar, 1982), and the average effects of multiple restrictive or permissive policies were assessed in a study in five European countries (Baccini and Carreras, 2014) (Table 3.6). Among the studies with individual-level data, the effects of restrictive interventions were assessed in eight studies in the USA (Hingson et al., 1983; Engs and Hanson, 1988; Williams and Lillis, 1988; George et al., 1989; Perkins and Berkowitz, 1989; Gonzalez, 1990; O'Malley and Wagenaar, 1991; Yu and Shacket, 1998) and one study in Denmark (Møller, 2002) (Table 3.7).

(a) Studies with population-level data

Smart and Schmidt (1975) assessed the effects of a reduction in the minimum legal purchase and drinking age from 21 years to 18 years in July 1971 in Ontario, Canada, on the monthly distribution of alcohol (proportion of annual volume of alcohol) to the province's on-premises and off-premises alcohol outlets. The distribution of alcohol to outlets for the period August–December 1970 (47.9%) was slightly lower than for the period August–December 1971 (48.8%) (*P* for difference not reported). The lower proportion of alcohol distributed in August–December in 1970 compared with 1971 was observed for both on-premises outlets (1970: 43.0%, 1971: 45.7%) and off-premises outlets (1970: 28.8%, 1971: 29.3%). [The strength of this study is the inclusion of monthly data, allowing for the assessment of alcohol distribution for the 5-month period in the year before and in the year of the policy change. The limitations of the study are that the results were descriptive, the lack of a geographical control, and that alcohol distribution was based on alcohol shipment data only.]

Smart (1977) assessed the effects of a reduction in the minimum legal drinking age in 25 states in the USA between 1970 and 1973 on the difference in the change in beverage-specific sales (volume in gallons per year) between the states with a reduction in minimum legal drinking age and adjacent states with no change from the year before the state-specific reduction in minimum legal drinking age to the year after the reduction. Beer sales increased by 5.7% more in states with a reduction compared with adjacent states ($P < 0.01$), but there was no difference in the change in wine sales or spirits sales ($P > 0.05$ for each). [The strengths of this study are the assessment of policy interventions in 25 states in

Table 3.6 Effects of regulations on minimum legal purchase or drinking age on alcoholic beverage consumption – studies with population-level data

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Smart and Schmidt (1975) Canada Total population 1970–1971	Interrupted time series Monthly distribution of alcohol data from the Liquor Control Board of Ontario Descriptive analysis	Ontario decreased the minimum legal purchase and drinking age from 21 years to 18 years in July 1971 No control or comparison province	Proportion of annual alcohol volume distributed to on-premises and off-premises alcohol outlets in August–December 1970: 47.9% 1971: 48.8% (*P* for difference, NR)		The policy intervention resulted in an additional 8.1% of the population being legally able to purchase and consume alcohol The proportion of annual alcohol volume distributed in August–December was lower in 1970 than in 1971 for both on-premises outlets (1970: 43.0%, 1971: 45.7%) and off-premises outlets (1970: 28.8%, 1971: 29.3%)
Smart (1977) USA Total population 1969–1974	Time series Data on alcohol sales from the Brewers Association, Washington, DC Mann–Whitney *U* test	Reductions in the state minimum legal drinking age during 1970–1973 From 21 years to 18 years: 16 states From 21 years to 19 years: 3 states From 21 years to 20 years: 1 state From 20 years to 18 years: 2 states From 20 years to 19 years: 2 states From 19 years to 18 years: 1 state Comparison: states adjacent to the intervention states with unchanged minimum legal drinking age in 1969–1974	Percentage difference in the change in beverage-specific sales per year (volume in gallons) of beer, wine, and spirits between states where the minimum legal drinking age changed compared with states where there was no change from the year before the reduction to the year after the reduction Beer: 5.7% ($P < 0.01$) Wine: NR ($P > 0.05$) Spirits: 4% ($P > 0.05$)		The analysis did not correct for cross-border purchases In analyses where the change in alcohol sales from the year before to the year of the intervention was assessed, there was a statistically significant increase ($P < 0.05$) in beer sales and spirits sales in states with a reduction in the minimum legal drinking age compared with states with no change in the minimum legal drinking age

Table 3.6 (continued)

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Wagenaar (1982) Maine, USA Total population 1970–1980	Interrupted time series Monthly alcohol distribution data from Maine Bureau of Alcoholic Beverages ARIMA	Reduction in the minimum legal drinking age from 20 years to 18 years for all alcoholic beverages in June 1972 and increase in the minimum legal drinking age from 18 years to 20 years in October 1977 in Maine No control or comparison groups	Percentage change in beverage-specific alcohol sales per year (volume in kilolitres) **From before (1970–June 1972) to after (July 1972–December 1980) the minimum legal drinking age was decreased:** Beer: 2.6% ($P \geq 0.01$) Wine: −4.1% ($P \geq 0.01$) Spirits: −3.7% ($P \geq 0.01$) **From before (1970–October 1977) to after (November 1977–December 1980) the minimum legal drinking age was increased:** Beer: −12.9% ($P < 0.01$) Wine: 10.2% ($P \geq 0.01$) Spirits: 3.6% ($P \geq 0.01$)	The model includes variables for seasonality, time, and both interventions	A mandatory beverage container deposit law was implemented at the same time that the minimum legal drinking age was increased in October 1977 Analyses of beer sales in New Hampshire revealed that decreased beer sales in Maine in the late 1970s were not due to additional cross-border beer purchases in New Hampshire by Maine residents
Xie et al. (2000) Canada Adult population aged ≥ 15 years 1968–1986	Panel Annual alcohol sales data by province from Statistics Canada Ordinary least-squares regression	Reduction in the minimum legal drinking age from 21 years to 18 years or 19 years in Canadian provinces; modelled as a 0–1 dummy variable (1 = minimum legal drinking age 18 years or 19 years; 0 = minimum legal drinking age 21 years)	Difference in the natural logarithm of alcohol sales per capita per year (litres of ethanol) between provinces with a minimum legal drinking age of 18 years or 19 years and provinces with a minimum legal drinking age of 21 years in the absence of alcohol outlets $\beta = 0.19$ ($P \leq 0.05$) Interaction term: Difference in the difference in the natural logarithm of alcohol sales per capita per year (litres of ethanol) between provinces with a minimum legal drinking age of 18 years or 19 years and provinces with a minimum legal drinking age of 21 years per 1-unit increase in the number of alcohol retail outlets per 10 000 population $\beta = -0.09$ ($P \leq 0.05$)	Alcohol outlet density, interaction between alcohol outlet density and minimum legal drinking age, alcohol price, AA membership, AA groups, population aged 35–54 years, unemployment rate, and personal disposable income	The number of provinces that reduced the drinking age was not reported The effective sample size was relatively small because of 3-year intervals between data points; 1 province was an outlier and was excluded from the models

Table 3.6 (continued)

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Baccini and Carreras (2014) 5 European countries Adult population aged ≥ 15 years 1961–2008 (see Allamani et al., 2014)	Time series Alcohol control policies and recorded alcohol consumption data from WHO GISAH (WHO, 2024c) Country-specific linear regression analyses using the same regression model followed by a random-effects meta-analysis	Changes in the minimum legal purchase age: Austria: decrease in 1966 Finland: increase in 1969 France: increase in 1991 Spain: increase in 1982 Switzerland: increase in 2002 No control or comparison countries	Percentage change in recorded alcohol consumption per capita per year (litres of ethanol) per 1-year increase in the minimum legal purchase age –9.8% (90% CI, –15.4% to –4.2%)	Implementation of restrictive alcohol advertising policies, restrictive alcohol availability policies, permissive alcohol availability policies, change in the BAC limit for driving, a linear time trend variable, logarithm of income, logarithm of price of the 2 main types of alcoholic beverages consumed during the study period, percentage of men aged > 65 years, and average urbanization level in the previous 2 years	Increasing the minimum legal purchase age was associated with reduced alcohol consumption in most countries (France, Spain, and Switzerland) but with an unexpected increase in alcohol consumption in Austria

AA, Alcoholics Anonymous; ARIMA, autoregressive integrated moving average; BAC, blood alcohol concentration; CI, confidence interval; GISAH, Global Information System on Alcohol and Health; NR, not reported; WHO, World Health Organization.

the USA over 4 years and the inclusion of adjacent states where the minimum legal drinking age did not change as geographical controls. The limitations of the study are that alcohol sales were analysed only by major type of alcoholic beverage and were based on recorded alcohol sales data only.]

Wagenaar (1982) assessed the effects of a reduction in the minimum legal drinking age from 20 years to 18 years in June 1972 and a subsequent increase in the minimum legal drinking age from 18 years to 20 years in October 1977 on the change in beverage-specific alcohol sales (volume in kilolitres per month) in Maine between 1970 and 1980. Controlling for long-term alcohol consumption trends and seasonality, the percentage change in monthly alcohol sales from before to after the reduction in the minimum legal drinking age in June 1972 was 2.6% for beer, −4.1% for wine, and −3.7% for spirits ($P \geq 0.01$ for each type of alcoholic beverage). The increase in the minimum legal drinking age in October 1977 was associated with a decrease in beer sales (–12.9%; $P < 0.01$) and an increase in wine sales (10.2%; $P \geq 0.01$) and spirits sales (3.6%; $P \geq 0.01$). Analyses of beer sales in New Hampshire revealed that decreased beer sales in Maine in the late 1970s were not due to additional cross-border beer purchases in New Hampshire by Maine residents. [The strengths of this study are the assessment of two policy interventions that occurred in the same state 5 years apart and the use of ARIMA models to account for underlying trends, autocorrelation, and seasonality. The limitations of the study are that the concurrent introduction of a state beer container deposit law may have biased the effect estimates for the increase in the minimum legal drinking age on alcohol sales, and that alcohol sales were analysed only by type of alcoholic beverage and were based on recorded alcohol sales data only.]

Xie et al. (2000) assessed the association between a reduction in the minimum legal drinking age and alcohol sales over the period from 1968 to 1986 in provinces in Canada. There was a statistical interaction between minimum legal drinking age and alcohol outlet density in relation to alcohol sales ($P \leq 0.05$), which shows that the association between minimum legal drinking age and alcohol sales decreased with increasing alcohol outlet density. For one alcohol outlet per 10 000 individuals, alcohol sales were [10.0%] higher in provinces with a minimum legal drinking age of 18 years or 19 years compared with provinces with a minimum legal drinking age of 21 years. [The strength of this study is the adjustment for income, price, and other control variables in the models. The limitations of the study are that the number of provinces with a lower minimum legal drinking age was not reported; that although data from a 19-year period were used, the effective sample size was relatively small because of 3-year intervals between data points; and that alcohol consumption was based on recorded alcohol sales data only.]

Baccini and Carreras (2014) (see also Sections 3.2.5 and 4.3.2(c)) used data from the AMPHORA project (Allamani et al., 2014) and conducted a time-series analysis and meta-analysis to assess the effect of alcohol policy interventions on the change in recorded alcohol consumption (litres of ethanol per capita per year) in 12 European countries from 1961 through 2008; the same model was fitted for each country, and country-specific estimates were meta-analysed. One of the alcohol policy interventions that was assessed in the study was minimum legal purchase age. This analysis was based on interventions in four countries that increased the minimum legal purchase age (Austria, in 1966; France, in 1991; Spain, in 1982; and Switzerland, in 2002) and one country that decreased the minimum legal purchase age (Finland, in 1969). After controlling for changes in other alcohol policy interventions during the study period, income, and other factors, a 1-year increase in the minimum legal purchase

age was associated with a reduction (–9.8%; 90% CI, –15.4% to –4.2%) in alcohol consumption. [The strengths of this study are the inclusion of 48 years of annual alcohol consumption data from multiple European countries with time variation in alcohol policy interventions; that the statistical models included control variables for unplanned contextual factors and planned policy variables, as well as a linear time trend variable for unobserved factors; that an auto-correlation test was implemented; and that the same model was used for each country to reduce bias due to heterogeneity. The limitations of the study are the lack of a geographical control and that alcohol consumption was based on recorded alcohol sales data only.]

(b) Studies with individual-level data

Hingson et al. (1983) assessed the effect of an increase in the minimum legal drinking age from 18 years to 20 years in 1979 in Massachusetts, USA, on alcohol consumption (drinks per day) among individuals aged 16–19 years using three waves of household survey data in 1979, 1980, and 1981; New York State was a control state because it retained a minimum legal drinking age of 18 years. Overall, there was no difference in the change in alcohol consumption between Massachusetts and New York State from 1979 to 1980–1981. Because increasing the minimum legal drinking age could also reduce alcohol consumption in younger age groups, Smith et al. (1984) assessed the effect of the change in the minimum legal drinking age in a subsample of individuals aged 16–17 years, among whom there also was no difference in the change in alcohol consumption between Massachusetts and New York State. [The strengths of this study are the inclusion of individuals from New York State as a geographical control and the consistently high survey response rates. The limitations of the study are that the statistical analysis does not account for underlying trends, that effect estimates were not reported, and the household sampling, which may not include residential college students.]

Engs and Hanson (1988) analysed data from three cross-sectional surveys (in 1982–1983, 1984–1985, and 1987–1988) conducted at 56 universities and colleges among all states in the USA to assess the effect of the national increase in the minimum legal purchase or drinking age by July 1987 on the prevalence of alcohol abstention (alcohol consumption less than once per year or not at all), which was 16.2% in 1982–1983. Compared with the prevalence of alcohol abstention in 1984–1985 (17.3%), the prevalence of alcohol abstention was higher in 1987–1988 (21.2%) (P for difference < 0.0001). [The strengths of this study are the high response rate for the 1987–1988 survey and that the demographic composition of the samples was similar across the three surveys. The limitations of the study are the lack of a geographical control and the non-probability sample of college and university students only.]

Williams and Lillis (1988) assessed the short-term (over 1 year) and long-term (over 3 years) effects of an increase in the minimum legal purchase age in New York State from 18 years to 19 years in December 1982 on the change in the prevalence of alcohol consumption (any consumption in the past 28 days) from 1982 to 1983 and from 1983 to 1985 using data from repeated cross-sectional surveys of individuals aged 16–20 years living in households. From 1982 to 1983, there was a decrease in the prevalence of alcohol consumption in all age groups; the decrease was greatest for individuals aged 18 years (–21.1%; $P < 0.01$) and smallest for individuals aged 19 years (–9.3%; $P < 0.05$). However, from 1983 to 1985, there were no statistically significant additional changes in the prevalence of alcohol consumption in any age groups (range, –10.9% for age 17 years to 11.6% for age 20 years). [The strength of this study is the inclusion of unaffected age groups for comparison. The limitations of the study are the lack of a geographical

Table 3.7 Effects of regulations on minimum legal purchase or drinking age on alcoholic beverage consumption – studies with individual-level data

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Hingson et al. (1983), Smith et al. (1984) Massachusetts (intervention) and New York State (control), USA Adults aged 16–19 years (wave 1, *n* = 2007; wave 2, *n* = 1990; wave 3, *n* = 1975) Survey years: 1979, 1980, and 1981	Repeated cross-sectional survey Probability sample of random-digit dialling telephone household surveys, sampling individuals aged 16–19 years; response rate range, 80% to 87% Log–linear regression and descriptive analysis	Increase in the minimum legal drinking age from 18 years to 20 years on 16 April 1979 in Massachusetts Comparison: New York State (excluding New York City and Nassau County), where the minimum legal drinking age of 18 years remained unchanged	Difference in the change in alcohol consumption (drinks per day) between Massachusetts and New York State from 1979 to 1980–1981 Ages 16–19 years: NR ($P > 0.05$) (Hingson et al., 1983) Ages 16–17 years: NR ($P > 0.05$) (Smith et al., 1984)		The age group studied in Smith et al. (1984) was not directly affected by the change in the law
Engs and Hanson (1988) USA 9317 college or university students Survey years: 1982–1983, 1984–1985, and 1987–1988	Repeated cross-sectional survey 3 waves of non-probability samples of students in sociology, health, or physical education courses at 56 colleges and universities (sample size between 2797 and 3375); 1987–1988 response rate > 98% Chi-squared test	State-specific minimum legal purchase or drinking age increased to 21 years no later than July 1987 No control or comparison group	Prevalence of alcohol abstention (alcohol consumption less than once per year or not at all) 1982–1983: 16.2% 1984–1985: 17.3% 1987–1988: 21.2% Test for difference between 1984–1985 and 1987–1988: $P < 0.0001$		The percentage of consecutive survey respondents who were of legal drinking age and the response rates for the first 2 surveys were not reported The demographic composition of the 1987–1988 sample approximated that of the earlier samples as well as the population attending baccalaureate institutions of higher learning in the USA

Table 3.7 (continued)

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Williams and Lillis (1988) New York State, USA Adults aged 16–20 years (wave 1, *n* = 1811; wave 2, *n* = 1798; wave 3, *n* = 1626) Survey periods: November 1982, December 1983, and November 1985	Repeated cross-sectional survey Probability-stratified samples of individuals aged 16–20 years living in households in New York State (excluding New York City) that answered a telephone survey; response rate: 76% in 1982, 86% in 1983, and 83% in 1985 *Z*-score proportions test	New York State increased the minimum legal purchase age from 18 years to 19 years in December 1982 No control or comparison group	Change in the prevalence of alcohol consumption in the past 28 days **Age 16 years:** From 1982 to 1983: –18.1% ($P < 0.01$) From 1983 to 1985: –7.5% ($P \geq 0.05$) **Age 17 years:** From 1982 to 1983: –13.5% ($P < 0.01$) From 1983 to 1985: –10.9% ($P \geq 0.05$) **Age 18 years:** From 1982 to 1983: –21.1% ($P < 0.01$) From 1983 to 1985: –1.6% ($P \geq 0.05$) **Age 19 years:** From 1982 to 1983: –9.3% ($P < 0.05$) From 1983 to 1985: –1.4% ($P \geq 0.05$) **Age 20 years:** From 1982 to 1983: –10.4 ($P < 0.05$) From 1983 to 1985: 11.6% ($P \geq 0.05$)		Excluded college samples
George et al. (1989) New York State, USA University students (wave 1, *n* = 785; wave 2, *n* = 627; wave 3, *n* = 287) Survey years: 1985, 1986, and 1987	Repeated cross-sectional survey Non-probability samples of introductory psychology students at 1 university who were administered the Daily Drinking Questionnaire 9.5 months before (in 1985) and 2.5 months after (in 1986) and 14.5 months after (in 1987) the policy intervention Not reported	New York State increased the minimum legal drinking age from 19 years to 21 years in December 1985 No control or comparison group	Difference in alcohol consumption per week (number of drinks) between 1985 and 1986 among individuals who consume alcohol **Men:** Ages ≤ 18 years: [–0.62] ($P > 0.05$) Ages 19–20 years: [–1.67] ($P > 0.05$) Ages ≥ 21 years: [0.96] ($P > 0.05$) **Women:** Ages ≤ 18 years: [0.64] (*P*, NR) Ages 19–20 years: [0.25] (*P*, NR) Ages ≥ 21 years: [1.80] (*P*, NR)		There was no difference in the prevalence of alcohol abstention in 1986 compared with 1985 for all age and sex groups (estimates not reported; all $P \geq 0.05$) A drink was defined as 12 ounces [354.9 mL] of beer, 4 ounces [118.3 mL] of wine, or 1 ounce [29.6 mL] of spirits

Table 3.7 (continued)

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Perkins and Berkowitz (1989) New York State, USA College students aged 17–23 years (wave 1, n = 797; wave 2, n = 860) Survey years: 1982 and 1984	Repeated cross-sectional survey Non-probability samples of first-year and second-year students at 1 liberal arts college that included the age affected by the policy change (age 18 years: wave 1, n = 203; wave 2, n = 240) Not reported	New York State increased the minimum legal drinking age from 18 years to 19 years in December 1982 No control or comparison group	Difference in prevalence of alcohol abstention (never consumed alcoholic beverages) between 1982 and 1984 among individuals aged 18 years 1.6 percentage points ($P \geq 0.05$) Difference in alcohol consumption in the past 2 weeks (number of drinks) between 1982 and 1984 among individuals aged 18 years –1.4 ($P \geq 0.05$)		The analysis excludes students aged 17 years because there were too few The prevalence of alcohol abstention did not increase among students aged 19–23 years A drink was defined as a beer, a glass of wine, a shot of spirits, or a mixed drink
Gonzalez (1990) Florida, USA 1546 university students Survey years: 1983–1988	Repeated cross-sectional survey Random sample of students at the University of Florida (51% women) who answered the Student Drinking Information Scale ANOVA	Gradual increase in minimum legal drinking age from 19 years to 21 years from 1 July 1985 to 1 July 1987 No control or comparison group	Difference in alcohol consumption per month (number of drinks) per 1-year increase in calendar year Men: NR ($P > 0.05$) Women: NR ($P > 0.05$)		The numbers of survey participants for whom drinking became illegal were not reported The average differences were comparable for students aged < 21 years and those aged ≥ 21 years (NR; $P > 0.05$)
O'Malley and Wagenaar (1991) USA High school seniors at time of first survey completion Survey years: 1976–1987	Repeated cross-sectional and longitudinal surveys Probability sample of high school seniors (~130 high schools) who completed a baseline survey in 1976–1981 (Monitoring the Future project, self-administered questionnaire) and follow-up mail surveys (1 year or 2 years after graduation and then every 2 years); response rate, ~83% Descriptive analysis	State-level (n = 21) increases in the minimum legal drinking age from 18 years to 19 years, 20 years, or 21 years No control or comparison group	Change in the prevalence of alcohol consumption in the past 30 days from the 3 years before to the 3 years after the intervention 21 states where minimum legal drinking age increased from 18 years to 19, 20, or 21 years: –13.3% of the total standard deviation 7 states where minimum legal drinking age increased from 18 years to 21 years: –28.2% of the total standard deviation		The analysis included 12 045 surveys among men and 12 682 surveys among women before the policy intervention, and 11 196 surveys among men and 12 187 surveys among women after the policy intervention The change in the prevalence of alcohol consumption in the past 30 days is expressed as a percentage of the total standard deviation (= 1.6)

Table 3.7 (continued)

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Yu and Shacket (1998) New York State, USA 11 069 adults aged 16–24 years Survey years: 1982, 1983, 1985, 1986, and 1996	Repeated cross-sectional survey Probability sample in 10 counties; response rate range, 76% to 93% Descriptive analysis	In New York State, the minimum legal purchase age increased from 18 years to 19 years in December 1982 and then from 19 years to 21 years in December 1985 No control or comparison group	Prevalence of alcohol consumption in the past 28 days among individuals aged 18 years 1982: 81% (95% CI, [76.6% to 85.4%]) 1983: 64% (95% CI, [58.7% to 69.3%]) Prevalence of alcohol consumption in the past 28 days among individuals aged 19 years 1985: 76% (95% CI, [71.0% to 81.0%]) 1986: 62% (95% CI, [55.7% to 68.3%]) Prevalence of alcohol consumption in the past 28 days among individuals aged 20 years 1985: 83% (95% CI, [78.6% to 87.4%]) 1986: 62% (95% CI, [55.9% to 68.1%])		
Møller (2002) Denmark ~8000 students Survey years: 1997, 1998, 1999, and 2000	Repeated cross-sectional survey Probability sample of schools in Denmark with students in grades 5–10 (approximate ages, 11–17 years); ~95% of the schools approached participated in the study, and 95% of the students in the participating schools completed the questionnaire Logistic regression	A law that prohibited the sale of alcohol to people younger than 15 years was implemented on 1 July 1998 No control or comparison group	OR of alcohol consumption during the past month 1998 vs 1997: 0.84 ($P = 0.086$) 1999 vs 1997: 0.78 ($P = 0.012$) 2000 vs 1997: 0.73 ($P = 0.001$)	Age, grade, sex, attitude towards school, absence due to illness (≥ 1 sick day in past 30 days), and truancy (≥ 1 day away in past 30 days)	In stratified analyses by grade, the prevalence of alcohol consumption during the past month was lower in 2000 than in 1997 for both grades 5–7 (OR, 0.64; $P = 0.005$) and grades 8–10 (OR, 0.83; $P = 0.086$)

ANOVA, analysis of variance; CI, confidence interval; NR, not reported; OR, odds ratio.

control and the household sampling, which may not include residential college students.]

Yu and Shacket (1998) used data from the same three cross-sectional surveys (in 1982, 1983, and 1985) used by Williams and Lillis (1988) and from two additional surveys (in 1986 and 1996) of individuals aged 16–24 years in New York State to assess the change in the prevalence of alcohol consumption (any consumption in the past 28 days) after the state's minimum legal purchase age was increased from 18 years to 19 years in 1982 and then from 19 years to 21 years in 1985. After the policy change in 1982, the prevalence of alcohol consumption among individuals aged 18 years decreased from 81% (95% CI, [76.6% to 85.4%]) in 1982 to 64% (95% CI, [58.7% to 69.3%]) in 1983. After the policy change in 1985, the prevalence of alcohol consumption in the past month among individuals aged 19 years decreased from 76% (95% CI, [71.0% to 81.0%]) in 1985 to 62% (95% CI, [55.7% to 68.3%]) in 1986, and among individuals aged 20 years decreased from 83% (95% CI, [78.6% to 87.4%]) in 1985 to 62% (95% CI, [55.9% to 68.1%]) in 1986. [The strength of this study is the assessment of two interventions that occurred in the same state and 3 years apart. The limitations of the study are the lack of a geographical control and the household sampling, which may not include residential college students.]

George et al. (1989) analysed repeated cross-sectional survey (in 1985, 1986, and 1987) data from introductory psychology students at one university in New York State, USA, to assess the effect of the increase in the minimum legal drinking age from 19 years to 21 years in 1985 on the difference in alcohol consumption (number of drinks per week) between 1985 and 1986. Among individuals who reported consuming alcohol and were aged 19–20 years, the age group most likely to be affected by the policy change, the difference in alcohol consumption between 1985 (i.e. 9.5 months before the policy intervention) and 1986 (i.e. 2.5 months after the policy intervention) was [–1.67] ($P > 0.05$) for men and [0.25] (P not reported) for women. No meaningful differences were observed for individuals aged ≤ 18 years and individuals aged ≥ 21 years. [The strength of this study is the estimation of the immediate effect of the policy intervention. The limitations of the study are the lack of a geographical control and the non-probability sample of students from one course and one university.]

Perkins and Berkowitz (1989) assessed the effect of the increase in the minimum legal drinking age in New York State from 18 years to 19 years in December 1982 using alcohol consumption data from surveys conducted in the spring of 1982 and the spring of 1984 among students at an undergraduate liberal arts institution of higher education. Among individuals aged 18 years, the prevalence of alcohol abstention was 3.0% in the spring of 1982 and 4.6% in the spring of 1984 ($P \geq 0.05$), and the difference in the number of drinks consumed in the past 2 weeks between the 1982 and 1984 surveys was –1.4 ($P \geq 0.05$). [The strength of this study is the inclusion of unaffected age groups as controls. The limitations of the study are the lack of a geographical control and the non-probability sample of students from one institution.]

Gonzalez (1990) assessed the effects of the gradual increase in the minimum legal drinking age from 19 years to 21 years in Florida, USA, in 1985–1987 using repeated cross-sectional survey data (for 1983 to 1988) from students at the University of Florida. Year of survey was not associated with the average number of drinks per month that students consumed over the 6 years among women ($P > 0.05$) or men ($P > 0.05$), and the associations did not differ between students aged < 21 years and those aged ≥ 21 years. [The strength of this study is the assessment of linear trends in alcohol consumption using data from six survey waves. The limitations of the study are the lack of a geographical control, that the sample

only included students from a single university, and that effect estimates were not reported.]

O'Malley and Wagenaar (1991) analysed data from six annual cross-sectional surveys of high school seniors in 1976–1981 and multiple follow-up surveys (until 1987) to assess the effect of increasing the minimum legal drinking age from 18 years to 19 years, 20 years, or 21 years between 1976 and 1986 on the change in the prevalence of alcohol consumption in the past 30 days (expressed as a percentage of the total standard deviation = 1.6) in 21 states in the USA. For states where the minimum legal drinking age increased from 18 years, the prevalence of alcohol consumption decreased by 13.3% of the total standard deviation from the 3 years before the increase in the minimum legal drinking age to the 3 years after the increase. For the seven states where the minimum legal drinking age increased from 18 years to 21 years specifically, the prevalence of alcohol consumption decreased by 28.2% of the total standard deviation. [The strength of this study is the assessment of multiple policy interventions in different states over a 10-year period. The limitations of the study are the descriptive analysis, the lack of a geographical control, and the implementation of other alcohol policy interventions during the study period.]

In Denmark, a new law that prohibited the sale of alcohol to individuals aged < 15 years was implemented in July 1998. Møller (2002) assessed the effect of this alcohol policy intervention on the prevalence of alcohol consumption during the past month using data from four cross-sectional surveys (in November 1997, 1998, 1999, and 2000) completed by representative samples of school students in grades 5–10 (approximately aged 11–17 years). Compared with the year before the ban on alcohol sales to young people, the prevalence of alcohol consumption during the past month was lower in 1998 (OR, 0.84; $P = 0.086$), 1999 (OR, 0.78; $P = 0.012$), and 2000 (OR, 0.73; $P = 0.001$). In stratified analyses by grade, the prevalence of alcohol consumption during the past month was lower in 2000 than in 1997 for both grades 5–7 (OR, 0.64; $P = 0.005$) and grades 8–10 (OR, 0.83; $P = 0.086$). [The strengths of this study are the high school and student participation rates and the adjustment for control variables in the models. The limitation of the study is the lack of a geographical control.]

3.2.4 Total and partial bans on alcohol sales

(a) Total bans

The effects of total bans on alcohol sales on alcoholic beverage consumption were assessed in two studies with population-level data (Miron and Zwiebel, 1991; Manthey et al., 2019) and two studies with individual-level data (Maphisa Maphisa and Ndlovu, 2023; Chakrabarti et al., 2024). The effects of total bans on alcohol sales on proxy outcomes for alcohol consumption were assessed in five studies with population-level data (Dills and Miron, 2004; Chrystoja et al., 2020; Law and Marks, 2020; Chu et al., 2022; Barron et al., 2024) (Table 3.8). The studies were conducted in both high-income countries (Canada and the USA) and low- and middle-income countries (Botswana, India, and South Africa).

(i) Total bans in the USA

During the second half of the 1910s, many states in the USA enacted various laws (i.e. prohibitions) that banned all alcohol sales (Dills and Miron, 2004; Law and Marks, 2020). For many states, there was a time lag, in some instances 2 years, between enactment and implementation of the statewide alcohol prohibitions, and in some states, individual counties were already dry (Law and Marks, 2020). The National Prohibition became effective in January 1920 under the 18th Amendment to the United States Constitution, which illegalized alcohol production, alcohol transportation, and all alcohol sales. However, the inability to restrict the illegal

Table 3.8 Effects of total bans on alcoholic beverage consumption and proxy outcomes – studies with population-level data

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Total bans in the USA					
Miron and Zwiebel (1991) USA Total population 1900–1950	Interrupted time series Estimated total alcohol consumption based on deaths from liver cirrhosis and from alcoholism from Vital Statistics of the United States, per capita admissions for alcohol psychosis from hospital records, and rate of arrests for drunkenness from the Moderation League Descriptive analysis	National Prohibition on the sale, production, and transportation of alcohol became effective in January 1920 and was repealed in December 1933 No control or comparison group	Alcohol consumption per capita per year (gallons of ethanol) After implementation of Prohibition: 30% of pre-Prohibition levels By the end of Prohibition: Plateaued to 60–70% of pre-Prohibition levels In the decade after Prohibition: Returned to pre-Prohibition levels		
Dills and Miron (2004) USA and United Kingdom Total population 1900–1997	Panel Annual state-level liver cirrhosis mortality rates and annual country-level liver cirrhosis mortality rates from the WHO World Health Statistics annual report; for 10 states liver cirrhosis data are available beginning in 1900, whereas in other states the first year of data varies Linear regression	33 states in USA passed state-level prohibition laws before 1920 (4 before 1900; 2 states temporarily repealed theirs before 1920); National Prohibition on the sale, production, and transportation of alcohol from 1920 to 1933 Control: United Kingdom	Difference (95% CI) in liver cirrhosis deaths per 100 000 people per year between states with a prohibition law and states without a prohibition law during 1900–1997 $\beta = -0.09$ ([−0.23 to 0.05]) Difference (95% CI) in liver cirrhosis deaths per 100 000 people due to National Prohibition between the Prohibition period (1920–1933) and non-Prohibition periods (1900–1916, 1934–1997) $\beta = -0.44$ ([−0.66 to −0.21])	Age, sex, state, and liver cirrhosis mortality in the previous year; the model for state prohibition additionally adjusted for year; the model for National Prohibition additionally adjusted for state prohibition, federal alcohol policies period, income, and liver cirrhosis mortality in the United Kingdom	State prohibitions varied in the regulation of importation and home production of alcoholic beverages, and state prohibition enactment rather than implementation was assessed The Pearson correlation coefficient between alcohol consumption and liver cirrhosis mortality rate was 0.75 In a sensitivity analysis of data from 1900 through 1919, there was no association between state prohibitions and liver cirrhosis mortality rates The results were weighted by state population

Table 3.8 (continued)

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Law and Marks (2020) USA Total population 1900–1920	Panel Annual state-level cause-specific mortality data from the Census Bureau for 35 states; not all 35 states had suitable data available since 1900 Linear regression	Countywide and statewide alcohol prohibition laws implemented before 1920 included bans on alcohol sales and in some states other bans (e.g. on home production); a prohibition coverage variable was computed as the proportion of a state's population that lived in a dry county during each year and was based on the timing of prohibition enforcement and the presence of countywide prohibitions Comparison: N/A	Difference in cause-specific deaths per 100 000 people per year between states with statewide prohibition laws and states without statewide or countywide prohibition laws during 1900–1920 Alcoholism: $\beta = -2.83$ (SE = 1.12; $P < 0.05$) Liver cirrhosis: $\beta = -1.25$ (SE = 0.34; $P < 0.01$) Homicides: $\beta = -1.94$ (SE = 0.75; $P < 0.05$) Accidents: $\beta = -10.58$ (SE = 5.48; $P < 0.10$)	State, year, state-specific trends, urbanization rate, illiteracy rate, women's suffrage, and proportion of the population that was non-White, foreign-born, female, aged 15–25 years, aged ≥65 years, and Catholic	The effect estimates were based on a prohibition coverage variable that was computed as the proportion of a state's population that lived in a dry county during each year; the effect estimates shown are for states with 100% of the population covered by statewide prohibition laws compared with states with 0% of the population covered by either statewide or countrywide prohibition laws In sensitivity analyses with area-level data from 1910–1920 for 12 states, inverse associations of prohibition with mortality rates from liver cirrhosis ($\beta = -4.49$; SE = 1.94; $P < 0.01$) and accidents ($\beta = -2.26$; SE = 4.29; $P > 0.10$) were observed In a sensitivity analysis of prohibitions in relation to non-alcohol-related mortality rates (negative control outcome), no significant associations were observed ($P > 0.10$)

Table 3.8 (continued)

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Total bans in South Africa					
Chu et al. (2022) Cape Winelands–Overberg region of the Western Cape Province, South Africa General surgery trauma-related patients at Worcester Regional Hospital 1 January 2019–23 December 2020	Interrupted time series Retrospective analysis of secondary data from the hospital electronic database Poisson regression	COVID-19 pandemic total alcohol sales ban during 27 March–31 May 2020 (total ban period 1) and 13 July–17 August 2020 (total ban period 2) No control or comparison site	IRR (95% CI) of trauma admissions per 100 days for the non-ban period (1 January–26 March 2020) compared with the total ban periods 2.41 (1.78 to 3.24)		Other COVID-19 pandemic restrictions also started on the same day as the first total ban on alcohol sales Compared with 1 January–26 March 2020, trauma admission rates decreased by 59% during the first total alcohol sales ban period Compared with 2019, trauma admission rates in 2020 were lower during each of the total ban periods (IRR ≤ 0.49; $P < 0.001$)
Barron et al. (2024) South Africa Total population January 2017–December 2020	Interrupted time series National daily mortality data for unnatural deaths, which included deaths precipitated by unintentional injuries, interpersonal violence, and suicide Ordinary least-squares regression	Implementation of a 5-week national ban on the sale and transportation of alcohol and of a curfew (initially 21:00–04:00, then 22:00–04:00) from 13 July to 17 August 2020 during the COVID-19 pandemic No control or comparison site	Difference in unnatural deaths per day (number) between the 5-week ban period (13 July–17 August 2020) and the non-ban periods during 2017–2020 Both men and women: $\beta = -21.99$ (95% CI, −32.58 to −11.39) Men: $\beta = -21.43$ (95% CI, −30.74 to −12.13) Women: $\beta = -0.55$ (95% CI, −3.35 to 2.25)	Presence of other COVID-19 pandemic restrictions; weekly, monthly, seasonal, and yearly time trends in mortality	In addition to some of the proxy outcomes defined in Section 3.1.3, this study also included suicide, which was the least common unnatural death included in the analysis In a sensitivity analysis using police records from 2020, the ban was associated with a decrease in interpersonal violence (homicide, assaults, and rape)

Table 3.8 (continued)

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Other total bans					
Manthey et al. (2019) All WHO Member States Adult population aged ≥ 15 years 1990–2016	Time series Alcohol consumption data from the WHO Global Information System on Alcohol and Health Descriptive analysis	The WHO Eastern Mediterranean Region (includes North Africa and the Middle East) has a high proportion of countries with total bans on alcohol sales (WHO, 2024c) Comparison: global data	APC during 1990–2016 WHO Eastern Mediterranean Region: < 1 L Globally: > 5 L Prevalence of lifetime abstinence (per year) during 1990–2016 WHO Eastern Mediterranean Region: > 90% Globally: < 50%		The study did not present country-specific estimates
Chrystoja et al. (2020) Alberta, British Columbia, Manitoba, New Brunswick, Nova Scotia, Ontario, and Saskatchewan, Canada Total population 1901–1956	Interrupted time series Liver cirrhosis mortality rate data were from provincial and national vital statistics sources ARIMA and fixed-effects meta-analysis	Introduction of total alcohol sales bans in 7 provinces in Canada, in 1916 (Alberta, Manitoba, Nova Scotia, and Ontario) and 1917 (British Columbia, New Brunswick, and Saskatchewan); the duration of the bans ranged between 4 years and 14 years No control or comparison province	Difference (95% CI) in annual liver cirrhosis deaths per 100 000 people between the ban periods and the non-ban periods during 1901–1956 Overall: −0.39 (−0.72 to −0.06) Alberta: −0.14 (−1.39 to 1.10) Manitoba: −0.82 (−2.99 to 1.35) Nova Scotia: −0.91 (−1.48 to −0.33) Ontario: −0.09 (−0.73 to 0.92) British Columbia: −0.90 (−4.68 to 2.88) New Brunswick: −0.14 (−0.72 to 0.43) Saskatchewan: 0.09 (−1.08 to 1.26)		Other provinces in Canada (Newfoundland, Prince Edward Island, and Quebec) were not included because of very short or partial alcohol sales bans or missing mortality data Liver cirrhosis mortality rate data were available only from 1921 for Alberta, Manitoba, New Brunswick, and Saskatchewan In sensitivity analyses, the overall differences in liver cirrhosis mortality rates were similar after a 5-year lag (−0.40; 95% CI, −0.77 to −0.03) and a 10-year lag (−0.44; 95% CI, −0.79 to −0.09), but when a 20-year lag period was used, the overall difference was attenuated (−0.11; 95% CI, −0.44 to 0.22)

Table 3.8 (continued)

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Maphisa Maphisa and Ndlovu (2023) Botswana 1326 adults 21 October–7 November 2021	Cross-sectional survey Online survey of residents of Botswana including a modified AUDIT-C; participants retrospectively recalled their alcohol use before, during, and after the alcohol sales ban McNemar test	70-day alcohol sales ban during the COVID-19 pandemic restrictions in Botswana from 28 June to 5 September 2021 No control or comparison group	Prevalence of recalled hazardous drinking (AUDIT-C score of 3 for women or 4 for men) Before the ban (before 28 June 2021): 52.6% (95% CI, 49.9% to 55.3%) During the ban (28 June 2021–5 September 2021): 33.9% (95% CI, 31.4% to 36.5%) After the ban (after 5 September 2021): 43.1% (95% CI, 40.4% to 45.8%) (*P* for difference among time points < 0.001)		Convenience sample of participants recruited from a social media platform
Chakrabarti et al. (2024) India 10 733 men and 88 188 women in Bihar; 38 674 men and 284 820 women in neighbouring states Survey periods: 2005–2006, 2012, 2013, 2015–2016, and 2019–2021	Repeated cross-sectional survey Nationally representative household surveys before the policy intervention (National Family Health Surveys in 2005–2006 and 2015–2016, Annual Health Survey in 2013, and District Level Household Survey in 2012) and after the policy intervention (National Family Health Survey in 2019–2021) Ordinary least-squares regression (DiD)	Statewide ban on the manufacture, transportation, sale, and consumption of alcohol in Bihar in April 2016; the ban was enforced with strict imposition of penalties and punishments Comparison: neighbouring states of Bihar	Difference in the change in the prevalence of at least weekly alcohol consumption (percentage points) between men in Bihar and men in neighbouring states from 2015–2016 to 2019–2021 –7.1 (95% CI, –9.6 to –4.6)	Age, urban residence, education level, number of cigarettes or bidis smoked, health insurance, family size, religion, caste, below poverty line card, and household wealth characteristics; regressions were estimated with sampling weights to provide representative estimates	Results are shown here only for men because < 1% of the female population in Bihar and neighbouring states consumed alcohol before the alcohol ban Social desirability bias from self-reporting can be ruled out because of consistency with other survey outcomes that are proxies for alcohol consumption (e.g. violence) Among men in Bihar, the ban resulted in a decrease in the prevalence of weekly alcohol consumption, from 15.0% in 2015–2016 to 7.8% in 2020–2021

APC, total adult alcohol per capita consumption; ARIMA, autoregressive integrated moving average; AUDIT-C, Alcohol Use Disorders Identification Test-Consumption; CI, confidence interval; DiD, difference in difference; IRR, incidence rate ratio; N/A, not applicable; NR, not reported; SE, standard error; WHO, World Health Organization.

trade of alcohol and corruption decreased the population's support for the Prohibition, and the 18th Amendment was repealed by the 21st Amendment in December 1933 (Miron and Zwiebel, 1991).

Miron and Zwiebel (1991) assessed the effect of the United States National Prohibition on the change in alcohol consumption from 1900 through 1950. Alcohol consumption was estimated by modelling statistics for deaths from liver cirrhosis and from alcoholism, per capita admissions for alcohol psychosis, and the rate of arrests for drunkenness. After implementation of the National Prohibition in 1920, alcohol consumption decreased to 30% of pre-Prohibition levels. Subsequently, alcohol consumption gradually increased during Prohibition and plateaued by the end of Prohibition to 60–70% of pre-Prohibition levels. In the decade after Prohibition, alcohol consumption returned to pre-Prohibition levels. [The strength of this study is the inclusion of 51 years of annual data covering multiple years before and after the alcohol ban. The limitations of the study are the lack of a geographical control, the descriptive analysis, and the estimation of alcohol consumption based on proxy outcomes.]

Dills and Miron (2004) used state-level data from 1900–1997 to assess the effect of state-level alcohol prohibitions enacted in 33 states before 1920 and the 1920–1933 National Prohibition on liver cirrhosis mortality rates. The covariate-adjusted difference in liver cirrhosis mortality rates (number of deaths per 100 000 people per year) between states with alcohol prohibition laws and states without alcohol prohibition laws during 1900–1997 was −0.09 (95% CI, [−0.23 to 0.05]). The difference in liver cirrhosis mortality rates between the National Prohibition period (1920–1933) and non-National Prohibition periods (1900–1916, 1934–1997) was −0.44 (95% CI, [−0.66 to −0.21]). [The strengths of this study are the inclusion of 98 years of annual data covering multiple years before and after the National Prohibition, the assessment of both state alcohol prohibitions and the National Prohibition, and the adjustment for liver cirrhosis mortality rates in the United Kingdom, income, and other control variables in the models. The limitations of the study are the assessment of state alcohol prohibition enactment rather than implementation, which may have underestimated the effect of state alcohol prohibition, and that the analysis included only proxy outcomes for alcohol consumption.]

Law and Marks (2020) assessed the effects of alcohol prohibition laws introduced in states or counties in the USA by 1920 on cause-specific mortality rates (number of deaths per 100 000 people per year) during 1900–1920 in 35 states. The effect of state-level alcohol prohibition laws was estimated using a prohibition coverage variable that was computed as the proportion of a state's population that lived in a dry county during each year. The difference in annual cause-specific mortality rates between states with statewide prohibition laws and states with no statewide or countywide prohibition laws was −2.83 (95% CI, [−5.04 to −0.63]) for alcoholism, −1.25 (95% CI, [−1.91 to −0.59]) for liver cirrhosis, −1.94 (95% CI, [−3.41 to −0.47]) for homicides, and −10.58 (95% CI, [−21.32 to 0.16]) for accidents. [The strengths of this study are the computation of a time-varying measure of state-level alcohol prohibition enforcement and the presence of dry counties, the adjustment for time trends and other potential confounders in the models, and the sensitivity analysis that assessed the effects of alcohol prohibition in urban areas that were wet and non-urban areas within the same state on mortality from liver cirrhosis and from accidents. The limitations of the study are the assumption that local, state, and national alcohol prohibition laws have the same effect, and that the analysis included only proxy outcomes for alcohol consumption.]

(ii) Total bans in South Africa

In South Africa, multiple waves of alcohol restrictions in response to the COVID-19 pandemic were introduced starting on 19 March 2020 (Barron et al., 2024). On 27 March 2020, the government declared a national restriction banning the sale and transportation of alcohol. On 1 June 2020, off-premises and online alcohol sales from Monday to Thursday between 09:00 and 17:00 were reintroduced. On 13 July, the government abruptly reintroduced a second ban on the sale and transportation of alcohol, along with a curfew from 21:00 to 04:00. On 18 August, off-premises and on-premises alcohol sales were permitted again; however, the curfew and other COVID-19 pandemic restrictions were maintained.

Chu et al. (2022) assessed the association between the 2020 total bans on alcohol sales in South Africa during the COVID-19 pandemic (27 March–31 May and 13 July–17 August) and trauma admission rates in one regional hospital. Overall, trauma admission rates were higher during the non-ban period in 2020 (1 January–26 March) than during the total ban periods (incidence rate ratio, 2.41; 95% CI, 1.78 to 3.24). [The strength of this study is the assessment of two nationwide total bans on alcohol sales in South Africa that occurred in the same year. The limitations of the study are the lack of a geographical control, possible confounding because a national lockdown started on the same day as the first total ban on alcohol sales, the small sample size, and the assessment of only proxy outcomes for alcohol consumption.]

Barron et al. (2024) assessed the effects of the 5-week alcohol sales ban period from 13 July to 17 August 2020 on unnatural mortality (deaths per day) using data for 2017–2020. Among men and women combined, the ban on alcohol sales reduced unnatural mortality by 21.99 deaths per day (95% CI, 11.39 to 32.58) during the 5-week alcohol sales ban period compared with the non-ban periods during 2017–2020. The reduction in the number of unnatural deaths associated with the ban was –21.43 ($P < 0.01$) deaths per day among men and –0.55 ($P \geq 0.10$) deaths per day among women. [The strength of this study is the inclusion of control variables for some other COVID-19 pandemic restrictions and for temporal and seasonal changes in the multivariable adjusted model. The limitations of the study are the lack of a geographical control, possible confounding due to simultaneous introduction of the alcohol ban and a curfew (from 21:00 to 04:00) (although the numbers of unnatural deaths increased after the ban was lifted and the curfew remained), and the assessment of only proxy outcomes for alcohol consumption.]

(iii) Other total bans

Manthey et al. (2019) assessed the changes in total alcohol consumption per capita per year from 1990 through 2015 or 2016 in the WHO Eastern Mediterranean Region, where some countries have alcohol prohibitions, compared with the global average. The alcohol consumption (APC) in the WHO Eastern Mediterranean Region was stable at < 1 L, and the global total alcohol per capita consumption was stable at > 5 L. The prevalence of lifetime abstinence was > 90% in the WHO Eastern Mediterranean Region and < 50% globally. [The strength of this study is the inclusion of 26 years of alcohol consumption data from all world regions. The limitations of the study are that it did not present country-specific estimates, and that the results were descriptive.]

Chrystoja et al. (2020) assessed the effect of total bans on alcohol sales in seven provinces in Canada that banned alcohol sales in 1916 or 1917 for 4–14 years on the liver cirrhosis mortality rates (deaths per 100 000 people per year) from 1901 to 1956. Liver cirrhosis mortality rates were lower during the ban periods compared with the non-ban periods in six of the seven provinces and in a meta-analysis of all seven provinces

(–0.39; 95% CI, –0.72 to –0.06). [The strengths of this study are the assessment of alcohol sales bans that occurred in seven provinces in Canada, the inclusion of up to 56 years of annual data covering multiple years before and after the ban for some of the provinces, and the use of ARIMA models to account for underlying trends and autocorrelation. The limitations of the study are the lack of a geographical control; the potential confounding by changes in the age composition of the population, tobacco use, obesity, coding practices, and sources of information; and the assessment of only proxy outcomes for alcohol consumption.]

Maphisa Maphisa and Ndlovu (2023) assessed the effects of a 70-day (28 June to 5 September 2021) alcohol sales ban during the COVID-19 pandemic restrictions in Botswana on self-reported, recalled hazardous drinking (defined as an AUDIT-Consumption [AUDIT-C] score of 3 for women or 4 for men) before, during, and after the alcohol sales ban. The prevalence of hazardous drinking was 52.6% (95% CI, 49.9% to 55.3%) before the ban, 33.9% (95% CI, 31.4% to 36.5%) during the ban, and 43.1% (95% CI, 40.4% to 45.8%) after the ban ($P < 0.001$ for difference in prevalence estimates among time points). [The strength of this study is the use of AUDIT-C, which is a recognized instrument for recording patterns of alcohol consumption. The limitations of the study are the lack of a control or comparison group, that the study used a convenience sample of participants recruited from a social media platform, that temporal patterns were inferred from a questionnaire administered at a single time point with current and recalled alcohol consumption, and that hazardous drinking was the only outcome measure of alcohol consumption.]

Chakrabarti et al. (2024) assessed the effect of the 2016 ban on the manufacture, transportation, sale, and consumption of alcohol in Bihar, India, on the prevalence of alcohol consumption (at least once per week) from repeated cross-sectional, nationally representative household surveys: the National Family Health Surveys (in 2005–2006, 2015–2016, and 2019–2021), Annual Health Survey (in 2013), and District Level Household Survey (in 2012). The alcohol ban resulted in a decrease in the prevalence of alcohol consumption among men in Bihar, from 15.0% in 2015–2016 to 7.8% in 2020–2021, but not in neighbouring states without an alcohol ban (adjusted difference in the change in the prevalence of alcohol consumption, –7.1 percentage points; 95% CI, –9.6 to –4.6 percentage points). [The strengths of this study are the use of data from large nationally representative repeated cross-sectional surveys, the inclusion of controls from the neighbouring states, and the adjustment for income and other control variables in the models. The limitations of the study are that social desirability bias may have led to an overestimation of the effects of the alcohol ban in Bihar, although the consistency with other proxy outcomes for alcohol consumption (e.g. violence) makes this less of a concern, and that only prevalence of alcohol consumption a least weekly was assessed.]

(b) Partial bans

The effects of a short-term partial ban on on-premises alcohol sales on alcoholic beverage consumption in the United Kingdom during the COVID-19 pandemic were assessed in one study with household-level data (Anderson et al., 2022) and one study with individual-level data (Hardie et al., 2022) (Table 3.9). In the United Kingdom, COVID-19 pandemic restrictions were introduced on 21 March 2020, confinement began on 23 March 2020, and all interventions were implemented by 26 March 2020. The restrictions included the closing of on-premises alcohol outlets (i.e. on-premises alcohol sales bans) on 21 March 2020, which remained fully closed until 4 July 2020 in England, 13 July 2020 in Wales, and 15 July 2020 in Scotland (although in Scotland, pubs and cafes were allowed to reopen

Table 3.9 Effects of partial bans on alcoholic beverage consumption – studies with household-level or individual-level data

Reference Location Study population Study period	Study type Data sources Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Anderson et al. (2022) United Kingdom 79 417 households 2015–2020	Repeated cross-sectional survey Off-premises sales from Kantar FMCG purchase panel data (barcode scanning of purchases at home); on-premises sales from the Scottish government website on Monitoring and Evaluating Scotland's Alcohol Strategy Linear regression	COVID-19 restrictions introduced on-premises alcohol sales bans from 21 March until 4 July 2020 (England), 13 July 2020 (Wales), or 15 July 2020 (Scotland) No control or comparison group	Difference in the percentage change in alcohol purchases per adult per household per day (grams of ethanol) between 23 March–15 July and 1 January–22 March from 2015–2019 to 2020 −0.90% (95% CI, −5.58% to 3.77%)	Day of the calendar year	The difference in the percentage change in off-premises alcohol purchases between 23 March–15 July and 1 January–22 March from 2015–2019 to 2020 was 37.5% (95% CI, 33.9% to 41.2%) [Anderson disclosed previous financial support from the alcohol industry]
Hardie et al. (2022) United Kingdom 253 148 adults in England and 41 507 adults in Scotland 2009–2020	Repeated cross-sectional survey On-premises and off-premises alcohol consumption from Kantar Alcovision survey and retrospective 1-week alcohol consumption diary data, which included self-reporting of occasions of alcohol consumption SARMA	COVID-19 restrictions introduced on-premises alcohol sales bans from 21 March until 4 July 2020 (England) or 6 July 2020 (outdoor seating, Scotland) or 15 July 2020 (indoor seating, Scotland) No control or comparison group	Difference in alcohol consumption per adult per week (number of units) between the ban period (March–June 2020) and the non-ban periods during 2009–2020 England: −0.95 (95% CI, −3.34 to 1.43) Scotland: −0.84 (95% CI, −6.76 to 5.09)	Models account for autocorrelation, seasonality, and trend	The on-premises alcohol sales partial ban coincided with other COVID-19 pandemic restrictions Participants could report up to 2 on-premises and 2 off-premises occasions per day Weekly units of off-premises alcohol consumption increased but on-premises consumption decreased both in England (off-premises: 1.18; 95% CI, 0.65 to 1.70; on-premises: −2.53; 95% CI, −2.86 to −2.20) and in Scotland (off-premises: 2.32; 95% CI, 0.61 to 4.02; on-premises: −2.84; 95% CI, −3.63 to −2.06) 1 unit = 8 g of ethanol

CI, confidence interval; FMCG, fast-moving consumer goods; SARMA, seasonal autoregressive moving average.

for outdoor seating with physical distancing on 6 July 2020).

In a longitudinal panel study (Anderson et al., 2022), the effects of the on-premises alcohol sales partial ban due to the COVID-19 pandemic restrictions on alcohol purchases (grams of ethanol per adult per household per day) in England, Scotland, and Wales were assessed. Alcohol purchases were estimated using off-premises alcohol purchases from Kantar fast-moving consumer goods (FMCG) panel data (which are derived from scanning the barcodes of all household purchases from selected retailers) and data on on-premises alcohol purchases from the Scottish government. The difference in the percentage change in alcohol purchases between 23 March–15 July (partial ban period) and 1 January–22 March from 2015–2019 to 2020 was similar (−0.90%; 95% CI, −5.58% to 3.77%). [Anderson disclosed previous financial support from the alcohol industry. The strength of this study is the inclusion of daily off-premises alcohol purchasing data and on-premises alcohol sales data covering multiple years before the intervention. The limitations of the study are the lack of a geographical control, the sample of participants from a household shopping panel used for commercial purposes, potential confounding by other COVID-19 pandemic restrictions, and the assessment of alcohol consumption based on household purchases (for off-premises alcohol consumption) and recorded alcohol sales data (for on-premises alcohol consumption) only.]

In the other study in the United Kingdom (Hardie et al., 2022), Kantar Alcovision repeated cross-sectional survey data (which include a retrospective 1-week alcohol consumption diary) were used to assess differences in total alcohol consumption (number of units per week) between the partial ban period (March–June 2020) and non-ban periods during January 2009–December 2020. There was no difference in total alcohol consumption between the partial ban period and the non-ban periods both in England (−0.95 units per week 95% CI, −3.34 to 1.43) and in Scotland (−0.84 units per week; 95% CI, −6.76 to 5.09). However, in analyses of off-premises and on-premises alcohol consumption, there were small, non-statistically significant increases in off-premises alcohol consumption both in England (1.18 units per week; 95% CI, 0.65 to 1.70) and in Scotland (2.32 units per week; 95% CI, 0.61 to 4.02), whereas on-premises alcohol consumption decreased both in England (–2.53 units per week; 95% CI, −2.86 to −2.20) and in Scotland (–2.84 units per week; 95% CI, −3.63 to −2.06). [The strength of this study is that the models accounted for underlying trends, autocorrelation, and seasonality. The limitations of the study are the lack of a geographical control, potential confounding by other COVID-19 pandemic restrictions, the sample of participants recruited for a market research panel, and that the indicator variable for the policy intervention period was for March–June 2020 but the intervention started on 21 March and ended in July 2020.]

3.2.5 Multiple alcohol availability policy interventions

The effects of coordinated multiple alcohol availability policy interventions that were introduced simultaneously at the national or subnational level on alcoholic beverage consumption were assessed in two studies with population-level data in Australia (Gray et al., 2000) and Canada (Clay et al., 2023) and one study with individual-level data in New Zealand (Huckle et al., 2011). The effects of other multiple alcohol availability policy interventions on alcohol consumption were assessed in one study with population-level data in 10 European countries (Baccini and Carreras, 2014) and one study with individual-level data from adolescents in 33 countries or regions in Europe and North America (Leal-López et al., 2020) (Table 3.10).

Table 3.10 Effects of multiple alcohol availability restrictions on alcoholic beverage consumption

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Coordinated multiple alcohol availability policy intervention					
Gray et al. (2000) Northern Territory, Australia Adult population aged ≥ 15 years July 1994–June 1997	Time series Quarterly data on purchases of alcoholic beverages by outlet type from the Liquor Commission Descriptive analysis	A trial of increased restrictions between August 1995 and February 1996 and restrictions fully implemented in March 1996: off-premises alcohol sales prohibited on Thursdays (the day of social welfare payments); sales of wine in casks of > 2 L in volume prohibited; sales of wine in casks of ≤ 2 L in volume restricted to 1 transaction per person per day; no wine to be sold in glass containers of > 1 L in volume; no third-party sales to taxi drivers; front bars in pubs to be closed on Thursdays; lounge bars not to open before 12:00 on Thursdays and Fridays; lounge bars to make food available; off-premises sales limited to between 12:00 and 21:00 on weekdays other than Thursdays; in front bars, wine to be sold only with substantial meals; in front bars, light beer to be the only alcoholic beverage sold between 10:00 and 12:00; and off-premises sales of fortified wines restricted to containers of ≤ 1.125 L in volume Comparison: all of the Northern Territory	Recorded alcohol consumption per capita per 12-month period (litres of ethanol) **Tennant Creek:** Q3 1994–Q2 1995: 25.3 (reference) Q3 1995–Q2 1996: 21.8 ([–13.8%]) Q3 1996–Q2 1997: 20.4 (–19.4%) **All of the Northern Territory:** Q3 1994–Q2 1995: 15.0 (reference) Q3 1995–Q2 1996: 14.3 ([–4.6%]) Q3 1996–Q2 1997: 14.8 ([–1.3%])		Q1 = January–March; Q2 = April–June; Q3 = July–September; Q4 = October–December

Table 3.10 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Huckle et al. (2011) New Zealand 16 546 individuals aged ≥ 12 years (this study was limited to individuals aged 14–65 years) Survey years: 1995 (wave 1), 2000 (wave 2), and 2004 (wave 3)	Repeated cross-sectional survey 3 nationally representative household surveys; the response rate was 76% in wave 1, 73% in wave 2, and 59% in wave 3 General linear model	Alcohol availability policy interventions in 1995 that introduced spirits-based mixed drinks (ready-to-drink alcoholic beverages, or alcopops) and in December 1999 that decreased the minimum legal purchase age from 20 years to 18 years, permitted sales of all types of alcohol on Sundays, and permitted sales of beer in supermarkets and grocery stores No control or comparison group	Prevalence of alcohol consumption in the past 12 months in 1995 and 2000 **Men:** 14–15 years: 69.0% and 72.2% ($P > 0.05$) 16–17 years: 89.1% and 83.5% ($P > 0.05$) 18–19 years: 89.9% and 91.7% ($P > 0.05$) 20–24 years: 94.6% and 94.4% ($P > 0.05$) 25–29 years: 91.6% and 89.5% ($P > 0.05$) 30–39 years: 90.7% and 89.3% ($P > 0.05$) 40–49 years: 88.8% and 87.4% ($P > 0.05$) 50–65 years: 85.7% and 88.8% ($P > 0.05$) **Women:** 14–15 years: 55.8% and 69.2% ([$P = 0.0128$]) 16–17 years: 89.9% and 83.8% ($P > 0.05$) 18–19 years: 78.5% and 89.1% ($P = 0.0064$) 20–24 years: 90.2% and 87% ($P > 0.05$) 25–29 years: 85.6% and 82.1% ($P > 0.05$) 30–39 years: 87.5% and 85.4% ($P > 0.05$) 40–49 years: 87.8% and 81.9% ($P = 0.0121$) 50–65 years: 81.5% and 81.0% ($P > 0.05$)		The 2000 survey included 2 new locations of alcohol consumption The study included estimates for 2004, but these are not shown here because they are likely to be confounded by the introduction of the tax on light spirits in 2003 [The authors of the published report clarified that the *P* value for the prevalence of alcohol consumption in 2000 among women aged 14–15 years was erroneously shown as $P = 0.128$]

Table 3.10 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Clay et al. (2023) British Columbia, Canada Adult population aged ≥ 15 years April 2017–April 2021	Panel Alcohol consumption was calculated based on alcohol sales data in 89 local health areas from the British Columbia Ministry of Public Safety and Solicitor General Liquor Distribution Branch Multilevel regression	Multiple alcohol sales restrictions implemented beginning in March 2020 in British Columbia, Canada, in response to the COVID-19 pandemic; RAPI (range, 0–100) is a continuous measure of 4 domains of restrictions: the number of hours of sales (on-premises and off-premises outlets), the number of people permitted on site (on-premises outlets only), outlet density (on-premises and off-premises outlets), and the extent of permissible alcohol takeaway or home delivery (on-premises and off-premises outlets) No control or comparison group	Percentage difference in recorded alcohol consumption per capita per week (standard drinks) for a 10-point increase in RAPI Total: −0.12% (95% CI, −0.15% to −0.09%) Off-premises: −0.12% (95% CI, −0.13% to 0.09%) On-premises: −0.43% (95% CI, −0.43% to −0.42%) Percentage difference in alcohol consumption per capita per week (standard drinks) for the most-restrictive policies (RAPI = 100) compared with the least-restrictive policies (RAPI = 0) Off-premises: −9% On-premises: −100%	Area-level Canadian Index of Multiple Deprivation, rate of new COVID-19 infections, pre-COVID-19 trend, seasonality, and autoregressive and moving average terms	In analyses of effect modification by each area deprivation index, the inverse association between RAPI and total alcohol consumption was observed only in the intermediate-deprived and most-deprived areas, but not in the least-deprived areas, based on economic dependency and situational vulnerability, and only in the least-deprived and the intermediate-deprived areas, but not in the most-deprived areas, based on ethnocultural composition and residential instability (all *P* for interaction ≤ 0.005) 1 standard drink = 13.45 g of ethanol Weekly population data were estimated to take into account new COVID-19 infections

Table 3.10 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Other evidence					
Baccini and Carreras (2014) 10 European countries Adult population aged ≥ 15 years 1961–2008 (see Allamani et al., 2014)	Time series Alcohol control policies and recorded alcohol consumption data from WHO GISAH (WHO, 2024c) Country-specific linear regression analyses using the same regression model followed by a random-effects meta-analysis	Alcohol availability policy restrictive and permissive scores are continuous variables based on the number of licensing policies and hours of alcohol sales policies A restrictive score is based on year-specific policies in Hungary (1977), Italy (1991 and 1998), the Netherlands (1991 and 2001), Poland (1982), Spain (1989 and 1990), Sweden (1977 and 1982), Switzerland (1980 and 1999), and the United Kingdom (1997 and 2001) A permissive score is based on year-specific policies in Finland (1969 and 1995), Hungary (1990), the Netherlands (1967), Norway (1973 and 1998), Poland (1984, 1990, 1993, and 1996), Sweden (1965, 2001, and 2004), Switzerland (1968), and the United Kingdom (2002) No control or comparison countries	Percentage change in recorded alcohol consumption per capita per year (litres of ethanol) associated with a 1-unit increase in the score Restrictive scores: −3.9% (90% CI, −8.8% to 1.1%) Permissive scores: 0.9% (90% CI, −4.6% to 6.3%)	A linear time trend variable, logarithm of income, percentage of men aged > 65 years, urbanization level, logarithm of price of the 2 main types of alcoholic beverages consumed during the study period, restrictive advertising policies, change in the minimum legal purchase age, and change in the BAC limit for driving	

Table 3.10 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups (if applicable)	Outcome Effect estimates	Covariates	Comments
Leal-López et al. (2020) 33 countries or regions in Europe and North America 671 084 adolescents aged 11, 13, and 15 years Survey years: 2001/2002, 2005/2006, 2009/2010, and 2013/2014	Repeated cross-sectional survey Alcohol consumption data from the Health Behaviour in School-aged Children study Multilevel regression	Alcohol availability index for the stringency of physical availability policies (range, 0–23.80) based on MLDA (no or yes), government monopoly (no, partial, or full), outlet density restriction (no, wine only, wine and spirits, or all alcoholic beverages), and sale time restrictions (none, on days or hours, or on days and hours); MLDA had higher weight in the overall index than the other policies based on the evidence of effectiveness in previous research; years of implementation varied by country and region	Difference in the prevalence per 1-unit increase in the availability index Lifetime alcohol consumption: $\beta = -0.02$ (SE = 0.011; $P > 0.05$) At least weekly alcohol consumption: $\beta = -0.03$ (SE = 0.011; $P < 0.01$)	Age, sex, time, advertising restrictions, and random effects for country, country–year, and school	Lifetime alcohol consumption was defined as consumption on at least 1 day over the lifespan

BAC, blood alcohol concentration; CI, confidence interval; GISAH, Global Information System on Alcohol and Health; MLDA, minimum legal drinking age; Q, quarter; RAPI, Restrictiveness of Alcohol Policy Index; SE, standard error; WHO, World Health Organization.

(a) Coordinated multiple alcohol availability policy interventions

Gray et al. (2000) assessed the effect of the implementation of alcohol availability restrictions in March 1996 (after a trial of increased alcohol availability restrictions implemented in August 1995 suggested efficacy in improving police incidents, public order, health, and welfare) in Tennant Creek, Northern Territory, Australia, on recorded alcohol consumption (litres of ethanol per capita per 12-month period) from July 1994–June 1995 (pre-intervention period) to July 1996–June 1997. These restrictions included, but were not limited to: prohibitions of off-premises and front bar alcohol sales on Thursdays (the day of social welfare payments); the volume of wine in casks and glass containers sold; the number of wine cask transactions per person per day; third-party sales to taxi drivers; and on-premises and off-premises hours of alcohol sales (see Table 3.10 for details). Compared with alcohol consumption in Tennant Creek before the introduction of alcohol availability restrictions, there was a [13.8%] reduction in alcohol consumption during the subsequent 12 months and a 19.4% reduction in alcohol consumption during the 24 months after the introduction of the restrictions. In contrast, over the same periods, alcohol consumption in the entire Northern Territory remained relatively stable. [The strength of this study is the comparison with the general trend in the Northern Territory. The limitations of the study are that the results are descriptive and that alcohol consumption was based on recorded alcohol sales data only.]

Huckle et al. (2011) analysed three nationally representative surveys (in 1995, 2000, and 2004) in New Zealand to assess the combined effect of the interventions in 1999 that decreased the minimum legal purchase age from 20 years to 18 years, permitted sales of all types of alcohol on Sundays, and permitted sales of beer in supermarkets and grocery stores on the prevalence of alcoholic beverage consumption within the past 12 months in each survey year. [Because the effect of the multiple alcohol availability interventions in 1999 on the prevalence of alcohol consumption in 2004 is likely to be biased due to the introduction of the tax on light spirits in 2003, only the prevalence of alcohol consumption in 1995 and 2000 is described here.] In age-stratified analyses, among men there was no statistically significant difference in the prevalence of alcohol consumption between 1995 and 2000 in any age strata. Among women, the prevalence of alcohol consumption was higher in 2000 than in 1995 in the 14–15 years (55.8% to 69.2%; [P = 0.0128]) and 18–19 years (78.5% to 89.1%; P = 0.0064) age strata, and was lower in 2000 than in 1995 in the 40–49 years age stratum (87.8% to 81.9%; P = 0.0121). [The strength of this study is the probability sampling of respondents to the survey. The limitation of the study is the lack of a geographical control.]

Clay et al. (2023) assessed the effect of a Restrictiveness of Alcohol Policy Index (RAPI) based on multiple alcohol sales restrictions implemented beginning in March 2020 in British Columbia, Canada, in response to the COVID-19 pandemic on alcohol consumption (standard drinks per capita per week) from April 2017 to April 2021. RAPI (range, 0–100) is a continuous measure of the alcohol sales restrictions that includes four domains: the number of hours of sales (on-premises and off-premises outlets), the number of people permitted on site (on-premises outlets only), outlet density (on-premises and off-premises outlets), and the extent of permissible alcohol takeaway or home delivery (on-premises and off-premises outlets). Overall, compared with the least-restrictive policies (RAPI = 0), the most-restrictive policies (RAPI = 100) were associated with 9% lower off-premises alcohol consumption and 100% lower on-premises alcohol consumption (the latter because of total closure of alcohol outlets). Effect

modification of the association between RAPI and alcohol consumption was assessed by four measures of area-level deprivation: economic dependency, situational vulnerability, ethnocultural composition, and residential instability (all *P* for interaction ≤ 0.005). [The strengths of this study are the inclusion of a combined area deprivation index and other control variables in the model and stratified analyses by individual area deprivation indexes. The limitation of the study is that alcohol consumption was based on recorded alcohol sales data only.]

(b) Other evidence

Baccini and Carreras (2014) (see also Section 3.2.3) assessed the effect of multiple alcohol availability policy interventions on the percentage change in recorded alcohol consumption (litres of ethanol per capita per year) in 10 European countries from 1961 through 2008. During this period, restrictive alcohol availability interventions on licensing and hours of alcohol sales were implemented in eight countries and permissive alcohol availability interventions on licensing and hours of alcohol sales were implemented in eight countries; in some countries, both periods of restrictive policies and periods of permissive policies were implemented. In a meta-analysis of the country-specific estimates of association, the percentage change in recorded alcohol consumption was −3.9% (90% CI, −8.8% to 1.1%) for restrictive policies and 0.9% (90% CI, −4.6% to 6.3%) for permissive policies. [The strengths and limitations of this study are described in Section 3.2.3. In addition, a limitation of the analysis of multiple alcohol availability policy interventions is the lack of detail provided on the interventions on licensing and hours of alcohol sales.]

Leal-López et al. (2020) (see also Sections 4.3.2 and 5.4.1) assessed the associations of alcohol policy interventions with the prevalence of lifetime alcohol consumption (defined as consumption on at least 1 day over the lifespan) and the prevalence of at least weekly alcohol consumption among adolescents using data from repeated cross-sectional surveys (in 2001/2002, 2005/2006, 2009/2010, and 2013/2014) collected from adolescents aged 11 years, 13 years, and 15 years in 33 countries or regions in Europe and North America who participated in the Health Behaviour in School-aged Children study. An alcohol availability index (range, 0–23.80) for the stringency of national alcohol availability policy interventions was calculated using information on minimum legal drinking age, presence of a government alcohol monopoly, restrictions on alcohol outlet density, and restrictions on days and hours of alcohol sales. Among the adolescents, a 1-unit increase in the availability index was not associated with the prevalence of lifetime alcohol consumption (–2%; $P > 0.05$) but was associated with a lower prevalence of at least weekly alcohol consumption (–3%; $P < 0.01$). [The strengths of this study are the inclusion of a large representative sample of adolescents in 33 countries or regions in Europe and North America, the 12-year period of data collection, the inclusion of a comprehensive set of control variables for alcohol policy interventions and affordability in the model, and the weighting of indexes for several policy areas to better assess their effect on alcohol consumption compared with counts. The limitation of the study is that the prevalence for the category of lifetime alcohol consumption included a wide range from 1–2 days to ≥ 30 days of alcohol consumption over the lifespan.]

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4. ALCOHOL MARKETING BANS

4.1 Definitions and general considerations

The World Health Organization (WHO) Global Alcohol Action Plan 2022–2030 (WHO, 2024a) describes proposed actions for Member States to reduce the harmful use of alcohol; Action 3 is "Implement, as appropriate in national contexts, high-impact and effective strategies and interventions, supported by legislative measures, addressing … the advertising and marketing of alcoholic beverages, through comprehensive and robust restrictions or bans across multiple types of media, including digital media".

The primary goal of this section is to assess the evidence on the effects of alcohol marketing bans on alcoholic beverage consumption (see Section 4.3). However, if there is a causal pathway, it is also important to understand the influence of alcohol marketing itself on consumption (see Section 4.2).

4.1.1 Defining alcohol marketing

Alcohol marketing was previously defined in the WHO Global Strategy to Reduce the Harmful Use of Alcohol as "any form of commercial communication or message that is designed to increase, or has the effect of increasing, the recognition, appeal and/or consumption of" alcohol (WHO, 2010). In a technical note providing background on alcohol marketing regulation and monitoring for the protection of public health, the Pan American Health Organization expanded the definition of alcohol marketing beyond mass media advertising, promotion, and sponsorship to also include "the design of alcohol products, brand stretching (using an established brand for a new product in another product category), co-branding (collaboration between different brands with the same advertising goals), depiction of alcohol products and brands in entertainment media, corporate social responsibility (CSR) activities undertaken by the alcohol industry, and the sale or supply of alcoholic beverages in educational and health settings. Trademarks and trade dress (label design, product configuration, and product packaging), which can serve multiple functions, are included when their goals align with those already mentioned previously" (PAHO, 2017).

Even that lengthy list is insufficient to describe current alcohol marketing, because marketers are embedding their products and messaging within a larger system of culture, consumption, and communication shaped by digital technologies and platforms (Carah and Brodmerkel, 2021). In this system, it is increasingly difficult to distinguish promotional messages from other forms of content in media channels and other environments, partly because consumers and other creators participate in

the creation and circulation of promotional messages. Marketers are more frequently integrating promotional and distribution elements of the marketing process, with advertisements via digital channels functioning as storefronts with direct links to online purchasing and delivery (Carah et al., 2023). In the current digital and programmatic marketing environments, automated models, which are trained by data accumulated from consumers as they use digital media and services, shape and optimize an individual's exposure to and engagement with promotional messages (Carah and Brodmerkel, 2021). Digital platforms continue to innovate the forms, content, and payment models used to support marketing in their ecosystems; this influences what is defined as alcohol marketing and how alcohol marketing is defined.

4.1.2 Extent and economic role of alcohol marketing

Based on the amount of money that marketers spend on it, alcohol marketing matters. High spending on alcohol marketing helps the global alcohol industry maintain its highly concentrated control by generating oligopoly profits that can, in turn, be used for more marketing spending (Jernigan and Ross, 2020). Thus, alcohol marketing and sales are concentrated in the hands of a few companies; the 10 largest spirits companies in the world sell two thirds of the alcohol in their sector, and the 10 largest beer companies sell more than half of the alcohol in their sector (Jernigan and Ross, 2020). According to *Advertising Age*, 5 transnational corporations rank among the 100 largest alcohol marketers in the world, and they spent a total of US$ 18.3 billion on advertising, direct marketing, and promotion in 2022 (Ad Age, 2023). For the United States market, as much as 61% of spending on alcohol advertising in 2023 was estimated to have been on digital media (Hardimon, 2024).

4.1.3 Alcohol marketing bans

The ubiquity of alcohol marketing, with spending spread across multiple activities, makes distinguishing between comprehensive and partial alcohol marketing bans critically important. Anything short of a comprehensive ban – defined as legislative adoption of bans on all forms of alcohol marketing for all types of alcoholic beverages – would probably result in expansion of marketing within and across sectors not subject to a ban. For example, instituting partial bans on broadcast marketing after certain time-of-day thresholds may reduce the exposure of the youngest viewers to advertising but increase the exposure of teenagers, because the advertising shifts to later times of day when watchers and listeners are disproportionately teenagers (Ross et al., 2013). Banning outdoor advertising may shift more spending to digital media. The ability to shift resources in response to a partial marketing ban on a single type of alcoholic beverage also underscores the importance of assessing the potential effects of alcohol marketing bans on total alcohol consumption. Online marketing, which WHO has termed "essentially cross-border in nature" (WHO, 2022), transcends national boundaries and renders national-level bans challenging to implement and enforce except in the relatively rare cases where language limits cross-border communication.

Another important consideration has been the historical lack of clear guidance on the role of private industry (termed "economic operators" by WHO) (WHO, 2010) in alcohol policy. Relevant to marketing, the WHO Global Alcohol Action Plan 2022–2030 (WHO, 2024a) calls on economic operators, which include private platforms such as Facebook and Google, to refrain from a wide range of activities that could interfere with or bias national alcohol policy-making or research. Continued industry activities, such as lobbying (also known as stakeholder marketing) and CSR

activities, may limit the effectiveness of alcohol marketing bans to reduce alcohol consumption.

Systematic reviews and meta-analyses on the effects of alcohol marketing bans on alcohol consumption (Siegfried et al., 2014; Manthey et al., 2024) have considered neither the multiple purposes and effects of alcohol marketing nor the significant differences between comprehensive and partial alcohol marketing bans. Because of the differences between comprehensive and partial bans, they should be evaluated separately (Casswell, 2024). However, truly comprehensive bans on alcohol marketing are extremely rare (Esser and Jernigan, 2018), and most studies on the effects of alcohol marketing bans on alcohol consumption have not assessed comprehensive bans. Therefore, the review and evaluation of the evidence on the effects of alcohol marketing bans on alcohol consumption in Section 4.3.2(b) focuses on studies that assessed strong bans, which are defined here as bans on alcohol marketing in at least one major media type (i.e. print, broadcast, or outdoor) for all types of alcoholic beverages. The studies of a mix of strong bans and partial bans, partial bans only, or the lifting of a ban are reviewed separately.

4.2 Alcohol marketing and alcohol consumption

4.2.1 Types of alcohol marketing

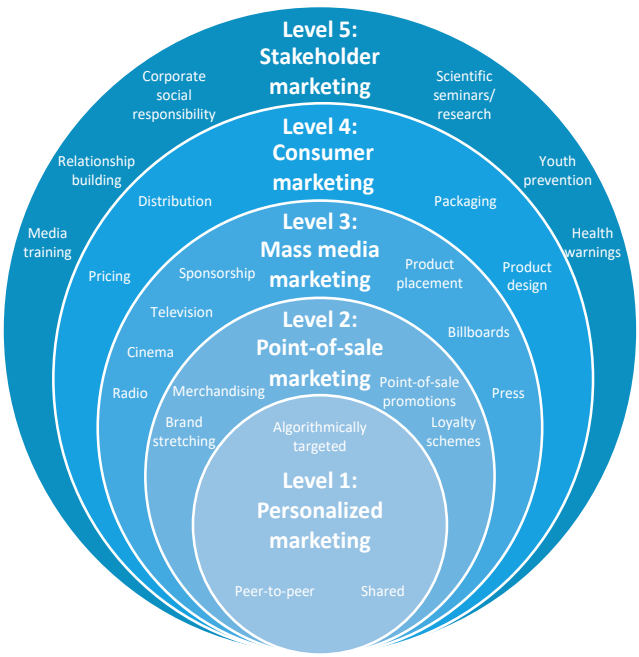
The United States National Cancer Institute developed a model of tobacco marketing (NCI, 2008) that the Working Group adapted to reflect the current landscape of alcohol marketing, which includes a greater focus on digital media. The alcohol marketing model (Fig. 4.1) illustrates the pervasive, immersive, personalized, and ubiquitous nature of alcohol marketing. The expansion of mass media to digital marketing has increased the reach, level of engagement, and technologies of alcohol marketing, which has increased cross-border marketing (Babor et al., 2023). In the alcohol marketing model, the nested nature of the types of marketing shows that, as marketing moves from Level 1 (the inner circle) to Level 5 (the outer circle) of the model, broader influence is exercised on consumers and alcohol policy.

(a) Level 1: Personalized marketing

Carah and Brodmerkel (2021) conceptualized the platform-centric perspective of digital alcohol marketing and described the ways in which digital platforms, such as YouTube, Facebook, and Instagram, create alcohol promotions and advertisements that are specific to a location, time, and context, with messages often delivered in an ephemeral (i.e. short-term) form. These platforms create detailed profiles of consumers using online data from their personal online and real-world behaviours. The profiles are used to deliver personalized marketing that is not currently available for public scrutiny. For example, Instagram Stories disappear after 24 hours (Instagram, 2024) and are not searchable. Platforms use artificial intelligence tools to decide which content will appear for which customers (Haleem et al., 2022).

The lines between paid, earned, shared, and owned media have become increasingly blurred as most, if not all, marketing is now driven by paid activities. Content may be shared by social media influencers or brand ambassadors, who are typically paid to share content, and content may be user- or company-generated. Digital platforms use data on customer engagement (i.e. how customers engage with media) to improve their communication with customers, and the content that ultimately reaches consumers is algorithmically targeted and shared among peers (Carah and Brodmerkel, 2021). In this way, digital platforms enable marketers to control and incentivize relationships of influence among consumers, who create their own extensions of the marketing content, incorporate marketing

Fig. 4.1 Model of the alcohol marketing landscape



Adapted from NCI (2008).

into their own identities, and feed information back to marketers (Zwick et al., 2008).

(b) Level 2: Point-of-sale marketing

Point-of-sale marketing is "designed to target consumers at the place where they will actually buy the product" (Howard et al., 2004). This includes point-of-sale promotions, alcohol-branded merchandise, brand stretching, and loyalty schemes (PAHO, 2017).

(c) Level 3: Mass media marketing

Mass media marketing describes media content paid for by the company that owns the product, including advertising via television, cinema, billboards, radio, press, or online ads, all of which reach mass audiences. Product, company, and brand sponsorship are included in this category. Cukier et al. (2018) described a mass media motive with regard to product placement in entertainment media; therefore, that type of marketing is included here.

(d) Level 4: Consumer marketing

Consumer marketing uses the product to create brand identities (NCI, 2008) or brand affiliations (Gabrielli et al., 2022). For example, an intriguing or eye-catching alcohol bottle design may influence a consumer's purchasing decision (Jones et al., 2022). Product design includes the formulation of the beverage itself (e.g. ingredients and flavours). Product packaging is the deliberate creation of packaging that appeals to intended audiences, as discussed in Section 4.2.1(f) on target marketing.

Consumer marketing also creates pricing strategies that, like those of the tobacco industry, place certain brands at certain price points, branding them as discount brands or higher-priced premium or super premium brands as part of that brand's identity (Chaloupka

et al., 2002; Booth et al., 2008; NCCDPHP, 2012). Connections are built between consumers and the alcohol brand (McClure et al., 2013), which could be a result of positive thoughts and feelings about the brand. These connections create brand loyalty among consumers (Maani Hessari et al., 2019). Product distribution (i.e. place of sale) increasingly plays a role in marketing (Babor et al., 2023). For example, in online sales, customers can purchase alcohol that is specifically marketed to them (Carah and Brodmerkel, 2021).

(e) Level 5: Stakeholder marketing

Stakeholder marketing is defined as "activities within a system of social institutions and processes for facilitating and maintaining value through exchange relationships with multiple stakeholders" (Hult et al., 2011). Stakeholder marketing involves, influences, and becomes part of national policies, often through alcohol industry lobbying, business models, research models, funding, and social and physical environments. CSR activities are a subset of stakeholder marketing in which marketers, including producers, distributors, and retailers, create largely ineffective programmes and policies, provide health warnings, and fund health information organizations that cast alcohol consumption in a favourable light (Mialon and McCambridge, 2018; Maani et al., 2023). Cause marketing is a subset of CSR activities in which alcohol companies use world events to engender community spirit and positive associations with their brands (e.g. Atkinson et al., 2021). For example, alcohol corporations used messages of "community support or feeling" to market alcohol consumption as an isolation activity during the COVID-19 pandemic (Martino et al., 2021). Pinkwashing is cause marketing that uses the breast cancer pink ribbon as a marketing tool, and alcohol marketers have used the pink ribbon to market alcoholic beverages (Mart and Giesbrecht, 2015) even though alcohol consumption is carcinogenic to humans (Secretan et al., 2009). All activities that fall under stakeholder marketing are strategic on the part of the for-profit corporations and serve to increase brand awareness and goodwill towards the brand among consumers (Yoon and Lam, 2013).

(f) Target marketing in all five levels

Target marketing is a strategy that the industry uses in all five levels of alcohol marketing. Certain segments of the population experience disproportionately negative effects of alcohol marketing; these include equity-deserving groups such as young people and women, as well as individuals who have or had alcohol use disorder or a family history of alcohol use disorder, are neurodiverse (Babor et al., 2017), or are living in low- and middle-income countries or communities (Babor et al., 2023).

One method of target marketing is the creation of brand identities that target specific market segments of the population (WHO, 2022), such as those grouped by gender, race, or ethnicity. These marketing methods include creating products to appeal to specific segments of the population via package design, as well as messaging (WHO, 2022). For example, brand identities that focus on appeals to women include pink packaging, messages about female friendships, and other so-called girly notions (Atkinson et al., 2022), or a focus on low-calorie alcoholic beverages (Cao et al., 2023). Targeted marketing also uses gendered themes to intentionally target women with messages about femininity; these messages have been accepted or rejected by their audience (Atkinson et al., 2024). Increasingly, marketing targets women with children, creating the false impression that alcohol is a solution to the struggles of parenthood (Bosma et al., 2022).

Product design is also implicated in the marketing of alcopops, which are premixed sweet drinks often referred to as ready-to-drink alcoholic beverages. These products are especially appealing to young people because of their

sweet taste, portability, and attractive packaging (Jones and Reis, 2011).

Alcohol marketing also targets individuals residing in low- and middle-income countries, where levels of alcohol consumption are increasing (Babor et al., 2023). Among these individuals, those who are owners of smartphones – an effective delivery device for digital alcohol marketing – are younger, have higher incomes, and have higher levels of education (Babor et al., 2023).

4.2.2 Effects of alcohol marketing on alcohol consumption

Advertising industry case studies of alcohol advertising campaigns, most of which used "appropriate quasi-experimental methods and longitudinal data", claimed a causal relationship between alcohol advertising and alcohol consumption, as well as clear intentions to target and influence individuals with heavy alcohol consumption (Maani Hessari et al., 2019). Current evidence shows that exposure to and/or engagement with alcohol marketing among young people increases alcohol consumption (Anderson et al., 2009; Jernigan et al., 2017), and a causal relationship between exposure to alcohol marketing and the alcohol consumption behaviour of young people has been established based on the Bradford Hill criteria (Sargent and Babor, 2020).

(a) Level 1: Personalized marketing

Researchers have investigated the association between time spent on social media and alcohol consumption (Purba et al., 2023). Influencers, as well as personalized, algorithmically targeted, and shared media, contribute to a normative drinking culture (Carah and Brodmerkel, 2021), which increases the likelihood of alcohol consumption, especially among young people, who are more susceptible to peer influence (Babor et al., 2023).

When the evidence was examined from more than 30 studies that assessed the effects of alcohol marketing on social media (e.g. exposure to friends' online pictures of alcohol consumption, user-generated messages about alcohol, and online industry-generated alcohol marketing), exposure to and/or engagement with alcohol marketing was associated with higher levels of consumption, intention to drink, alcohol-related problems, and positive attitudes about alcohol (Babor et al., 2023). However, the existing evidence does not encompass the effects of the current highly personalized algorithmic targeting on alcohol consumption.

Individuals with heavy alcohol consumption are more receptive to alcohol marketing (Babor et al., 2023) and are probable targets of the algorithms of digital marketing (Carah and Brodmerkel, 2021).

(b) Levels 2 and 3: Point-of-sale marketing and mass media marketing

The effects of exposure to and/or engagement with mass media alcohol advertising and portrayals of alcohol consumption in entertainment media on young people have been assessed in three systematic reviews (Anderson et al., 2009; Smith and Foxcroft, 2009; Jernigan et al., 2017). One of the reviews also assessed the effects of product placement in entertainment media, mentions of alcohol consumption in song lyrics and music videos, and promotional activities that included alcohol-branded merchandise (a form of point-of-sale marketing) on alcohol consumption among young people (Smith and Foxcroft, 2009). Overall, exposure to alcohol marketing was associated with initiation of alcohol consumption and higher levels of consumption among young people who had already started consuming alcohol.

(c) Level 4: Consumer marketing

Alcopops are one example of a product intentionally designed, priced, and sold in ways that appeal to young people (Jones and Reis, 2011). In 2010–2019, poison control centres in the USA received 1719 calls related to consumption of supersized alcopops (Rossheim et al., 2021), suggesting that these products were leading to higher-risk alcohol consumption.

(d) Level 5: Stakeholder marketing

As a subset of stakeholder marketing, CSR activities frame alcohol consumption as a personal responsibility, deflect the role of alcohol consumption in alcohol-attributable disease, and normalize alcohol consumption (Maani et al., 2023). The ubiquitous nature of alcohol marketing, including the broad influence of the Level 5 marketing activities described above, results in permissive drinking environments that encourage and increase alcohol consumption (WHO, 2023).

(e) Effects of alcohol marketing among populations who experience disproportionately negative effects

There is evidence that expansion of alcohol-related transnational corporations into low- and middle-income countries promotes the normalization of alcohol consumption and is associated with heavy alcohol consumption and intoxication (Babor et al., 2023). This expansion also increases marketing to specific target populations, including young people, women, and the rising middle class, and is associated with higher consumption in these groups (Esser and Jernigan, 2015).

Young people are especially susceptible to alcohol marketing because they seek peer approval and to be part of the normative culture (Babor et al., 2023). In the heuristic model proposed by McClure et al. (2013), alcohol marketing receptivity among young people progresses to noticing more marketing, remembering more marketing, liking certain brands, participating in marketing by wearing branded merchandise or visiting an alcohol company's website, communicating brand preferences to peers, and creating user-generated content. The reach of user-generated media can grow exponentially with digital sharing, thus creating more opportunities for exposure to and engagement with alcohol marketing, with a greater likelihood of increasing alcohol consumption.

Girls and women are seen by the alcohol industry as an untapped growth segment (Babor et al., 2023). Evidence suggests that among women greater exposure to alcohol marketing is positively associated with alcohol consumption behaviours (e.g. Amanuel et al., 2018).

4.3 Alcohol marketing bans

The regulation of alcohol marketing occurs within a larger regulatory context including actions related to consumer protection, digital services, and marketing in general. These contexts may be established at the subnational, national, regional, or global level. For example, in the USA, the Federal Trade Commission seeks to protect "the public from deceptive or unfair business practices and from unfair methods of competition through law enforcement, advocacy, research, and education" (FTC, 2024). In the European Union, the two main goals of the Digital Services Act and the Digital Markets Act are "to create digital spaces in which the fundamental rights of all users are protected" and "to establish a level playing field to foster innovation, growth, and competitiveness both in the European Single Market and globally" (European Commission, 2024). Globally, trade agreements, such as the World Trade Organization's Agreement on Trade-Related Aspects of Intellectual Property Rights (TRIPS) and General Agreement on Trade in Services (GATS), commit World Trade Organization members to adhere to certain basic

regulatory principles applicable to marketing regulation (WTO, 1994a, b). Decisions of judicial bodies, potentially at each of the geographical levels, also influence the shape and context of alcohol marketing regulations.

4.3.1 *Types of alcohol marketing bans and their geographical variability*

This section describes alcohol marketing regulations at the national or subnational level, types of alcohol marketing regulations, and global variation in their implementation and enforcement.

Among the levels of alcohol marketing shown in Fig. 4.1 (see Section 4.2.1), most alcohol marketing regulations focus on the point-of-sale marketing and mass media marketing described in Levels 2 and 3. The regulation of product pricing and distribution (Level 4) are discussed in Sections 2 and 3, and Level 5 activities continue to be largely unregulated. There have been a few efforts to regulate the personalized marketing activities described in Level 1. A review of case law and judicial or other (e.g. administrative or dispute-settlement) decisions regarding alcohol marketing is beyond the scope of this volume.

(a) Alcohol marketing restrictions and regulations

Esser and Jernigan (2018) identified four main approaches for restricting or regulating alcohol marketing: (i) no actions or restrictions at all; (ii) industry self-regulatory, co-regulatory, or voluntary actions, which rely on the adoption of codes of conduct, pledges, or other commitments by industry actors; (iii) legislative adoption of partial restrictions (referred to herein as partial bans) targeting one or more, but not all, specific forms of alcohol marketing (e.g. restrictions on marketing of specific types of alcoholic beverages or in specific types of media); and (iv) legislative adoption of bans on all forms of alcohol marketing for all types of alcoholic beverages (i.e. a comprehensive ban). The boundaries separating one approach from another are not always straightforward; for example, some voluntary actions may be integrated into a general legislative framework (i.e. co-regulation) (WHO, 2022). In addition, the extent of a partial ban can vary; some partial bans are closer to a comprehensive ban, whereas others are closer to the absence of a ban.

An important consideration is the distinction between the regulation of exposure to alcohol marketing, of content, and of engagement. The regulation of exposure seeks to limit the frequency of and ease of access to marketing materials, often among equity-deserving groups such as young people. The regulation of content limits what can be included in marketing communications, such as imagery, wording, and health claims. The regulation of engagement addresses marketing activities that encourage consumers to participate in or in some way become a part of the marketing. Strategies to limit exposure may include bans on marketing at certain times of day, to certain audiences (e.g. no marketing materials to be shown to audiences with more than a specific proportion of young people), for particular types of alcoholic beverages (e.g. banning advertising of spirits in broadcast media), in particular media or channels of delivery (e.g. print, broadcast, digital), in certain locations (e.g. near schools, parks, or playgrounds), or at particular events (e.g. civic festivals, sporting or cultural events). In the absence of a comprehensive ban on exposure to all forms of alcohol marketing, countries may use restrictions on content to limit the effect of marketing. Such restrictions may include bans on the use of lifestyle imagery; restricting content to basic information about the product (e.g. origin, ingredients); bans on nutritional and health claims made for alcohol; bans on the promotion of harmful alcohol consumption (e.g. in conjunction with dangerous activities, such as operating motor vehicles); bans on marketing content that

links alcohol consumption with social success, sports, or sexual performance (Smith et al., 2014); and bans on the use of celebrities and other influencers (WHO, 2022). Finally, efforts to ban or restrict engagement may include banning "share" or "like" buttons, games, or lotteries on alcohol brands' social media sites (Katainen et al., 2020).

The 1991 French Evin law on alcohol marketing took a unique approach to regulating alcohol marketing: rather than banning or limiting particular content, the law specifies the content and other activities that are permitted. If advertisers want to innovate beyond these limits, the law requires that they return to the French Parliament for modification of the law, effectively limiting the innovation of alcohol marketing beyond the limits provided in the law (Friant-Perrot and Garde, 2022).

Of the various types of bans used to regulate alcohol marketing, comprehensive bans have several advantages. More specifically, a comprehensive ban limits the opportunities to shift marketing investments from regulated to unregulated programmes, media, settings, or techniques (e.g. moving a larger investment to digital platforms after a ban on broadcast or outdoor advertising). In addition, enforcing and monitoring comprehensive bans is more straightforward than adjudicating violations of partial bans, particularly on content (WHO, 2019). Defining and distinguishing content that is attractive to young people can be challenging (Padon et al., 2018), particularly for digital marketing, because it is notoriously difficult to identify what may constitute content targeting children or other equity-deserving groups given the different platforms' current control over nearly all relevant data.

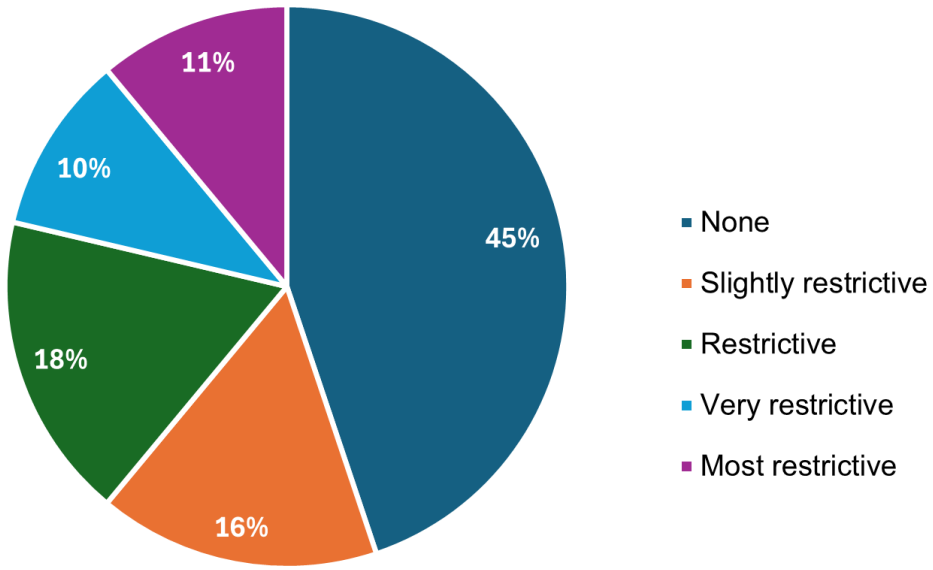
(b) Geographical variability in alcohol marketing regulations

In the current era when personalized marketing occurs simultaneously with mass media, comprehensive bans on alcohol marketing are rare. [Among 136 countries that permit alcohol consumption by their citizens and provided marketing data to the 2019 WHO Global Survey on Alcohol and Health, only one country reported a comprehensive ban on all forms of alcohol marketing across all media types; 54% of countries reported a partial ban, and 45% of countries reported no ban on alcohol marketing (WHO, 2024b).]

Esser and Jernigan (2014) developed and evaluated a scale for assessing the level of restrictiveness of alcohol marketing regulations. [Applying this scale across the three major types of alcoholic beverages (beer, wine, and spirits) and 11 media subtypes (national television, private television, national radio, local radio, print, billboards, point of sale, cinema, Internet, social media, and surrogate advertising) using the 2019 WHO Global Survey on Alcohol and Health data (WHO, 2024b) generates an assessment of the overall restrictiveness of alcohol marketing regulations for each of the 136 reporting countries. As shown in Fig. 4.2, 45% of the countries reported no restrictions, whereas the regulations were slightly restrictive for 16% of the countries and most restrictive for 11% of the countries.]

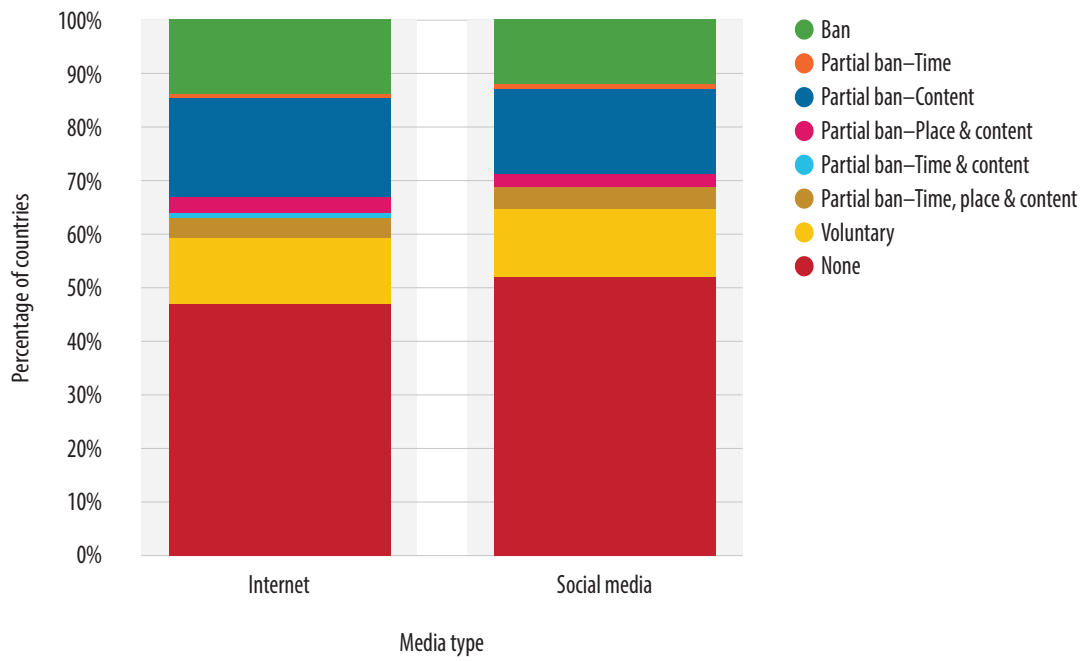
Beer is the alcoholic beverage least likely to be regulated worldwide (WHO, 2018); therefore, WHO reported on the status of beer marketing regulation in countries that provided marketing ban data on the Internet or social media in 2019. Fig. 4.3 shows the percentage of countries with various types of total bans, partial bans, or no bans on beer marketing on the Internet and social media in 2019; most countries had no bans or only partial bans in place (WHO, 2024c).

Fig. 4.2 Percentage of countries (*n* = 136) by level of alcohol marketing restrictiveness score, 2019



Compiled by the Working Group from the 2019 WHO Global Survey on Alcohol and Health data (WHO, 2024b).

Fig. 4.3 Restrictions on advertising for beer by media type and percentage of countries, 2019 (*n* = 125 countries for social media and *n* = 130 countries for the Internet)



Reproduced from WHO (2024c). Licence: CC BY-NC-SA 3.0 IGO.

4.3.2 Effects of alcohol marketing bans on alcoholic beverage consumption

(a) Study eligibility and methodological considerations

As discussed in Section 4.1, truly comprehensive bans on all forms of alcohol marketing for all types of alcoholic beverages are rare. Therefore, for this review and evaluation, the Working Group reviewed and evaluated the evidence on the effects of strong bans, which are defined here as a ban on alcohol marketing in at least one major media type for all types of alcoholic beverages, on alcoholic beverage consumption. The Working Group summarized, but did not evaluate, studies that included a mix of strong bans and partial alcohol marketing bans or partial bans only.

The general outcomes, study eligibility criteria, methodological considerations, and other issues that apply to all policies evaluated are described in Sections 1.3–1.6. In addition, for alcohol marketing bans specifically, the Working Group excluded studies that assessed the immediate (e.g. within hours) effects on alcohol consumption of viewing advertisements, television programmes, and/or movies that did or did not contain portrayals of alcohol, or that assessed voluntary or self-regulatory codes of conduct adopted by the alcohol industry.

As discussed in Section 4.2, marketing activities are often targeted to specific subpopulations. For example, an important target for alcohol marketing is young people, and alcohol marketing bans aim to limit their exposure to and engagement with alcohol marketing, to prevent initiation of alcohol consumption or increasing alcohol consumption. However, only one study assessed an alcohol marketing ban (a partial ban) among young people, and no study on marketing bans has focused specifically on other high-risk subgroups.

The methodological considerations that are particularly relevant to assessing the evidence on the effects of alcohol marketing bans on alcohol consumption include potential bias due to collinearity and endogeneity. More specifically, some studies included variables for alcohol marketing bans as both an independent variable and a component of a control variable (e.g. a total alcohol policy strictness index) or included variables for both a partial ban and a strong ban (which includes the partial ban) in the model. The inclusion of these variables in the same model can lead to collinearity or overspecification of the model. Furthermore, endogeneity can occur if alcohol marketing bans are affected by alcohol consumption or if both variables are affected by a third common variable, and not all studies tested for endogeneity or used statistical methods to reduce bias due to endogeneity.

(b) Strong bans

The effects of strong alcohol marketing bans on alcoholic beverage consumption were assessed in two panel-regression studies in 20 (Saffer and Dave, 2002) and 17 (Nelson, 2010) Organisation for Economic Co-operation and Development (OECD) countries, and in interrupted time-series studies in Norway (Rossow, 2021) and British Columbia, Canada (Smart and Cutler, 1976) (Table 4.1).

In a panel-regression study using annual data from 1970 through 1995 for 20 OECD countries, Saffer and Dave (2002) categorized the number of media types (i.e. television, radio, or print) in which advertising was banned for both alcoholic beverage groups (i.e. spirits, and beer and wine) into a total alcohol advertising ban score, a numerical variable ranging from 0 to 3; a score of 1 is a strong marketing ban in at least one media type. In a model with real alcohol price, real income, and alcohol culture (i.e. percentage of total alcohol consumed as beer and wine) as control variables, a 1-unit increase in the total alcohol advertising ban score was associated with [8.6%] ($P < 0.10$) lower alcohol consumption. [The strengths of this study are the

Table 4.1 Effects of strong alcohol marketing bans on alcoholic beverage consumption

Reference Location Study population Period	Description of intervention	Study type Data source(s) Method of analysis	Outcome Effect estimates	Covariates	Comments
Smart and Cutler (1976) British Columbia (intervention) and Ontario (comparison), Canada Total population 1962–1972	Temporary ban on alcohol advertising in newspapers and on radio, TV, billboards, and noticeboards originating in British Columbia from September 1971 to October 1972; the pre-ban years were 1962–1970, and the ban years were 1971–1972	Interrupted time series Alcohol consumption was computed separately for beer, wine, and spirits using alcohol sales data from Statistics Canada Mann–Whitney *U* test for difference in rankings of percentage of annual alcohol consumption (wine, beer, and spirits) relative to 1962	Difference in the annual change in beverage-specific alcohol consumption per capita per year between the pre-ban years and the ban years British Columbia: no differences ($z = 0.31$; $P > 0.05$) Ontario: higher in ban years ($z = 1.87$; $P < 0.03$)		Broadcast and print advertising originating outside British Columbia were not subject to regulation and were available to residents of British Columbia British Columbia lowered its legal drinking age in April 1970, whereas Ontario lowered its legal drinking age in July 1971
Saffer and Dave (2002) 20 OECD countries Adult population aged ≥ 15 years 1970–1995	For each country, a total alcohol advertising ban score is a variable ranging from 0 to 3 for each year based on the number of media types (i.e. TV, radio, or print) in which advertising was banned for 2 alcoholic beverage groups (i.e. spirits, and beer and wine); years of implementation varied by country	Panel regression Alcohol advertising ban and consumption data from BAC Two-equation structural models estimated by TSLS	Difference in the natural log of alcohol consumed per capita per year (litres of ethanol) for each 1-unit increase in the total alcohol advertising ban score $\beta = -0.0898$ ($P < 0.10$)	Real alcohol price, real income, and alcohol culture (percentage of total alcohol consumed as beer and wine)	Endogeneity between alcohol marketing bans and alcohol consumption was addressed using two-equation structural models estimated by TSLS In other models that included country dummy variables, beer production, and wine production, the effect estimate changed for the total alcohol advertising ban score variable ($\beta = 0.0367$ per 1-unit increase; $P > 0.10$), probably due to collinearity with the country dummy variables

Table 4.1 (continued)

Reference Location Study population Period	Description of intervention	Study type Data source(s) Method of analysis	Outcome Effect estimates	Covariates	Comments
Nelson (2010) 17 OECD countries Adult population aged ≥ 15 years 1975–2000	For each country, a ban on broadcast advertising for all types of alcoholic beverages except weak beer was a dummy variable for each year without (scored 0) or with (scored 1) a ban; years of implementation varied by country	Panel regression Alcohol advertising ban data principally from BAC and Österberg and Karlsson (2003); alcohol consumption data from WHO Weighted generalized least-squares regression	Difference in the natural log of recorded alcohol consumption per capita per year (litres of ethanol) between countries with the ban and countries without the ban Table 5, Model 1: $\beta = -0.039$ ($P < 0.05$)	Table 5, Model 1: spirits advertising bans, real income, real alcohol price index, wine sentiment (percentage of total alcohol consumed as wine)	There was evidence of endogeneity between alcohol marketing bans and alcohol consumption Model 2 contains a control index variable (range, 0–20) for stringency of alcohol policies that includes scores for statutory or voluntary marketing restrictions; the effect estimate changed for the ban variable ($\beta = 0.045$; $P < 0.05$), probably due to collinearity with the alcohol marketing bans [Nelson has at times, but not in this paper, disclosed support from alcohol industry interests (Bartlett and McCambridge, 2024)]
Rossow (2021) Norway Adult population aged ≥ 15 years 1960–2006	Ban in 1975 on all alcohol marketing in all media for all beverage types with an ethanol content > 2.5%; the ban variable was scored 0 in each of the pre-ban years (1960–1974), 0.5 in 1975, and 1 in each of the subsequent ban years (1976–2006)	Interrupted time series Alcohol sales data from Statistics Norway ARIMA	Change in the natural log of recorded alcohol sales per capita per year (litres of ethanol) from the pre-ban years to the ban years $\beta = -0.074$ (SE = 0.023; $P = 0.002$)	Real income and real alcohol prices	Approximately 20% of total alcohol consumption in Norway in 1973–1994 was unrecorded Exposure to alcohol advertising may have started after 1988 when satellite TV broadcasting from the United Kingdom was first allowed; in restricted time-series analysis from 1960 through 1995, the decrease in consumption associated with the ban was lower ($\beta = -0.067$; SE = 0.031; $P = 0.039$)

ARIMA, auto-regressive integrated moving average; BAC, Brewers Association of Canada; OECD, Organisation for Economic Co-operation and Development; SE, standard error; TSLS, two-stage least squares; TV, television; WHO, World Health Organization.

inclusion of 26 years of annual consumption data from 20 countries with time variation in alcohol marketing bans in different types of media; the inclusion of control variables for real alcohol price and real income for the demand function, and alcohol culture in the model; the assessment of the effects of the lack of normality of the ban variables because of the large number of zero values; the incorporation of the advertising response function and its role in endogeneity into the analysis; and the use of structural equation models to reduce bias due to endogeneity. The limitations of this study are that the alcohol marketing ban variable is a numerical integer, which required an assumption of linearity in the association between the ban variable and alcohol consumption; and that it is unclear whether the alcohol consumption outcome was based on recorded alcohol consumption data only.]

Using similar data sets but different definitions for strong bans and different statistical procedures than Saffer and Dave (2002), Nelson (2010) assessed the association between a marketing ban in broadcast media for all types of alcoholic beverages except weak beer and recorded alcohol consumption using annual data from 1975 through 2000 for 17 OECD countries. In the most informative model, after controlling for spirits advertising bans, real income, real alcohol price index, and wine sentiment (i.e. percentage of total alcohol consumed as wine), a strong ban was associated with [3.8%] ($P < 0.05$) lower recorded alcohol consumption. [The strengths of this study are the inclusion of 26 years of annual consumption data from 17 countries with time variation in strong alcohol marketing bans in broadcast media; the inclusion of control variables for real income, real alcohol price, and wine sentiment in the model; the sensitivity analysis that included treatment for non-stationary data, showing similar results; and the assessment of endogeneity between alcohol marketing bans and alcohol consumption, but none was observed. The limitations of this study are that the definition of a strong alcohol marketing ban was limited to broadcast media and had an exception for weak beer, which usually has the same brands as beers with higher ethanol content; that there is potential for collinearity, because a variable for spirits advertising bans was included in the model as well as the alcohol control index variable (Karlsson and Österberg, 2001); that the alcohol consumption outcome was based on recorded alcohol consumption data only; and that Nelson has at times, but not in this paper, disclosed support from alcohol industry interests (Bartlett and McCambridge, 2024).]

Rossow (2021) assessed the effects of Norway's 1975 strong ban on alcohol marketing in all media types for all beverage types with an ethanol content greater than 2.5% on the change in recorded alcohol sales from the pre-ban years to the ban years. In a model that controlled for real alcohol prices and real income, the ban led to a [7.1%] ($P = 0.002$) reduction in alcohol sales. [The strengths of this study are the inclusion of 47 years of annual sales data covering multiple years before and after the ban, and the inclusion of control variables for real alcohol prices and real income in the interrupted time-series model. The limitations of this study are that there was no control country for comparison, that endogeneity was not assessed, that the alcohol sales outcome was based on recorded alcohol sales data only, and that Rossow (2021) reports that approximately 20% of alcohol consumption in Norway between 1973 and 1994 was unrecorded.]

Smart and Cutler (1976) assessed the effect of a temporary strong marketing ban implemented in British Columbia, Canada, from September 1971 to October 1972 on the difference in the annual change in beer, wine, and spirits consumption between the pre-ban years and the ban years. The difference in consumption during the same years was also assessed in Ontario, Canada, a province with characteristics

similar to those of British Columbia but without a temporary ban. There was no difference in the annual change in alcohol consumption between the pre-ban years and the ban years in British Columbia ($z = 0.31$; $P > 0.05$). In contrast, alcohol consumption in Ontario increased significantly in the ban years compared with the pre-ban years ($z = 1.87$; $P < 0.03$). [The strengths of this study are the inclusion of a control comparison province, and that the difference in consumption between the pre-ban years and the ban period in each province was based on differences in rankings of the percentage of annual sales to detrend the time-series data because of the absence of control variables. The limitations of this study are that the ban was limited to media originating in British Columbia but did not ban marketing material originating outside the province, which could dilute any potential effects of the ban; that no other control variables were included in the analysis, such as those related to changes in the economic environment or other alcohol policies; that British Columbia lowered its legal drinking age in April 1970, whereas Ontario lowered its legal drinking age in July 1971, which could have contributed to the increase in alcohol consumption in Ontario during the ban period; and that the alcohol consumption outcome was based on recorded alcohol consumption data only.]

(c) Other evidence

The primary challenge in assessing the other evidence on the effects of alcohol marketing bans on alcoholic beverage consumption is the diversity of the bans included in the studies. A mix of strong bans and partial bans was assessed in a panel-regression study in 20 OECD countries (Saffer and Dave, 2002), a meta-analysis of time-series data from 5 European countries (Baccini and Carreras, 2014), a repeated cross-sectional survey study of individual-level data from adolescents in 33 countries or regions in Europe and North America (Leal-López et al., 2020), and a time-series study in three Baltic countries and Poland (Rehm et al., 2022). Marketing bans on spirits only were assessed in panel-regression studies in 50 states in the USA (Nelson, 2003) and 17 OECD countries (Nelson, 2010). The effect of lifting an alcohol marketing ban on beer and wine on alcohol consumption in Saskatchewan, Canada, was assessed in an interrupted time-series study (Makowsky and Whitehead, 1991) (Table 4.2).

(i) Mix of strong bans and partial bans

The panel-regression study of alcohol marketing bans and alcohol consumption in 20 OECD countries by Saffer and Dave (2002) was described in Section 4.3.2(b). In addition to assessing strong bans, they also categorized the number of media types (range, 0–3) in which advertising was banned for the number of alcoholic beverage groups (range, 0–2) into a partial alcohol advertising ban score, a numerical variable ranging from 0 to 6. In the model with real alcohol price, real income, and alcohol culture (i.e. percentage of total alcohol consumed as beer and wine) as control variables, a 1-unit increase in the partial alcohol advertising ban score was associated with [4.7%] ($P < 0.10$) lower alcohol consumption. [The strengths and limitations of this study are described in Section 4.3.2(b).]

The Alcohol Measures for Public Health Research Alliance (AMPHORA) project (Baccini and Carreras, 2014) is described in Section 3.2.3. The change in alcohol consumption from before to after the introduction of either strong or partial bans was assessed using data from five European countries (Austria, France, Italy, Norway, and Spain). In a meta-analysis that included the individual country-specific estimates of association, there was a 0.6% (90% confidence interval [CI], –3.6% to 2.3%) reduction in alcohol consumption after the introduction of an alcohol marketing ban. [The strengths and limitations of this study are described in Section 3.2.3. An additional weakness is that the alcohol marketing ban

Table 4.2 Other evidence on the effects of alcohol marketing bans on alcoholic beverage consumption

Reference Location Study population Study period	Description of intervention	Study type Data source(s) Method of analysis	Outcome Effect estimate	Covariates	Comments
Mix of strong bans and partial bans					
Saffer and Dave (2002) 20 OECD countries Adult population aged ≥ 15 years 1970–1995	For each country, a partial alcohol advertising ban score is a variable ranging from 0 to 6 for each year based on the number of media types in which advertising was banned for the number of alcoholic beverage groups; years of implementation varied by country	Panel regression Alcohol advertising ban and consumption data from BAC Two-equation structural models estimated by TSLS	Difference in the natural log of alcohol consumed per capita per year (litres of ethanol) for each 1-unit increase in the partial alcohol advertising ban score $\beta = -0.0486$ ($P < 0.10$)	Real alcohol price, real income, and alcohol culture (percentage of total alcohol consumed as beer and wine)	Endogeneity between alcohol marketing bans and alcohol consumption was addressed using two-equation structural models estimated by TSLS In other models that included country dummy variables, beer production, and wine production, the effect estimate changed for the partial alcohol advertising ban score variable ($\beta = 0.0367$ per 1-unit increase; $P > 0.10$), probably due to collinearity with the country dummy variables
Baccini and Carreras (2014) 5 European countries Adult population aged ≥ 15 years 1961–2008 (see Allamani et al., 2014)	For each country, a mix of strong bans and partial bans was a dummy variable scored 0 for each year before the introduction of the ban and scored 1 for the year the ban was introduced and each subsequent year; year of initial implementation varied by country: Austria, 1974; Norway, 1975; France, 1987; Spain, 1988; and Italy, 2001	Time series Alcohol control policies and recorded alcohol consumption data from WHO GISAH (WHO, 2024b) Country-specific linear regression analyses using the same model followed by a random-effects meta-analysis	Meta-analytic coefficient (×100) is the percentage change in recorded alcohol consumption per capita per year (litres of ethanol) from the pre-ban years to the ban years −0.6% (90% CI, −3.6% to 2.3%)	A linear time-trend variable, logarithm of income, percentage of men aged > 65 years, urbanization level, logarithm of price of the 2 main types of alcoholic beverages consumed during the study period, restrictive availability policies, permissive availability policies, change in the minimum purchase age, and change in blood alcohol concentration limit for driving	In a separate report of data during the period 1960–2008 in Spain only (Matrai et al., 2014), a 1988 advertising ban on alcoholic beverages > 20 proof [> 10% ethanol content] was associated with a reduction in alcohol consumption ($\rho = -0.12$; 90% CI, −0.17 to −0.07) unadjusted for other alcohol policy changes enacted between 1982 and 1998

Table 4.2 (continued)

Reference Location Study population Study period	Description of intervention	Study type Data source(s) Method of analysis	Outcome Effect estimate	Covariates	Comments
Leal-López et al. (2020) 33 countries or regions in Europe and North America 671 084 adolescents aged 11, 13, and 15 years Survey years: 2001/2002, 2005/2006, 2009/2010, and 2013/2014	For each country, an alcohol advertising index was a proportional restrictiveness score (range, 0–2.60) for each year based on whether there was no restriction, a voluntary self-regulated code, partial statutory restriction, or a complete ban in each type of media (i.e. print, broadcast, billboards, sport sponsorship, and Internet) for each type of alcoholic beverage; years of implementation varied by country and region	Repeated cross-sectional survey Alcohol consumption data from the Health Behaviour in School-aged Children Study Multilevel modelling among 4 waves of data collection	Difference in the prevalence of lifetime alcohol consumption per 1-unit increase in the alcohol advertising index $\beta = -0.11$ (SE = 0.096; $P > 0.05$) Difference in the prevalence of weekly alcohol consumption per 1-unit increase in the alcohol advertising index $\beta = -0.17$ (SE = 0.096; $P > 0.05$)	Age, sex, time, availability ban; random effects for country, country–year, and school	Lifetime consumption was defined as consumption on at least 1 day over the lifespan
Rehm et al. (2022) Estonia, Latvia, Lithuania, and Poland Adult population aged ≥ 15 years 2000–2020	Year of implementation and type of ban varied by country: Estonia (November 2008), broadcast advertising prohibited from 07:00 to 21:00; Latvia (2013), outdoor advertising of all alcoholic beverages prohibited; Lithuania (2008), advertising on TV and radio prohibited during daytime; Lithuania (2018), full ban on TV, radio, and Internet advertisements; Poland (2001), ban on beer advertising lifted; Poland (2003), return of beer advertising on billboards and reduction of ban on TV, radio, and theatre advertisements between 06:00 and 20:00; for each country, the marketing ban variable was a dummy variable for each year without (scored 0) or with (scored 1) a ban	Time series Alcohol consumption data from WHO estimates of APC (recorded and unrecorded) Linear regression	Immediate (i.e. within 1 year) change in APC associated with implementation of an alcohol marketing ban $\beta = 0.04$ (95% CI, −0.65 to 0.73)	Year; stricter taxation and availability policies; looser taxation, availability, and marketing policies; country	Sensitivity analysis using alcohol consumption data defined by each country had similar results ($\beta = -0.01$; 95% CI, −0.59 to 0.57)

Table 4.2 (continued)

Reference Location Study population Study period	Description of intervention	Study type Data source(s) Method of analysis	Outcome Effect estimate	Covariates	Comments
Marketing bans on distilled spirits					
Nelson (2003) 45 states in the USA Adult population aged ≥ 14 years 1982–1997	For each state, both bans on billboard advertising of distilled spirits and bans on price advertising of distilled spirits were dummy variables for each year without (scored 0) or with (scored 1) a ban; years of implementation varied by state	Panel regression State bans on billboard and price advertising of distilled spirits from *Summary of State Laws & Regulations Relating to Distilled Spirits*; alcohol consumption data from United States NIAAA Generalized least-squares analysis	Difference in the natural log of total alcohol consumption per capita per year (gallons of ethanol) between states with the ban and states without the ban Billboard ban: $\beta = 0.054$ ([$P < 0.05$]) Price advertising ban: $\beta = -0.009$ ([$P < 0.05$])	Real income, real alcohol price, cross-price elasticities for cigarettes, tourism, percentage aged 18–24 years, percentage aged ≥ 65 years, unemployment rate, retail monopoly, MLDA, dummy variables for three regions (relative to fourth region); billboard ban and price advertising ban were mutually adjusted	Test of significance [i.e. *P* value] was based on the Huber–White robust *t* statistics and a sample size of 720 observations described in Table II Subanalysis showed similar results for 33 licence states ($\beta = 0.023$ for billboard bans, and $\beta = -0.01$ for price advertising bans) [Nelson has at times, but not in this paper, disclosed support from alcohol industry interests (Bartlett and McCambridge, 2024)]
Nelson (2010) 17 OECD countries Adult population aged ≥ 15 years 1975–2000	For each country, a ban on broadcast advertising of distilled spirits was a dummy variable for each year without (scored 0) or with (scored 1) a ban; years of implementation varied by country	Panel regression Alcohol advertising ban data principally from BAC and Österberg and Karlsson (2003); alcohol consumption data from WHO Weighted generalized least-squares models	Difference in the natural log of recorded alcohol consumption per capita per year (litres of ethanol) between countries with the ban and countries without the ban Table 5, Model 1: $\beta = 0.084$ ($P < 0.05$)	Table 5, Model 1: broadcast advertising ban on all beverages except weak beer, real income, real alcohol price index, wine sentiment (percentage of alcohol consumed as wine)	There was no evidence of endogeneity between alcohol marketing bans and alcohol consumption Model 2 contains a control index variable (range, 0–20) for stringency of alcohol policies that includes scores for statutory or voluntary marketing restrictions; the effect estimate changed for the ban variable ($\beta = 0.030$; $P < 0.05$) [Nelson has at times, but not in this paper, disclosed support from alcohol industry interests (Bartlett and McCambridge, 2024)]

Table 4.2 (continued)

Reference Location Study population Study period	Description of intervention	Study type Data source(s) Method of analysis	Outcome Effect estimate	Covariates	Comments
Lifting of alcohol marketing ban on beer and wine					
Makowsky and Whitehead (1991) Saskatchewan (intervention) and New Brunswick (control), Canada Adult population aged ≥ 15 years April 1981–March 1987	Lifting of a nearly 58-year advertising ban in October 1983, allowing broadcast marketing for beer and wine (but not spirits) and print marketing for beer, wine, and spirits in Saskatchewan; a dummy variable for each month before the lifting of the ban (April 1981–September 1983; scored 0) and after the lifting of the ban (October 1983–March 1987; scored 1); a similar ban remained in effect in New Brunswick	Interrupted time series Comptrollers of the Saskatchewan and New Brunswick Liquor Commissions Box–Jenkins time-series analysis: ARIMA abrupt permanent models	Change in total alcohol sales per month (litres of ethanol) from the period before to the period after the lifting of the advertising ban Saskatchewan: –11 110 ($P > 0.05$) New Brunswick: 2.26 ($P > 0.05$)		Saskatchewan: sales (litres of ethanol) of spirits decreased (–22 490 L; $P < 0.05$), but sales of beer (14 890 L; $P < 0.05$) and wine (1149 L; $P > 0.05$) increased New Brunswick: change in sales (litres of ethanol) of spirits (–2465 L; $P > 0.05$), beer (–17 880 L; $P > 0.05$), and wine (1879 L; $P > 0.05$)

APC, total adult alcohol per capita consumption (sum of recorded and unrecorded alcohol consumed per person aged ≥ 15 years over a calendar year); ARIMA, auto-regressive integrated moving average; BAC, Brewers Association of Canada; CI, confidence interval; GISAH, Global Information System on Alcohol and Health; MLDA, minimum legal drinking age; NIAAA, National Institute on Alcohol Abuse and Alcoholism; OECD, Organisation for Economic Co-operation and Development; SE, standard error; TSLS, two-stage least squares; TV, television; WHO, World Health Organization.

variable was a dummy-coded variable for all five countries and treats all bans equally regardless of level of restrictiveness.]

The study by Leal-López et al. (2020), which included four cross-sectional waves of survey data from adolescents in 33 countries or regions, is described in detail in Section 3.2.5. An alcohol advertising index (range, 0–2.60) was a proportional score based on whether there was a complete ban, partial statutory restrictions, a voluntary self-regulated code, or no restrictions, in each type of media (i.e. print, broadcast, billboards, sport sponsorship, and Internet) for each type of alcoholic beverage. A 1-unit increase in the alcohol advertising index was associated with an 11% ($P > 0.05$) lower prevalence of lifetime alcohol consumption and a 17% ($P > 0.05$) lower prevalence of weekly alcohol consumption. [The strengths and limitations of this study are described in Section 3.2.5.]

Rehm et al. (2022) assessed the association between the change in alcohol marketing bans and the immediate (i.e. within 1 year) change in the total adult alcohol per capita consumption (APC) using time-series data for 2000 through 2020 from Estonia, Latvia, Lithuania, and Poland. During the period of analysis, most countries implemented or lifted partial alcohol marketing bans, whereas a strong ban on marketing was implemented in Lithuania in 2018. Alcohol marketing bans were not associated with an immediate change in APC (0.04 L; 95% CI, −0.65 L to 0.73 L). [The strength of this study is the inclusion of 21 years of annual APC data from four geographically neighbouring countries with time variation in different types of partial or complete alcohol marketing bans. The limitations of this study are that endogeneity was not assessed, that the alcohol marketing ban variable was a dummy-coded variable for all four countries and therefore treats all bans equally regardless of restrictiveness, and that only the effect of a marketing ban on the immediate change in alcohol consumption was assessed.]

(ii) Marketing bans on spirits

Nelson (2003) assessed the associations of bans on billboard marketing and price advertising for spirits only with total alcohol consumption in 45 states in the USA for the period 1982 through 1997. Billboard marketing bans were associated with [5.5%; $P < 0.05$] higher alcohol consumption, whereas the price advertising ban was associated with [1%; $P < 0.05$] lower alcohol consumption. [The strengths of this study are the inclusion of 16 years of annual consumption data from 45 states in the USA with time variation in two different types of alcohol marketing bans, which were mutually adjusted for in the model, and the inclusion of control variables for real income, real alcohol price, tourism, unemployment, age distributions, retail monopolies, minimum legal drinking age, region, and cross-price elasticities for cigarettes in the model. The limitations are that there was no testing for endogeneity, that it is unclear whether the alcohol consumption outcome was based on recorded alcohol consumption data only, and that Nelson has at times, but not in this paper, disclosed support from alcohol industry interests (Bartlett and McCambridge, 2024).]

The study by Nelson (2010) of strong alcohol marketing bans on broadcast advertising and alcohol consumption in 17 OECD countries is described in Section 4.3.2(b). In that study, the association between a broadcast advertising ban on spirits and alcohol consumption also was assessed. In a model including strong marketing bans, real income, real alcohol price index, and wine sentiment, the spirits advertising ban was associated with [8.8%] ($P < 0.05$) higher alcohol consumption. [The strengths and limitations of this study are described in Section 4.3.2(b). An additional limitation is that there is potential for collinearity because a variable for broadcast advertising bans on all alcoholic beverages except weak beer was included in the model.]

(iii) Lifting of a marketing ban on beer and wine

Makowsky and Whitehead (1991) assessed the effects of the lifting of a nearly 58-year marketing ban on broadcast marketing for beer and wine (the broadcast marketing ban remained in effect for spirits) and on print marketing for beer, wine, and spirits in Saskatchewan, Canada, in October 1983 on the change in monthly total alcohol sales. The change in monthly total alcohol sales in New Brunswick, Canada, where a similar ban remained in effect, was also assessed. In Saskatchewan, there was a reduction in total alcohol sales from the period before to the period after the lifting of the ban, which was not statistically significant (–11 110 litres per month; $P > 0.05$), and in New Brunswick there was no change in alcohol sales (2.26 litres per month; $P > 0.05$). [The strengths of this study are the inclusion of a control province, the use of monthly sales data, and the analyses accounting for autocorrelation of the data. The limitations of this study are that no other control variables were included in the analysis, that endogeneity was not assessed, and that the alcohol sales outcome was based on recorded alcohol sales data only.]

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5. COORDINATED AND OTHER MULTIPLE ALCOHOL POLICY INTERVENTIONS

5.1 General considerations

In addition to studies on the effects of alcohol policy interventions related to taxation and pricing, availability, and marketing on alcoholic beverage consumption, the effects of multiple alcohol policy interventions on alcohol consumption have also been assessed in several studies.

For this volume, coordinated multiple alcohol policy interventions are defined as government alcohol monopolies or as a set of interventions that are enacted and implemented as part of a national action plan to reduce alcohol-related harms. Government alcohol monopolies or other coordinated multiple alcohol policy interventions are often implemented at the national or subnational level during a specific time period to curb the alcohol-related harms attributable to increasing or high levels of consumption of alcoholic beverages in the target population. The Working Group evaluated studies on the effects of government alcohol monopolies (Section 5.2) and other coordinated multiple alcohol policy interventions (Section 5.3) on alcohol consumption.

In addition, there is evidence from studies of multiple alcohol policy interventions that were implemented at different time points (not using a coordinated approach) on alcohol consumption. This evidence includes observational studies of multiple alcohol policy restrictiveness scores (Section 5.4.1) and controlled trials of alcohol policy community action interventions (Section 5.4.2). The Working Group reviewed these studies but did not take them into consideration for the evaluation of the multiple alcohol policy interventions.

The general outcomes, study eligibility criteria, methodological considerations, and other issues for the studies reviewed in this section are described in Sections 1.3–1.6. In addition, in Sections 5.3 and 5.4, only studies in which the interventions included at least two of the three policy areas discussed in Sections 2, 3, and 4 (i.e. taxation and pricing, availability, and marketing) were reviewed.

5.2 Government alcohol monopolies

5.2.1 Key concepts

A government alcohol monopoly exists when responsibility for all or part of the alcohol market is allocated to a national or subnational governmental department or authority (WHO, 2018). A government monopoly can facilitate coordinated multiple alcohol policy interventions, and jurisdictions with such a monopoly typically have stricter alcohol policies (e.g. lower availability, higher prices, and stricter marketing controls) than jurisdictions without a government alcohol monopoly (Lima, 2019).

Important motivations for implementing a government alcohol monopoly are to decrease alcohol-related harms and to maintain the reductions. Two other motivations, which were more important in the past, are to provide an organized market for the benefit of farmers – as an outlet for their produce – and to generate revenue from the sale of alcoholic beverages (Room, 2020). A government alcohol monopoly at the retail level provides the greatest beneficial effects on the health and welfare of a population (Room, 2000), whereas a government monopoly at the wholesale and importation levels can assist a government retail monopoly in controlling the alcohol market. A government alcohol monopoly at the retail level can control advertising and other promotions, as well as price competition in the alcohol market. A government retail monopoly also makes it easier to regulate on-premises and off-premises hours of sale, the density and locations of alcohol sales outlets, the minimum legal purchase and drinking age, and the sobriety of purchasers, and to combine alcohol sales with adequate information on risks (Room, 2000).

The organization and operation of government alcohol monopolies at the retail level can vary substantially. For example, current government alcohol monopolies primarily regulate off-premises sales, at least in high-income countries. In contrast, the health- and welfare-oriented government monopolies in the late 19th century and early 20th century primarily regulated on-premises sales, in part because they were often the primary location of alcohol consumption at that time. Government alcohol monopolies also vary with respect to where they are located within the government's structure. When the goal is to limit alcohol-related harms to health and welfare, monopolies should be controlled by a government's health or welfare department, rather than its revenue or treasury department.

According to the Global Information System on Alcohol and Health (GISAH), among the 154 countries that reported to the 2019 World Health Organization (WHO) Global Survey on Alcohol and Health about progress on the United Nations Sustainable Development Goals (SDGs) Target 3.5, 113 countries reported no government monopoly on retail sales at the national level, and 23 countries reported a government retail monopoly at the national level for spirits, 25 countries for wine, and 21 countries for beer, but information was missing for some countries (WHO, 2024a). Information on subnational alcohol monopolies is also incomplete.

Because very few government alcohol monopolies have been established in recent decades, the Working Group reviewed and evaluated studies on the effects of the weakening or dissolution of government alcohol monopolies on alcoholic beverage consumption (Section 5.2.2). Studies that adjusted for mediators of availability, such as outlet density, were excluded because part of the effect of the government monopoly may be through mediators, and therefore such studies lack a measure of the overall effect of the government monopoly on alcohol consumption. Studies in which bias due to cross-border shopping was ruled out were influential in the evaluations.

In addition, some countries, such as the Nordic countries Finland, Iceland, Norway, and Sweden, have a long tradition of using government monopolies to reduce historically very high levels of alcohol consumption. The Nordic government alcohol monopolies and examples of studies of their effects on alcohol consumption are briefly summarized in Section 5.2.3.

5.2.2 Effects of government alcohol monopolies on alcoholic beverage consumption

The effects of partial or complete privatization of government alcohol monopolies on alcoholic beverage consumption were assessed in nine studies with population-level alcohol consumption data (Macdonald, 1986; Smart, 1986; Holder and Wagenaar, 1990; Wagenaar and Holder, 1991, 1995; Mäkelä, 2002; Trolldal, 2005a, 2005b; Świątkiewicz et al., 2014), two studies with individual-level data from repeated cross-sectional population surveys (Fitzgerald and Mulford, 1992) or with household-level alcohol purchasing data (Barnett et al., 2020), and one study with both population-level and individual-level data (Mulford and Fitzgerald, 1988) (Table 5.1). These studies were based on privatization of government alcohol monopolies in two provinces in Canada, two countries in Europe, and nine states in the USA.

Macdonald (1986) used 1961–1977 alcohol sales time-series data to assess the effects of four state-level alcohol policy interventions in the USA that increased the availability of wine in grocery stores on wine, beer, and spirits sales per capita (litres of ethanol) in the year of the policy intervention in each state. The policy interventions allowed for wine to be sold in grocery stores in Idaho and Maine (1971), a greater selection of wines in grocery stores in Washington State (1969), and the addition of fortified wines to the table wines already available in grocery stores in Virginia (1974). The policy interventions resulted in higher observed wine sales in the year of the intervention than expected based on the trend in the previous years for Idaho ($P < 0.001$), Maine ($P < 0.001$), and Washington State ($P < 0.01$) but not for Virginia ($P > 0.05$). The observed state beer and spirits sales were not statistically significantly different from the expected sales in any of the four states. [The strength of this study is the assessment of four policy interventions that occurred in four states over 6 years. The limitations of the study are that the effect estimates were not systematically reported, that alcohol sales were reported only by major types of alcoholic beverages and were based on recorded alcohol sales data only, and that for Virginia the effect of the intervention on sales of ethanol from wine may be underestimated because of the approximation in the calculation of ethanol sales.]

Smart (1986) assessed the effects of allowing bottled wine that was locally produced or bottled by the government monopoly to be sold in grocery stores in Quebec, Canada, beginning in June 1978. The difference in annual change in alcohol sales between Quebec and Ontario, the bordering province where bottled wine was sold only in provincial stores, from 1967–1977 to 1978–1983 was assessed. There was no significant difference in annual change in alcohol sales in Quebec compared with Ontario. [The strength of this study is the inclusion of a control province. The limitations of the study are that the effect estimates were not reported and that alcohol sales were based on recorded alcohol sales data only.]

Trolldal (2005b) also assessed the effect of the privatization of wine sales from 1978 through 1983, as well as allowing the sale of wine in large grocery store chains in 1984 in Quebec, Canada, on annual wholesale total alcohol sales per capita (litres of ethanol) from 1950 to 2000, with the rest of Canada as a control site. The 1978 intervention resulted in a 1% ($P \geq 0.10$) increase in alcohol sales, and the 1983–1984 intervention resulted in a 4% ($P \geq 0.10$) increase in alcohol sales. [The strengths of this study are the inclusion of 50 years of annual sales data covering multiple years before and after the two policy interventions; the adjustment for general trends in national data, income, and other control variables in the models; and the use of auto-regressive integrated moving average (ARIMA) models to account for underlying trends and autocorrelation. The limitations of the study are that the

Table 5.1 Effects of the weakening or dissolution of a government alcohol monopoly on alcoholic beverage consumption

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Macdonald (1986) Idaho, Maine, Virginia, and Washington State, USA Adult population aged ≥ 21 years 1961–1977	Interrupted time series Sales data for wine, beer, and spirits for each state from the Brewers Almanac Observed to expected based on linear regression in years before intervention	Retail sales of wine allowed in grocery stores in Idaho (1 July 1971) and Maine (1 January 1971); greater selection of wines in grocery stores in Washington State (1969); addition of fortified wines as privatized sales in Virginia (1 July 1974), but fortified wines could not be sold for a lower price than the least expensive bottle of wine No control or comparison state	Difference in alcohol sales per capita (litres of ethanol) between the observed alcohol sales for the year of the policy intervention and the expected alcohol sales based on the regression trends during the 8–13 years before the policy intervention **Idaho:** Wine: [0.74] ($P < 0.001$) Beer: > 0 ($P > 0.05$) Spirits: > 0 ($P > 0.05$) **Maine:** Wine: > 0 ($P < 0.001$) Beer: [−0.06] ($P > 0.05$) Spirits: [0.15] ($P > 0.05$) **Washington State:** Wine: > 0 ($P < 0.01$) Beer: < 0 ($P > 0.05$) Spirits: NR ($P > 0.05$) **Virginia:** Wine: < 0 ($P > 0.05$)		> 0 refers to positive differences and < 0 to negative differences if actual effect estimates were not reported For Virginia, results for sales of beer and spirits were not reported, and the approximate calculation of volume of ethanol may have resulted in underestimation of the effect of the intervention on sales of wine The number of retail outlets in the year of the intervention increased in Idaho (from 70 to 1000), in Maine (from 65 to 1400), and in Washington State (from ~300 to > 4000) but not in Virginia Compared with the prices in state-controlled stores, wine prices in the grocery stores were ~25% higher in Washington State and 15–20% higher in Virginia
Smart (1986) Quebec (intervention) and Ontario (control), Canada Adult population aged ≥ 15 years 1967–1983	Interrupted time series Per capita alcohol sales data from Statistics Canada reports and from statistical reports of the Addiction Research Foundation Ordinary least squares regression (DiD)	Quebec allowed retail sales of wines made in the province or bottled by the government in small grocery stores and corner stores from June 1978 Control: Ontario, where the provincial government monopoly was maintained	Difference in the change in alcohol sales per capita per year (litres of ethanol) between Quebec and Ontario from 1967–1977 to 1978–1983 DiD: NR ($P > 0.05$)	Dummy variable for the period of the intervention, dummy variable for the province of the intervention, linear term for time, 2-way interactions, and 3-way interaction	Statistical test showed potential autocorrelation for total alcohol sales Similar findings as for total alcohol sales were reported for wine sales ($P > 0.05$); the linear trend for the change in alcohol sales over time did not differ between Quebec and Ontario for total alcohol sales or for wine sales (both $P > 0.05$) Beer was available in Quebec grocery stores before June 1978 The number of retail outlets for wine sales in Quebec increased from 353 to ~9000

Table 5.1 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Mulford and Fitzgerald (1988) Iowa, USA Adult population aged ≥ 18 years Survey periods: February–April 1985, April 1986 Sales data period: July 1983–August 1987	Interrupted time series and repeated cross-sectional survey Monthly per capita sales of alcohol and probability sample of self-reported population surveys (n = 1007 aged ≥ 18 years in February–April 1985, and n = 1000 aged ≥ 18 years in April 1986) Method not reported	Iowa legalized the private wholesale and retail sale of bottled wine on 1 July 1985, making wine available in grocery and convenience stores No control or comparison group	Alcohol sales per capita per month (centilitres of ethanol) February–March 1985: 58.8 March–April 1986: 61.8 $P > 0.05$ Self-reported alcohol consumption in the past 30 days (centilitres of ethanol) February–April 1985: 19.8 April 1986: 19.2 $P > 0.05$ Prevalence of self-reported consumption in the past 30 days February–April 1985: 53.6% April 1986: 55.8% $P > 0.05$	1986 survey weighted to match the 1985 baseline survey distribution on status of alcohol consumption, education, and marital status	Seasonality not fully accounted for Federal excise taxes on spirits effective 1 October 1985 may have attenuated the effect of the privatization The number of off-premises outlets for wine sales increased from 214 in June 1985 to ~800 by September 1985 [The authors disclosed that this study was supported by the alcohol industry]
Holder and Wagenaar (1990) USA Total population January 1968–March 1989	Interrupted time series Monthly total volume of alcohol sales to licenced retail establishments from the Distilled Spirits Council of the United States (spirits), the Bureau of Alcohol, Tobacco, and Firearms, United States Department of the Treasury (wine), and the Beer Institute (beer) ARIMA	Iowa closed all state liquor stores on 28 February 1987 and privatized the retail sales of distilled spirits on 1 March 1987 but retained its state government monopoly on wholesale of distilled spirits Control: all other states in the USA	Percentage change (95% CI) in alcohol sales per month (litres of ethanol) from January 1968–February 1987 to March 1987–March 1989 Distilled spirits: 9.5% (4.4% to 14.9%) Wine: –12.1% (–19.3% to –4.3%) Beer: 1.3% (–2.6% to 5.4%)	National covariates for beverage-specific sales in all other states and stocking effects	After all state liquor stores in Iowa were closed, annual total alcohol sales increased by 24 000 L of ethanol (no statistical test), but there were no changes in alcohol sales in the 6 bordering states

Table 5.1 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Wagenaar and Holder (1991) USA Total population 1968–1987	Interrupted time series Data on monthly quantities of wine distributed and monthly sales of beer and distilled spirits in each state ARIMA	End of state government monopolies on retail sales of wine in West Virginia (1 July 1981) and Iowa (1 July 1985, and by March 1987 almost all state retail wine stores were closed; end of state government monopoly on wholesale of wine on 1 July 1986) Control: all other states in the USA	Percentage change (90% CI) in alcohol sales per month (litres of beverage) **West Virginia: from January 1968–June 1981 to July 1981–December 1987** Wine: 48.2% (35.9% to 61.5%) Beer: 12.0% (8.3% to 15.8%) Distilled spirits: –13.8% (–19.7% to –7.5%) **Iowa: from January 1968–June 1985 to July 1985–December 1987** Wine: 93.0% (72.7% to 115.7%) Beer: –3.1% (–7.3% to 1.2%) Distilled spirits: –5.4% (–9.3% to –1.4%)	Nationwide alcohol sales minus the sales in the state of interest, long-term trend, regular cycles within each state, and stocking effects	The privatizations resulted in increases in monthly total alcohol sales (litres of ethanol) for both West Virginia (by 25 235 L) and Iowa (by 28 602 L); no statistical test There were no changes in wine sales in bordering states In the 12 months after privatization in West Virginia, 933 licences were issued, representing new wine outlets added to the existing 165 state stores and agencies
Fitzgerald and Mulford (1992) Iowa, USA Adults aged ≥ 18 years (wave 1, n = 1007; wave 2, n = 1000; wave 3, n = 1025) Survey periods: February–April 1985, April 1986, and April 1989	Repeated cross-sectional survey Alcohol consumption from 3 waves of Iowa survey data from non-institutional adults recruited using probability sampling; non-response rates were similar in all 3 surveys (29%, 32%, and 28%) Yates corrected chi-squared test	Iowa privatized wholesale and retail sale of bottled wine (1 July 1985) and retail sale of bottled spirits (1 March 1987) No control or comparison group	Prevalence of heavy alcohol consumption in the past 30 days (average of ≥ 10 cL of ethanol per day) February–April 1985: 0.4% April 1986: 0.5% April 1989: 0.8% $P > 0.05$ for all pairwise comparisons	The 1986 survey was weighted to match the 1985 survey on education level, family status, and alcohol consumption status; the 1989 survey was weighted to match the 1985 survey on education level and family status	Seasonality not fully accounted for The prevalence of ever consumption of alcohol decreased from 75% in the February–April 1985 survey to 63% in the April 1986 survey The privatizations resulted in a rapidly increasing number of off-premises wine and spirits outlets, extended hours of sales, and the possibility to purchase on credit terms; approximately 2 months after private retail spirits sales were legalized, Sunday sales and advertising were allowed [The authors disclosed support from alcohol industry interests in another paper, but not in this paper]

Table 5.1 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Wagenaar and Holder (1995) USA Total population 1968–1991	Interrupted time series Monthly alcohol wholesale shipments from the Distilled Spirits Council of the United States (spirits), the Bureau of Alcohol, Tobacco, and Firearms, United States Department of the Treasury (wine), and the Beer Institute (beer) ARIMA	Elimination of state government monopolies on the retail sales of table wine in Maine (1 January 1971), Idaho (July 1971), New Hampshire (August 1978), Montana (October 1979 for wine with ≤ 14% alcohol by volume; extended to wine with ≤ 16% alcohol by volume in 1985), and Alabama (October 1980; 3 counties allowed wine sales in private stores from October 1973) Controls: all other states in the USA	Percentage change (95% CI) in alcohol sales per month (litres of ethanol) **Alabama: from January 1968–September 1980 to October 1980–December 1991** Wine: 42.0% (13.4% to 77.7%) Beer: –7.4% (–14.7% to 0.6%) Spirits: –5.0% (–9.7% to 0.1%) **Idaho: from January 1968–June 1971 to July 1971–December 1991** Wine: 150.1% (129.2% to 172.9%) Beer: 9.5% (–7.0% to 28.8%) Spirits: 6.8% (–0.3% to 14.4%) **Maine: from January 1968–December 1970 to January 1971–December 1991** Wine: 136.7% (112.6% to 163.5%) Beer: 3.2% (–5.5% to 12.7%) Spirits: –1.9% (–7.3% to 4.0%) **Montana: from January 1968–September 1979 to October 1979–December 1991** Wine: 75.3% (56.9% to 96.0%) Beer: –4.9% (–13.0% to 4.0%) Spirits: –4.4% (–14.0% to 6.4%) **New Hampshire: from January 1969–July 1978 to August 1978–December 1991** Wine: 13.0% (1.2% to 26.2%) Beer: –2.2% (–10.5% to 6.8%) Spirits: –1.2% (–7.6% to 5.7%)	Nationwide alcohol sales minus the sales in the state of interest, long-term trend, regular cycles within each state, and stocking effects	No information on the change in total alcohol sales Increases in wine sales after the privatizations were unlikely to be due to shifts in sales from bordering states, where the change in wine sales ranged from –3.3% to 10.8% (each $P > 0.05$) The privatization in Idaho caused a dramatic increase in the promotion of wine in the state

Table 5.1 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Mäkelä (2002) Finland Adult population aged ≥ 15 years 1968–1969	Interrupted time series Data on annual alcohol sales and deliveries from the government alcohol monopoly; sales of medium-strength beer consisted of sales by government monopoly stores (including to licenced restaurants) until 1968 and deliveries from breweries to outlets from 1969 Descriptive analysis	In 1969, Finland introduced medium-strength beer (≤ 4.7% ethanol by volume) in grocery stores and cafés, and government alcohol monopoly stores were permitted in rural areas; beer restaurants obtained more extensive licences No control or comparison site	Percentage difference in recorded alcohol consumption per capita (litres of ethanol) between 1968 and 1969 46%		The results are descriptive, and data on the consumption of medium-strength beer in 1969 also include the stock of licenced establishments Compared with 1968, in 1969 the consumption of beer increased by 125%, of medium-strength beer increased by 242%, and of distilled spirits increased by 12% In 1969, the number of licenced restaurants increased by 46% and the number of government monopoly stores increased by 22%
Trolldal (2005a) Canada Adult population aged ≥ 15 years 1950–2000	Interrupted time series Annual wholesale alcohol sales from Statistics Canada ARIMA	Privatization of retail alcohol sales in Alberta: beer (1974) and wine (1985) allowed in private stores, cold beer stores authorized (1989), hotels in rural areas (1990) and non-rural areas (1992) allowed to sell wine and spirits for off-premises consumption, and all liquor stores privatized (5 March 1994) Controls: all other Canadian provinces	Difference in the natural logarithm of alcohol sales per capita per year (litres of ethanol) for total privatization of retail sales compared with no privatization $\beta = 0.05$ (SE = 0.04; $P \geq 0.10$)	Disposable income, alcohol price, 1980 strike in publicly owned liquor stores, and alcohol sales in the rest of Canada	For each year, the extent to which retail sales of alcohol were privatized was a variable that ranged from 0 (for no privatization) to 1 (for total privatization of all types of alcoholic beverages) Because privatization may affect alcohol price, controlling for alcohol price may result in underestimation of the effect of privatization on alcohol sales Alcohol wholesale remained monopolized, and no sales were allowed in grocery stores Spirits sales increased by 12% ($P < 0.01$), wine sales decreased by 1% ($P \geq 0.10$), and beer sales increased by 1% ($P \geq 0.10$) Opening hours were generally extended during the privatization, and the number of off-premises spirits or wine outlets almost tripled between 1989 and 1994

Table 5.1 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Trolldal (2005b) Canada Adult population aged ≥ 15 years 1950–2000	Interrupted time series Annual wholesale alcohol sales from Statistics Canada ARIMA	Privatization of retail wine sales in Quebec: grocery stores allowed to sell wine produced in Quebec or bottled by the government (1978), extended to wine bottledin Quebec (1983) and to larger grocery stores (1984) Controls: all other Canadian provinces	Difference in the natural logarithm of alcohol sales per capita per year (litres of ethanol) 1978 intervention: from 1950–1977 to 1978–2000 $\beta = 0.01$ (SE = 0.02; $P \geq 0.10$) 1983–1984 intervention: from 1950–1982 to 1983–2000 $\beta = 0.04$ (SE = 0.03; $P \geq 0.10$)	Alcohol price; disposable income; store strikes in 1964, 1968, and 1979; other wine sale policy interventions; and alcohol sales in the rest of Canada	Because privatization may affect alcohol price, controlling for alcohol price may result in underestimation of the effect of privatization on alcohol sales Policy intervention in 1978: 10% increase in wine sales ($P < 0.01$) and 1% decrease in spirits sales ($P > 0.10$); policy intervention in 1983–1984: 1% decrease in wine sales ($P > 0.10$) and 8% increase in spirits sales ($P > 0.10$); no change in beer sales Allowing grocery stores to sell wine resulted in the number of stores that sold wine increasing from 353 to ~9000
Świątkiewicz et al. (2014) Poland Adult population aged ≥ 15 years 1961–2008	Interrupted time series Recorded alcohol consumption data for Poland from WHO GISAH (WHO, 2024a) Method not reported	Privatization of bulk sales of alcohol in 1990 No control or comparison site	Difference in annual APC between 1961–1989 and 1990–2008 9% ($P < 0.10$)	Not reported	The extent to which changes in other alcohol policy interventions during the same period may have influenced the results is unclear

Table 5.1 (continued)

Reference Location Study population Study period	Study type Data source Method of analysis	Policy intervention Control or comparison groups	Outcome Effect estimates	Covariates	Comments
Barnett et al. (2020) Washington State (intervention) and 10 other states (control), USA Household sample from metropolitan and surrounding areas 2010–2014	Repeated cross-sectional survey Data on household alcohol purchases registered using an in-home barcode scanner (Nielsen Consumer Panel Dataset) for 510 276 household-months Negative binomial regression (DiD)	Privatization of retail sales and distribution of liquor in Washington State was completed on 1 June 2012; taxes and fees equalling 10% of the wholesale price and 17% of the retail price were introduced Controls: 10 states with government alcohol monopolies (Alabama, Idaho, Mississippi, Montana, New Hampshire, North Carolina, Ohio, Oregon, Vermont, and Virginia)	Difference in the change in household alcohol purchases per month (ounces of ethanol) between Washington State and control states from January 2010–May 2012 to June 2012–December 2014 DiD = 2.01 (95% CI, 0.64 to 3.38)	Income, employment levels, age group, marital status, having children in the household, racial group, Hispanic ethnicity, time trends, annual unemployment rates, and period of decriminalization and legal retail of marijuana	Household purchases did not include on-premises alcohol consumption After the privatization, in households in Washington State, liquor (82%) and beer (12%) purchases increased and wine purchases decreased ([–51%]) The DiD corresponded to 3.35 standard drinks per household per month (1 standard drink = 0.6 ounces [17.7 mL] of ethanol) Sensitivity analysis of households included throughout 2011–2013 showed similar results overall (DiD, 1.56; 95% CI, 0.68 to 2.44) and among households in the low stratum of alcohol purchases (< 5.25 standard drinks per adult per week) and middle stratum of alcohol purchases (> 5.25 to 10.50 standard drinks per adult per week); the privatization was associated with decreased purchases in households in the highest stratum of alcohol purchases (> 10.5 standard drinks per adult per week before the privatization) Privatization resulted in liquor prices increasing by an average of 15.5%, an almost 5-fold increase in the number of off-premises liquor sales outlets, and an almost doubling of the trading hours

APC, total adult alcohol per capita consumption; ARIMA, autoregressive integrated moving average; CI, confidence interval; DiD, difference in difference; GISAH, Global Information System on Alcohol and Health; NR, not reported; SE, standard error; WHO, World Health Organization.

models adjusted for alcohol price but because privatization may affect alcohol price, controlling for alcohol price may result in underestimation of the effect of privatization on alcohol sales, and that alcohol consumption was based on alcohol wholesale data only.]

Mulford and Fitzgerald (1988) assessed the effect of allowing the private wholesale and retail sale of bottled wine in Iowa, USA (1 July 1985) on alcohol sales per capita per month (centilitres of ethanol) in February–March 1985 compared with March–April 1986 using population-level data, and on self-reported amount of alcohol consumption in the past 30 days (centilitres of ethanol) and prevalence of alcohol consumption in the past 30 days in February–April 1985 (1007 adults in Iowa) compared with April 1986 (1000 adults in Iowa) using individual level-data. There was little difference in total alcohol sales for the 2-month period in 1985 (58.8 cL) compared with the 2-month period in 1986 (61.8 cL) ($P > 0.05$). Similarly, there was little difference in self-reported amount of alcohol consumption in the past 30 days (from 19.8 cL in February–April 1985 to 19.2 cL in April 1986; $P > 0.05$) and in the prevalence of alcohol consumption in the past 30 days (from 53.6% in February–April 1985 to 55.8% in April 1986; $P > 0.05$). [The authors disclosed that this study was supported by the alcohol industry. The strength of this study is that both population-level and individual-level data were reported for the same intervention and study period. The limitations of the study are that there was no control or comparison group, that the statistical analysis was not described, and that the study did not fully account for seasonal variation in alcohol consumption.]

In a later study, Fitzgerald and Mulford (1992) used data from the sample of 1007 adults in Iowa who completed the February–April 1985 survey mentioned above and another sample of 1025 adults in Iowa who were interviewed in April 1989 to assess the effects of privatizing both the wholesale distribution and the retail sale of bottled wine in July 1985 and the retail sale of bottled spirits in March 1987 on the prevalence of heavy alcohol consumption (average of ≥ 10 cL of ethanol per day) in the past 30 days. The prevalence of heavy alcohol consumption in the past 30 days was 0.4% in February–April 1985 and 0.8% in April 1989 ($P > 0.05$). [The strength of this study is the assessment of the effects of two privatizations. The limitations of the study are that only the prevalence of heavy alcohol consumption was assessed, that there was no control or comparison group, that the study did not fully account for seasonal variation in alcohol consumption, and that the authors disclosed support from alcohol industry interests in another paper, but not in this paper.]

Holder and Wagenaar (1990) assessed the effects of closing all state liquor stores on 28 February 1987 and privatizing the retail sales of distilled spirits on 1 March 1987 in Iowa in a time-series analysis of monthly beer, wine, and spirits sales (litres of ethanol) during 1968–1989. After all state liquor stores were closed and state retail sales of distilled spirits were privatized, annual total alcohol sales increased by 24 000 L of ethanol. In beverage-specific analyses, monthly spirits sales increased (9.5%; 95% confidence interval [CI], 4.4% to 14.9%), monthly beer sales increased (1.3%; 95% CI, –2.6% to 5.4%), and monthly wine sales decreased (–12.1%; 95% CI, –19.3% to –4.3%). Purchase data in bordering states showed no substantial changes in cross-border shopping. [The strengths of this study are the inclusion of 22 years of monthly sales data covering multiple years before and after the privatization; the control for general trends in national data; the use of ARIMA models to account for underlying trends, autocorrelation, seasonality, and stocking effects; and the analysis of bordering states for effects of cross-border shopping. The limitations of the study are that there was no statistical testing for total alcohol consumption and that alcohol consumption was based on recorded alcohol sales data only.]

Wagenaar and Holder (1991) assessed the effects of privatizing wine sales in West Virginia (in July 1981) and in Iowa, USA (in July 1985) on monthly alcohol sales (litres of beverage) during 1968–1987. In beverage-specific analyses that adjusted for national trends, in West Virginia, from January 1968–June 1981 to July 1981–December 1987, wine sales increased (48.2%; 90% CI, 35.9% to 61.5%), beer sales increased (12.0%; 90% CI, 8.3% to 15.8%), and distilled spirits sales decreased (–13.8%; 90% CI, –19.7% to –7.5%). In Iowa, from January 1968–June 1985 to July 1985–December 1987, wine sales increased (93.0%; 90% CI, 72.7% to 115.7%), distilled spirits sales decreased (–5.4%; 90% CI, –9.3% to –1.4%), and beer sales did not change substantially (–3.1%; 90% CI, –7.3% to 1.2%). Monthly total alcohol sales (litres of ethanol) increased by 25 235 L per month in West Virginia and by 28 602 L per month in Iowa. [The strengths of this study are the assessment of the privatization of wine sales that occurred in two states, the fact that models controlled for general trends in national data, and the use of ARIMA models to account for underlying trends, autocorrelation, seasonality, and stocking effects. The limitations of the study are that alcohol consumption was analysed primarily by alcohol type, that there was no statistical testing for total alcohol consumption, and that alcohol consumption was based on recorded alcohol sales data only.]

Wagenaar and Holder (1995) also assessed the percentage changes in monthly state alcohol sales (litres of ethanol) by type of alcoholic beverage during 1968–1991 resulting from the elimination of five state government monopolies on the retail sales of table wine in the USA. Compared with trends in other states, there were no major changes in the monthly sales of beer or spirits after the privatization of the retail sales of table wine. The monthly sales of wine increased in all five states after the policy change in Alabama in 1980 (42.0%; 95% CI, 13.4% to 77.7%), in Idaho in 1971 (150.1%; 95% CI, 129.2% to 172.9%), in Maine in 1971 (136.7%; 95% CI, 112.6% to 163.5%), in Montana in 1979 (75.3%; 95% CI, 56.9% to 96.0%), and in New Hampshire in 1978 (13.0%; 95% CI, 1.2% to 26.2%). [The strengths of this study are the inclusion of 24 years of monthly sales data covering multiple years before and after the five statewide policy interventions that occurred over 10 years, the fact that models controlled for general trends in national data, and the use of ARIMA models to account for underlying trends, autocorrelation, seasonality, and stocking effects. The limitations of the study are that alcohol consumption was analysed only by type of alcoholic beverage and was based on recorded alcohol sales data only.]

In 1969, Finland introduced medium-strength beer (≤ 4.7% ethanol by volume) in grocery stores and cafés, and government alcohol monopoly stores were permitted in rural areas; beer restaurants obtained more extensive licences. Based on annual alcohol consumption data from 1950–1975 in Finland (Österberg, 1979), Mäkelä (2002) assessed the percentage difference between 1968 and 1969 in recorded alcohol consumption per capita per year (litres of ethanol). Compared with 1968, recorded total alcohol consumption was 46% higher in 1969. This difference was driven by 125% higher beer consumption and 12% higher spirits consumption in 1969 compared with 1968. [The strength of this study is the inclusion of data on multiple types of alcoholic beverages. The limitations of the study are that the results were descriptive, that there was no geographical control for comparison, that data on the consumption of medium-strength beer in 1969 also included the stock of licensed establishments, and that alcohol consumption was based on recorded alcohol consumption data only.]

Trolldal (2005a) assessed the effects of the privatization of retail sales of alcohol between 1974 and 1994 in Alberta, Canada, on annual wholesale total alcohol sales per capita (litres of ethanol) during 1950–2000 compared with sales

in the rest of Canada, where provincial government monopolies continued. The extent to which retail sales of alcohol were privatized in Alberta for each year was a variable that ranged from 0 for no privatization to 1 for total privatization of all types of alcoholic beverages. Total privatization was associated with a 5% ($P \geq 0.10$) increase in total alcohol sales. In beverage-specific analyses, total privatization was associated with a 12% ($P < 0.01$) increase in sales of spirits, but there was no substantial change in the sales of other major types of alcoholic beverages (all $P \geq 0.10$). [The strengths of this study are the inclusion of 50 years of annual sales data covering multiple years before and after the policy intervention; the adjustment for general trends in national data, income, and other potential confounders in the model; and the use of ARIMA models to account for underlying trends and autocorrelation. The limitations of the study are that the model adjusted for alcohol price but because privatization may affect alcohol price, controlling for alcohol price may result in underestimation of the effect of privatization on alcohol sales, and that alcohol consumption was based on alcohol wholesale data only.]

In an interrupted time-series study from the Alcohol Measures for Public Health Research Alliance (AMPHORA) project, Świątkiewicz et al. (2014) assessed the effect of the privatization of bulk sales of alcohol in Poland in 1990 on the difference in the average total adult alcohol per capita consumption (APC) during 1961–2008. There was a 9% increase in the APC after the privatization of bulk sales of alcohol ($P < 0.10$). [The strength of this study is the inclusion of 48 years of annual alcohol consumption data covering multiple years before and after the privatization of bulk sales of alcohol. The limitations of the study are that there was no geographical control, that the changes in the political system and other alcohol policy interventions during the same period may also have influenced the change in alcohol consumption, and that the control variables in the model were not reported.]

Barnett et al. (2020) assessed the effects of the privatization of the retail sale and distribution of liquor in Washington State in 2012 on the difference between Washington and 10 control states that maintained their monopoly in the change in average monthly household alcohol purchases (ounces of ethanol) from January 2010–May 2012 to June 2012–December 2014. Data on household alcohol purchases were obtained from a repeated consumer purchasing survey using in-home barcode scanners (the Nielsen Consumer Panel Dataset). After the privatization, the average monthly household ethanol purchases in Washington State increased by 2.01 ounces (95% CI, 0.64 to 3.38 ounces) [59.4 mL (95% CI, 18.9 to 100 mL], which corresponded to a 26% increase, compared with the change in the 10 other states. In beverage-specific analyses, there was an 82% increase in spirits purchases ($P < 0.01$), a 12% increase in beer purchases ($P = 0.04$), and a [51%] decrease in wine purchases ($P > 0.05$) in Washington State compared with the 10 other states. The sensitivity analysis of the subset of households with continued participation in 2011–2013 showed similar results overall and among households in the low or middle stratum of alcohol purchases before the privatization, but among households in the highest stratum of alcohol purchases, monthly purchases decreased ($P < 0.05$). [The strengths of this study are the inclusion of households from 10 states that maintained their alcohol monopolies as a control group, the adjustment for income and other control variables in the models, and the sensitivity analysis of households with continuous data included throughout 2011–2013. The limitation of the study is that the purchase data came from off-premises alcohol purchases, which did not include on-premises consumption.]

5.2.3 The Nordic government alcohol monopolies

The five Nordic countries (i.e. Denmark, Finland, Iceland, Norway, Sweden) share a history of substantial alcohol consumption, a pattern that is also common in eastern Europe and that used to be dominated by relatively heavy consumption of spirits on weekends and little alcohol consumption on weekdays. There were strong temperance movements in all the Nordic countries except Denmark. As an alternative to alcohol prohibition, these countries established government monopolies to control off-premises alcohol sales, and by the 1920s there were limited numbers of retail outlets and limited opening hours (Room, 2002). On-premises alcohol sales also were subject to restrictions.

Liberalizations in the alcohol policy systems in the Nordic countries began in the 1960s. There has been research on the effects of these liberalizations on alcohol consumption, which was typically presented in reports of national alcohol sales data rather than in peer-reviewed research papers (Room, 2002). For example, similar to the study on the introduction of medium-strength beer in grocery stores in Finland (Mäkelä, 2002) (see Section 5.2.2), the 1965 introduction of the sale of medium-strength beer in grocery stores in Sweden led to a 15% increase in total alcohol sales between 1961–1965 and 1965–1977 ($P < 0.01$); subsequently, after sales of medium-strength beer in grocery stores were withdrawn in 1977, total alcohol sales decreased by 15% between 1965–1977 and 1977–1980 ($P < 0.01$) (Noval and Nilsson, 1984).

The gradual liberalization of these government alcohol monopoly systems was accelerated by conditions imposed by the entry of Finland and Sweden into the European Union in 1995. Although the government alcohol monopolies were able to keep their off-premises retail stores, both countries were forced to abandon their government monopolies at the production, importation, and wholesale levels. In particular, the government alcohol monopoly system in Finland was greatly reduced from its position as a "state within a state". Until this change, alcohol policy had been dealt with primarily at the national level, with government monopolies often funding the research. In succeeding years, efforts to reduce alcohol-attributable harms were increasingly under the control of the local and municipal levels (Karlsson and Tigerstedt, 2004). For example, the Local Alcohol Policy programme, a community-based project in Finland, led to changes in the environment in which alcohol is consumed and grassroots enforcement of legislation, which resulted in the reduction of the availability of alcohol to minors and customers who are intoxicated (Holmila and Warpenius, 2013). Similarly, Nilsson et al. (2020), noting that "in the past 15 years Sweden has placed increasing emphasis on local alcohol prevention initiatives", found that these increases were "associated with a decrease in alcohol consumption and in alcohol-related mortality". In an analysis from the Nordic countries of "who drinks more and less" after 9 changes in the hours and days of sale and 15 changes in the range of alcoholic beverages distributed through national government monopolies in the Nordic countries over 50 years, individuals who consumed large amounts of alcohol (and who accounted for the majority of alcohol consumption) were the most likely to change their alcohol consumption (Mäkelä et al., 2002).

5.3 Other coordinated multiple alcohol policy interventions in selected countries

An alternative approach to government alcohol monopolies in countries with increasing alcoholic beverage consumption (and alcohol-attributable harms) is the enactment and implementation of specific coordinated multiple alco-

hol policy interventions (defined in Section 5.1), hereafter referred to as coordinated interventions. These coordinated interventions have often included the WHO "best buy" alcohol policy interventions (WHO, 2017). For some countries, there is evidence on the effects of implemented coordinated interventions on changes in alcohol consumption. In this section, the Working Group reviewed and evaluated the associations reported for Estonia, Lithuania, the Russian Federation, and Thailand (Table 5.2).

When assessing the evidence on the associations of coordinated interventions with changes in alcoholic beverage consumption, the Working Group carefully considered other secular changes in the population that could affect the association. For example, the 2008–2009 global recession had substantial effects on consumer spending and therefore could lead to a reduction in the APC in the population. In addition, to better understand the effects of the coordinated interventions on changes in alcohol consumption, when data were available, the trends in consumption before and after the periods of the implementation of coordinated interventions, the trends during the implementation, and the trends in surrounding countries (i.e. geographical controls) were considered (Fig. 5.1). Finally, the Working Group did not quantify differences or disentangle the effects of individual interventions from those of the coordinated interventions.

5.3.1 Estonia

(a) Recent history of coordinated interventions

After Estonia regained independence from the Soviet Union in 1991, social changes in the country led to a free-market ideology, which led to increases in alcoholic beverage consumption and marked increases in alcohol-attributable harms (e.g. decreases in life expectancy), followed by the development, enactment, and implementation of alcohol policies (Moskalewicz and Simpura, 2000; Stoppel et al., 2024). Although various alcohol policy interventions were implemented in Estonia during the first decade of the 21st century, alcohol consumption continued to increase until the 2008–2009 global recession.

In the next decade, influenced by the 2010 WHO Global Strategy to Reduce the Harmful Use of Alcohol (WHO, 2010), Estonia sought to develop and implement a more coherent strategy for reducing alcohol consumption and alcohol-attributable harms. After consultations with many national stakeholders, a strategy was drafted in 2012 and summarized in the Green Paper on Alcohol Policy (GP) (Estonian Ministry of Social Affairs, 2014). The GP was approved by the Estonian government in 2014. In reaction to high levels of alcohol consumption in the general population (Estonia was consistently ranked in the 90th percentile of APC globally; WHO, 2004, 2014), targets 1 and 2 of the GP explicitly aimed to lower the APC in the Estonian population. The three targets of the GP were (1) to permanently reduce the APC to 8 L of ethanol per year, (2) to increase the age of initiation of alcohol consumption and to reduce the prevalence of alcohol consumption and intoxication in adolescents, and (3) to reduce alcohol-attributable injuries and injury fatalities (WHO Regional Office for Europe, 2024). The GP also provided suggestions for a wide range of activities intended to reduce alcohol consumption and alcohol-attributable harms, the first of which was implemented in 2013. The specific alcohol policy interventions that were implemented are listed in Table 5.2. For detailed background information on alcohol control policies in Estonia in the 21st century, see Stoppel et al. (2024) and Pärna (2020).

The core period of the GP lasted from the beginning of 2013 until the end of 2019, which was the last full year before the onset of the COVID-19 pandemic. There is evidence that decreases in the APC during this period were driven, at least in part, by increases in alcohol excise taxes, which decreased the affordability of alcoholic beverages in 2016–2018 (Rehm

Table 5.2 Effects of national coordinated alcohol policy interventions on alcoholic beverage consumption

Country Reference Study population Study period	Alcohol policy	Study type	Period or category	Outcome(s) Measure or effect estimate		Covariates and/or comments
Estonia						
WHO Regional Office for Europe (2024) APC: adult population aged ≥ 15 years 2001–2022 HBSC study surveys: adolescents aged 11, 13, and 15 years 2009–2010, 2013–2014, 2017–2018, 2021–2022	Green Paper on Alcohol Policy (core period 2013–2019): increase excise taxes, penalties for making alcohol available to minors, penalties and tax on illegal alcohol, prohibit alcohol consumption in public places, prevent sales of alcohol-containing cosmetic or other products, develop a health-care system for treatment and rehabilitation of alcohol dependence and for early detection and counselling, restrict alcohol advertising and sponsorship, limit alcohol sales in retail stores to separate areas, and actions related to drink-driving, education to reduce harms, monitoring, and evaluations 2019: excise taxes decreased by 25%	APC: time series Descriptive	Period	Mean (SD) APC (litres)	Mean (SD) difference in APC from the previous year (litres) during the period	2001–2012: mean annual APC and difference in APC from the previous year excludes 2009 (because of the financial crisis)
			2001–2012	12.39 (1.33)	0.47 (0.84)	
			2013–2019	10.63 (0.64)	−0.25 (0.39)	
			2020–2022	11.02 (0.19)	0.27 (0.15)	
		HBSC: repeated cross-sectional study	Survey years	Prevalence of alcohol consumption, OR (95% CI)		
			2009–2010	1.0 (Ref.)		ORs adjusted for age and sex
			2013–2014	0.48 (0.44 to 0.54)		
			2017–2018	0.31 (0.28 to 0.34)		
			2021–2022	0.22 (0.20 to 0.24)		
Lithuania						
WHO (2024b) Adult population aged ≥ 15 years 2000–2020	Coordinated interventions implemented in 2008–2009 and in 2017–2018: tax increases that reduced the affordability of alcoholic bever-ages, availability restrictions that affected off-premises trading hours, and bans on alcohol advertising on television, radio, and the Internet	Time series	Period	Mean (SD) APC (litres)	Mean (SD) difference in APC from previous year (litres) during the period	
			2001–2007	[12.99 (1.15)]	[0.50 (0.25)]	
			2008–2010	[13.75 (0.81)]	[−0.55 (0.46)]	
			2011–2016	[14.86 (1.07)]	[0.047 (1.40)]	
			2017–2019	[11.79 (0.27)]	[−0.50 (0.61)]	
			2020	[12.10 (N/A)]	[0.40 (N/A)]	

Table 5.2 (continued)

Country Reference Study population Study period	Alcohol policy	Study type	Period or category	Outcome(s) Measure or effect estimate		Covariates and/or comments
Russian Federation						
Nemtsov et al. (2019) Adult population aged ≥ 15 years 1980–2016	1985–1988: anti-alcohol campaign in which the government controlled alcohol production, distribution, and sales through the government monopoly, reduced the production of alcoholic beverages, increased prices, limited sales hours and availability, and expanded services for the treatment of alcohol use disorders and public education about alcohol	Time series	Year	APC (litres)	Difference from previous year (litres)	APC was estimated using the method of Nemtsov (2011) The alcohol policy timeline is based on Neufeld et al. (2020a) and WHO Regional Office for Europe (2019)
			1984	18.8	Ref.	
			1985	17.8	[–1.0]	
			1986	15.2	[–2.6]	
			1987	13.6	[–1.6]	
	1990–1994: repeal of government alcohol monopoly and provisions of the anti-alcohol campaign, and loosening of existing interventions		1990	16.5	Ref.	
			1991	16.1	[–0.4]	
			1992	18.5	[2.4]	
			1993	21.7	[3.2]	
			1994	23.6	[1.9]	
	1995–1998: introduction of Federal Law No. 108 and Federal Law No. 171 and key interventions on alcohol licensing, availability, and marketing		1995	21.0	[–2.6]	
			1996	19.2	[–1.8]	
			1997	17.5	[–1.7]	
			1998	17.2	[–0.3]	
	1999–2003: state-owned distillery enterprise formed and alcohol market restructured; few other interventions implemented		1999	17.9	[0.7]	
			2000	19.4	[1.5]	
			2001	20.4	[1.0]	
			2002	21.0	[0.6]	
			2003	21.2	[0.2]	
	2004–2007: key interventions on alcohol advertising, alcohol pricing, drink-driving, and availability of unrecorded alcohol implemented		2004	21.0	[–0.2]	
			2005	20.4	[–0.6]	
			2006	19.3	[–1.1]	
			2007	18.4	[–0.9]	

Table 5.2 (continued)

Country Reference Study population Study period	Alcohol policy	Study type	Period or category	Outcome(s) Measure or effect estimate		Covariates and/or comments
Nemtsov et al. (2019) (cont.)	2008–2009: National Concept to Reduce Alcohol Abuse and Alcohol Dependence in the Population (2010–2020) published, and Federal Service for Alcohol Market Regulation formed; few other interventions introduced		2008 2009	18.1 17.3	[−0.3] [−0.8]	
	2010–2013: key interventions on alcohol pricing, availability, and marketing and drink-driving legislation introduced 2014–2018: temporary loosening and freezing of some interventions, followed by their reinstatement		2010 2011 2012 2013 2014 2015 2016	16.6 16.2 16.0 15.5 15.9 15.0 14.6	[−0.7] [−0.4] [−0.2] [−0.5] [0.4] [−0.9] [−0.4]	2011–2012: increased alcohol excise taxes and minimum prices for spirits decreased the affordability of alcoholic beverages
Radaev et al. (2020) Russia Longitudinal Monitoring Survey: 34 514 individuals aged 14–80 years 2006–2017	After 2011, alcohol sales were banned between 23:00 and 08:00 and in medical, educational, sports, and cultural facilities and in all public places; advertisements for beer were prohibited on television and radio and in public spaces; zero tolerance for alcohol when driving was confirmed; from 2011 to 2017, > 2-fold increase in excise tax rates for strong spirits and beer was implemented, which decreased the affordability of alcoholic beverages	Repeated cross-sectional study	Year(s) 2006 2017 2006 2017 2006–2011/ 2012–2017 2006–2011/ 2012–2017	Prevalence of abstention (descriptive analyses) **Women:** 27.7% 44.1% **Men:** 18.7% 33.7% Abstention prevalence ratio (multivariable analyses) **Women:** $\beta = 0.081$ ($P < 0.05$) **Men:** $\beta = 0.14$ ($P < 0.01$)		From 2006 to 2017, the prevalence of abstention increased the most among individuals aged 14–25 years (from 35.5% to 62.3% among women, and from 29.7% to 61.6% among men) Covariates in multivariable analysis for the difference in the prevalence of abstention and for the percentage change in the natural log of the amount of alcohol consumed: age, 10-year birth cohort, per capita income, education level, marital status, composition of household, body weight, ethnicity, residence type, regional per capita income, and regional climate

Table 5.2 (continued)

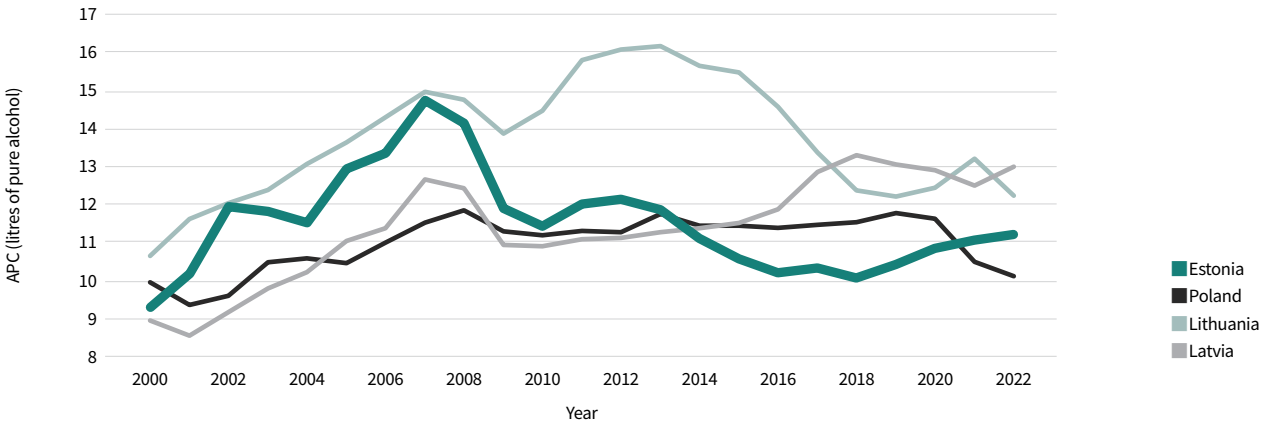
Country Reference Study population Study period	Alcohol policy	Study type	Period or category	Outcome(s) Measure or effect estimate		Covariates and/or comments
Radaev et al. (2020) (cont.)				Percentage change in the amount of alcohol consumed (descriptive analyses)		
			2006 to 2017	–35%		
				Difference in the natural log of the amount of alcohol consumed in the past 30 days per capita (grams of ethanol) between the 2 periods (multivariable analyses)		
			2006–2011 and 2012–2017	**Women:** $\beta = -0.063$ ($P < 0.001$)		
			2006–2011 and 2012–2017	**Men:** $\beta = -0.0996$ ($P < 0.001$)		
Salagay et al. (2021) Adult population aged ≥ 15 years 2008–2020	2010–2020: National Concept to Reduce Alcohol Abuse and Alcohol Dependence in the Population	Time series	Year	APC (litres)	Percentage change in APC, 2008 to 2019	APC estimated by the Central Research Institute of Alcohol and Biotechnology of the Ministry of Health of the Russian Federation
			2008	15.7		
			2019	9.1	–42%	
Kolosnitsyna (2024) Adult population aged ≥ 15 years 2010–2014 and 2017–2021	2010–2014: key interventions: national night ban on alcohol sales, increases in excise taxes and minimum prices on vodka and excise taxes on beer 2015–2021: loosening of alcohol policy interventions: decrease in minimum prices on vodka in 2015 and freezing of excise tax rates in 2015–2016 and in 2017–2019; no new regional restrictions on hours of sales	Time series	Year	Recorded APC (litres)	Difference in recorded APC from previous year (litres)	[Recorded APC was estimated by the author based on official sales data from the Federal State Statistics Service of the Russian Federation and provided by the author to the Working Group]
			2010	10.7		
			2014	9.3	[–1.4]	
			2017	7.2		
			2021	7.7	[0.5]	

Table 5.2 (continued)

Country Reference Study population Study period	Alcohol policy	Study type	Period or category	Outcome(s) Measure or effect estimate		Covariates and/or comments
Thailand						
Assanangkornchai et al. (2020) Students aged 13–17 years (in 2007: *n* = 50 033, 49.9% female; in 2016: *n* = 38 535, 49.8% female) 2007 and 2016	Alcoholic Beverage Control Act of 2008: increasing the minimum legal purchase age from 18 years to 20 years, banning alcohol sales and consumption in places frequented by young people, and [comprehensively] banning alcohol advertisements [including at specific times of the day in broadcast media]	Repeated cross-sectional survey Response rates: > 95% in both years	Lifetime alcohol consumption, prevalence ratio (2016/2007) Girls: 2.1 (95% CI, 1.8 to 2.5) Boys: 1.4 (95% CI, 1.2 to 1.5) 12-month alcohol consumption, prevalence ratio (2016/2007) Girls: 1.9 (95% CI, 1.6 to 2.2) Boys: 1.2 (95% CI, 1.1 to 1.3)			The National Alcohol Policy Strategy of 2011–2020 included targets for preventing initiation and reducing the prevalence of alcohol consumption among adolescents aged 15–19 years Covariates: type of school, grade level in survey year, and sex
Rehm et al. (2024c) Adult population aged ≥ 15 years (control country: Viet Nam) 2010–2019	In Thailand, the Alcoholic Beverage Control Act of 2008, the Excise Tax Act of 2017, and the Road Traffic Act of 1979 (in 2016, the legal blood alcohol concentration limit was 0.05% for the general population, for professional or commercial drivers, and for young or novice drivers; in 2017, it was revised to 0.02% for young or novice drivers) In Viet Nam, 2 increases in excise tax occurred, in 2016 and 2019	Time series	Year	APC (litres)	Percentage change in APC, 2010 to 2019	APC based on WHO estimates In Thailand, the National Alcohol Policy Strategy of 2011–2020 was certified by the National Health Assembly and approved by the cabinet ministers in 2010 In Viet Nam, the Law on Prevention and Control of Harmful Effects of Alcoholic Beverages was enacted in 2020
				Thailand:		
			2010	7.57		
			2013	8.16		
			2016	8.28		
			2019	7.85	3.7%	
				Viet Nam:		
			2010	7.37		
			2013	8.28		
			2016	9.17		
			2019	9.34	26.8%	

APC, total adult alcohol per capita consumption; CI, confidence interval; HBSC, Health Behaviour in School-aged Children; N/A, not applicable; OR, odds ratio; Ref., reference; SD, standard deviation; WHO, World Health Organization.

Fig. 5.1 Total adult alcohol per capita consumption (APC) in litres of pure ethanol in the three Baltic countries and Poland, 2000–2022



et al., 2022, 2024a). As shown in Fig. 5.1 (WHO Regional Office for Europe, 2024), there were no decreases in the APC in Poland or Latvia during the core period of the GP in Estonia; there were decreases in Lithuania, which also implemented coordinated interventions in 2016–2018 (Rehm et al., 2023a; see also Section 5.3.2). The end of the GP core period in Estonia coincided with several key events, including (i) the Baltic tax war, when Estonia competed with Latvia for tourists from Finland crossing the border to buy low-cost alcoholic beverages, (ii) the reduction of alcohol excise taxes in Estonia by 25% in July 2019, which increased the affordability of alcoholic beverages (Pärna, 2020), and (iii) the onset of the COVID-19 pandemic (WHO Regional Office for Europe, 2024).

(b) Effects of coordinated interventions on alcoholic beverage consumption

The effects of the coordinated interventions implemented during the GP core period in Estonia on the change in alcoholic beverage consumption were assessed by examining trends in the APC for Estonia before, during, and after the core period (WHO Regional Office for Europe, 2024) (Table 5.2). Before the GP core period (excluding 2009 because of the financial crisis), from 2001 through 2012 the mean APC was 12.39 L and there was a trend towards increasing APC (mean difference in APC from the previous year, 0.47 L). During the GP core period (from 2013 through 2019), the mean APC was 10.63 L and there was a trend towards decreasing APC (mean difference in APC from the previous year, −0.25 L). The mean difference in APC from the previous year was significantly different for the GP core period compared with before the GP core period (P = 0.038). During the COVID-19 pandemic (2020–2022), the mean APC was 11.02 L and the APC increased (mean difference in APC from the previous year, 0.27 L). [The strength of this analysis is the assessment of trends in the APC before, during, and after implementation of the GP. The limitations of the analysis are the lack of geographical controls and the lack of control variables for potential confounding factors.]

The effects of coordinated interventions implemented during the GP core period on

alcohol consumption among adolescents (aged 11, 13, and 15 years), a key indicator for the success of the GP, also were assessed (WHO Regional Office for Europe, 2024) (Table 5.2). Compared with the prevalence of alcohol consumption reported by adolescents in 2009–2010, there was a steady reduction in the prevalence of alcohol consumption reported by adolescents in 2013–2014 (odds ratio [OR], 0.48; 95% CI, 0.44 to 0.54), 2017–2018 (OR, 0.31; 95% CI, 0.28 to 0.34), and 2021–2022 (OR, 0.22; 95% CI, 0.20 to 0.24). [The strengths of this analysis are the comparison of the prevalence of alcohol consumption over four periods, the focus on minors, and the inclusion of control variables for sex and age. The limitation of the analysis is the lack of geographical controls.]

5.3.2 Lithuania

(a) Recent history of coordinated interventions

Lithuania regained independence from the Soviet Union in 1990. Subsequently, Lithuania tried to associate with the European Union and many changes occurred that led to a growing free-market ideology. During this period, the Lithuanian government developed and enacted several alcohol policies, which included restrictions and liberalizations. Despite these efforts, alcoholic beverage consumption and alcohol-attributable harms increased (Miščikienė et al., 2020).

Frequent changes to alcohol policies continued until 2008, when the Lithuanian government sought to reduce alcohol consumption and alcohol-attributable harms by declaring 2008 "the year of sobriety" and by enacting different policies, including banning alcohol advertising on television and radio during the daytime, enacting stricter laws for drink-driving, and increasing alcohol excise taxes, which decreased the affordability of alcoholic beverages (Miščikienė et al., 2020; Rehm et al., 2021, 2023b). Other policies enacted in 2009 included banning off-premises sales of alcoholic beverages between 22:00 and 08:00 and further increasing alcohol excise taxes, although not by enough to decrease the affordability of alcoholic beverages (Miščikienė et al., 2020).

After 2009, there was another period of relative inaction in alcohol policy in Lithuania, and by 2013 the APC was > 16 L, which was among the highest in the WHO European Region (Shield et al., 2016). Subsequently, to reduce the APC and alcohol-attributable harms in the population, the Lithuanian government implemented coordinated interventions consistent with the WHO "best buys" for reducing alcohol-related harm (WHO, 2017). In March 2017, excise taxes on beer and wine were increased by > 100% and excise taxes on spirits were increased by 23%, which decreased the affordability of alcoholic beverages, and drink-driving laws were strengthened (Miščikienė et al., 2020; Rehm et al., 2023b). Alcohol availability restrictions further reduced the retail hours for off-premises sales of alcoholic beverages, with sales hours being restricted to 10:00–20:00 on Mondays to Saturdays and 10:00–15:00 on Sundays, and increased the minimum legal purchase age from 18 years to 20 years. There was also a comprehensive ban on alcohol advertising on television, radio, and the Internet (Miščikienė et al., 2020). These population-level interventions were accompanied by the development and implementation of individual-level interventions, including an addiction consultant model (with low-threshold practice) and the implementation of brief interventions in primary health care. During the two periods (i.e. 2008–2009 and 2017–2018) of implementation of coordinated interventions in Lithuania, there were substantial reductions in alcohol-attributable mortality (Rehm et al., 2024b).

(b) Effects of coordinated interventions on alcoholic beverage consumption

The effects of the coordinated interventions on the change in alcoholic beverage consumption in Lithuania were assessed by examining

trends in the APC for Lithuania before, during (including 1 year after), between, and after the two periods of implementation (WHO, 2024b) (Table 5.2).

From 2001 through 2007, before the first period of implementation of coordinated interventions, the mean APC was [12.99 L] and there was a trend towards increasing APC (mean difference in APC from the previous year, [0.50 L]). During and up to 1 year after the first period of implementation of coordinated interventions (2008–2010), the mean APC was [13.75 L] but there was a trend towards decreasing APC (mean difference in APC from the previous year, [−0.55 L]). From 2011 through 2016, there were small increases in the mean difference in APC from the previous year ([0.047 L]). During and up to 1 year after the second period of implementation of coordinated interventions (2017–2019), the mean APC was [11.79 L] and there was another trend towards decreasing APC (mean difference in APC from the previous year, [−0.50 L]). In 2020, the APC was [0.40 L] higher than in 2019. [The strength of this analysis is the assessment of trends in the APC before, during, between, and after the two periods of implementation of coordinated interventions. The limitations of the analysis are the lack of geographical controls and the lack of control variables for potential confounding factors.]

5.3.3 Russian Federation

The Russian Federation has a long history of changes in alcohol consumption, alcohol-attributable harms, and alcohol policies (reviewed in detail in Nemtsov, 2011; WHO Regional Office for Europe, 2019).

(a) History of coordinated interventions beginning in 1985

After the Second World War and during the Soviet era, there was a substantial increase in alcohol consumption and alcohol-attributable harms that led to several attempts to reduce alcohol consumption and culminated in a comprehensive anti-alcohol campaign in 1985 (Nemtsov, 2011). Alcohol production, distribution, and sales were controlled through the Soviet government monopoly, which was central to the campaign. The campaign reduced the production of alcoholic beverages (particularly spirits), increased prices to decrease the affordability of alcoholic beverages, and limited alcohol sales hours and availability in public spaces. The campaign also expanded services for the treatment of alcohol use disorders and public education about alcohol to raise awareness of the health risks associated with alcohol consumption (WHO Regional Office for Europe, 2019). This [very intense] period of activity has been credited with saving an estimated 1.22 million lives between 1986 and 1991 (Nemtsov, 2002) and increasing life expectancy among men by 3 years between 1984 and 1987 (Nemtsov, 2005). However, the 1985 anti-alcohol campaign was unpopular with the public, and it led to an increase in illegal alcohol production, black market activities, and consumption of alcohol surrogates (WHO Regional Office for Europe, 2019). The 1985 anti-alcohol campaign was repealed in 1988.

Neufeld et al. (2020a) described a total of seven distinct subsequent periods, which were characterized as phases of intense implementation of multiple policies, less-active phases, or phases when policies were loosened (see Table 5.3; Neufeld et al., 2020a and WHO Regional Office for Europe, 2019).

From 1990 through 1994, most of the anti-alcohol campaign policy interventions were repealed, the Soviet Union was dissolved, and the government monopoly on alcohol production and sales was abolished (Nemtsov, 2011). In the newly formed Russian Federation, alcohol production, sales, and importation were privatized and increased rapidly, alcohol prices were no longer fixed, hours of alcohol sales were extended, and the Occupational Therapy

Table 5.3 Implementation of coordinated multiple alcohol policy interventions and their intensity in the Russian Federation

Period[a]	Coordinated multiple alcohol policy interventions	Intensity[b]
1985–1988	Anti-alcohol campaign in which the Soviet government controlled alcohol production, distribution, and sales through its government monopoly, reduced the production of alcoholic beverages, increased prices to reduce the affordability of alcoholic beverages, limited alcohol sales hours and availability in public spaces, and expanded services for the treatment of alcohol use disorders and public education about alcohol to raise awareness of the health risks associated with alcohol consumption	[Very intense]
1990–1994	Repeal of the government alcohol monopoly and provisions of the anti-alcohol campaign, loosening of existing interventions, lack of state control	Baseline
1995–1998	Introduction of Federal Law No. 171 (On State Regulation of Production and Turnover of Ethyl Alcohol, Alcoholic and Alcohol-Containing Products) and Federal Law No. 108 (On Advertising: Restrictions on Alcohol Advertisement) and key interventions on alcohol licensing, availability, and marketing	Intense
1999–2003	Formation of Rosspirtprom (state-owned distillery enterprise) and restructuring of the alcohol market; few or no interventions introduced	Inactive or indifferent
2004–2007	Introduction of key interventions on drink-driving and alcohol advertising, pricing, and availability, and specific interventions to reduce the availability of unrecorded alcohol	Very intense
2008–2009	Few new interventions introduced; publication of the National Concept to Reduce Alcohol Abuse and Alcohol Dependence in the Population (2010–2020) and formation of the Federal Service for Alcohol Market Regulation	Less intense
2010–2013	Introduction of key interventions on alcohol pricing (increase in alcohol excise tax and minimum pricing), availability, and marketing and drink-driving legislation	Very intense
2014–2018	Temporary loosening of some marketing restrictions and pricing interventions; freeze on alcohol excise tax; temporary reduction of minimum price for vodka in 2014–2015, which was subsequently reinstated to the original level and then increased; and introduction of additional interventions to reduce the availability of unrecorded alcohol	Moderate and indifferent, but generally stricter

[a] Neufeld et al. (2020a) described 7 distinct periods from 1990 through 2018 during which there were varying alcohol policy activities. The Working Group added the time period 1985–1988, during which an anti-alcohol campaign was implemented as a result of increasing alcohol consumption and alcohol-associated harms before 1985 (WHO Regional Office for Europe, 2019).

[b] The intensity of the coordinated multiple alcohol policy interventions during each period, except 1985–1988, was classified by Neufeld et al. (2020a) on the basis of (i) the number of new alcohol policies introduced to restrict or regulate alcohol, (ii) the number of repealed policies or new policies introduced to loosen alcohol restrictions or regulations, and (iii) the evidence on the effectiveness of the introduced policies. The 1990–1994 period was defined as the baseline, during which almost no alcohol policies were implemented and many provisions of the 1985–1987 anti-alcohol campaign were repealed. Using the same criteria, the Working Group classified the 1985–1988 anti-alcohol campaign period as very intense. Adapted from Neufeld et al. (2020a).

Reha-bilitation Centres established previously (i.e. in 1967) for the mandated inpatient treatment of severe alcohol dependence were abolished (WHO Regional Office for Europe, 2019). Although the government monopoly on alcohol production and sales was restored in 1993, it was generally not enforced, and in 1994 alcohol advertising was allowed on television (WHO Regional Office for Europe, 2019). By 1992–1994, increased alcohol-attributable mortality rates, particularly from alcohol poisoning and accidents among men of working age, were reported (Treisman, 2010), and "the negative consequences of alcohol consumption reached levels that threatened the physical, psychological and social health of the Russian population" (Nemtsov, 2011). An estimated 33% of all deaths in the Russian Federation in 1994 were directly or indirectly related to alcohol consumption (Nemtsov, 2002).

As a result of the increase in alcohol-attributable harms, in 1995 the Federal Assembly of the Russian Federation passed Federal Law No. 171: On State Regulation of Production and Turnover of Ethyl Alcohol, Alcoholic and Alcohol-Containing Products. The law required producers and sellers of products containing alcohol to be licensed and to declare the quantities of production and sales; it also harmonized tax and fraud protections through excise stamps and stipulated penalties for violations of the law (WHO Regional Office for Europe, 2019). The law also limited the importation of products containing alcohol, prohibited the establishment of foreign-invested organizations for the importation, bottling, and sale of products containing > 12% ethanol by volume (Nemtsov, 2011), and banned the sale of spirits in kiosks. Also in 1995, Article 33 of Federal Law No. 108: On Advertising: Restrictions on Alcohol Advertisement was adopted, which banned advertising of alcoholic beverages, except beer, on television and radio between 07:00 and 22:00. Beginning in 1996, there was a complete ban on advertising of spirits on television (WHO Regional Office for Europe, 2019). Also in 1996, a minimum price on vodka was established, but this had no effect because of high inflation. In 1997, new anti-counterfeit excise stamps were introduced, and the sale of beverages with an alcohol content of > 12% ethanol by volume in vending machines, kiosks, sales tents, and other small retail outlets was banned (Nemtsov et al., 2019).

In 1999–2003, few or no policies were implemented. However, an important restructuring of the alcohol market occurred. In early 2000, the state-owned distillery enterprise Rosspirtprom was established to consolidate most spirits production under government control (WHO Regional Office for Europe, 2019).

In 2004–2007, key interventions on drink-driving and alcohol advertising, pricing, and availability were implemented, as well as specific interventions to reduce the availability of unrecorded alcohol. In 2004, Federal Law No. 108 was amended to include new restrictions on alcohol advertising, including on beer and beer-based beverages, and to mandate health warnings in advertisement spaces or during advertising time (Khaltourina and Korotayev, 2015; Nemtsov et al., 2019). In 2005 and 2006, amendments to Federal Law No. 171 were adopted, which introduced stricter licensing regulations for alcohol producers and distributors and the Unified State Automated Information System (EGAIS) to monitor ethyl alcohol production and the importation, distribution, and sales of alcoholic beverages. Some interventions targeted unrecorded alcohol consumption, such as specific denaturing requirements for ethanol-based products that were frequently misused as alcohol surrogates (Khaltourina and Korotayev, 2015; Neufeld and Rehm, 2018). Excise taxes on alcoholic beverages were increased by 50%, and regions were given autonomy to introduce regional restrictions on hours of sales (which some regions did, but most did not). In addition, sales of alcoholic beverages with an ethanol content of > 15% were banned in certain public places unless a specific licence was

obtained (Kolosnitsyna and Dubynina, 2019). In 2007, sales of alcoholic beverages on the Internet were banned (WHO Regional Office for Europe, 2019).

In late 2008, the Federal Service for Alcohol Market Regulation was formed as a separate body under the Ministry of Finance of the Russian Federation to oversee and regulate the production, distribution, and sales of ethanol and alcoholic beverages, enforce compliance with alcohol laws, and implement public health initiatives to reduce alcohol-attributable harms (WHO Regional Office for Europe, 2019). In 2009, the Russian Federation published a national alcohol strategy, known as the National Concept to Reduce Alcohol Abuse and Alcohol Dependence in the Population (2010–2020) (Neufeld et al., 2020b). This strategy aimed to significantly reduce alcohol consumption and alcohol-attributable harms through increased public awareness, strengthened legal regulations, improved health-care services, and a coordinated approach across various sectors.

In 2010–2013, several key interventions on alcohol pricing (increase in alcohol excise tax and minimum pricing), availability, and marketing and drink-driving legislation were introduced. For example, in 2011–2012, further amendments to Federal Law No. 171 imposed greater restrictions on the sale and consumption of alcoholic beverages, prohibited public alcohol consumption, introduced broader restrictions on alcohol advertising, including on the Internet, and banned the sale of alcoholic beverages in kiosks, except for beer (which was banned in 2013). The law also banned alcohol sales between 23:00 and 08:00, but regions retained the authority to impose stricter regulations. Concurrently, changes to the tax code and additional decrees substantially increased alcohol excise taxes and minimum prices for spirits starting in 2011; the largest increases occurred in 2012, when excise taxes and minimum prices were increased twice within the year (WHO Regional Office for Europe, 2019).

Starting in 2014, some key interventions were loosened temporarily (WHO Regional Office for Europe, 2019). For example, beer advertising was allowed at sporting events starting in 2014, and television and radio advertising of domestic wine was allowed starting in January 2015. The minimum price for vodka was reduced in 2015 and was increased again in 2016 but to a lower price than the 2013 minimum price, and instead of the planned gradual increase in excise taxes on spirits by 32%, they were frozen from 2015 through 2016. In 2016–2018, the minimum prices for spirits and sparkling wine increased, and excise taxes increased by only 4.6% and then remained frozen until 2019.

(b) Effects of coordinated interventions on alcoholic beverage consumption

The effects of the coordinated interventions since 1985 on alcoholic beverage consumption were assessed in three studies with population-level data (Nemtsov et al., 2019; Salagay et al., 2021; Kolosnitsyna, 2024) and one study with individual-level data (Radaev et al., 2020) (Table 5.2).

Nemtsov et al. (2019) published APC data from 1980 through 2016, which allow for an assessment of changes in the APC over each of the described periods of policy changes, although the final period is only partially covered. As a result of the 1985–1987 anti-alcohol campaign, which was officially repealed in 1988, the APC decreased from 1985 through 1987 ([–5.2 L]) compared with the APC in 1984. After the rapid liberalization and privatization of the alcohol market, the APC increased from 1991 through 1994 ([7.1 L]) compared with the APC in 1990. With the enactment of Federal Law No. 171 and Federal Law No. 108 in 1995 and other interventions through 1998, the APC again decreased, from a peak of 23.6 L in 1994 to 17.2 L in 1998 ([–6.4 L]). However, from 1999 through

2003, no major alcohol policy interventions were effectively implemented, and the APC increased ([4.0 L]). The subsequent period, 2004–2007, was a period of very intense alcohol policy interventions, and the APC decreased ([–2.8 L]) compared with the APC in 2003. In 2008–2009, there was less-intense alcohol policy activity and the APC decreased ([–1.1 L]) compared with the APC in 2007. The period from 2010 through 2013 was a period of very intense alcohol policy activity, and the APC decreased ([–1.8 L]) compared with the APC in 2009. In 2014–2018, some key interventions were loosened temporarily and subsequently reinstated, and from 2014 through 2016 the APC decreased only slightly ([–0.9 L]) compared with the APC in 2013. [The strength of this study is the availability of APC data to assess changes in the APC over the eight periods of policy changes. The limitations of the study are the lack of geographical controls and the lack of control variables for potential confounding factors.]

Salagay et al. (2021) assessed the completeness of the implementation of the National Concept to Reduce Alcohol Abuse and Alcohol Dependence in the Population (2010–2020) interventions and the effects of the interventions on alcohol consumption. Among the 21 interventions in the National Concept, 15 were implemented, 12 fully and 3 partially. From 2008 through 2019, the APC decreased from 15.7 L to 9.1 L (–42%). [The strength of this study is the assessment of the number of interventions implemented. The limitations of the study are the lack of geographical controls and the lack of control variables for potential confounding factors.]

In the most recent study with population-level data, Kolosnitsyna (2024) assessed trends in recorded alcohol consumption in the Russian Federation from 2010 through 2014 and from 2017 through 2021. Notably, the affordability of alcoholic beverages increased starting in 2014, and the number of regions with stricter regulations on hours of alcohol sales did not change from 2017 through 2021, with nearly half of the population living in areas with lenient restrictions. During the implementation of several key interventions in 2010–2014, the recorded APC decreased ([–1.4 L]). After the loosening of several interventions beginning in 2015, the recorded APC increased slightly, from 7.2 L in 2017 to 7.7 L in 2021. [The strengths of this study are the results showing increased affordability of alcoholic beverages and the number of regions with restrictions on hours of alcohol sales. The limitations of the study are the lack of geographical controls, the lack of control variables for potential confounding factors, and that the APC was based on recorded APC data only.]

Radaev et al. (2020) used individual-level repeated cross-sectional data from the Russia Longitudinal Monitoring Survey to assess changes in the prevalence of abstention from alcohol consumption and in the volume of alcohol consumption from 2006 through 2017. In descriptive analyses, from 2006 to 2017, the prevalence of abstention increased among all women (from 27.7% to 44.1%) and among all men (from 18.7% to 33.7%). The increase in the prevalence of abstention was highest among individuals aged 14–25 years (from 35.5% to 62.3% among women, and from 29.7% to 61.6% among men). In multivariable regression analyses, the prevalence of abstention was higher for 2012–2017 than for 2006–2011 among both women ([8.4%]; $P < 0.05$) and men ([15%]; $P < 0.01$). In descriptive analyses, the volume of alcohol consumption was 35% lower in 2017 than in 2006 among all men and all women. In multivariable regression analyses, the volume of alcohol consumption was lower for 2012–2017 than for 2006–2011 among both women ([–6.1%]; $P < 0.001$) and men ([–9.5%]; $P < 0.001$). [The strengths of this study are the sex-specific analyses and the inclusion of control variables for income and other factors in the multivariable model. The limitation of the study is the lack of geographical controls.]

5.3.4 Thailand

In Thailand, the rapid economic growth and the transition from a low-income agrarian society to an upper-middle-income country have affected alcohol consumption among the citizens and the related alcohol policies. This economic transition, spanning the past four decades, has driven urbanization, industrialization, and an expanding middle class (World Bank Group, 2024). The predominately Buddhist culture of Thailand, influenced by the five precepts, has historically discouraged the consumption of intoxicants, including alcohol, and has shaped Thai attitudes and behaviours related to alcoholic beverage consumption (Groves, 2014). However, the complexities of globalization and modernization have challenged these traditional values, resulting in a dynamic interaction between cultural norms and economic realities that shapes the national alcohol policy in Thailand. The alcoholic beverage market evolved from a government production monopoly to an oligopoly in 1985; currently, the alcohol market in Thailand is dominated by two powerful conglomerates (Thamarangsi, 2006; Talek et al., 2024). The APC in Thailand increased from 0.3 L in 1961 (WHO, 2018) to almost 8 L in 2007 (Talek et al., 2024).

(a) Recent history of coordinated interventions

To address the increase in alcohol consumption, Thailand enacted the relatively comprehensive Alcoholic Beverage Control (ABC) Act in 2008 (Ministry of Public Health of Thailand, 2008). This act included restrictions on alcohol marketing, increased access to treatment and rehabilitation of alcohol dependence, and limitation of the availability of alcoholic beverages. More specifically, the act prohibits alcohol advertising that promotes alcohol consumption but allows advertisements or public notifications to provide information or social creative knowledge without illustrating alcoholic beverages or their packages. The ABC Act authorizes the issuing of policies and guidelines about treatment of alcohol use disorder, and it enables individuals with alcohol use disorder, groups, or public or private organizations that aim to treat or rehabilitate individuals with alcohol use disorder to request support for treatment or rehabilitation from the Royal Thai Government (Ministry of Public Health of Thailand, 2008). It also restricts sales locations (e.g. alcohol sales are not allowed at places of worship, public health facilities, schools, petrol stations, and public parks), imposes limitations on hours of alcohol sales (alcohol sales are allowed only 11:00–14:00 and 17:00–24:00), prohibits alcohol sales on five major Buddhist holy days and on pre-election day, bans certain sales practices (e.g. using an automatic vending machine, hawking, providing a discount, special services, and free samples), and increases the minimum purchase age to 20 years (Ministry of Public Health of Thailand, 2008). In addition, other laws governing alcohol policies in Thailand have evolved (Talek et al., 2024). Since 2017, the alcohol taxation system has been modified, after the enactment of the Excise Tax Act of 2017 (Ministry of Finance of Thailand, 2017), to use a combination of both specific and ad valorem tax rates; the intention was to reduce the consumption of alcoholic beverages with high ethanol content, but this resulted in a price increase of only 0.2% (Talek et al., 2024). In addition to these excise taxes, value-added tax (7%), customs duty (on imported alcoholic beverages), and an earmarked tax (17.5% of alcohol tax revenue) – which goes towards community development (10%), social activities (Thai Public Broadcast Service, 1.5%; National Sports Development Fund, 2%; Older Persons Fund, 2%), and public health (Thai Health Promotion Foundation, 2%) – are also applied. The Road Traffic Act of 1979 also established drink-driving regulations (Ministry of Interior of Thailand, 2022; Talek et al., 2024). Since 1994, the legal blood alcohol concentration limit has been 0.05% for the

general population and 0.02% for four specific groups: drivers younger than 20 years, drivers with a temporary driving licence, drivers with the incorrect type of driving licence, and drivers whose licences were revoked or suspended (Ministry of Interior of Thailand, 1994).

Since the enactment of the ABC Act in 2008, the National Alcohol Policy Strategy of 2011–2020 was certified by the National Health Assembly and approved by the cabinet ministers in 2010. This strategy aligns with the ABC Act and outlines interventions and related policies, such as the Road Traffic Act, the Excise Tax Act, and civil society campaigns, to effectively control the magnitude and severity of alcohol consumption and alcohol-attributable harms. The National Alcohol Policy Strategy includes five key strategies: controlling the availability of alcoholic beverages, modifying attitudes towards alcohol consumption, reducing risks, implementing policies in every setting, and establishing supportive mechanisms (Joint Assessment Mission to Review Alcohol Control Policy and Strategy in Thailand, 2020). In 2021, the National Alcohol Action Plan Phase II was implemented for 2021–2027; it is structured using the framework of the WHO SAFER initiative, with two additional strategies: managing the environment in which alcoholic beverages are consumed, and providing information and education (Talek et al., 2024).

(b) Effects of coordinated interventions on alcoholic beverage consumption

The effects of the coordinated interventions (since 2008) on alcoholic beverage consumption in Thailand were assessed in one population-level time-series study (Rehm et al., 2024c) and one individual-level repeated cross-sectional survey study of adolescents (Assanangkornchai et al., 2020) (Table 5.2).

In a descriptive analysis of time-series data, Rehm et al. (2024c) assessed the APC at four time points (i.e. 2010, 2013, 2016, and 2019) both in Thailand, where the ABC Act was enacted in 2008 and there were intermittent increases in the alcohol excise tax rates (Sornpaisarn and Kaewmungkun, 2014), and in Viet Nam, where two increases in excise tax occurred, in 2016 and 2019, but no major coordinated interventions were implemented until 2020, when the Law on Prevention and Control of Harmful Effects of Alcoholic Beverages was enacted. In Thailand, the APC was 7.57 L in 2010 and 7.85 L in 2019 (an increase of 3.7%), whereas in Viet Nam, the APC was 7.37 L in 2010 and 9.34 L in 2019 (an increase of 26.8%). [The strengths of this study are the assessment of the APC at four time points and the inclusion of a geographical control of a second member of the Association of Southeast Asian Nations with a growing economy. The limitations of the study are that it is descriptive and lacks control for potential confounding variables or hypothesis testing, that no trend analysis was conducted, and that the time between the implementation of some policy changes (e.g. excise tax increase in 2017) and the assessed APC may be too short to enable the detection of a potential effect of the policy.]

In a study that focused on adolescents, Assanangkornchai et al. (2020) assessed the effects of the ABC Act on the prevalence of lifetime alcohol consumption and on alcohol consumption in the 12 months before completing the survey among adolescents aged 13–17 years in 2016 compared with 2007. The prevalence was higher in 2016 than in 2007 both for lifetime alcohol consumption (prevalence ratio, 2.1; 95% CI, 1.8 to 2.5 among girls and 1.4; 95% CI, 1.2 to 1.5 among boys) and for 12-month alcohol consumption (prevalence ratio, 1.9; 95% CI, 1.6 to 2.2 among girls and 1.2; 95% CI, 1.1 to 1.3 among boys). [The strengths of this study are the large sample sizes with very high response rates, the use of the same questionnaire on alcohol consumption at both time points, the selection of a nationally representative sample of the target population, and the administration of the

surveys at the same time of year. The limitations of the study are the use of data from only one survey 1 year before the implementation and one survey 8 years after the implementation of the ABC Act, which does not allow for comparisons of potential trends in consumption before and after implementation of the policies, and that during the 8-year period between the implementation of the ABC Act and the second survey, there were other civil society campaigns to control alcohol consumption among individuals younger than the legal drinking age, which were not controlled for in the statistical analysis.]

5.4 Other evidence

5.4.1 Alcohol policy scores

Researchers have constructed composite restrictiveness indices or scales of multiple alcohol policy interventions, which are converted to scores (e.g. Brand et al., 2007; Carragher et al., 2014; Ferreira-Borges et al., 2015; Casswell et al., 2022). These alcohol policy indices or scales were designed to rate the existence, implementation, enforcement, and/or effects of the alcohol policy environment within a jurisdiction. Many methods and sources of jurisdiction-specific alcohol policy data have been used to construct alcohol policy indices or scales, and there is variability in the specific policy interventions included. In general, each alcohol policy intervention included in an index or scale is assigned a weighted score based on evidence of the effects of the intervention on alcoholic beverage consumption and/or alcohol-related harms, and then the weighted scores are summed across all interventions to yield a total score for each jurisdiction; a higher value indicates a more restrictive and/or more effective set of alcohol policies. The association of alcohol policy total scores with outcomes such as alcohol consumption or alcohol-related harms has been assessed in many studies.

In this section, the Working Group briefly reviewed five cross-sectional ecological studies (Brand et al., 2007; Carragher et al., 2014; Ferreira-Borges et al., 2015; Madureira-Lima and Galea, 2018; Casswell et al., 2022), one single-point-in-time cross-sectional survey study (Casswell et al., 2023), and two repeated cross-sectional survey studies of adolescents (Xuan et al., 2015; Leal-López et al., 2020) in which the association between alcohol policy total scores and alcohol consumption was assessed (Table 5.4). These studies were selected to represent associations for different regions of the world (with high-income countries most commonly represented), for different alcohol policy total scores, and for different age groups (i.e. total population and adolescents, separately). Of note, the sources of alcohol policy data varied among the studies: some studies used the WHO GISAH database, which is based on self-reporting by countries, whereas others relied on collaborating researchers, their own analyses of policy documents, or legal epidemiology at the national level.

Brand et al. (2007) developed the Alcohol Policy Index (total score range, 0 to 100) to rate the restrictiveness of country-specific alcohol policies on the basis of 16 interventions among five domains: physical availability, alcohol consumption context, alcohol prices, alcohol advertising, and motor vehicle regulations. An Alcohol Policy Index score was calculated for each of 30 Organisation for Economic Co-operation and Development (OECD) countries, among which the total scores ranged from 14.5 to 67.3. Higher total scores were correlated with lower APC (Pearson correlation coefficient = −0.57; P = 0.001), and in linear regression analyses a 10-point increase in the total score was associated with a 1 L reduction in the APC.

Carragher et al. (2014) modified the method of Brand et al. (2007) to develop the Toolkit for Evaluating Alcohol policy Stringency and Enforcement-16 (TEASE-16) to assess the

Table 5.4 Effects of alcohol policy scores on alcoholic beverage consumption

Reference Locations Time period	Study type	Policies or interventions	Outcome measures Effect estimates	Comments
Population-level data				
Brand et al. (2007) 30 OECD countries Alcohol policy data: 2000–2005 APC data: 2003 for 28 countries and 2001 for 2 countries	Cross-sectional	The Alcohol Policy Index score (total score range, 0 to 100) is a restrictiveness score for country-specific alcohol policies based on the sum of weighted scores for each of 16 alcohol policy interventions across 5 domains: physical availability, alcohol consumption context, alcohol prices, alcohol advertising, and motor vehicle regulations Median score, 42.4; range, 14.5–67.3	APC Pearson correlation coefficient = −0.57 (P = 0.001) APC (95% CI) per 1-point increase in the total score β = −0.10 (−0.15 to −0.04)	Unadjusted effect estimates
Carragher et al. (2014) Western Pacific: Australia, China, Hong Kong Special Administrative Region, Japan, Malaysia, New Zealand, Philippines, Singapore, and Viet Nam Alcohol policy data, 2008–2012 APC data, 2011	Cross-sectional	Modified from the Alcohol Policy Index (Brand et al., 2007), the TEASE-16 score (total score range, 0 to 100) rates the level of stringency and enforcement of 16 alcohol policy interventions across 5 domains: physical availability, alcohol consumption context, alcohol prices, alcohol advertising, and drivers of motor vehicles Median score, 56.4; range, 24.1–67.5	Income-adjusted APC in 2011 Pearson correlation coefficient = −0.88 (P = 0.001) Difference in income-adjusted APC per 1-point increase in the total score = −1.8% (P, NR)	Alcoholic consumption per capita in 2011 is adjusted for income (litres of ethanol per 1000 international dollars of GDP per capita) Unadjusted effect estimates
Ferreira-Borges et al. (2015) 46 countries in Africa Alcohol policy and APC data, 2012	Cross-sectional	Modified from the Alcohol Policy Index (Brand et al., 2007) (total score range, 0 to 100) to include 10 alcohol policy interventions across 5 domains: pricing, taxation, physical availability, marketing, and drink-driving Mean score, 44.1; range, 9.1–75.0	APC Spearman correlation coefficient = −0.35 (P = 0.005)	Spearman correlation coefficient is for individuals who consume alcohol Unadjusted estimates

Table 5.4 (continued)

Reference Locations Time period	Study type	Policies or interventions	Outcome measures Effect estimates	Comments
Madureira-Lima and Galea (2018) 167 countries Alcohol policy and APC data, 2012	Cross-sectional	ACPI score (total score range, 0 to 100) based on the 10 policy areas in the 2010 WHO global strategy (WHO, 2010): leadership, awareness, and commitment; health services response; community action; drink-driving policies and countermeasures; availability of alcohol; marketing of alcoholic beverages; pricing policies; reducing the negative consequences of alcohol consumption; reducing the public health impact of illicit and informally produced alcohol; and monitoring and surveillance	APC (95% CI) per 1-point increase in the total score Simple mean ACPI binary variable: $\beta = -0.014$ (−0.034 to 0.005) Theory weighting on ACPI binary variable: $\beta = -0.020$ (−0.043 to 0.002) Simple mean on ACPI *Z* scores: $\beta = -0.019$ (−0.040 to 0.002) Factor analysis on ACPI *Z* scores: $\beta = -0.024$ (−0.043 to −0.004) Theory weighting on ACPI *Z* scores: $\beta = -0.023$ (−0.046 to −0.000)	Covariates: GDP per capita, population aged 0–14 years, urban population, and world region
Casswell et al. (2022) Australia, Chile, England and Scotland (United Kingdom), Mongolia, the Nether-lands, New Zealand, Saint Kitts and Nevis, South Africa, Thailand, Türkiye, and Viet Nam Alcohol policy data and recorded APC data, 2012–2020	Cross-sectional	IACPI score (total score range, 0 to 25) based on on-premises and off-premises trading hours and days of sale and outlet density, tax rate as percentage of price, legally binding restrictions on different types of marketing, and blood alcohol content laws and enforcement	APC (recorded only) [Pearson] correlation coefficient = −0.91	Unadjusted estimate
Individual-level data				
Casswell et al. (2023) Australia, England and Scotland (United Kingdom), Mongolia, New Zealand, Saint Kitts and Nevis, South Africa, Thailand, Türkiye, and Viet Nam Survey data, 2011–2016	Cross-sectional survey	IACPI score (total score range, 0 to 25) based on on-premises and off-premises trading hours and days of sale and outlet density, tax rate as percentage of price, legally binding restrictions on different types of marketing, and blood alcohol content laws and enforcement IACPI score range, 5.0–13.9	Percentage difference in total volume of ethanol consumed in the past 6 months per 1-point increase in the total score = −16.5% ($P = 0.001$)	Covariates: country as a random effect, and age group, sex, and education level as fixed effects

Table 5.4 (continued)

Reference Locations Time period	Study type	Policies or interventions	Outcome measures Effect estimates	Comments
Xuan et al. (2015) USA: 50 states and Washington, DC High school students in grades 9–12 Biennial surveys from 1999 through 2011	Repeated cross-sectional survey Youth Risk Behavior Surveys	Alcohol Policy Scale score (total score range, 0 to 100) characterizing the strength of the efficacy and/or legislative implementation of state-level youth-oriented alcohol policy interventions (n = 10) and population-oriented alcohol policy interventions (n = 19) Alcohol Policy Scale scores for the adjusted OR analyses were based on the legislative implementation of the interventions for each state–year	Adjusted OR (95% CI) per 10 percentage points increase in the total score Any alcohol consumption: 0.92 (0.90 to 0.95) HED: 0.93 (0.91 to 0.96)	Any alcohol consumption is defined as consuming ≥ 1 alcoholic beverages in the past 30 days HED is defined as consuming ≥ 5 alcoholic beverages ("within a couple of hours") on ≥ 1 day in the past 30 days Covariates: year, state-level sociodemographic covariates (proportion of adults aged ≥ 21 years, sex distribution, race/ethnicity, degree of urbanization, median household income, religiosity, police officers per capita, and geographical region), and individual-level covariates (age, sex, race/ethnicity, and tobacco use in the past month)
Leal-López et al. (2020) 33 countries or regions in Europe and North America Adolescents aged 11, 13, and 15 years Surveys in 2001–2002, 2005–2006, 2009–2010, and 2013–2014	Repeated cross-sectional survey Health Behaviour in School-aged Children study	Total alcohol policy index for stringency of alcohol policies (total score range, 0 to 26.11) included minimum legal drinking age (no, yes), government alcohol monopoly (no, partial, full), restrictions on alcohol outlet density (no, wine only, wine and spirits, all beverages), restrictions on alcohol sales times (none, on hours or days, on both hours and days), and bans on alcohol advertising in print, broadcast, billboards, sports sponsorship, and the Internet (no restrictions, voluntary self-regulated code, partial statutory restrictions, complete ban)	Difference in prevalence per 1-point increase in the total score Lifetime alcohol consumption: $\beta = -0.02$ (SE = 0.011; $P < 0.05$) At least weekly alcohol consumption: $\beta = -0.04$ (SE = 0.013; $P < 0.001$)	Lifetime alcohol consumption was defined as consumption on at least 1 day over the lifespan Covariates: age, sex, time, affordability, family affluence scale, and random effects for country, country–year, and school

ACPI, Alcohol Control Policy Index; APC, total adult alcohol per capita consumption; CI, confidence interval; GDP, gross domestic product; HED, heavy episodic drinking; IACPI, International Alcohol Control Policy Index; NR, not reported; OECD, Organisation for Economic Co-operation and Development; OR, odds ratio; SE, standard error; TEASE-16, Toolkit for Evaluating Alcohol policy Stringency and Enforcement-16; WHO, World Health Organization.

stringency and enforcement of alcohol policy interventions (total score range, 0 to 100). Among the nine areas in the Western Pacific included in the analysis, the total scores ranged from 24.1 to 67.5. The total score was inversely associated with the income-adjusted APC (Pearson correlation coefficient = −0.88; $P = 0.001$), and a 1-point increase in the total score was associated with a reduction of 1.8% in the APC.

Ferreira-Borges et al. (2015) also modified the method of Brand et al. (2007) to develop an alcohol policy restrictiveness index (total score range, 0 to 100) to assess the restrictiveness of alcohol policies in relation to the APC. Among the 46 countries in Africa included in the analysis, the total scores ranged from 9.1 to 75.0. There was an inverse association between the total score and the APC among individuals who consume alcohol (Spearman correlation coefficient = −0.35; $P = 0.005$).

Madureira-Lima and Galea (2018) developed an Alcohol Control Policy Index (ACPI) based on the 10 policy areas described in the 2010 WHO global strategy (WHO, 2010). Five different methodological approaches were used to calculate the ACPI scores (total score range, 0 to 100) for each of 167 countries. Among the five methodological approaches, a 1-point increase in the total score was consistently associated with a reduction in the APC, ranging from −0.024 L (95% CI, −0.043 to −0.004 L) to −0.014 L (95% CI, −0.034 to 0.005 L) in multivariable-adjusted analyses.

Casswell et al. (2022) developed the International Alcohol Control Policy Index (IACPI) to measure both the restrictiveness and the impact of four domains of alcohol policies (availability, pricing policy, marketing, and drink-driving) that could easily be used both in high-income countries and in low- and middle-income countries. The association between the IACPI score (total score range, 0 to 25) and alcohol consumption was assessed based on data from 12 diverse national or subnational jurisdictions. Among these 12 jurisdictions, the total scores ranged from 5.0 to 13.9. There was a strong inverse association between the total score and the recorded APC ([Pearson] correlation coefficient = −0.91).

Casswell et al. (2023) also assessed the association between the IACPI score and alcohol consumption in a separate analysis based on survey data from individuals aged 16–65 years who consume alcohol in five high-income and five middle-income national or subnational jurisdictions. A 1-point increase in the total score was associated with a reduction of 16.5% ($P = 0.001$) in the volume of ethanol consumed in the past 6 months.

Xuan et al. (2015) used repeated cross-sectional survey data collected from adolescents (in grades 9–12) in the USA (all 50 states and Washington, DC) to assess the association of an Alcohol Policy Scale (total score range, 0 to 100) for the legislative implementation of 29 alcohol policy interventions (10 youth-oriented and 19 population-oriented interventions) with alcohol consumption. A 10 percentage points increase in the total score was associated with a reduction in any alcohol consumption (OR, 0.92; 95% CI, 0.90 to 0.95) and a reduction in heavy episodic drinking (OR, 0.93; 95% CI, 0.91 to 0.96).

In the study by Leal-López et al. (2020) (described in Section 3.2.5(b)), which included data from four cross-sectional surveys between 2001 and 2014 of adolescents in 33 countries or regions in Europe and North America, an alcohol policy index (total score range, 0 to 26.11) was computed as the sum of an availability index (range, 0 to 23.80; see Section 3.2.5(b)) and an alcohol advertising index (range, 0 to 2.60; see Section 4.3.2(c)(i)). Among the adolescents, a 1-unit increase in the total score was associated with a lower prevalence of lifetime alcohol consumption ($\beta = -0.02$; $P < 0.05$) and a lower prevalence of at least weekly alcohol consumption ($\beta = -0.04$; $P < 0.001$).

5.4.2 Community action trials

Communities play a pivotal role in enforcing alcohol policies and advocating for policy change. The involvement of communities ensures that alcohol policies are not just imposed top-down but are actively supported and maintained through collective, grass-roots efforts. The 2010 WHO global strategy states that "communities can be supported and empowered by governments and other stakeholders to use their local knowledge and expertise in adopting effective approaches to prevent and reduce the harmful use of alcohol by changing collective rather than individual behaviour while being sensitive to cultural norms, beliefs and value systems" (WHO, 2010). Community action typically involves a core group that supports and encourages collaboration and engagement among government officials and local stakeholders for decision-making about stores, outlets, schools, and local organizations, for example. Community mobilization is central to alcohol policy change and enforcement. The specific alcohol policy interventions can vary according to the interest, needs, and conditions of the community.

Many studies have assessed the effects of various community action interventions on alcoholic beverage consumption or other outcomes. In this section, only studies of controlled trials with a design that enables assessment of the effect of multiple alcohol policy interventions at the community level on alcohol consumption are briefly reviewed (Holder et al., 2000; Wagenaar et al., 2000; Shakeshaft et al., 2014) (Table 5.5). It is important to note that community action intervention trials are difficult to design, because they involve intensive contact with local policy-makers and stakeholders. In addition, there is potential for the interventions to contaminate the control communities, which could attenuate experimental effects. An important advantage of community action trials is that the degree of implementation and enforcement can potentially be assessed and can then be included in analyses on the effects of the interventions.

The Community Trials Project (April 1992–December 1996) was designed as a non-randomized controlled efficacy trial and was conducted in three experimental communities and three control communities (matched on community structure) in California and South Carolina, USA (Holder et al., 2000). Each community had about 100 000 inhabitants. The interventions comprised mobilizing the community, encouraging responsible beverage service, reducing access to alcoholic beverages among individuals younger than the minimum legal drinking age, increasing local enforcement of drink-driving laws, and limiting access to alcoholic beverages through local zoning and other municipal controls on alcohol outlet density. Compared with the control communities, in the experimental communities there was a reduction in the frequency of alcohol consumption (–1%; 95% CI, –4% to 2%) and in the number of alcoholic beverages consumed per occasion (–6%; 95% CI, –12% to –1%) during the project period among individuals who reported alcohol consumption.

The Communities Mobilizing for Change on Alcohol trial (1992–1995) was a randomized community intervention trial designed to reduce the accessibility of alcoholic beverages to individuals younger than the legal drinking age (Wagenaar et al., 2000). A total of 15 communities in Minnesota and western Wisconsin, USA (average population, 20 836) were pair-matched (or one triplet-matched) to the experimental or control conditions. The intervention sought to reduce the number of alcohol outlets that sell to young people; reduce the availability of alcohol to young people from non-commercial sources such as parents, siblings, and older peers; and reduce community tolerance of alcohol consumption among individuals younger than the legal drinking age and adult provision of alcohol to young people. The intervention was organized

Table 5.5 Effects of community action interventions on alcoholic beverage consumption

Reference Locations Time period	Trial name Design Data collection	Description of intervention	Outcome measures Effect estimates	Comments
Holder et al. (2000) 3 experimental communities and 3 control communities in California and South Carolina, USA April 1992–December 1996	Community Trials Project Non-randomized controlled trial Alcoholic beverage consumption was assessed in 1 adult (aged ≥ 18 years) per household for each of 120 randomly selected households per community through telephone surveys per month during the project period	Mobilize the community, encourage responsible beverage service, reduce access to alcoholic beverages among individuals younger than the legal drinking age, increase local enforcement of drink-driving laws, and limit access to alcoholic beverages through local zoning and other municipal controls on alcohol outlet density	Difference in the percentage change in the frequency of alcohol consumption in the previous 6 months between experimental and control communities during the project period –1% (95% CI, –4% to 2%) Difference in the percentage change in the number of alcoholic beverages consumed per occasion between experimental and control communities during the project period –6% (95% CI, –12% to –1%)	Response rates ranged from 58% to 67% across the years of the surveys Effect estimates are for individuals who reported alcoholic beverage consumption Frequency of alcohol consumption is the number of days on which ≥ 1 alcoholic beverage was consumed within a standardized 28-day period
Wagenaar et al. (2000) 7 experimental communities and 8 control communities in Minnesota and Wisconsin, USA 1992 (baseline survey) and 1995 (follow-up survey)	Communities Mobilizing for Change on Alcohol Randomized controlled trial Alcoholic beverage consumption was assessed in individuals aged 18–20 years and in school-based surveys in high-school seniors through baseline and follow-up telephone surveys	Reduce the number of alcohol outlets that sell to young people; reduce the availability of alcohol to young people from non-commercial sources such as parents, siblings, and older peers; and reduce community tolerance of alcohol consumption among individuals younger than the legal drinking age and adult provision of alcohol to young people. The intervention was organized for action through public institutions (e.g. local city councils, schools) and private institutions (e.g. merchants, business associations, and the media)	Difference in the percentage change in the 30-day prevalence of alcohol consumption between experimental and control communities from baseline to follow-up Individuals aged 18–20 years: –6.55% ($P = 0.07$) 12th-grade students: –1.52% ($P = 0.37$) Difference in the percentage change in the number of occasions when alcohol was consumed in the past month between experimental and control communities from baseline to follow-up Individuals aged 18–20 years: –3.92% ($P = 0.19$) 12th-grade students: –7.14% ($P = 0.14$)	12th-grade students = high-school seniors Survey response rates for individuals aged 18–20 years were 92.5% in 1992 and 93.9% in 1995, and rates for 12th-grade students were 89.3% in 1992 and 83.5% in 1995 Covariates for 30-day prevalence, individuals aged 18–20 years: male, older, White, college town, not a college student, never married Covariates for 30-day prevalence, 12th-grade students: White, no adults after school, less parent education, have older sibling, no younger sibling, more spending money Covariates for number of occasions when alcohol was consumed in the past month: NR

Table 5.5 (continued)

Reference Locations Time period	Trial name Design Data collection	Description of intervention	Outcome measures Effect estimates	Comments
Shakeshaft et al. (2014) 20 pair-matched communities in New South Wales, Australia 2005 (pre-intervention survey) and 2010 (post-intervention survey)	Alcohol Action in Rural Communities Cluster-randomized controlled trial Alcoholic beverage consumption was assessed in sex and 5-year age strata random samples of adults aged 18–62 years through pre-intervention and post-intervention 10-item AUDIT surveys with a standard drink chart	Community engagement, general practitioner training in alcohol SBI and feedback on their prescribing of medications to treat alcohol dependence, feedback to key stakeholders, media campaigns, workplace policies and practices training, school-based intervention, community pharmacy-based SBI, web-based SBI, Aboriginal Community Controlled Health Services support for SBI, Good Sports programme for sports clubs, identifying and targeting high-risk weekends, and hospital emergency department-based SBI	Difference in the change in the number of standard drinks consumed per respondent per week between experimental and control communities from pre-intervention to post-intervention surveys –1.90 (95% CI, –3.37 to –0.43)	Response rates were 40% for the pre-intervention survey and 24% for the post-intervention survey Covariates: age, sex, and pre-intervention alcohol consumption

AUDIT. Alcohol Use Disorders Identification Test; CI, confidence interval; NR, not reported; SBI, screening and brief intervention.

for action through public institutions (e.g. local city councils, schools) and private institutions (e.g. merchants, business associations, and the media). Among individuals aged 18–20 years, compared with the control communities, in the experimental communities there was a reduction in the 30-day prevalence of alcohol consumption (–6.55%; P = 0.07) and in the number of occasions when alcohol was consumed in the past month (–3.92%; P = 0.19) from baseline to follow-up. Among 12th-grade students (i.e. high-school seniors; age not reported), there was also a reduction in the 30-day prevalence of alcohol consumption (–1.52%; P = 0.37) and in the number of occasions when alcohol was consumed in the past month (–7.14%; P = 0.14) from baseline to follow-up.

The Alcohol Action in Rural Communities trial (2005–2010) was a cluster-randomized trial in Australia in which 20 communities (with populations of 5000 to 20 000) were pairwise randomized to the experimental or control conditions (Shakeshaft et al., 2014). A total of 13 interventions were implemented in the experimental communities. The interventions included community engagement, general practitioner training in alcohol screening and brief intervention (SBI) and feedback on their prescribing of medications to treat alcohol dependence, feedback to key stakeholders, media campaigns, workplace policies and practices training, school-based intervention, community pharmacy-based SBI, web-based SBI, Aboriginal Community Controlled Health Services support for SBI, Good Sports programme for sports clubs, identifying and targeting high-risk weekends, and hospital emergency department-based SBI. Compared with the control communities, in the experimental communities there was a reduction in the number of standard drinks consumed per week (–1.90; 95% CI, –3.37 to –0.43) from pre-intervention to post-intervention surveys.

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6. HEALTH CARE-BASED INTERVENTIONS

6.1 General considerations

6.1.1 Key concepts

Health care-based interventions for addressing harm among individuals with "disorders due to use of alcohol", which include primarily a "harmful pattern of use of alcohol" and "alcohol dependence" (WHO, 2024d), as well as individuals with other health conditions caused by "harmful use of alcohol", are among the set of recommended alcohol policy interventions described in the World Health Organization (WHO) 2010 Global Strategy to Reduce the Harmful Use of Alcohol (WHO, 2010a). The WHO Global Alcohol Action Plan 2022–2030 includes a recommendation to "support the development and implementation of sustainable programmes on the identification and management of hazardous and harmful drinking" in health-care settings and to "promote screening and brief interventions, as well as other interventions with proven effectiveness" (WHO, 2024a).

Currently, an estimated 7% of the global population aged ≥ 15 years live with an alcohol use disorder (AUD), but only an estimated 14% of these individuals receive treatment services (WHO, 2024b). Policies to support widespread dissemination of treatment services could increase treatment rates for individuals with an AUD. Moreover, assuming that treatment services are available and sufficiently utilized, services ranging from mutual help to pharmacotherapy have the potential to reduce the prevalence of individuals who consume alcohol and the rates of alcohol-related harms at the population level (Holder and Parker, 1992; Smart and Mann, 1993, 2000). However, access to effective screening, brief interventions, and treatment services depends on policy-level decisions to allocate resources for and support provider training on delivering and disseminating these services.

Clinical guidelines, tools for health professionals, and education modules provide instructions for screening and a wide range of interventions in health-care settings (Humeniuk et al., 2010; WHO, 2014, 2023; NIAAA, 2023). WHO has developed guidance to help health-care entities facilitate patient access to screening, brief interventions, and treatment services as part of the SAFER initiative (WHO, 2024c).

This section summarizes the evidence on the effects of health care-based screening and brief interventions and longer-term psychosocial interventions with or without pharmacotherapy on alcoholic beverage consumption among individuals with an AUD. More specifically, the summarized evidence focuses on interventions that aim to reduce or stop alcohol consumption, including relapse prevention, rather than the management of all "disorders due to use of alcohol" (e.g. alcohol intoxication, single episode

of harmful use of alcohol, alcohol withdrawal, alcohol-induced psychotic disorder) (WHO, 2024d).

6.1.2 Study eligibility and methodological considerations

The evidence from systematic reviews, including meta-analyses and umbrella reviews, on the effects of the most widely used screening and brief interventions, and psychosocial and pharmacotherapy interventions, on decreasing alcohol consumption is summarized in Sections 6.2 and 6.3, respectively. When multiple reviews that addressed the same intervention were identified, only the most recent and comprehensive review was typically summarized. When available, the effect estimates are shown for the method of intervention delivery, for specific subgroups (such as adolescents and pregnant women), and for low- and middle-income countries. The Working Group prioritized the reviews in which most or all studies were conducted in health-care settings (e.g. primary care, emergency department, and inpatient settings) and for which a meta-analysis (or network meta-analysis; NMA) of effect estimates was provided. In addition, reviews of interventions that could feasibly be implemented in health-care settings but may not have been specifically tested in those settings (e.g. digitally delivered or distance-delivered interventions) were included.

The primary outcomes in the reviews selected to be included are the amount of alcohol consumed (e.g. grams of ethanol per day), heavy drinking-related outcomes, and the proportion of participants who exceeded a specified level of alcohol consumption; if these were not available, other outcomes were included. Heavy drinking was not consistency defined or included in all reviews summarized; however, when available, the definition of heavy drinking is provided. If the outcomes in reviews were not reported in their native units (e.g. grams of ethanol per day or number of drinks per week), then the Working Group extracted other measures, such as standardized mean difference (SMD), which shows differences between groups in units of standard deviations (e.g. an SMD of 0.5 corresponds to a difference of one half of a standard deviation between groups). SMDs were reported as Cohen's *d* or its more commonly used modification Hedges' *g*, which have the same interpretation. A general rule of thumb for SMDs is that 0.20, 0.50, and 0.80 represent small, medium, and large effects, respectively (Cohen, 1992). Meta-analytic results were extracted when available; otherwise, the Working Group extracted information from narrative summaries. As noted in the descriptions of some meta-analyses, the effect estimates were reverse-scored to standardize the direction of the effects, such that a positive effect estimate indicated a beneficial treatment outcome. For those studies, the Working Group clarified the direction of the effect estimate as appropriate.

Several limitations should be considered when reviewing the evidence summarized in Sections 6.2 and 6.3. First, this evidence relies on self-reported alcohol consumption outcomes. Because study participants were aware of their treatment assignment, the results may be vulnerable to social desirability bias (Del Boca and Noll, 2000; Del Boca and Darkes, 2003). Although some research among university students suggests minimal to no social desirability bias (Kypri et al., 2016; McCambridge et al., 2019), the number of studies is limited.

Second, the findings reflect the effects of interventions that are implemented in the research setting, which may be difficult to achieve in real-world settings. A substantial amount of training and time is required to successfully disseminate the knowledge and skills needed to deliver the interventions. For example, professional development courses typically require about 10–30 hours of training for beginning-level competency in motivational interviewing (Brown and Wood, 2024; ITTI, 2024; UMass Chan Medical School, 2024; University of Wisconsin–Madison, 2024).

Maintaining the evidence-based principles and procedures of interventions while permitting adaptability to the setting and cultural norms also presents a challenge and may make dissemination challenging. A review of 84 studies assessed factors that influence the implementation of interventions when scaled up for delivery in real-world settings by general practitioners and primary care nurses in the routine delivery of screening and brief interventions for alcohol consumption in adults (Rosário et al., 2021). In that review, the most commonly reported barriers included clinicians' concerns about their ability to deliver screening and brief interventions "to help patients to cut down", lack of "alcohol-related knowledge", and time constraints; important facilitators were support and training.

Third, some of the pharmacotherapies used in the interventions are not registered for the treatment of AUD or included in the national essential medicines list, basic insurance package, or national treatment guidelines in some countries (Krupchanka et al., 2023; WHO, 2024b). Various issues, such as national policies, financial limitations, and a lack of physician confidence and experience in administering these medicines, probably limit the number of patients who can benefit from their prescriptions (Oliva et al., 2011; Hagedorn et al., 2019). Another concern is that systematic reviews of most pharmacotherapy interventions primarily use data from clinical trials, some of which are funded by pharmaceutical companies. Publication bias could occur if studies that do not support the efficacy of these pharmacotherapies are less likely to be published than studies that demonstrate favourable treatment outcomes.

Fourth, much of this research, particularly research on face-to-face brief interventions among adults in health-care settings, was conducted more than 20 years ago. Some reviews have found smaller effects in more recently published studies (O'Connor et al., 2018; McCambridge, 2021). The reasons for the smaller effects are unknown but bring into question the degree to which the intervention effect sizes reported in some of the older literature would be comparable in current, real-world settings (O'Connor et al., 2018; McCambridge, 2021). With the rise of widespread access to computers, smartphones, and the Internet, studies were conducted in the past 20 years that explored technology-based digitally delivered approaches, which may be more acceptable in the current environment (Riper et al., 2018). However, reliance on technology-based interventions may disadvantage individuals with limited technological literacy and access to computers, potentially creating or exacerbating inequities among these groups.

Finally, the assessed interventions cannot affect the broader context of an individual's circumstances that have important effects on their health habits, including social determinants of health (e.g. access to affordable treatment, transportation, and technology platforms) and population-level alcohol policy interventions (McCambridge, 2021). Little is known about how the effects of screening, brief interventions, and treatments to reduce alcohol consumption vary based on an individual's circumstances, and best practices for adapting interventions based on cultural or personal circumstances are not well established.

6.2 Screening and brief interventions

Many screening tools have been developed to identify adolescents and adults with hazardous or harmful patterns of use of alcohol. The Alcohol Use Disorders Identification Test (AUDIT) (WHO, 2001) and a three-item abbreviated version of the AUDIT, the AUDIT-Concise (Bush et al., 1998), have been translated into dozens of languages (Saunders, 2024) and are among the most widely used instruments worldwide (Babor

and Robaina, 2016). WHO has published a guideline for using the AUDIT in primary health care (WHO, 2010b). AUDIT results are categorized into four risks levels, where higher levels of risk can lead to increasingly intensive interventions. AUDIT scores of 0–7 indicate a lower risk level, and guidance suggests education about alcohol-related harms as an appropriate intervention. Simple advice is suggested for AUDIT scores of 8–15, simple advice plus brief counselling and continued monitoring are suggested for AUDIT scores of 16–19, and referral to a specialist is suggested for AUDIT scores of 20–40. Evidence suggests that the AUDIT and AUDIT-Concise have acceptable accuracy for detecting problematic alcohol consumption in younger people (aged 9–18 years), pregnant women, and older adults (aged ≥ 60 years), in addition to general adult populations (Wood et al., 2024). The Alcohol, Smoking and Substance Involvement Screening Test (ASSIST) was developed "to assist with early identification of substance use related health risks and substance use disorders in primary health care, general medical care and other settings" (WHO, 2010b).

Among individuals who screen positive for potentially hazardous or harmful patterns of use of alcohol, subsequent steps should be tailored to the needs and resources of the individual. Brief interventions may be an appropriate initial step. Brief interventions are typically one session or a few brief sessions and commonly include education about the potential harms of alcohol consumption, advice to reduce alcohol consumption to within recommended limits, personalized normative feedback showing how an individual's alcohol consumption compares with that of others, and motivational interviewing (Humeniuk et al., 2010; SAMHSA, 2013).

The goal of motivational interviewing is to help individuals work through ambivalence and commit to change (Miller et al., 1992). Motivational interviewing techniques include adopting an empathic, non-judgemental stance, listening reflectively, developing discrepancy, rolling with resistance, avoiding argumentation, and supporting efficacy to change (Miller et al., 1992). These sessions can involve, for example, helping individuals explore their personal decisions related to alcohol consumption (i.e. pros and cons) and supporting their autonomy in making decisions related to changing their alcohol consumption patterns.

Motivational enhancement therapy, which is sometimes used in brief interventions, combines motivational interviewing with personalized normative feedback. FRAMES is a multistep framework that is commonly incorporated into motivational enhancement therapy; it comprises feedback about personal risk, personal responsibility for change, advice, having a menu of strategies, using an empathic style of interaction, and supporting self-efficacy (Searight, 2007). In the feedback step, interventions that provide normative feedback about alcohol consumption appear to be most effective when the norms are taken from a population perceived to be similar to the participant (Rodriguez et al., 2020). More intensive counselling may be required for some patients, such as those scoring as high-risk on screening tools, those meeting criteria for a harmful pattern of use of alcohol or alcohol dependence, or those who have comorbidities or polysubstance use (Humeniuk et al., 2010; SAMHSA, 2013; NIAAA, 2021). Additional treatments may include more extended outpatient treatment, inpatient or residential treatment, and pharmacotherapy; these interventions are discussed in Section 6.3.

This section summarizes the evidence on the effects of screening and brief interventions on alcohol consumption outcomes (Table 6.1).

Table 6.1 Systematic reviews of the effects of screening and brief interventions on alcoholic beverage consumption

Reference No. of studies Location (no. of studies)	Study populations Setting	Interventions Controls	Outcome Comparison	No. of comparisons (no. of participants)	Post-treatment or follow-up (months)	Effect estimates (95% CI)	Comments
Beyer et al. (2023) 201 RCTs USA (105), Europe (42), United Kingdom (21), Australasia (13), Asia (6), Africa (5), Canada (5), South America (2), Russian Federation (1), USA and Canada (1)	People with hazardous or harmful alcohol consumption who were not seeking treatment for alcohol consumption (no age restriction) Online, community, or health-care settings where the intervention was immediately accessible without referral	Brief interventions that include personalized feedback to reduce alcohol consumption (1–5 sessions) Practitioner-delivered: 118 RCTs delivered with a conversation in real time Digitally delivered: 89 RCTs delivered via a device, including text messages or other non-synchronous interaction Control: interventions that provided minimal alcohol-related information; assessment-only	**Quantity (g/week):**			**NMA MD**	Number of direct comparisons for all analyses was substantially lower, and for the comparison of practitioner-delivered vs digitally delivered interventions only 1 study reported results at 6 months or later
			Practitioner vs minimal control	52 (26 777)	6	−29 (−39 to −19)	
				45 (25 288)	12	−19 (−32 to −7)	
			Practitioner vs assessment-only	52 (26 777)	6	−28 (−37 to −18)	
				45 (25 288)	12	−21 (−31 to −11)	
			HED episodes (*n*/month):				
			Practitioner vs minimal control	19 (7 300)	6	−0.8 (−1.4 to −0.1)	
				19 (11 636)	12	−0.3 (−0.7 to 0.1)	
			Practitioner vs assessment-only	19 (7 300)	6	−0.9 (−1.4 to −0.5)	
				19 (11 636)	12	−0.4 (−0.6 to −0.3)	
			Quantity (g/week):				
			Digital vs minimal control	52 (26 777)	6	−15 (−28 to −3)	
				45 (25 288)	12	−14 (−33 to 5)	
			Digital vs assessment-only	52 (26 777)	6	−14 (−22 to −6)	
				45 (25 288)	12	−16 (−32 to 1)	
			HED episodes (*n*/month):				
			Digital vs minimal control	19 (7 300)	6	−0.1 (−1.0 to 0.8)	
				19 (11 636)	12	−0.4 (−1.2 to 0.3)	
			Digital vs assessment-only	19 (7 300)	6	−0.3 (−0.9 to 0.3)	
				19 (11 636)	12	−0.6 (−1.2 to 0.0)	
			Quantity (g/week):				
			Practitioner vs digital	52 (26 777)	6	−14 (−25 to −3)	
				45 (25 288)	12	−6 (−24 to 12)	
			HED episodes (*n*/month):				
			Practitioner vs digital	19 (7 300)	6	−0.6 (−1.3 to 0.1)	
				19 (11 636)	12	0.1 (−0.5 to 0.8)	

Table 6.1 (continued)

Reference No. of studies Location (no. of studies)	Study populations Setting	Interventions Controls	Outcome Comparison	No. of comparisons (no. of participants)	Post-treatment or follow-up (months)	Effect estimates (95% CI)	Comments
Kaner et al. (2018) 69 RCTs USA (34), United Kingdom (10), Spain (6), Australia (4), Canada (2), Finland (2), Sweden (2), Brazil (1), Denmark (1), France (1), Germany (1), Kenya (1), Poland (1), South Africa (1), Switzerland (1), Thailand (1)	People with harmful or hazardous alcohol consumption attending routine health-care visits whose alcohol consumption was identified by a screening tool General practice, emergency department, other primary care	Brief interventions to reduce hazardous or harmful alcohol consumption (1–5 sessions, total of ≤ 60 minutes); excluding digital interventions Control: assessment-only or minimal intervention	Quantity (g/week) HED episodes (*n*/week) HED (%) Quantity (g/week), primary care Quantity (g/week), emergency care	34 (15 197) 15 (6946) 10 (4456) 24 (8811) 10 (6386)	12 12 12 12 12	**MD:** −20.1 (−28.4 to −11.8) **MD:** −0.08 (−0.14 to −0.02) **RD:** −0.07 (−0.12 to −0.02) **MD:** −25.7 (−37.3 to −14.1) **MD:** −9.7 (−17.5 to −1.9)	
McQueen et al. (2011) 14 RCTs United Kingdom (5), USA (4), Taiwan, China (2), Australia (1), Finland (1), Germany (1)	People aged ≥ 16 years admitted to general inpatient hospital care for any reason other than alcohol consumption-related treatment and who screened positive for hazardous or harmful patterns of use of alcohol General medical wards, trauma centres, medical or surgical units	Brief interventions to reduce alcohol consumption (1–3 sessions) Control: assessment-only, usual care, minimal intervention	Quantity (g/week) Quantity (g/week)	4 (453) 4 (1073)	6 12	**MD** −69.4 (−128.1 to −10.7) −33.6 (−82.3 to 15.0)	

Table 6.1 (continued)

Reference No. of studies Location (no. of studies)	Study populations Setting	Interventions Controls	Outcome Comparison	No. of comparisons (no. of participants)	Post-treatment or follow-up (months)	Effect estimates (95% CI)	Comments
Foxcroft et al. (2016) 84 RCTs	Young adults (aged 15–24 years); 70 of the 84 studies in high-risk individuals or different settings (e.g. hazardous alcohol consumption, social risk factors, higher and vocational education and training, health system, military recruitment, social welfare, and criminal justice settings)	Motivational interviewing to reduce or prevent hazardous alcohol consumption (1–6 sessions) Control: no intervention, assessment-only, alternative interventions without motivational interviewing components				**SMD**	
			Quantity (drinks/week)	33 (7971)	≥ 4	−0.11 (−0.15 to −0.06)	
			Frequency (days/week)	17 (4377)	≥ 4	−0.14 (−0.21 to −0.07)	
			HED episodes (*n*/week)	12 (5479)	≥ 4	−0.04 (−0.09 to 0.02)	
			Peak BAC (%)	5 (901)	≥ 4	−0.12 (−0.20 to −0.05)	
Kohler and Hofmann (2015) 6 RCTs USA (5), Brazil (1)	Adolescents and young adults (aged 13–25 years) who screened positive for current or previous risky alcohol consumption Emergency department	Motivational interviewing to reduce alcohol consumption Control: no intervention, other intervention	**Quantity (multiple definitions)**			**SMD**	The 6 RCTs included a total of 1633 participants After excluding the study from Brazil, the effect estimate for quantity was SMD: −0.12 (95% CI, −0.24 to −0.01) and for frequency was SMD: −0.21 (95% CI, −0.34 to −0.07)
			Largest mean differences	NR	3–12	−0.09 (−0.22 to 0.04)	
			Smallest mean differences	NR	3–12	0.02 (−0.09 to 0.13)	
			Frequency (multiple definitions)				
			Largest mean differences	NR	3–12	−0.17 (−0.32 to −0.02)	
			Smallest mean differences	NR	3–12	−0.03 (−0.14 to 0.08)	

Table 6.1 (continued)

Reference No. of studies Location (no. of studies)	Study populations Setting	Interventions Controls	Outcome Comparison	No. of comparisons (no. of participants)	Post-treatment or follow-up (months)	Effect estimates (95% CI)	Comments
O'Logbon et al. (2024) 11 RCTs included in the meta-analysis of digital interventions and alcohol consumption USA (5), New Zealand (2), Australia (1), Denmark (1), Switzerland (1), United Kingdom (1)	≥ 50% of participants were adolescents and young adults (aged 10–24 years) with problematic alcohol consumption, dependence, or addiction Colleges or universities, high schools, social media, emergency departments	Digital interventions (delivered with the support of computers, mobile phones, or portable devices) with the primary aim of changing substance use-related behaviours Control: no restrictions	**Quantity (weekly consumption, units varied)** Digital vs control Digital vs face-to-face	 11 (5210) 2 (NR)	 NR NR	**SMD** −0.12 (−0.17 to −0.06) −0.11 (−0.29 to 0.07)	
Popova et al. (2023) 26 RCTs and quasi-experimental studies USA (16), South Africa (2), Brazil (1), Ireland (1), Israel (1), Netherlands (1), Norway (1), Spain (1), Sweden (1), United Kingdom (1)	Pregnant women Obstetrics or prenatal clinics within hospitals, clinics or health-care centres in rural or urban areas, midwives' offices, maternity care or women's health centres, inpatient pre-delivery, and emergency unit of the hospital	Brief interventions to reduce alcohol consumption Control: no intervention, usual care	**Quantity (drinks/week)** **Abstinence** All studies RCTs only	2 (176) 12 (2620) NR (NR)	NR NR NR	**SMD:** −0.21 (−0.78 to 0.36) **OR** 1.56 (1.15 to 2.13) 1.86 (1.39 to 2.49)	For quantity of consumption, the 1 study included 2 intervention arms and 1 control arm; for abstinence, the 10 studies included 12 intervention arms and 10 control arms

Table 6.1 (continued)

Reference No. of studies Location (no. of studies)	Study populations Setting	Interventions Controls	Outcome Comparison	No. of comparisons (no. of participants)	Post-treatment or follow-up (months)	Effect estimates (95% CI)	Comments
Ghosh et al. (2022) 14 RCTs South Africa (7), India (3), Brazil (1), China (1), Thailand (1), Zimbabwe (1)	People aged 15–65 years with non-dependent, harmful, or hazardous alcohol consumption living in low- and middle-income countries Primarily health-care settings	Brief interventions that include motivational interviewing to reduce alcohol consumption (1–6 sessions) Control: waitlisted to receive treatment at end of trial, usual care, brief or general advice	**HED episodes (*n*/month)**			**SMD**	
				2 (346)	6	0.03 (−0.18 to 0.25)	
				2 (427)	12	0.03 (−0.16 to 0.22)	
			Low-risk alcohol consumption			**OR**	
				3 (818)	3	1.20 (0.86 to 1.68)	
				4 (1668)	6	0.97 (0.64 to 1.49)	
				4 (1134)	12	1.44 (0.73 to 2.86)	

BAC, blood alcohol concentration; CI, confidence interval; HED, heavy episodic drinking; MD, mean difference between groups; NMA, network meta-analysis; no., number; NR, not reported; OR, odds ratio; RCT, randomized controlled trial; RD, risk difference; SMD, standardized mean difference.

6.2.1 Effects of screening and brief interventions

The effects of different types of brief interventions in decreasing alcoholic beverage consumption have been assessed in many studies. These studies can be grouped into face-to-face practitioner-delivered interventions and digitally delivered interventions (i.e. via devices, including text messages or other non-synchronous methods). In one of the most comprehensive reviews identified, Beyer et al. (2023) conducted a systematic review and NMA to assess the effects of practitioner-delivered brief interventions and digitally delivered brief interventions on alcohol consumption compared with each of two types of control conditions: screening or assessment-only and minimal intervention (e.g. education leaflet). In addition, the effects of practitioner-delivered brief interventions on alcohol consumption were compared with the effects of digitally delivered brief interventions. That review included 201 randomized controlled trials (RCTs) on brief interventions, which were delivered in both health-care and non-health-care settings, that included personalized feedback for people who were not seeking treatment but who screened positive for hazardous or harmful patterns of use of alcohol. Compared with assessment-only controls, practitioner-delivered brief interventions resulted in lower reported alcohol consumption at the 6-month follow-up (NMA mean difference [MD], –28 g of ethanol per week; 95% confidence interval [CI], –37 to –18 g) and 12-month follow-up (NMA MD, –21 g of ethanol per week; 95% CI, –31 to –11 g); consumption was also lower at both follow-up times when compared with the minimal intervention condition. Fewer heavy episodic drinking (HED) episodes (usually defined as ≥ 4 drinks for women and ≥ 5 drinks for men in a single occasion) were also observed at both the 6-month and 12-month follow-ups for practitioner-delivered brief interventions compared with each of the control conditions. Digitally delivered brief interventions also led to lower alcohol consumption (range, –16 to –14 g of ethanol per week) and fewer HED episodes, although not statistically significantly fewer, at the 6-month and 12-month follow-ups compared with each control condition. Compared with digitally delivered brief interventions, practitioner-delivered brief interventions led to a greater reduction in alcohol consumption at the 6-month follow-up (MD, –14 g of ethanol per week; 95% CI, –25 to –3 g), but this reduction was attenuated by the 12-month follow-up (MD, –6 g of ethanol per week; 95% CI, –24 to 12 g).

6.2.2 Effects of brief interventions when provided in health-care settings

Kaner et al. (2018) conducted a systematic review and meta-analysis to assess the effects of brief interventions delivered specifically in health-care settings (i.e. general practice or emergency departments) on alcohol consumption compared with usual care; RCTs conducted in 16 countries were included. This meta-analysis included studies that assessed interventions lasting ≤ 60 minutes, and most interventions included a mention of FRAMES, motivational interviewing, or elements of these approaches. Overall, brief interventions delivered by clinicians in health-care settings to patients who screened positive for hazardous alcohol use led to lower alcohol consumption after 1 year (MD, –20.1 g of ethanol per week; 95% CI, –28.4 to –11.8 g) and fewer HED episodes (MD, –0.08; 95% CI, –0.14 to –0.02) compared with control conditions. Also, larger effects on alcohol consumption were observed in primary care settings (MD, –25.7 g of ethanol per week; 95% CI, –37.3 to –14.1 g) than in emergency department settings (MD, –9.7 g of ethanol per week; 95% CI, –17.5 to –1.9 g). In another systematic review and meta-analysis of RCTs of brief interventions delivered in inpatient settings in high-income countries (McQueen et al., 2011),

larger reductions in alcohol consumption were observed when compared with control conditions at 6 months (MD, −69.4 g of ethanol per week; 95% CI, −128.1 to −10.7 g) and, to a lesser extent, at 12 months (MD, −33.6 g of ethanol per week; 95% CI, −82.3 to 15.0 g) of follow-up.

6.2.3 Effects of brief interventions in adolescents and young adults

The effects of brief interventions on alcohol consumption may differ for young people, who have less experience with consuming alcohol and whose developmental stage may call for different interventions than the general adult population. The effectiveness of motivational interviewing and motivational enhancement therapy on decreasing alcohol consumption was assessed in a large systematic review and meta-analysis (Foxcroft et al., 2016). It covered both universal prevention (delivered broadly, regardless of level of risk) and indicated prevention (limited to individuals identified as being at higher risk) compared with assessment-only or alternative intervention control groups in people aged 15–24 years. Overall, there were small but statistically significant reductions in the quantity of alcohol consumed (SMD, −0.11; 95% CI, −0.15 to −0.06), the frequency of alcohol consumption (SMD, −0.14; 95% CI, −0.21 to −0.07), and the estimated peak blood alcohol concentration (SMD, −0.12; 95% CI, −0.20 to −0.05) but not the number of HED episodes.

Focusing on studies conducted in health-care settings, in a systematic review and meta-analysis of RCTs of motivational interviewing among people aged 13–25 years, Kohler and Hofmann (2015) found that when limited to emergency departments there was no reduction in the quantity of alcohol consumed after 3–12 months (SMD, −0.09; 95% CI, −0.22 to 0.04), but there was a small reduction in the frequency of alcohol consumption (SMD, −0.17; 95% CI, −0.32 to −0.02). Finally, there was a small reduction in the quantity of alcohol consumed (SMD, −0.12; 95% CI, −0.17 to −0.06) in a meta-analysis of 11 RCTs of digitally delivered interventions compared with controls among people aged 10–24 years (O'Logbon et al., 2024); based on data from 2 of the 11 RCTs, the quantity of alcohol consumed was non-significantly lower for digitally delivered interventions compared with face-to-face interventions (SMD, −0.11; 95% CI, −0.29 to 0.07).

6.2.4 Effects of brief interventions in pregnant women

The Working Group identified one systematic review and meta-analysis that included 26 RCTs and quasi-experimental studies conducted among pregnant women (Popova et al., 2023). Brief interventions resulted in an increased likelihood of abstinence (overall: odds ratio [OR], 1.56; 95% CI, 1.15 to 2.13; among RCTs only: OR, 1.86; 95% CI, 1.39 to 2.49) and a modest reduction in weekly alcohol consumption (SMD, −0.21; 95% CI, −0.78 to 0.36).

6.2.5 Effects of brief interventions in low- and middle-income countries

The effects of brief interventions delivered in low- and middle-income countries, which have mostly been studied within health-care settings, were assessed by Ghosh et al. (2022). That systematic review and meta-analysis included RCTs in South Africa (7 studies), India (3 studies), Brazil (1 study), China (1 study), Thailand (1 study), and Zimbabwe (1 study). The brief interventions involved either a single session (8 studies) or a range of two to six sessions, typically using elements of FRAMES and motivational interviewing. Overall, there were no differences in the number of HED episodes (defined as ≥ 4 standard drinks for women and ≥ 6 standard drinks for men in one occasion) between the intervention and control groups at 6 months or 12 months of

follow-up. [Although only two to five RCTs were included in any subgroup analysis, the effect sizes for HED episodes were very close to zero, indicating that the lack of statistical significance was not due to lack of statistical power.] There was no consistent pattern of effect for the percentage of participants who transitioned from high-risk to low-risk alcohol consumption at 3 months, 6 months, and 12 months of follow-up.

6.3 Treatment interventions

6.3.1 Psychosocial interventions

Psychosocial interventions are recommended by WHO for the treatment of AUD (WHO, 2023). These interventions can be used either independently or with pharmacotherapies as part of a more comprehensive treatment plan to reduce alcohol consumption, promote abstinence, and prevent relapse. Several psychosocial interventions have been developed, evaluated, and gradually adopted into various treatment settings. In general, these interventions reinforce positive behavioural change and increase adherence to treatment and medications, and they can be delivered in various settings, including unspecialized and specialized outpatient and inpatient short-term treatment settings and long-term residential treatment settings (WHO, UNODC, 2020). WHO also recommends that dependent patients and their family members should be encouraged to engage with mutual help groups (e.g. Alcoholics Anonymous; AA) (WHO, 2012). This section summarizes recent reviews of psychosocial interventions used in clinical practice for treating AUD and relapse prevention (Table 6.2).

(a) Effects from a network meta-analysis of psychosocial interventions

Zhang et al. (2022) conducted a systematic review and comprehensive NMA of psychosocial interventions for the treatment of AUD that included data from 23 RCTs (22 arms). Motivation enhancement therapy, couples therapy, motivation enhancement therapy plus couples therapy, supportive therapy, and 12-step facilitation with couples therapy, but not cognitive behavioural therapy, were superior to treatment as usual in increasing the percentage of abstinent days. However, there was no clear evidence that any one of the efficacious psychosocial approaches had larger effects than the others.

(b) Effects of 12-step facilitation interventions

Twelve-step facilitation (TSF) is a professionally delivered intervention that is based on the 12 steps of AA and aims to link individuals to community AA groups (Humphreys, 1999; Alcoholics Anonymous, 2001). Kelly et al. (2020) conducted a systematic review and meta-analysis to compare manualized (standardized content delivered in a linear or modular fashion so that the same treatment is delivered across time and different sites, ensuring replicability) and non-manualized peer-led AA or TSF with other established treatments (e.g. cognitive behavioural therapy) or with other types of TSF. Manualized AA or TSF was superior in maintaining abstinence (relative risk [RR], 1.21; 95% CI, 1.03 to 1.42; at 12 months of follow-up, reported by 2 studies) and at decreasing the number of drinks per drinking day (MD, –1.02; 95% CI, –1.83 to –0.21; at 36 months of follow-up, reported by 1 study) compared with other established treatments, but was not statistically significantly better than other types of TSF. Non-manualized AA or TSF performed better than other established interventions in increasing the number of abstinent days (RR, 1.71; 95% CI, 0.70 to 4.18; at 9 months of follow-up, reported by 1 study) and decreasing the number of drinks per drinking day (MD, –1.76; 95% CI, –2.23 to –1.29; at 12 months of follow-up, reported by 2 studies; at the 9-month follow-up; reported by 1 study).

Table 6.2 Systematic reviews of the effects of psychosocial interventions on alcoholic beverage consumption

Reference	Population Setting	Intervention Comparators	Interventions vs comparator	No. of study comparisons (no. of participants)	Outcome Effect size (95% CI)	Comments
Network meta-analysis of psychosocial interventions						
Zhang et al. (2022)	Adult patients (aged ≥ 18 years) with AUD, AD, or alcohol abuse 9 countries and regions 74% male	Interventions: psychotherapies (e.g. CBT, CT, MET, SP, TSF) Comparator: TAU		23 (780 in 22 arms)	**Percentage of abstinent days: NMA MD**	Among the 23 studies, results for percentage of abstinent days were available from 22 arms
			CBT vs TAU		−20 (−44 to 3.7)	
			MET vs TAU		35 (12 to 59)	
			CT vs TAU		29 (13 to 44)	
			MET + CT vs TAU		35 (15 to 55)	
			TSF + CT vs TAU		26 (0.44 to 51)	
			SP vs TAU		22 (4.1 to 39)	
Twelve-step facilitation interventions						
Kelly et al. (2020)	Adults (aged 34–51 years) with AUD, alcohol abuse, or AD attending either outpatient treatment or an inpatient residential facility 50–100% male	Interventions: peer-led AA or professionally delivered treatments that facilitate AA involvement (TSF interventions) that are manualized or non-manualized Comparators: A = other established psychological clinical interventions (e.g. MET and CBT); B = other TSF programme			**Complete abstinence: relative risk**	Manualized refers to standardized content delivered in a linear or modular fashion so that the same treatment is delivered across time and different sites to ensure replicability
			Manualized AA/TSF vs A	2 (1936)	Follow-up, 12 months: 1.21 (1.03 to 1.42)	
			Manualized AA/TSF vs B	1 (307)	Follow-up, 12 months: 1.28 (1.00 to 1.63)	
			Non-manualized AA/TSF vs A	1 (93)	Follow-up, 9 months: 1.71 (0.70 to 4.18)	
			Non-manualized AA/TSF vs B	1 (382)	Follow-up, 12 months: 1.15 (1.02 to 1.29)	
					Percentage of abstinent days: MD	
			Manualized AA/TSF vs A	4 (199)	Follow-up, 12 months: 3.03 (−4.36 to 10.43)	
			Manualized AA/TSF vs B	1 (135)	Follow-up, 12 months: 16.40 (5.12 to 27.68)	
			Non-manualized AA/TSF vs A	1 (93)	Follow-up, 9 months: 3.00 (0.31 to 5.69)	
			Non-manualized AA/TSF vs B	NR	NR	

Table 6.2 (continued)

Reference	Population Setting	Intervention Comparators	Interventions vs comparator	No. of study comparisons (no. of participants)	Outcome Effect size (95% CI)	Comments
Kelly et al. (2020) (cont.)					**Drinks per drinking day: MD**	
			Manualized AA/TSF vs A	1 (806)	Follow-up, 36 months: −1.02 (−1.83 to −0.21)	
			Manualized AA/TSF vs B	1 (42)	Follow-up, 6 months: −4.10 (−10.4 to 2.24)	
			Non-manualized AA/TSF vs A	1 (93)	Follow-up, 9 months: −1.76 (−2.23 to −1.29)	
			Non-manualized AA/TSF vs B	NR	NR	
Contingency management interventions						
Coughlin et al. (2023)	Adults (aged ≥ 18 years or ≥ 21 years, or college students aged 18–26 years) 7 of the 9 alcohol-focused studies included abstinence outcomes	Remotely delivered (telephone, Internet, app, wearable): 4 studies monitored abstinence via remote transdermal alcohol monitors; 4 studies monitored abstinence via breath alcohol concentration with cell phone video recordings, an app with facial recognition, or a breathalyser with built-in facial recognition Comparators: no or delayed treatment, alternative therapy, or repeated measures of participation in ≥ 2 treatment arms			**Percentage of negative biological samples**	Systematic review; no MA conducted
			CM vs non-contingent	1 (30)	Follow-up, NR: 87.1 vs 66.9	
			CM vs non-contingent	1 (29)	Follow-up, NR: 89 vs 70	
			CM vs non-contingent	1 (9)	Follow-up, 9 weeks: 49 vs 27	
					Percentage of abstinent days	
			CM vs non-contingent	1 (20)	Follow-up, week 1: 82.4 vs 23.3	
			CM vs non-contingent	1 (40)	Follow-up, 1 month: 85 vs 38	
			CM vs non-contingent	1 (36)	Follow-up, 6 months: 86 vs 40	
			CM vs non-contingent	1 (30)	Follow-up, 1 month: 54.3 vs 31.2	

Table 6.2 (continued)

Reference	Population Setting	Intervention Comparators	Interventions vs comparator	No. of study comparisons (no. of participants)	Outcome Effect size (95% CI)	Comments
Other psychosocial interventions						
Kiyak et al. (2023)	Adults (aged ≥ 18 years) diagnosed with AUD Outpatients (6 studies), inpatients (5 studies)	Interventions: CET only (5 studies), CET and CST (4 studies), CET and ECET (1 study), CET and CBT (1 study) Comparators: daily contact (1 study), relaxation (3 studies), CBT (6 studies), or TAU (1 study)	CET alone or CET with CST or CET/ECET with CBT vs CBT or daily contact or TAU	7 (557)	**Number of drinks per day: SMD** Follow-up, 6–8 months: −0.35 (−0.72 to 0.03)	
			CET alone or CET with CST or CET with CBT vs daily contact or CBT or relaxation or TAU	9 (779)	**Number of drinking days: SMD** Follow-up, 6–8 months: −0.30 (−0.54 to −0.06)	
Batschelet et al. (2020)	Experimental laboratory studies of non-clinical participants, or clinical RCTs of treatment-seeking patients with AUD	Alcohol consumption-specific computerized interventions (AtBM, ApBM, and IT) (1–8 sessions) Comparators: sham, no training, no intervention, psychoeducation, non-specific exposure	AtBM	4 (NR)	**Drinking behaviour** Inconsistent effects among clinical patients with AUD	Systematic review; no MA conducted Mostly non-clinical participants and students who were young and cognitively strong but had no motivation to change drinking behaviour
			ApBM	4 (NR)	Consistent beneficial effects among clinical patients with AUD	
			IT	9 (NR)	Inconsistent effects among non-clinical participants	
Song et al. (2023)	Married or unmarried heterosexual adults (mean age, 42.2 years), couples with at least one of the spouses meeting at least one of the excessive use or dependence criteria for psychoactive substance use disorders (mainly drug use or alcohol consumption)	BCT intervention (9–32 sessions, 60–120 minutes/session); 15 of 19 RCTs addressed AUD Comparators: IBT, PACT, or other treatments	BCT vs all controls	19 (976)	**Frequency of substance use (alcohol or other drugs): SMD** Post-treatment: 0.17 (0.04 to 0.30)	MA included data from 12 studies based on 19 RCTs The effect estimates for frequency of substance use were reverse-scored to standardize the direction of the effects among the included studies
			BCT vs all controls	16 (748)	Follow-up, 12 months: 0.32 (0.18 to 0.47)	
			BCT vs IBT	7 (371)	Post-treatment: 0.23 (0.02 to 0.43)	

Table 6.2 (continued)

Reference	Population Setting	Intervention Comparators	Interventions vs comparator	No. of study comparisons (no. of participants)	Outcome Effect size (95% CI)	Comments
Multiple or combined psychosocial and pharmacotherapy interventions						
Ray et al. (2020)	Adults (mean age, 29 years) with AUD or SUD 72% male 70% in USA Participants recruited from specialty substance use or mental health clinics (20 studies), medical settings (5 studies), or community advertising (5 studies)	Intervention: CBT individual, group, or mixed sessions (median, 16 sessions; range, 4–48); most common medications: naltrexone and acamprosate; 15 of the 30 studies focused only on alcohol Comparators: A = TAU + Pharm; B = another specific therapy + Pharm	CBT + TAU + Pharm vs A CBT + TAU + Pharm vs A CBT + Pharm vs B	2 (NR) 2 (NR) 3 (NR)	**Frequency: SMD** Post-treatment: 0.28 ([−0.43 to 0.99]) Latest follow-up: 0.02 ([−0.18 to 0.22]) **Quantity: SMD** Post-treatment: 0.31 ([−0.22 to 0.84])	The effect estimates shown are for the alcohol-focused studies only The effect estimates for frequency and quantity of substance use were reverse-scored to standardize the direction of the effects among the included studies The COMBINE Study, a large alcohol trial, influenced other trials; its medication management control is not comparable to TAU
van Amsterdam et al. (2022)	People with AUD, no age restrictions listed Setting not described	RCTs testing the efficacy of (i) psychotherapy as an add-on to Pharm, and (ii) Pharm as an add-on to psychotherapy for the treatment of AUD	Pharm + psychotherapy vs Pharm alone Pharm + psychotherapy vs psychotherapy alone	9 (1185) 19 (2894)	**Relapse prevention** More effective in 3 studies; no significant difference in 6 studies More effective in 10 studies; no significant difference in 9 studies	

Table 6.2 (continued)

Reference	Population Setting	Intervention Comparators	Interventions vs comparator	No. of study comparisons (no. of participants)	Outcome Effect size (95% CI)	Comments
van Ginneken et al. (2021)	"Adults patients with harmful or hazardous alcohol or substance use"	Interventions: targeted based on prior risk screening and primarily intended to address alcohol consumption	Lay health worker-led CBT or counselling vs enhanced TAU	2 (684) 3 (786) 2 (930)	**Quantity: SMD** Post-intervention: −0.37 (−0.52 to −0.22) 1–6 months post-intervention: −0.23 (−0.56 to 0.09) > 6 months post-intervention: −0.11 (−0.29 to 0.06)	Only results from studies in low- and middle-income countries are described All outcomes and effect estimates are alcohol-specific

AA, Alcoholics Anonymous; AD, alcohol dependence; ApBM, approach bias modification; AtBM, attentional bias modification; AUD, alcohol use disorder; BCT, behavioural couples therapy; CBT, cognitive behavioural therapy; CET, cue exposure therapy; CI, confidence interval; CM, contingency management; COMBINE, Combined Pharmacotherapies and Behavioral Interventions; CST, coping skills treatment; CT, couples therapy; ECET, emotional cue exposure therapy; IBT, individual-based therapy; IT, inhibition training; MA, meta-analysis; MD, mean difference between groups; MET, motivation enhancement therapy; NMA, network meta-analysis; no., number; NR, not reported; PACT, psychoeducational attention control treatment; Pharm, pharmacotherapy; RCT, randomized controlled trial; SMD, standardized mean difference; SP, supportive psychotherapy; SUD, substance use disorder; TAU, treatment as usual; TSF, 12-step facilitation.

(c) *Effects of contingency management interventions*

Contingency management interventions are based on the operant conditioning principle, in which positive reinforcement, such as monetary incentives, is given contingent upon behavioural change (Ainscough et al., 2017). Delivery of contingency management can be facilitated through digital platforms, minimizing the necessity for clinic visits to evaluate treatment outcomes and automating the delivery of incentives (Getty et al., 2019). In a systematic review of 39 studies, Coughlin et al. (2023) assessed remotely delivered contingency management interventions for the treatment of substance use. The contingency management interventions for alcohol consumption were administered for time periods ranging from 2 weeks to 12 weeks. In 7 of the 8 studies in which specific alcohol consumption outcomes were assessed, the treatment resulted in a statistically significant increases in the percentage of abstinent days (e.g. 86% for treatment vs 40% for control at the 6-month – i.e. longest – follow-up in the largest study) or the percentage of participants with biological samples that were negative for alcohol consumption (e.g. 87.1% for treatment vs 66.9% for control – follow-up time not reported – in the largest study) (Coughlin et al., 2023).

(d) *Effects of other psychosocial interventions*

Other psychosocial interventions with some evidence of efficacy in treating AUD include cue exposure therapy, mindfulness-based interventions, cognitive bias modification, and couples therapy. Cue exposure therapy addresses urges to consume alcohol through exposure to addictive stimuli (Marlatt, 1990); in one systematic review and meta-analysis, small to medium effects of cue exposure therapy on the number of drinks per day and the number of drinking days were observed (Kiyak et al., 2023). Cognitive bias modification, a broad class of interventions, often involves computer-based training programmes that present individuals with scenarios related to alcohol consumption. Through repeated exposure and practice, individuals can learn to identify and challenge cognitive biases, developing more balanced and adaptive thinking patterns. However, the effect of cognitive bias modification has been inconsistent (Batschelet et al., 2020). Behavioural couples therapy involves increased support for abstinence for the partner or spouse by restructuring the dysfunctional bond between partners. In a systematic review and meta-analysis of individuals with psychoactive substance (predominantly alcohol consumption) use disorders, there were small but statistically significant effects of behavioural couples therapy on decreasing the frequency of substance use compared with control conditions or individual-based therapy after treatment and at 12 months of follow-up (Song et al., 2023).

6.3.2 Pharmacotherapy interventions

Pharmacotherapy interventions, typically combined with psychosocial approaches (Anton et al., 2006; Ray et al., 2020), aim to curb cravings for alcohol, lessen the reward associated with consuming alcohol, and deter alcohol consumption (Swift and Aston, 2015). At present, three medications (acamprosate, disulfiram, and naltrexone) are approved for the treatment of AUD by the United States Food and Drug Administration (FDA), and one (nalmefene) is approved by the European Medicines Agency (Stokłosa et al., 2023). In addition, WHO recommends considering baclofen for the treatment of adults with alcohol dependence after detoxification (WHO, 2023). Other medications have been used off-label in some countries, such as gabapentin and topiramate (Bahji et al., 2022; McPheeters et al., 2023a). This section describes the findings of selected systematic reviews (and meta-analyses) in which the efficacy of these

medications in treating AUD was assessed (Table 6.3).

(a) Effects of acamprosate, disulfiram, and oral naltrexone

Bahji et al. (2022) conducted a large systematic review and NMA of 156 RCTs evaluating 27 medications, including FDA-approved and off-label medications, and alternative pharmacotherapies among adults with AUD. The comparators were placebo, other medications, or non-pharmacotherapy or behavioural interventions (e.g. individual supportive therapy or cognitive behavioural therapy). Most of the RCTs were conducted in outpatient settings (153 studies), most evaluated combined pharmacotherapy and adjunctive psychotherapy approaches (92 studies), and many of the RCTs were conducted in the USA (69 studies). The median treatment duration was 12 weeks (range, 4–52 weeks). Acamprosate, disulfiram, and oral naltrexone were the most extensively studied agents and had the most consistent evidence of efficacy. For all three medications, there were increased total abstinence rates and decreased rates of heavy drinking (defined as ≥ 4 standard drinks for females and ≥ 5 standard drinks for males in one sitting) compared with placebo.

McPheeters et al. (2023b) conducted a systematic review and meta-analysis of 118 pharmacotherapy RCTs; 87 studies included psychosocial co-interventions and 100 included a co-intervention such as medical management, harm reduction, or counselling. The length of the treatment varied from 12 weeks to 52 weeks. Most of the studies were conducted in the USA and Europe. Compared with placebo, acamprosate (number needed to treat, 11; 95% CI, 1 to 32) and oral naltrexone at 50 mg per day (number needed to treat, 18; 95% CI, 4 to 32) decreased the rate of return to any drinking; oral naltrexone at 50 mg per day also decreased rates of heavy drinking (defined as ≥ 4 drinks per day for women and ≥ 5 drinks per day for men; number needed to treat, 11; 95% CI, 5 to 41). Direct comparisons between acamprosate and naltrexone showed no difference in the rates of return to any drinking (range, 75.0–80.5% with acamprosate, 65.0–83.0% with naltrexone; 3 trials; n = 800) or return to heavy drinking (range, 50.0–72.7% with acamprosate, 50.9–73.6% with naltrexone; 4 trials; n = 1141). In that review, there was no effect of disulfiram in decreasing the rate of return to any drinking or the percentage of drinking days.

(b) Effects of extended-release injectable naltrexone

McPheeters et al. (2023b) and Bahji et al. (2022) also assessed the effects of extended-release injectable naltrexone (XR-NTX) compared with placebo. In those meta-analyses, XR-NTX decreased the percentage of drinking days and heavy drinking days (McPheeters et al., 2023b) but had no effect on the rates of return to any drinking (McPheeters et al., 2023b) or heavy drinking (Bahji et al., 2022; McPheeters et al., 2023b) (Table 6.3). In a larger meta-analysis of XR-NTX, Murphy et al. (2022) conducted a systematic review with a meta-analysis that included seven RCTs of adults with AUD who received monthly injections of either XT-NTX or placebo for 2–6 months and a psychosocial intervention. All seven RCTs were conducted in outpatient clinic settings, including clinics specializing in alcohol and substance use, and HIV clinics in the USA or Europe. Four RCTs required 3–7 days of lead-in abstinence before enrolment. The XR-NTX group had fewer drinking days per month (weighted MD, –2.0; 95% CI, –3.4 to –0.6; 5 studies, n = 576) and fewer heavy drinking (defined as ≥ 4 drinks per day for women and ≥ 5 drinks per day for men) days per month (weighted MD, –1.2; 95% CI, –0.2 to –2.1; 7 studies, n = 1500) than the placebo group. Greater reductions in heavy drinking days per month were observed among studies not requiring lead-in abstinence and among studies with longer duration of treatment. However, the

Table 6.3 Systematic reviews of the effects of pharmacotherapy interventions on alcoholic beverage consumption

Outcome	No. of trials	Effect type: size (95% CI)	No. of studies	Effect type: size (95% CI)	No. of studies	Effect type: size (95% CI)	No. of studies	Effect type: size (95% CI)
		Acamprosate		***Disulfiram***		***Oral naltrexone***		***XR naltrexone***
Total abstinence	96	NMA RR: 1.33 (1.15 to 1.54)[a]	96	NMA RR: 1.71 (1.39 to 2.10)[a]	96	NMA RR: 1.15 (1.01 to 1.32)[a]	96	NMA RR: 1.31 (0.83 to 2.09)[a]
Return to any drinking	20	RR: 0.88 (0.83 to 0.93)[b]	3	RR: 1.03 (0.90 to 1.17)[b]	16	RR: 0.93 (0.87 to 0.99)[b]	2	RR: 0.96 (0.90 to 1.03)[b]
Heavy drinking	76	NMA RR: 0.78 (0.70 to 0.86)[a]	76	NMA RR: 0.19 (0.10 to 0.35)[a]	76	NMA RR: 0.81 (0.73 to 0.90)[a]	76	NMA RR: 1.01 (0.74 to 1.37)[a]
Drinking days (%)	14	WMD: −8.3 (−12.2 to −4.4)[b]	2	WMD: NS[b]	15	WMD: −5.1 (−7.16 to −3.04)[b]	2	WMD: −4.99 (−9.49 to 0.49)[b]
Heavy drinking days (%)	2	WMD: −3.4 (−6.45 to 5.86)[b]	0	–[b]	7	WMD: −4.3 (−7.60 to −0.91)[b]	3	WMD: −4.68 (−8.63 to −0.73)[b]
Drinks per drinking day (*n*)	2	WMD: 0.6 (−1.43 to 2.64)[b]	0	–[b]	9	WMD: −0.49 (−0.92 to −0.06)[b]	0	–[b]
		Baclofen		***Gabapentin***		***Topiramate***		***Nalmefene***
Total abstinence	96	NMA RR: 1.80 (1.39 to 2.34)[a]	96	NMA RR: 1.66 (1.04 to 2.67)[a]	96	NMA RR: 1.27 (1.00 to 1.62)[a]	–	–
Return to any drinking	12	RR: 0.87 (0.77 to 0.99)[c]	3	RR: 0.92 (0.83 to 1.02)[b]	1	53.8% vs 72.2%[b]	–	–
Heavy drinking	76	NMA RR: 0.72 (0.57 to 0.91)[a]	76	NMA RR: 0.82 (0.65 to 1.04)[a]	76	NMA RR: 0.79 (0.56 to 1.13)[a]	–	–
Abstinent days	16	MD (%): 9.07 (3.30 to 14.85)[c]	–	–	–	–	5	NMA SMD long-term: 0.07 (−0.04 to 0.17)[d]
Drinking days (%)	5	WMD: −5.55 (−18.79 to 7.69)[b]	1	WMD: NS[b]	8	WMD: −7.2 (−14.3 to −0.1)[b]	–	–
Heavy drinking days	13	SMD: −0.18 (−0.48 to 0.11)[c]	3	WMD (%): NS[b]	9	WMD (%): −6.2 (−10.9 to −1.4)[b]	6	NMA SMD long-term: −0.28 (−0.37 to −0.18)[d]
Drinks per drinking day (*n*)	9	MD: −0.45 (−1.20 to 0.30)[c]	2	WMD: NS[b]	7	WMD: −2.0 (−3.1 to −1.0)[b]	–	–
Total alcohol consumption	–	–	–	–	–	–	6	NMA SMD long-term: −0.25 (−0.35 to −0.16)[d]

CI, confidence interval; MD, mean difference; NMA, network meta-analysis; no., number; NS, not significant; RR, relative risk; SMD, standardized mean difference; WMD, weighted mean difference; XR, extended-release injectable.

[a] Bahji et al. (2022).

[b] McPheeters et al. (2023b).

[c] Agabio et al. (2023).

[d] Kotake et al. (2024); long-term refers to a treatment duration > 24 weeks.

risk differences in return to any drinking and in return to heavy drinking were not statistically significant between the treatment and placebo groups. The superiority of XR-NTX compared with placebo in decreasing the percentage of heavy drinking days was also observed in one other review (Kedia et al., 2023).

(c) Effects of baclofen

The benefits and harms of baclofen in achieving and maintaining abstinence or decreasing alcohol consumption in people with AUD was assessed in a large systematic review and meta-analysis of 17 RCTs (Agabio et al., 2023). In 10 of the RCTs, participants underwent detoxification before treatment, whereas in the other 7 RCTs, participants continued consuming alcohol before starting treatment. The treatment duration was 12 weeks for 15 studies and up to 48 weeks for 2 studies. Compared with placebo, baclofen decreased the rate of return to any drinking (RR, 0.87; 95% CI, 0.77 to 0.99) and increased the percentage of abstinent days but did not have statistically significant effects on the number of heavy drinking days or the number of drinks per drinking day. These findings were consistent with the findings of Bahji et al. (2022) and McPheeters et al. (2023b), in which, compared with placebo, baclofen increased total abstinence rates and decreased rates of heavy drinking (Bahji et al., 2022) (Table 6.3) and decreased rates of return to any drinking (McPheeters et al., 2023b) but had no effect on the percentage of drinking days (McPheeters et al., 2023b) (Table 6.3).

(d) Effects of other pharmacotherapy interventions

Studies of gabapentin suggest that compared with placebo, there are beneficial effects on total abstinence rates (Bahji et al., 2022) and the rates of return to any drinking (McPheeters et al., 2023b) and to heavy drinking (Bahji et al., 2022) (Table 6.3). Studies of topiramate suggest that compared with placebo, there are beneficial effects after 12 weeks of treatment on total abstinence (Bahji et al., 2022) and other alcohol consumption outcomes (McPheeters et al., 2023b) (Table 6.3). Studies of gamma-hydroxybutyrate show higher total abstinence rates than placebo (NMA RR, 1.90; 95% CI, 1.03 to 3.53) (Bahji et al., 2022). There is little evidence of beneficial effects of other medications (ondansetron, varenicline, and prazosin) on alcohol consumption outcomes (McPheeters et al., 2023a).

(e) Effects of duration of pharmacotherapy interventions

Kotake et al. (2024) conducted a systematic review and NMA that included 55 RCTs to assess the efficacy of pharmacotherapy on decreasing alcohol consumption stratified by duration of treatment. Ten of the studies assessed long-term treatment (> 24 weeks): 6 on nalmefene, 2 on naltrexone, and 1 study each on valproate and XR-NTX. Nalmefene was the only medication that demonstrated long-term efficacy compared with placebo, decreasing total alcohol consumption (NMA SMD, −0.25; 95% CI, −0.35 to −0.16) and the number of heavy drinking days (NMA SMD, −0.28; 95% CI, −0.37 to −0.18) but not the number of abstinent days.

6.3.3 Multiple or combined psychosocial and pharmacotherapy interventions

Combining psychosocial and pharmacotherapy interventions is recommended in clinical practice to enhance treatment outcomes (WHO, 2023). Psychosocial treatments aim to foster motivation for decreasing or ceasing alcohol consumption, whereas pharmacotherapy approaches target reward and craving pathways. The reviews described in Sections 6.3.1 and 6.3.2 evaluated the effects of psychosocial or pharmacotherapy interventions alone compared with placebo, no treatment, minimal treatment, or treatment as usual. The reviews discussed in this section focus on the combined effects of

psychosocial and pharmacotherapy interventions (Table 6.2).

Ray et al. (2020) conducted a systematic review and meta-analysis to assess the efficacy of cognitive behavioural therapy in combination with pharmacotherapy among individuals with AUD and other substance use disorders. The effect estimates for the frequency and quantity of alcohol consumption were reverse-scored to standardize the direction of the effects among the included studies. Among the studies focused only on alcohol consumption, compared with combined usual care and pharmacotherapy, cognitive behavioural therapy as an addition to usual care and pharmacotherapy did not appear to reduce the frequency of alcohol consumption after treatment or at the latest follow-up, and combined cognitive behavioural therapy and pharmacotherapy did not reduce the quantity of alcohol consumed.

In one systematic review that included 28 studies, combined therapies (pharmacotherapy and psychosocial) were compared with monotherapies (pharmacotherapy or psychosocial) in the treatment of AUD (van Amsterdam et al., 2022). Among the 9 studies that compared combined therapies with pharmacotherapy alone, combined therapy was more effective than pharmacotherapy alone in preventing relapse to alcohol consumption in 3 studies. [However, one of the studies had a high dropout rate and the other two were limited to men who have sex with men, which may not be representative of people with AUD overall.] Among 19 studies that compared combined therapies with psychosocial intervention alone, the added value of combined therapies over psychotherapy alone in preventing relapse to alcohol consumption was observed in 10 studies. The authors concluded that combined therapy and pharmacotherapy alone can effectively treat AUD, but psychotherapy works best as combined therapy with pharmacotherapy.

6.3.4 Interventions implemented in low- and middle-income countries

The effectiveness of psychosocial or pharmacotherapy interventions on decreasing alcohol consumption has been assessed in only a few studies in low- and middle-income countries (Botwright et al., 2023). van Ginneken et al. (2021) assessed the effects of cognitive behavioural therapy and counselling delivered by lay health workers to "adult patients with harmful or hazardous alcohol or substance use" in low- and middle-income countries compared with treatment as usual. After treatment, there was a reduction in alcohol consumption (SMD, −0.37; 95% CI, −0.52 to −0.22). However, the effect size was smaller and no longer statistically significant at longer follow-up (Table 6.2).

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7. SUMMARY

7.1 Overview of *IARC Handbooks* Volume 20B

Consumption of alcoholic beverages is a major risk factor for the global burden of disease and mortality, including that due to cancer. In 2010, the World Health Assembly endorsed the Global Strategy to Reduce the Harmful Use of Alcohol (resolution WHA63.13), which describes a set of alcohol policies – defined as a set of interventions or actions "in a jurisdiction or society aimed at minimizing the health and social harms from alcohol consumption" – that are grouped into 10 recommended target areas. Three cost-effective "best buy" alcohol policy interventions to reduce alcohol-related harm were identified by the World Health Organization (WHO) in 2017 and recently reconfirmed: "1. Increase excise taxes on alcoholic beverages; 2. Enact and enforce bans or comprehensive restrictions on exposure to alcohol advertising (across multiple types of media); 3. Enact and enforce restrictions on the physical availability of retailed alcohol (via reduced hours of sale)".

For Volume 20B of the *IARC Handbooks*, the Working Group reviewed and evaluated the strength of evidence on the potential for alcohol policy interventions related to (i) taxation and pricing policies, (ii) policies to limit physical availability, (iii) alcohol marketing policies, and (iv) government alcohol monopolies and other coordinated multiple alcohol policy interventions to reduce alcohol consumption. These interventions were selected because their implementation aims to reduce alcohol consumption at the population (i.e. national or subnational) level, which has been shown to effectively reduce alcohol-attributable harms.

The primary outcome considered in this volume is average change (or difference) in total alcohol consumption – defined as the sum of at least the three major alcoholic beverage types (i.e. spirits, wine, and beer) – over time. If outcome data on total alcohol consumption were not available, then studies with data on consumption of all major beverage types separately were included. In some studies, the outcome assessed was alcohol sales or purchases. Exceptionally, for some alcohol policy interventions where few studies on alcohol consumption outcomes were available, studies using proxy outcomes for alcohol consumption (e.g. liver cirrhosis mortality) were included if an association with alcohol consumption was previously established.

Several types of studies with population-level or with individual-level or household-level measures of alcohol consumption contributed to the body of evidence.

General methodological issues considered include control for important confounding factors (e.g. income) and inclusion of appropriate control groups or jurisdictions. In studies

with individual-level or household-level data, biases may result from differential sampling, non-response, or reporting of alcohol consumption. In studies with population-level data, the proportion of the total adult alcohol per capita consumption (APC) that is due to unrecorded alcohol consumption, which can be substantial in some countries, may be relevant to studies that assessed the effects of alcohol policy interventions on recorded alcohol sales data only.

7.2 Taxation and pricing policies

7.2.1 General concepts and considerations

(a) Key concepts and definitions

Alcohol taxation and pricing policies may be used by countries for public health purposes to reduce alcohol-related harms. Taxation and pricing policies that increase the price of alcoholic beverages generally reduce the affordability of alcoholic beverages for the consumer, which can be defined as the proportion of income needed to purchase a given amount of alcohol. In addition to affordability, price elasticity of demand also plays an important role in the effects of tax increases on the demand for alcoholic beverages. One type of price elasticity is own-price elasticity, which measures the proportionate change in the demand for a product in response to a proportionate change in the price of the product.

(b) Types of taxation and pricing policies and their global variation

Three types of taxes may be applied to alcoholic beverages: general taxes (such as sales tax, value-added tax, and goods and services tax), excise taxes, and customs taxes. Pricing policies include minimum pricing (by reference either to the volume of the product or to the ethanol content of the product), restrictions on discounting and price promotions (such as offering lower prices if a customer buys multiple alcohol products), and restrictions on subsidies. As of July 2022, 148 WHO Member States applied national-level excise taxes to at least one type of alcoholic beverage. Member States that did not apply an excise tax on alcoholic beverages tended to have a comprehensive ban on alcohol sales.

(c) Study eligibility and methodological considerations

In addition to the general study eligibility criteria, studies that were considered for inclusion had to assess a taxation or pricing policy for at least one of the three major types of alcoholic beverages (i.e. spirits, wine, and beer). Studies on the effects of a single alcohol taxation or pricing policy or on the effects of multiple policies of the same type of taxation or pricing policy implemented during the same period or within the period of repeated measurement were also eligible for inclusion. However, studies on the effects of a combination of different types of alcohol policies (e.g. changes in taxation and cross-border allowances) were excluded.

The sample size, appropriate length of follow-up, weighting for non-probability sampling and statistical analysis that included seasonality, and presence of sensitivity analyses were considered in assessing study quality.

7.2.2 Effects of taxation and pricing policies on alcoholic beverage consumption

(a) Taxation

The effects of alcohol taxes on alcoholic beverage consumption were assessed in four informative studies with population-level data: two in the USA, one in 14 countries in the Organisation for Economic Co-operation and Development (OECD), and one in Sweden. Increases in excise taxes were assessed in two studies, an increase in sales tax was assessed in the third study, and a revenue-neutral tax reformulation was assessed in the fourth study.

Among these studies, different robust analytical strategies were used. Key confounding factors were effectively controlled for in most studies. Overall, increases in alcohol taxes were associated with reductions in alcohol consumption or sales. The magnitude of the effect varied by context and by tax policy. In the most informative study, the 50% increase in alcohol sales tax (from 6% to 9%) in 2011 in Maryland, USA, resulted in a reduction in total alcohol sales (–3.78%; 95% confidence interval [CI], –4.82% to –2.74%) during the 18 months after the tax increase relative to expected sales estimated from trends in the 18 months before the tax increase.

The effects of alcohol taxes on alcoholic beverage consumption also were assessed in four informative studies with individual-level or household-level data: two repeated cross-sectional survey studies and one longitudinal panel study in the USA and one repeated cross-sectional study in Australia. In one of the most informative studies using a difference-in-difference model, the effect of a 2009 excise tax increase (21% for beer, 90% for wine, and 90% for spirits) in Illinois, USA, on the change in total alcohol purchases compared with that in other states of the USA without a tax increase was assessed. From the pre-tax-increase period to the tax-increase period, there was a 6.4% ($P < 0.05$) reduction in total alcohol purchases in the heavy alcohol consumption category and a 4.3% ($P < 0.10$) reduction in the other alcohol consumption category in Illinois compared with the other states in the USA. In another study, in Australia, the effect of an excise tax increase (70%) during 2008–2009 on ready-to-drink alcoholic beverages on the change in alcohol consumption was assessed. In the most fully adjusted difference-in-difference model, 9 years after the intervention there was a decrease (–8.9%; $P < 0.001$) in alcohol consumption among adults aged 15–69 years compared with adults aged ≥ 70 years.

Two other bodies of evidence were considered in evaluating the effects of alcohol taxation policies on alcoholic beverage consumption. First, there is consistent evidence that alcohol tax changes are reflected in near-equivalent changes in the prices that consumers pay for alcohol (full pass-through rate). Second, there is a large and consistent body of evidence from around the world that increasing the price of alcoholic beverages results in lower total alcohol consumption.

(b) Minimum pricing

The effects of alcohol minimum pricing policies on alcoholic beverage consumption were assessed in four studies with population-level data, in the United Kingdom ($n = 1$), Saskatchewan, Canada ($n = 1$), and the Northern Territory, Australia ($n = 2$), and in six studies with individual-level or household-level data in Scotland, United Kingdom. In the well-controlled study with population-level data in the United Kingdom, off-premises sales of alcohol decreased by at least 3.0% after the introduction of the minimum unit price (MUP) in May 2018 in Scotland, relative to the changes in alcohol sales in other United Kingdom jurisdictions without the policy. The minimum pricing policies also led to a reduction in alcohol consumption in the other three studies. Among the six well-controlled studies with individual-level or household-level data in which the effect of the May 2018 Scotland MUP policy was assessed, there were consistent decreases in purchases of alcoholic beverages after implementation of the policy. In the study with the longest period after implementation of the MUP policy, the reduction in off-premises alcohol purchases from the pre-MUP period (January 2015–April 2018) to a mid-term MUP period (January–July 2020) was 7.06 g of ethanol per adult per household per day (95% CI, –7.47 to –6.66 g).

(c) Bans on discounting

Three studies assessed the effects of the same October 2011 Scotland, United Kingdom, ban on multibuy discounts (e.g. buy two, get one free) for off-premises alcohol purchases, and included control for or comparison with off-premises alcohol sales or purchases in England and Wales combined, United Kingdom. In the study with population-level data, the ban led to a decrease in off-premises alcohol sales. In the two studies with household-level data, there was no difference between Scotland and England and Wales combined in the percentage change in off-premises alcohol purchases from the pre-ban period to the ban period in one study, whereas in the other study there was an increase in off-premises alcohol purchases between the pre-ban period and the ban period.

7.3 Policies to limit physical availability

7.3.1 General concepts and considerations

(a) Key concepts

National and subnational governments can control all or parts of the alcoholic beverage distribution chain to restrict availability of alcoholic beverage products. An important aspect of the distribution chain that is specific to alcoholic beverages is the distinction between the retail sale of a product to be consumed elsewhere (off-premises) (e.g. stores selling primarily alcoholic beverages or grocery stores) and the sale and service of alcoholic beverages to individuals who consume alcohol on the premises of the seller (on-premises) (e.g. bars and restaurants).

A government has four main options for establishing systems to regulate the availability of alcoholic beverages: (i) do nothing beyond general controls of consumables, (ii) license nongovernmental parties to do business (i.e. privatize), (iii) run at least part of the chain (i.e. monopolize it), or (iv) establish and enforce total bans (i.e. prohibitions) on all or parts of the distribution chain, and in some jurisdictions on alcohol consumption.

(b) Types of alcohol policy interventions to limit physical availability

Interventions to limit the physical availability of alcoholic beverages include: total bans and partial bans on alcohol sales; regulations on the number, density, and location of alcohol outlets; regulations on the days and hours of alcohol sales; regulations on the minimum legal purchase and drinking age; regulations on the quantities and types of alcoholic beverages sold; and, most recently, regulations on online alcohol retail sales and delivery services.

Globally, there is considerable variability in both the types of regulatory systems and alcohol policy interventions that affect alcohol availability. However, most countries that permit alcohol consumption by their citizens impose a minimum legal purchase age and drinking age as well as restrictions on the days and hours of alcohol sales.

(c) Study eligibility and methodological considerations

In addition to the general study eligibility criteria, studies that were eligible for inclusion had to assess an availability policy intervention for at least one of the three major types of alcoholic beverages (i.e. spirits, wine, and beer). For alcohol availability policy interventions that affect alcohol outlet density and for total bans on alcohol sales, there was a paucity of studies in which alcohol consumption was the outcome. Therefore, for those interventions, studies on proxy outcomes were included. Studies that focused only on special populations (e.g. pregnant women) and studies on the effects of interventions that target patrons who are intoxicated, specific types of alcoholic beverages that constitute a very small proportion of the alcohol market

(e.g. alcopops), the volume of packaged alcohol, and alcohol purchase quantities were excluded, because these policy interventions were unlikely to have meaningful effects on alcohol consumption in the general population.

Methodological considerations that are particularly relevant to the evaluation of studies on alcohol availability policy interventions include the potential for bias due to cross-border shopping, and stockpiling of alcoholic beverages, for example during the COVID-19 pandemic.

7.3.2 Effects of alcohol policy interventions to limit physical availability on alcoholic beverage consumption

(a) Regulations on alcohol outlet density or location

Six studies in high-income countries were available in which the effect of alcohol policy interventions that regulate alcohol outlet **density** on alcoholic beverage consumption or proxy outcomes were assessed.

The effects of alcohol policy interventions that **restrict** alcohol outlet density on proxy outcomes were assessed in two studies with population-level data. In one study in the USA, a restriction on the retail sale of alcohol and its enforcement, which led to a reduction in the density of on-premises alcohol outlets in one community, was associated with a 2-fold greater reduction in exposure to violent crime relative to comparison communities. In the other study, in the United Kingdom, annual alcohol-related hospital admissions were 2.3% lower ($P < 0.05$) in local jurisdictions with the highest intensity of enforcement of licensing scrutiny compared with jurisdictions with no activity.

The effects of **permissive** alcohol outlet density policy interventions on alcohol consumption or proxy outcomes were assessed in three studies with population-level data and one study with individual-level data. In one study in Seattle, Washington, USA, each additional alcohol outlet in a given census block was associated with an increase in the annual number of aggravated assaults of 7.7% (95% CI, 3.3% to 12.2%) for off-premises outlets and 4.6% (95% CI, 3.3% to 5.9%) for on-premises outlets, and an increase in the annual number of non-aggravated assaults of 6.1% (95% CI, 2.2% to 10.1%) for off-premises outlets and 5.0% (95% CI, 3.8% to 6.3%) for on-premises outlets. Similarly, in a study in British Columbia, Canada, each additional off-premises alcohol outlet per 10 000 adults was associated with an increase in annual per capita alcohol consumption of 0.57 L of ethanol (95% CI, 0.09 L to 1.04 L). In the other two studies, interventions that permitted the sale of wine or the sale of alcoholic beverages in grocery stores were not associated with an increase in alcohol sales or, among adolescents, with the probability of transitioning from alcohol abstention to low-risk alcohol consumption, although the probability of transitioning from alcohol abstention to high-risk regular alcohol consumption increased.

In three nationwide population-based studies, the associations of **restrictions** on the **location** of alcohol outlets with recorded alcohol consumption were inconsistent.

(b) Regulations on days or hours of alcohol sales

The effects of **bans** on Sunday alcohol sales were assessed in two studies with individual-level data, and the effects of **permitting** Saturday or Sunday alcohol sales were assessed in two studies with population-level data and two studies with individual-level data; all the studies were in high-income countries. In all six studies, one extra day of alcohol sales was positively associated with alcohol consumption. For example, in the only controlled trial, permitting Saturday off-premises alcohol sales in Sweden increased monthly per capita alcohol consumption by 3.7% ($P < 0.001$). In the other study with population-level data, in the USA, state-level

alcohol policy interventions permitting Sunday off-premises alcohol sales led to a small increase in annual alcohol consumption (2.8%; $P = 0.05$). In the largest study with individual-level data, in the USA, state-level bans on Sunday off-premises alcohol sales were associated with lower daily alcohol consumption among individuals who consumed alcohol in the past month; the reduction in daily alcohol consumption was largest in the group with high education (–8.6%; 95% CI, –9.5% to –7.7% among women; –3.9%; 95% CI, –4.9% to –3.0% among men), with smaller reductions in the groups with medium education and low education.

Among the four studies on alcohol policy interventions that regulate the **hours** of alcohol sales, the effects of interventions that **restricted** hours of sales were assessed in two studies with population-level data and one study with individual-level data. Earlier closing of on-premises alcohol outlets in Australia was associated with decreased alcohol consumption (–2.9 L of ethanol per capita per year; 95% CI, –3.4 L to –2.4 L). In a study in the Baltic countries and Poland, a ≥ 20% reduction in weekly hours of off-premises alcohol sales was also associated with decreased alcohol consumption (–0.33 L of ethanol per capita per year; 95% CI, –1.06 L to 0.41 L). In a study of adolescents, a 10% higher stringency score for hours of alcohol sales was associated with a lower prevalence of alcohol consumption (odds ratio [OR], 0.80; 95% CI, 0.69 to 0.94). The effect of an intervention that **increased** hours of sales was assessed in one study in the United Kingdom with population-level data. In that study, permitting an extra hour of alcohol sales in restaurants was associated with an increase of 9% (90% CI, 5% to 13%) in alcohol consumption.

(c) Regulations on minimum legal purchase or drinking age

The effects of regulations on minimum legal purchase or drinking age on alcohol consumption were assessed in five studies with population-level data and nine studies with individual-level data. Ten of the studies were conducted in the USA, where the minimum legal drinking age fluctuated between 18 years and 21 years throughout the 1970s and the 1980s; the four other studies were in other high-income countries.

In seven of the nine studies with individual-level data, establishment of a minimum legal purchase or drinking age or an increase in the minimum legal purchase or drinking age was associated with reduced alcohol consumption. For example, in a study in New York State, USA, an increase in the minimum legal purchase age from 18 years to 19 years led to a decrease of 21.1% ($P < 0.01$) in the prevalence of alcohol consumption during the past month in a representative sample of individuals aged 18 years. In a representative sample of students aged 11–17 years in Denmark, compared with the year before the implementation of a minimum legal purchase age of 15 years in July 1998, the prevalence of alcohol consumption during the past month was lower in each of the subsequent 3 years (1998: OR, 0.84; $P = 0.086$; 1999: OR, 0.78; $P = 0.012$; 2000: OR, 0.73; $P = 0.001$).

In a study with population-level data that assessed four increases and one decrease in the minimum legal purchase age in five European countries, a 1-year increase in the minimum legal purchase age was associated with a decrease (–9.8%; 90% CI, –15.4% to –4.2%) in alcohol per capita consumption. In the four other studies with population-level data, a decrease in the minimum legal purchase or drinking age was associated with an increase in alcohol consumption.

(d) Total and partial bans on alcohol sales

The effects of total bans on alcohol sales on alcohol consumption were assessed in two studies with population-level data and two studies with individual-level data, and the effects of total bans on alcohol sales on proxy outcomes were assessed in five studies with population-level data.

The studies were conducted in both high-income and low- and middle-income countries in Africa, North America, and South Asia.

Total bans on alcohol sales were consistently associated with large decreases in alcohol consumption. In the most informative study with population-level data, during implementation of the National Prohibition in the USA from 1920 to 1933 there was an initial 70% decrease in alcohol per capita consumption, which gradually increased before plateauing at 60–70% of pre-Prohibition levels. In the most informative study with individual-level data, after the 2016 ban on alcohol sales in Bihar, India, the prevalence of alcohol consumption (at least once per week) among men decreased (–7.1 percentage points; 95% CI, −9.6 to −4.6 percentage points) from 2015–2016 to 2020–2021 compared with neighbouring states.

Total bans on alcohol sales were consistently associated with decreases in liver cirrhosis mortality, alcohol-related mortality, injury-induced mortality, alcoholism, trauma admission rates, and homicides in five other studies.

The effects of a short-term partial ban on on-premises alcohol sales in the United Kingdom during the COVID-19 pandemic were assessed in two studies. The studies showed no change in alcohol purchases or alcohol consumption, respectively.

(e) Multiple alcohol availability policy interventions

The effects of the introduction or weakening of multiple alcohol availability regulations, including alcohol outlet density, days or hours of alcohol sales, and minimum legal purchase or drinking age, on alcohol consumption were assessed in three studies with population-level data and two studies with individual-level data. In general, the restrictive interventions were associated with a reduction in alcohol consumption or alcohol sales, whereas the permissive interventions were associated with an increase.

7.4 Alcohol marketing bans

7.4.1 Definitions and general considerations

Alcohol marketing is ubiquitous, and alcohol marketing activities are rapidly evolving. Because marketers can shift their spending from more-regulated to less-regulated activities, anything short of a comprehensive ban – defined as legislative adoption of bans on all forms of alcohol marketing for all types of alcoholic beverages – may result in expansion of marketing within and across sectors not subject to a comprehensive ban. This ability to shift resources underscores the importance of assessing the potential effects of alcohol marketing bans on total alcohol consumption.

Most studies on the effects of alcohol marketing bans on alcoholic beverage consumption have not assessed comprehensive bans. The available evidence is based on studies that assessed strong bans – defined as bans on alcohol marketing in at least one major type of media (i.e. print, broadcast, or outdoor) for all types of alcoholic beverages – and studies of a mix of strong bans and partial bans or partial bans only.

7.4.2 Alcohol marketing and alcohol consumption

Alcohol marketing can be described across five levels: personalized marketing, point-of-sale marketing, mass media marketing, consumer marketing, and stakeholder marketing. Exposure to and engagement with all types of alcohol marketing creates a normative culture that encourages and supports alcohol consumption. The shift from mass media towards digital and social media marketing has increased the reach, level of engagement, and cross-border nature of alcohol marketing. Contemporary marketing specifically targets subpopulations who experience disproportionately negative effects from alcohol marketing. There is substantial evidence that exposure to and/or engagement with alcohol

marketing is generally associated with increased alcohol consumption, and a causal role has been established among young people.

7.4.3 Alcohol marketing bans

(a) Types of alcohol marketing bans and their geographical variability

There are four principal approaches for regulating alcohol marketing: (i) no restrictions; (ii) industry self-regulatory, co-regulatory, or voluntary actions; (iii) legislative adoption of partial restrictions; and (iv) legally mandated comprehensive bans on all forms of alcohol marketing for all types of alcoholic beverages. Efforts to ban alcohol marketing have focused primarily on mass media and point-of-sale marketing, and most countries have taken little or no action on banning alcohol marketing in digital and social media. WHO has called for greater global collaboration to address the challenge of alcohol marketing types that transcend national borders.

(b) Effects of alcohol marketing bans on alcoholic beverage consumption

(i) Study eligibility and methodological considerations

In addition to the general study eligibility criteria, experimental studies of the immediate effects of alcohol marketing bans or studies of alcohol industry self-regulation of marketing were excluded. Methodological considerations that are particularly relevant to the evaluation of studies on alcohol marketing bans include the potential for bias due to collinearity and endogeneity.

(ii) Strong bans

The effects of strong alcohol marketing bans on alcoholic beverage consumption were assessed in four studies with population-level data. In one interrupted time-series study using data from 1960 through 2006 in Norway, a 1975 ban on alcohol marketing in all media types for all beverage types with an ethanol content greater than 2.5% was associated with a 7.1% ($P = 0.002$) reduction in alcohol consumption. However, that study lacked a control. Two panel-regression studies used similar data from 20 and 17 OECD countries, respectively. In the study with data from 1970 through 1995, a 1-unit increase in the total alcohol advertising ban score was associated with an 8.6% ($P < 0.10$) reduction in alcohol consumption. In the study with data from 1975 through 2000, a ban in broadcast media for all types of alcoholic beverages except weak beer was associated with a 3.8% ($P < 0.05$) reduction in alcohol consumption. In a fourth study, which assessed a 14-month alcohol marketing ban in a single province in Canada, there was no change in alcohol consumption during the ban years compared with the pre-ban years, whereas consumption increased in a similar province without a ban.

(iii) Other evidence

Seven studies were available to assess other evidence on the effects of alcohol marketing bans on alcoholic beverage consumption. These included four studies of a mix of strong bans and partial bans in European and North American countries, two studies of marketing bans on distilled spirits only in OECD countries and states in the USA, and one study on the lifting of a partial alcohol marketing ban in a province in Canada. Among these studies, the effects of the alcohol marketing bans on alcohol consumption were inconsistent.

7.5 Coordinated and other multiple alcohol policy interventions

7.5.1 General considerations

Coordinated multiple alcohol policy interventions are defined here as government alcohol monopolies or other coordinated multiple

alcohol policy interventions (i.e. a set of interventions that are enacted and implemented as part of a national action plan to reduce alcohol-related harms).

7.5.2 Government alcohol monopolies

A government alcohol monopoly exists when responsibility for all or part of the alcohol market is allocated to a national or subnational governmental department or authority. Such a monopoly can facilitate coordinated multiple alcohol policy interventions. The aim, organization, and operation of government alcohol monopolies can vary substantially. A government alcohol monopoly can control the price, availability, and marketing of products containing ethanol to limit alcohol consumption. In recent decades, very few government alcohol monopolies have been established, but some have been weakened or dissolved.

The effects of weakening or dissolution of government alcohol monopolies on alcoholic beverage consumption were assessed in 12 informative studies covering the period 1965–2012. Nine studies were based on population-level data, one study was based on individual-level, one study was based on household-level data, and one study was based on both population-level and individual-level data. Most of the studies were in the USA, and the others were in Canada, Finland, and Poland.

The effects of partial or complete privatization of government alcohol monopolies on alcoholic beverage consumption were assessed in 9 studies with population-level data, 2 studies with individual-level or household-level data, and 1 study with both population-level and individual-level data. In the two studies that assessed the dissolution of government monopolies on all alcoholic beverages, there was a 9% increase in APC ($P < 0.10$) in Poland and a 5% increase in alcohol sales ($P \geq 0.10$) in a province in Canada. State-level privatization of sales of distilled spirits was associated with increases in the monthly household alcoholic beverage purchases (by 2.01 ounces of ethanol; 95% CI, 0.64 to 3.38 ounces) [59.4 mL; 95% CI, 18.9 to 100 mL] in one state and in annual total alcohol sales (by 24 000 L of ethanol) in another state in the USA. In the six studies of the privatization of the sales of wine, wine consumption increased by at least 13.0% (95% CI, 1.2% to 26.2%), and consumption of other types of alcoholic beverages did not change. In the other two studies, privatization of the sales of wine and spirits or medium-strength beer was associated with increased prevalence of heavy alcohol consumption or total alcohol consumption.

7.5.3 Other coordinated multiple alcohol policy interventions in selected countries

(a) Estonia

In Estonia, a coordinated multiple alcohol policy intervention strategy for reducing alcohol consumption and alcohol-attributable harms was drafted in 2010: the Green Paper on Alcohol Policy (GP). The GP was approved by the Estonian government in 2014, although implementation began in 2013.

The effects of the coordinated interventions on APC are described in a report from the WHO Regional Office for Europe. Before the GP period (2001 to 2012, excluding 2009), the APC increased (mean difference from the previous year, 0.47 L); during the GP core period (2013–2019), the APC decreased (mean difference from the previous year, −0.25 L), and during the COVID-19 pandemic (2020–2022), the APC increased again (mean difference from the previous year, 0.27 L). Among adolescents, compared with the prevalence of alcohol consumption in 2009–2010, the prevalence of alcohol consumption also decreased over time (OR, 0.48 for 2013–2014, 0.31 for 2017–2018, and 0.22 for 2021–2022; all ORs had $P < 0.05$).

(b) Lithuania

In 2008, the Lithuanian government declared a "year of sobriety", and in 2008–2009 several coordinated interventions were implemented. However, there was a period of relative inaction in alcohol policy after 2009, and by 2013 the APC in Lithuania was among the highest in the WHO European Region. In March 2017, new coordinated interventions at the population level, including those consistent with the WHO "best buys", and at the individual level were implemented.

The effects of the coordinated interventions were assessed based on descriptive analyses of trends in the APC from 2001 through 2019. During and 1 year after implementation of the coordinated interventions, the APC decreased (mean difference from the previous year, −0.55 L for 2008–2010 and −0.50 L for 2017–2019). However, before, between, and after the periods of implementation of the coordinated interventions, the APC increased (mean difference from the previous year, 0.50 L for 2001–2007, 0.047 L for 2011–2016, and 0.40 L for 2020).

(c) Russian Federation

The Russian Federation has a long history of changes in alcohol consumption, alcohol-attributable harms, and alcohol policies. After the Second World War, there was a substantial increase in alcohol-attributable harms. An anti-alcohol campaign was put in place in 1985–1988 (period 1; very intense activity). That anti-alcohol campaign was unpopular, and in 1990–1994, most of the anti-alcohol campaign policy interventions were repealed (period 2; baseline or dissolution of policies) and the government monopoly on alcohol production and sales was abolished. In 1995–1998 (period 3; intense activity), the Federal Assembly of the Russian Federation passed federal laws encompassing numerous restrictive interventions. In 1999–2003 (period 4; inactive or indifferent), few or no interventions were implemented. In 2004–2007 (period 5; very intense activity), key restrictive interventions were implemented, as well as specific interventions to reduce availability of unrecorded alcohol. In 2008–2009 (period 6; less-intense activity), few new interventions were implemented, but the Federal Service for Alcohol Market Regulation was formed and the National Concept to Reduce Alcohol Abuse and Alcohol Dependence in the Population (2010–2020) was published. In 2010–2013 (period 7; very intense activity), several key interventions were introduced. In 2014–2018 (period 8; moderate and indifferent, but generally stricter), alcohol policies fluctuated.

The effects of changes in alcohol policy interventions on alcohol consumption were assessed in three informative studies based on population-level data and one study based on individual-level data.

The study with the longest duration (i.e. 1980–2016) allowed for an assessment of changes in the APC during each of the eight periods of policy changes. During the four periods of very intense or intense restrictive alcohol policy activities, alcohol consumption decreased by 5.2 L (1985–1987), 6.4 L (1995–1998), 2.8 L (2004–2007), and 1.8 L (2010–2013). During the two periods of less-intense or moderate activities, there were smaller decreases in alcohol consumption (–1.1 L in 2008–2009 and −0.9 L in 2014–2016). In contrast, during the periods classified as baseline (1990–1994) or inactive or indifferent (1999–2003), alcohol consumption increased, by 7.1 L and 4.0 L, respectively. In another study with population-level data, the APC decreased by 42% between 2008 and 2019 when 15 of the 21 interventions in the national concept were implemented. In a more recent study with population-level data, the implementation of several key interventions in 2010–2014 was associated with a decrease in the recorded APC (–1.4 L), whereas after the loosening of

several interventions, there was a modest increase (0.5 L) in the recorded APC from 2017 through 2021.

In a repeated cross-sectional survey study, compared with 2006–2011, the prevalence of abstention from alcohol consumption for 2012–2017 was higher among both women (8.4%; $P < 0.05$) and men (15%; $P < 0.01$), and the volume of alcohol consumption was lower among both women (–6.1%; $P < 0.001$) and men (–9.5%; $P < 0.001$).

(d) Thailand

The predominately Buddhist culture of Thailand historically discouraged alcohol consumption, but globalization and modernization have challenged these traditional values. The APC in Thailand increased from 0.3 L in 1961 to almost 8 L in 2007. In 2008, Thailand enacted the Alcoholic Beverage Control Act, which included restrictions on alcohol marketing and availability of alcoholic beverages and increased access to treatment and rehabilitation of alcohol dependence. Other laws governing alcohol policies also evolved, including modifications to the alcohol excise tax system and drink-driving countermeasures.

The effect of the Alcoholic Beverage Control Act on alcoholic beverage consumption was assessed in two studies. In a descriptive analysis, the percentage increase in the APC from 2010 through 2019 in Thailand (3.7%) was lower than that in Viet Nam (26.8%), where no major alcohol policy changes occurred until 2020. In a repeated cross-sectional survey study of adolescents, the prevalence was higher in 2016 than in 2007 both for lifetime alcohol consumption (prevalence ratio, 2.1; 95% CI, 1.8 to 2.5 among girls and 1.4; 95% CI, 1.2 to 1.5 among boys) and for 12-month alcohol consumption (prevalence ratio, 1.9; 95% CI, 1.6 to 2.2 among girls and 1.2; 95% CI, 1.1 to 1.3 among boys).

7.5.4 Other evidence

(a) Alcohol policy scores

Researchers have developed composite restrictiveness indices or scales of multiple alcohol policy interventions to rate the existence, implementation, enforcement, and/or effects of the alcohol policy environment within a jurisdiction. Each alcohol policy intervention included in an index or scale is assigned a weighted score, and the weighted scores are summed to yield a total score; a higher value indicates a more restrictive and/or more effective set of alcohol policy interventions.

The Working Group reviewed a representative sample of eight studies based on different world regions, alcohol policy restrictiveness indices or scales, and age groups. The largest study included population-level data from 167 countries and an alcohol policy index total score based on all 10 of the alcohol policy areas in the 2010 WHO global strategy. Among the five methods used to estimate the total score in that study, there was a consistent inverse association between the score and the APC (ranging from –0.024 L to –0.014 L per 1-point increase in the score). In the seven other studies, consistent inverse associations were observed between the total scores and the APC.

(b) Community action trials

Communities play an important role in enforcing alcohol policies, advocating for policy change, and ensuring that alcohol policies are not just imposed top-down but are actively supported and maintained through collective efforts. Community action typically involves a core group that supports and encourages collaboration and engagement among government officials and local stakeholders for decision-making about the specific intervention needs in a community.

The effects of multiple alcohol policy interventions at the community level on alcohol

consumption were assessed in three controlled trials. In a cluster-randomized controlled trial of 20 communities in Australia, implementation of 13 interventions in the experimental communities resulted in a reduction in alcohol consumption (–1.90 standard drinks per week; 95% CI, –3.37 to –0.43) compared with the control communities. The other two trials were both in the USA. Compared with the control communities, in the experimental communities there were reductions in the 30-day prevalence of alcohol consumption and in the number of occasions when alcohol was consumed in the past month among young people, and reductions in the frequency of alcohol consumption and in the number of alcoholic beverages consumed per occasion among individuals who reported alcohol consumption.

7.6 Health care-based interventions

7.6.1 General considerations

Services are needed for individuals with alcohol use disorders and other health conditions caused by alcoholic beverage consumption. Screening in primary care and other health-care settings is a valuable approach for identifying such individuals. A positive screening result may be followed by brief interventions, longer-term psychosocial interventions, which may be combined with pharmacotherapy, or referral for specialist treatment.

7.6.2 Screening and brief interventions

Many screening tools have been developed to identify adolescents and adults with hazardous or harmful patterns of use of alcohol. Individuals who screen positive may receive brief interventions, which are typically one session or a few brief sessions and commonly include education about the potential harms of alcohol consumption, advice to reduce alcohol consumption, personalized normative feedback, and motivational interviewing.

Numerous randomized controlled trials have been conducted in both health-care and non-health-care settings, primarily in high-income countries, to assess the effectiveness of brief interventions in reducing alcohol consumption. Overall, interventions delivered by a practitioner result in decreased total alcohol consumption and heavy episodic drinking frequency compared with control conditions at 6-month follow-up, but effects appear to be slightly smaller after 12 months and when delivered digitally. Reductions in alcohol consumption are similar in primary care and inpatient settings but smaller in emergency department settings. The effects of screening and brief interventions are also smaller among adolescents and young adults compared with adults.

7.6.3 Treatment interventions

Psychosocial interventions are implemented in treatment programmes to target behavioural, psychological, motivational, and other psychosocial aspects of alcohol use disorder. Pharmacotherapy interventions are typically delivered in combination with psychosocial interventions to reduce alcohol cravings or the reward of alcohol consumption. Many systematic reviews have reported evidence from randomized controlled trials on the effectiveness of psychosocial or pharmacotherapy interventions in treating alcohol use disorders. Most studies were conducted in high-income countries; participants were predominantly male and aged 40 years on average. There is evidence supporting the efficacy of various types of psychosocial interventions in reducing alcohol consumption or increasing abstinence compared with no or little treatment, or treatment as usual. These interventions include manualized peer-led Alcoholics Anonymous or other 12-step facilitation interventions, contingency management,

cue exposure therapy, and behavioural couples therapy. Most studies reported small to moderate effects, and there is little evidence on efficacy beyond 12 months of follow-up.

The efficacy of pharmacotherapy interventions, which are typically delivered in combination with minimal psychosocial interventions, varies by type of outcome and duration of follow-up. In the short term, acamprosate, disulfiram, naltrexone, baclofen, and gabapentin are superior to placebo in increasing the rate of abstinence and had beneficial effects on other outcomes. Acamprosate, baclofen, naltrexone, nalmefene, and topiramate decrease the percentage of drinking days or heavy drinking days; they also increase the number of abstinent days. Naltrexone and topiramate reduce the number of drinks per drinking day in the short term. In the longer term (i.e. over 24 weeks), nalmefene is the only medication that has been found to reduce total alcohol consumption and the number of heavy drinking days.

8. EVALUATIONS

8.1 Taxation and pricing policies

8.1.1 Alcohol taxes

There is *sufficient evidence* that increases in alcohol taxes that increase prices lead to a reduction in alcoholic beverage consumption.

Rationale. Among all studies reviewed, alcohol taxes were inversely associated with alcohol consumption. Studies that did not control for income or its proxies were excluded; thus, potential biases due to the effects of income or its proxies on alcohol consumption could be ruled out with reasonable confidence. The evaluation is supported by strong evidence from all regions of the world that an increase in alcohol taxes is passed through to alcohol prices and that the resulting increase in alcohol prices reduces alcohol consumption.

8.1.2 Minimum pricing

There is *sufficient evidence* that alcohol minimum pricing leads to a reduction in alcoholic beverage consumption.

Rationale. Several well-controlled studies consistently showed an inverse association between alcohol minimum pricing and alcohol consumption. The consistency was observed among studies with different designs, analytical approaches, and study populations.

8.1.3 Bans on discounting

There is *inadequate evidence* that bans on discounts of alcoholic beverages lead to a reduction in alcoholic beverage consumption.

Rationale. Three studies of the same partial ban on alcohol discounting implemented in one country but with different study designs, data sources, or analytical strategies reported highly inconsistent results.

The Working Group noted that it is plausible that bans on alcohol discounting lead to a reduction in alcoholic beverage consumption, because these bans will influence the price of alcohol.

8.2 Policies to limit physical availability

8.2.1 Restrictions on alcohol outlet density

There is *sufficient evidence* that restrictions on density of alcohol outlets lead to a reduction in alcoholic beverage consumption.

Rationale. Several high-quality studies consistently showed an association in the expected direction between the policy change (i.e. increasing or decreasing alcohol outlet density) and alcohol consumption or proxy outcomes (i.e. violence and alcohol-related hospital admissions). The most informative studies were based on proxy outcomes. Potential confounding by income or other factors as another explanation

for the observed association could be ruled out with reasonable confidence.

The Working Group noted that the evidence is based on studies that precede the growth of online alcohol retail sales and home delivery.

8.2.2 Restrictions on days or hours of alcohol sales

There is *sufficient evidence* that restrictions on days or hours of alcohol sales lead to a reduction in alcoholic beverage consumption.

Rationale. All studies reviewed, including several high-quality studies, consistently showed an association in the expected direction between the policy change (i.e. increasing or decreasing days or hours of alcohol sales) and alcohol consumption. All studies used a control group, controlled for relevant confounding factors (e.g. alcohol price), or both, allowing bias and confounding as other explanations for the observed association to be ruled out with reasonable confidence.

8.2.3 Minimum legal purchase or drinking age

There is *sufficient evidence* that increasing the minimum legal purchase or drinking age leads to a reduction in alcoholic beverage consumption.

Rationale. In all better-quality studies, the interventions showed an association in the expected direction between the policy change (i.e. decreasing or increasing minimum legal age) and alcohol consumption. The majority of the studies assessed policy interventions that increased the minimum legal age, thus assessing a restrictive intervention. The effects of the interventions were observed in the specific age group affected by the policy. Potential confounding by other alcohol availability policies was controlled for in the strongest studies, thus allowing confounding as another explanation for the observed association to be ruled out with reasonable confidence.

8.2.4 Total bans on alcohol sales

There is *sufficient evidence* that banning alcohol sales leads to a reduction in alcoholic beverage consumption.

Rationale. In all studies reviewed, an inverse association was consistently observed between total bans on alcohol sales and alcohol consumption or proxy outcomes (i.e. liver cirrhosis mortality, violence, and injuries), with large effect estimates. The studies were conducted across a range of settings in low- and middle-income countries and high-income countries.

8.3 Alcohol marketing bans

8.3.1 Strong alcohol marketing bans

There is *sufficient evidence* that strong alcohol marketing bans lead to a reduction in alcoholic beverage consumption.

Rationale. All studies consistently showed an association in the expected direction between strong alcohol marketing bans and alcohol consumption. The body of evidence included two very large studies with data from 17 and 20 countries in the Organisation for Economic Co-operation and Development, which are methodologically strong and controlled for potential confounding by alcohol price and other factors.

The Working Group noted that the evidence is based on studies that predate highly personalized digital marketing.

8.4 Coordinated and other multiple alcohol policy interventions

8.4.1 Coordinated multiple alcohol policy interventions

There is *sufficient evidence* that government alcohol monopolies or other coordinated multiple alcohol policy interventions lead to a reduction in alcoholic beverage consumption.

Rationale. The implementation of government alcohol monopolies or other coordinated multiple alcohol policy interventions was consistently associated with reduced alcohol consumption, whereas the weakening or dissolution of a government alcohol monopoly was consistently associated with increased alcohol consumption. The effects were observed in studies conducted in multiple countries, in both high-income countries and low- and middle-income countries, and some studies spanned over very long periods. The effects were observed in studies with both individual-level and population-level data on consumption among adults and adolescents. Potential confounding by income and other factors as another explanation for the observed association was ruled out with reasonable confidence.

9. FINAL CONSIDERATIONS

Consistent with the framework described in the Preamble to the *IARC Handbooks* for Primary Prevention, the evaluation of alcohol policy interventions in relation to cancer incidence proceeded in a two-step process by way of the intermediate outcome "reduction or cessation of alcohol consumption". The Working Groups for *IARC Handbooks* Volumes 20A and 20B concluded that (i) there is *sufficient evidence* that reduction or cessation of alcohol consumption leads to a reduction in the incidence of oral cancer and oesophageal cancer (Volume 20A), and (ii) there is *sufficient evidence* that selected population-level alcohol policy interventions lead to a reduction in alcohol consumption (Volume 20B). As a result, it is reasonable to infer that alcohol policy interventions could lead to a reduction in cancer incidence. Thus, these evaluations contribute to the broader body of evidence that can support governments in their efforts to reduce the alcohol-related cancer burden.

9.1 Considerations about the evaluations

In reaching the evaluations for this volume, the Working Group acknowledged several issues related to the body of evidence.

The available studies were conducted in a limited number of countries, mostly high-income countries; this restricts the direct generalizability of their findings. Nevertheless, the evidence remains valuable for informing the development of alcohol policies in low- and middle-income countries, because there is little evidence or theory to suggest that the same conclusions would not hold true in these settings, while recognizing the need for context-specific adaptation and further research.

An important aim of alcohol policy interventions is to reduce alcohol consumption among young people. Although numerous interventions – particularly school-based and educational approaches – have been developed specifically for this age group, the evidence shows that these interventions have little effect on alcohol consumption behaviours (Cho and Cho, 2021; WHO, 2025); the notable exception is regulations on minimum legal purchase and drinking age (see Section 3.2.3). Nevertheless, population-wide alcohol policy interventions that target the general population, such as taxation, marketing restrictions, and availability restrictions, also reduce alcohol consumption among young people (Babor et al., 2023).

In reviewing the literature, the Working Group noted the large number of and substantial heterogeneity in the types of policy interventions assessed in studies; some policy interventions or combinations of policy interventions were not evaluated, primarily because the interventions were not clearly defined, for

example in studies that assessed the effects of a mix of strong and partial bans on alcohol marketing, different types of partial bans on alcohol marketing, different types of partial bans on alcohol sales, and multiple availability restrictions. Nevertheless, the Working Group considered that partial restrictions may be better than no restrictions at all, because they can raise awareness of the harm associated with alcohol and support momentum towards more stringent alcohol policies and the denormalization of alcohol consumption.

The Working Group evaluated the effects of coordinated multiple alcohol policy interventions (see Section 5) but did not quantify the magnitude of the effects of individual or multiple interventions. However, the Working Group considered that it is reasonable to expect that a combination of policies would be more effective than a single policy implemented in isolation. This expectation is supported by evidence from other fields where implementation of multiple policies has demonstrated substantial effects, including, for example, the MPOWER tobacco control package in reducing smoking prevalence and smoking-attributable deaths, multicomponent interventions in obesity prevention, and integrated climate policies to achieve greater emissions reductions than single interventions (Elvsaas et al., 2017; van den Bergh et al., 2021; Lyle and Hendrie, 2024).

Finally, the Working Group noted that many evaluations rely on data collected before the rapid expansion of digital technologies such as digital marketing, online retail sales, and home delivery services. As a result, this limits their relevance to the current alcohol marketing and alcohol availability context. This is further discussed below.

9.2 How digital technologies challenge current and shape future alcohol policies

With the advent of the digital era, the previously distinct parts of the marketing process and sales to consumers have evolved into a seamless logistic flow from marketing exposure to purchasing decisions. As a result, alcohol policies must address both alcohol marketing and alcohol availability simultaneously.

9.2.1 Digital alcohol marketing

The evidence that strong alcohol marketing bans lead to a reduction in alcoholic beverage consumption is based on studies conducted before the advent of algorithmically targeted marketing in digital and social media. Because of the significant evolution of alcohol marketing that has occurred, at least three key issues must be considered when studying the effects of bans on digital alcohol marketing on alcohol consumption.

First, there has been a proliferation of new forms of media that are unprecedentedly personalized and interactive, primarily because of algorithmically driven digital and social media platforms. Therefore, to better conceptualize digital alcohol marketing, it is necessary to differentiate between and appropriately measure exposure to, receptivity to, and engagement with alcohol marketing, to assess its effects on alcohol consumption.

Second, one of the challenges with assessing the effects of alcohol marketing bans in the digital era is the speed at which digital media and formats evolve. Therefore, unless a ban changes at the same speed, it is by default a partial and perhaps outdated ban. Such bans are difficult to study because they seek to address what is essentially a moving target, are implemented in the context of marketing innovation, and, in some

cases, stimulate further innovation as marketers develop strategies to circumvent the bans.

Third, research, regulatory, and accountability frameworks for alcohol marketing rely on the assumption that it is public. Paradoxically, in the digital era, just as consumers become more visible to marketers, advertising itself as a public form disappears because of algorithmically targeted marketing (Carah and Brodmerkel, 2021). Digital marketing has two important characteristics: (i) the advertisements themselves are no longer visible to the greater public, and so the advertisements and who is seeing them are not observable; and (ii) the power of digital marketing lies in the automated models that build audiences and optimize the placement of advertisements, but digital corporations that own and operate the models hold them, and the audience data that support them, out of public view (Carah and Brodmerkel, 2021).

Enabling access to the extensive and more accurate data about exposure, engagement, and indicators of consumption held by digital platforms would provide the opportunity to advance our understanding of the effects of the cumulative personalized sequences of advertisements that users see over time on alcohol consumption. However, no digital platform offers an accessible library of alcohol advertisements stored over time, and useful information about volume, spend, targeting, or reach is also not publicly available in most parts of the world. The exception is in the European Union, where the recent Digital Services Act (European Parliament, 2022; European Commission, 2024) requires very large digital platforms to create repositories of advertisements published on their platforms together with information about the advertiser, content, and targeting and delivery criteria. Because digital platforms exercise unprecedented private control over public life, strong and enforceable national and supranational frameworks for observability and accountability and for accessing the abundant digital data could substantially improve research on the potential adverse effects of alcohol marketing, or the beneficial effects of alcohol marketing bans, on alcohol consumption.

9.2.2 Online sales and home delivery of alcohol

Online retail sales and home delivery of alcoholic beverages have been rapidly expanding and are changing the landscape of alcohol availability. These changes may weaken the effects of alcohol availability restrictions, for example by allowing alcohol sales to those who may be intoxicated or who are younger than the minimum legal purchase or drinking age (Mojica-Perez et al., 2019; Coomber et al., 2024). Similarly, the effectiveness of alcohol availability policies that restrict alcohol outlet density and days and hours of alcohol sales could also decrease if online retail sales and home delivery of alcoholic beverages are not regulated. To address these changes, alcohol availability policies need to be adapted to minimize the risk that their effects will be weakened or circumvented. In several countries, policies have been adapted or are being adapted to address the changes in the alcohol availability landscape, including, for example, policies to allow the delivery of alcoholic beverages only a set number of hours after ordering, or to allow delivery only during off-premises days or hours of alcohol sales (NordAN, 2025).

9.3 How to make policies most effective

The alcohol industry, civil society, and governments each play their part in a rapidly changing environment where industry is very innovative and regulators are tasked with reacting to new developments in the market. Evaluations should support policy-makers, public health professionals, and community advocates in developing, enacting, implementing, and enforcing

evidence-based alcohol policies, actions, and interventions.

9.3.1 Safeguarding policy from industry influence

The separation between public health and commercial interests requires effective regulation. Policy-makers often face conflicts of interest, because alcohol is a widely consumed product. Mechanisms to prevent industry interference – including transparency requirements, conflict-of-interest policies, and restrictions on lobbying and campaign contributions – are essential to ensure that decisions are guided by public health priorities rather than commercial interests.

9.3.2 Strengthening national and international regulatory capacity

The globalization of the economy and the concentration of the alcohol industry have amplified its influence, making strong collaboration within the health sector essential at the international level and multisectoral collaboration critical at national and subnational levels. Strengthening regulatory capacity requires institutional settings that bring together health, finance, justice, transportation, education, and other sectors, while ensuring that subnational authorities have the technical and financial resources to fulfil their responsibilities effectively. At the same time, governments face persistent challenges in regulating cross-border issues, such as digital marketing, taxation and pricing, and other trade-related matters. The World Health Organization (WHO) has specifically referenced the need for "collaboration" among national and subnational jurisdictions "and an international mechanism for effective cooperation" in the regulation of cross-border policies as "an integral part of public health strategies" to address alcohol-related harm (WHO, 2022). Although a WHO Framework Convention on Alcohol Control has been proposed (Casswell and Thamarangsi, 2009; Au Yeung and Lam, 2019), similar to the WHO Framework Convention on Tobacco Control (WHO FCTC, 2024), the absence of political will and of the necessary resources limits its feasibility in the near term.

9.3.3 Implementing effective taxes

The body of evidence consistently shows that increases in alcohol taxes that increase prices lead to a reduction in alcoholic beverage consumption. Therefore, taxation should be a central part of a comprehensive, long-term strategy for tackling alcohol-related harm. According to the *WHO Technical Manual on Alcohol Tax Policy and Administration* (WHO, 2023), recommended approaches include relying on excise taxes instead of customs taxes, maintaining commitments relating to harmonization of taxes, and ensuring that tax differentials between different categories of alcoholic beverages are proportionate to their ethanol content and the relative health risks posed to the population. When appropriately designed and administered, alcohol taxes not only reduce alcohol consumption and related harms but also provide a predictable source of government revenue, which may be invested in health and community programmes.

9.3.4 Ensuring enforcement and monitoring

Enforcement is important for strengthening the effectiveness of and compliance with alcohol regulations. Towards this end, it is necessary to devote sufficient resources for effectively enforcing such regulations and to sanction breaches with penalties (WHO, 2019). Governments may consider involving civil society organizations in the monitoring of applicable regulatory frameworks. For example, in France, civil society organizations that bring successful complaints under the Evin law are awarded the equivalent

of the fines levied on the alcohol industry for those violations (Friant-Perrot and Garde, 2022). Such models improve accountability, broaden enforcement capacity, and ensure that laws are upheld effectively.

9.3.5 Raising public awareness of the carcinogenicity of alcoholic beverages

Increasing awareness among populations can help generate support for stronger governmental and policy measures to regulate alcohol consumption. Information campaigns, labelling requirements for containers and advertisements, and integration of alcohol-related cancer risks into broader health promotion strategies have the potential to shift social norms and strengthen acceptance of policy interventions. Public awareness not only has the potential to empower individuals but may also create the conditions for stronger political will and policy action.

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IARC HANDBOOKS

In 2020, an estimated 741 300 new cancer cases, or 4.1% of all new cancer cases globally, were attributable to alcohol consumption. Alcoholic beverage consumption is a risk factor for numerous other health conditions in addition to cancer.

A Working Group of 20 independent international experts, convened by the International Agency for Research on Cancer (IARC) from June to October 2024, reviewed the body of evidence on the effects of selected alcohol policies on reducing alcoholic beverage consumption.

Population-level alcohol policy interventions related to (i) taxation and pricing, (ii) physical availability, (iii) alcohol marketing, and (iv) coordinated and other multiple alcohol policy interventions were evaluated.

In addition, health care-based interventions (i.e. screening and brief interventions, and psychosocial and pharmacotherapy interventions) were reviewed but not evaluated.

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