ISDUP and EUPC on regulatory alcohol policy measures

A foundation document

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Introduction

This document reviews the scientific evidence for efficacy or effectiveness of regulatory alcohol policy measures as described in UNODC's International Standards of Drug Use Prevention (ISDUP) and in EMCDDA's/EUDA's European Prevention Curriculum (EUPC). As these reports are important guidelines for evidence-based prevention work to reduce alcohol and drug related harms, it is essential that the referenced evidence base is accurate and reliable. Preferably, the referenced literature should also be updated and cover the extant literature adequately.

This document serves as a reference document (or foundation document) for a shorter and less detailed version in Norwegian. This document is produced as part of project, involving collaboration between the Norwegian Directorate of Health and the Norwegian Public Health Institute (NIPH). In addition to examining the evidence base for effective alcohol and drug prevention, which is the focus of the present document, the collaborative project will include also an assessment of suitability for implementation of effective interventions in a Norwegian context.

Alcohol policy measures are a first priority to consider here because 'alcohol policies' (along with tobacco policies') are the only interventions rated as having 'excellent' evidence of efficacy in the ISDUP and EUPC.

This document includes the following parts:

- Specification of 'alcohol policies' and alcohol policy intervention measures
- Identification of referenced literature on 'alcohol policies' in the ISDUP and EUPC
- Description of method employed for the assessment of the referenced literature
- Assessment of the referenced literature for each alcohol policy measure
- References, appendices and endnotes

Specification of 'alcohol policies' and alcohol policy measures

The International Standard of Drug Use Prevention (hereafter ISDUP) came in two editions; the first (ISDUP ,1st ed.) in 2015 [1] and the second (ISDUP, 2nd ed) in 2018 [2]. ISDUP (1st ed.) [1] describes alcohol policies along with tobacco policies. For alcohol policies the following is noted:

> *Tobacco and alcohol use, dependence and associated disorders, are much more prevalent than drug use disorders and the global burden of disease is much higher. Their use in early adolescence, when the brain is still developing, considerably increases the likelihood of developing substance use disorders and addiction later in life. Moreover, young people who use drugs, often also use alcohol in excessive quantities and/or in combination with other substances. That is why efforts to prevent and reduce tobacco and alcohol use by young people, including harmful patterns of use, are relevant to an overall drug prevention strategy, besides being crucial to any public health policy.*

Available evidence

Six good reviews and six acceptable reviews reported findings with regard to alcohol policies[1] According to these studies, raising the price of alcohol and tobacco reduces their consumption in the general population. With regard to alcohol, the impact appears to affect both moderate and heavy drinkers and an increase of 10 per cent has been found to be associated with a 7.7 per cent decrease in alcohol consumption. Raising prices has also been found to reduce heavy drinking among college students. Finally, higher alcohol prices are associated with decreased violence.

In ISDUP (1st ed.) [1] it is further noted the importance of supportive policy and regulatory framework (in Chapter III: Characteristics of an effective prevention system). Here, it is stated that "*an effective national system would be embedded in a comprehensive and health-centred system of drug control*" [ ] and that "*the delivery of programmes can be greatly enhanced if it is mandated and supported at the national level by appropriate regulation*". However, there is no mentioning of any regulatory alcohol policies in this regard.

Table 1 (pp 8-10) in ISDUP (1st ed.) provides a summary of interventions and policies that have been found to yield positive results in preventing substance abuse. The table is organized along two dimensions; age related developmental periods (from prenatal/infancy to adulthood) and the setting in which the intervention type is implemented (e.g. family, school, community, health sector). Within the table, the various groups/types of interventions are placed along these two dimensions. A third dimension, target population, categorizes interventions into universal, selective or indicative, and is noted for each intervention type. The intervention types are also rated with regard to efficacy (i.e. ranging from 'limited' to 'excellent'). Alcohol and tobacco policies constitute the only intervention group assessed as having 'excellent efficacy'.

ISDUP (2nd ed) [2] describes alcohol policies separately and as follows:

A series of policies and interventions to reduce the harmful use of alcohol defined as drinking that causes detrimental health and social consequences for the drinker, the people around the drinker and society at large, and to reduce the patterns of drinking associated with increased risk of adverse health outcomes.

Available evidence

No new reviews were identified in the new overview of systematic reviews[2].

There is, however, a note on WHO's global strategy to reduce the harmful use of alcohol which summarizes evidence-based guidance with to effective interventions and policies[3]

[1] Anderson, 2009; Bühler, 2008; Campbell, 2009; Elder, 2010; Hahn, 2010; Hahn, 2012; Middleton, 2010; Popova, 2009; Rammohan, 2011; Smith, 2009; Spoth, 2008, Wagenaar & Toomey, 2002.

[2] It is not specified for which years searches for new reviews were conducted

[3] The note on WHO's global strategy reads as follows: The WHO global strategy to reduce the harmful use of alcohol of 2010 36 summarizes clear evidence based guidance with regard to interventions and policies that reduce the harmful use of alcohol, grouping them in 10 target areas. Besides leadership, awareness and commitment in protecting the population, responses are called for in the health sector, namely screening and brief interventions in primary health-care and other settings, including for pregnant women and women of child-bearing age. Another area of action concerns the mobilization and empowering of communities in preventing the sale of alcohol to underage drinkers and other at-risk groups and in developing alcohol-free environments and events. Drink-driving policies and countermeasures should be complemented with carefully planned, high-intensity, well-executed public awareness and information campaigns. Another crucial area of policy is the regulation of the availability of alcohol through measures such as establishing a licensing system for retail sales,

Thus, In ISDUP 1st ed. [1], the text on 'alcohol policies' refers solely to price regulation. However, the referenced literature on efficacy of alcohol policies covers various other regulatory policies, including restrictions on days and hours of sale, government monopoly on sales, minimum legal age for purchase/drinking, restrictions on outlet density, restrictions on advertising/marketing and enforcement strategies, including dram shop liability.

In ISDUP 2nd ed. [2], the note on WHO's global strategy on alcohol pertains to a broad range of interventions or policies including screening and brief intervention, drink-driving countermeasures, licensing systems for retail sales, government monopolies, regulating density of alcohol outlets, and establishing an appropriate minimum legal age for purchase or consumption of alcohol.

Thus, while there is no clear definition of what kind of interventions or policy measures that subsume to the term 'alcohol policies' in ISDUP, it seems that various regulatory alcohol policy measures clearly are included in 'alcohol policies'.

In the further review of the scientific evidence that is given for the assessment of efficacy of 'alcohol policies' in ISDUP, the following regulatory policies/interventions are considered:

Pricing policies (i.e. alcohol taxes), regulating physical availability (i.e. regulating outlet density, regulating days and hours of sale, government monopoly and minimum legal age for purchase/drinking), restrictions on advertising and marketing, and dram shop liability. These are all policies/interventions that are included in the referenced literature (i.e. the 12 reviews noted in footnote 1).

In the European Prevention Curriculum (EUPC) [3], ISDUP provides one of the two documents that provide the main foundation for the curriculum. In addition, EUPC provides information about registries of evidence-based programmes in accordance with the standards in ISDUP. The second source for EUPC, is a European framework for conducting substance use prevention (EDPQS). While ISDUP focusses on the content, structure and most appropriate instructional strategy of the interventions, the EDPQS focusses on how to plan for, select and implement prevention interventions to assure quality [3].

The EUPC [3] presents in Tables 3 – 6 (pp. 52-56) an overview of types of prevention interventions by age-related developmental periods and level of risk targeted (i.e. target population) quite similar to that in ISDUP, Table 1 (op.cit.), and alcohol and tobacco policies are, like in ISDUP, the only

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or public health-oriented government monopolies, regulating the number and location of on-premise and off-premise alcohol outlets, regulating days and hours of retail sales, regulating modes of retail sales of alcohol, regulating retail sales in certain places or during special events, establishing an appropriate minimum age for purchase or consumption of alcoholic beverages, and adopting policies to prevent sales to intoxicated persons and to reduce the impact of marketing. It is particularly important to protect young people
from the content of alcohol marketing, especially in low- and middle-income countries where there is currently a low prevalence of alcohol consumption among adolescents and they are now being targeted as new markets.
In addition, increasing the price of alcoholic beverages through an effective and efficient system of taxation matched by adequate tax collection and enforcement is one of the most effective interventions to reduce the harmful use of alcohol. Complementary policies include reducing the harm from alcohol intoxication and drinking without necessarily affecting the underlying alcohol consumption, particularly with regard to driving, and enacting management policies relating to responsible serving of beverages on premises and training staff in relevant sectors how better to prevent, identify and manage intoxicated and aggressive drinkers. Further areas of action are reducing the public health impact of illicit alcohol and informally produced alcohol, and, monitoring and surveillance.
These policies are also recommended by WHO to prevent unintentional injury (road injury) among adolescents, youth violence and sexual and other forms of gender-based violence, and child maltreatment.

intervention type assessed as having excellent evidence of efficacy. In the EUPC [3] (Chapter 3, p. 56) the scientific support for efficacy of alcohol policies is described along with tobacco policies as follows*: "Alcohol and tobacco policies have excellent scientific support for effectiveness (Table 6). As tobacco and alcohol use is more prevalent than illicit drug use and the associated population health burden is greater, delaying the use of these substances among young people can have a significant societal impact. Evidence-based tobacco and alcohol policies are those that reduce access to underage children and adolescents and reduce the availability of tobacco and alcohol products. Successful policies are those that increase the minimum age for the sale of these products and also increase prices through taxation. Banning the advertising of tobacco and restricting the advertising of alcohol products targeting young people have also been shown to reduce use. Active and consistent enforcement of these policies and the involvement of retailers through educational programmes are part of the effective approaches to tobacco and alcohol use."*

In the EUPC [3], efficacy of alcohol policies is further described, with reference to ISDUP (Chapter 7 on Environment/population policies on tobacco and alcohol, pp. 121-122). Here, it is noted that "*increasing the price of [tobacco and] alcohol through taxation is an important evidence-based intervention for substance use*." "*Specifically, the review of research on environment-based substance use interventions, conducted during the development of the International Standards, found that raising the price of alcohol had several positive outcomes. It had an impact on both moderate and heavy drinkers, including heavy drinking among college-age young people. Furthermore, it found that increasing the price of alcohol by only 10 % was associated with a 7.7 % decrease in alcohol consumption in the general population and that increased prices for alcohol were also associated with decreases in violence*."

"Other strategies that have been shown to reduce use include increasing the minimum purchase age for [tobacco and] alcohol products and enforcing this rule. Restricting and banning advertising and other forms of marketing of [tobacco and] alcohol to young people have also been shown to be effective. "

In the EUPC, it is further noted that "As with many prevention interventions, *combining environmental interventions can have a more powerful impact than single interventions. Accordingly, comprehensive prevention interventions to keep underage young people from purchasing tobacco and alcohol involve: - active and ongoing law enforcement; - the education of retailers through a variety of strategies (personal contact, media and information materials); and - media- and school-based prevention interventions to reinforce these messages*."

Thus, the regulatory alcohol policy measures that are assessed in the EUPC include: - pricing policy/taxation, minimum purchase age, and restrictions/ban on advertising/marketing. Moreover, it is also noted that active and ongoing law enforcement in combination with environmental interventions (presumably minimum legal age for purchase and strictions/ban on advertising/marketing) can have a more powerful impact.

To sum up: the ISDUP seems to have included a broader set of regulatory alcohol policy measures when referring to alcohol policies than the EUPC. While both include pricing policy/taxation, restrictions on advertising/marketing and regulating physical availability in terms of minimum legal age for purchase, the ISDUP – at least implicitly, in terms of the referenced literature – also includes

regulating alcohol outlet density, regulating days and hours of sale, government monopoly and dram shop liability.

Thus, in the subsequent review of the scientific evidence that is given for the assessment of efficacy of 'alcohol policies' in ISDUP and EUPC, the following regulatory policies/interventions are considered:

- Pricing policies (i.e. alcohol taxes),
- Regulating physical availability (i.e. regulating outlet density, regulating days and hours of sale, government monopoly and minimum legal age for purchase/drinking),
- Restrictions on advertising and marketing, and
- Dram shop liability.

It is of interest not only to consider the introduction of such interventions/policies but also the enforcement of these regulations.

Identification of referenced literature on 'alcohol policies' in the ISDUP and EUPC

In ISDUP (1st ed.) [1], there is reference to 12 reviews regarding efficacy of alcohol policies (see foot note 1) and these will all be assessed. In an appendix to the ISDUP (1st ed) (Appendix II, Annex V) an overview of the findings in these 12 reviews [4-15] is presented. In ISDUP (2nd ed.) [2] no further references were given.

In the EUPC [3], there is no direct reference to the scientific evidence regarding efficacy of alcohol policy measures, but the assessment is clearly built on ISDUP. In addition to the EUPC document, two additional sources of scientific evidence are provided by the EUDA (formerly EMCDDA): The Best Practice Portal – Evidence Database [16] and the Xchange Prevention Registry [17]. By adding information on the evidence base from these two additional sources, it could be assumed that a more updated and comprehensive evidence base than that in ISDUP from 2015, would be obtained.

The Best Practice Portal is organized with a list of evidence summaries and for each intervention/program there is information about: area (e.g. prevention, harm reduction, treatment); substance (e.g. alcohol, cannabis, opioids, any substance), target group or setting (e.g. families, people who inject drugs, school, partygoers/nightlife); and evidence rating (e.g. beneficial, likely beneficial, possibly beneficial, evidence of ineffectiveness, unknown effectiveness)[4]. For each intervention/program in the list, there is a short summary of the evidence and references to the literature. The list contains around 300 interventions/programs. Several search strategies were employed to identify which regulatory alcohol policy interventions were listed in the Best Practice Portal and furthermore how efficacy of these interventions was assessed, and which literature was referenced to support evidence of efficacy (or effectiveness). First, using only the search term 'alcohol policy' (no other restrictions) yielded 28 hits (i.e. programs/interventions) and none of these were

[4] Copied from Best Practice Portal:

Evidence ratings

The available information on the effects of specific interventions are examined and then ranked them as described below.

- **Beneficial:** Interventions for which precise measures of the effects in favour of the intervention were found in the systematic reviews of randomised controlled trials (RCTs), and that were recommended in guidelines with reliable methods for assessing evidence (such as GRADE*). An intervention ranked as 'beneficial' is suitable for most contexts.
- **Likely to be beneficial:** Interventions that were shown to have limited measures of effect, that are likely to be effective but for which evidence is limited, and/or those that are recommended with some caution in guidelines with reliable methods for assessing evidence (such as GRADE). An intervention ranked as 'likely to be beneficial' is suitable for most contexts, with some discretion.
- **Trade-off between benefits and harms:** Interventions that obtained measures of effects in favour of harm reduction and/or are recommended in guidelines with reliable methods for assessing evidence (such as GRADE), but that showed some limitations or unintended effects that need to be assessed before providing them.
- **Unknown effectiveness:** Interventions for which there are not enough studies or where available studies are of low quality (with few patients or with uncertain methodological rigour), making it difficult to assess if they are effective or not. Interventions for which more research should be undertaken are also grouped in this category.
- **Evidence of ineffectiveness:** Interventions that gave negative results if compared with a standard intervention, for example.

* GRADE is an approach to grading the quality of evidence and strength of recommendations. Some of the available reviews do not include measures of effect, and in these cases we therefore report the rank about evidence in the same narrative fashion as provided by the authors. In the coming year, we will produce a synthesis of the available studies with the adoption of the GRADE profiler.

The categories of effectiveness were created following those adopted by BMJ Clinical Evidence that were originally developed in the Cochrane Collaboration first editorial group for the publication "A guide to effective care in pregnancy and childbirth".

relevant in our context (i.e. none of the interventions pertained to regulatory alcohol policy measures). Similar searches employing more specific terms for alcohol policies (i.e. 'alcohol tax', 'minimum legal age', 'alcohol advertising') yielded similar results, that is numerous hits, none of which were relevant. Second, searches were conducted specifying 'Prevention area', 'Alcohol' and 'Desired outcome: i) reduce alcohol sales' or ii) reduce injuries/harm' or iii) reduce harms, or iv) reduce mortality' with no restrictions on target group/setting or evidence rating. These searches yielded 2 hits, 7 hits, 0 hits and 0 hits, respectively. Only one intervention – among the nine in total – pertained to regulatory alcohol policy measures (i.e. restricting opening hours to reduce alcohol related injuries). Next, searches were conducted specifying 'Prevention area', and 'Alcohol', and 'Beneficial evidence rating' or 'Likely beneficial evidence rating' with no restrictions on target group/setting or desired outcome ', which yielded 8 and 12 hits, respectively. None of these pertained to regulatory alcohol policy measures. (However, three of the programs with likely beneficial evidence pertained to law enforcement in nightlife settings, one of which included enforcement checks to reduce underage serving).

Thus, from the Best Practice Portal only one regulatory alcohol policy measure was found among the approximately 300 interventions listed. This measure – 'Restricting opening hours to reduce alcohol related injuries' was assessed as having 'unknown effectiveness' with reference to Calafat et al., 2009 [18]. One additional intervention identified through these searches is, however, also of interest as it relates to enforcement of minimum legal drinking age; that is 'Electronic Age Verification (EAV) devices to increase the frequency of age verification at recreational premises'. This was rated as having 'evidence of ineffectiveness' with reference to Calafat et al., 2009 [18].

The Xchange Prevention Registry is an online registry of thoroughly evaluated prevention interventions. Since 2020, Xchange broadened the scope to non-manualised interventions and included also local environmental prevention strategies, including regulatory environmental prevention.

Like the Best Practice Portal, Xchange is organized as a list of interventions with a rating of evidence f or efficacy[5]. Again, a number of searches were conducted to identify regulatory alcohol policy measures.

[5] Copied from Exchange Prevention Registry:
About Xchange ratings
Beneficial: Interventions for which convincing, consistent and sustained effects for relevant outcomes are in favour of the intervention as found in two or more studies of excellent quality in Europe.
Likely to be beneficial: Interventions for which convincing and consistent effects for relevant outcomes are in favour of the intervention as found in at least one evaluation study of excellent quality in Europe.
Possibly beneficial: Interventions for which some effects for relevant outcomes are in favour of the intervention as found in at least one evaluation study of acceptable quality in Europe. An intervention ranked as 'possibly beneficial' is suitable for application in the context of more rigorous evaluations.
Additional studies recommended: Interventions for which concerns about evaluation quality or consistency of outcomes in Europe make it difficult to assess if they are effective or not, even if outcomes seem to be in favour of the intervention.
Unlikely to be beneficial: Interventions for which at least one evaluation of excellent quality in Europe shows convincing evidence of no effects on relevant outcomes.
Possibly harmful: Interventions for which some effects for relevant outcomes of the intervention are considered harmful, as found in at least one evaluation study of acceptable quality in Europe. An intervention ranked as 'possibly harmful' is unsuitable for application except within a framework of other priorities and with rigorous and strictly supervised evaluations.

First, the search term 'Alcohol policy' was employed with no other restrictions. This search yielded two relevant interventions. The first, 'Extending opening hours of on-premise alcohol sales' was rated as 'possibly harmful' and two references were given [19, 20]. The other intervention was 'Local environmental alcohol licensing strategies', which was rated 'possibly beneficial' and with five references provided: [21-25].

Next, using the search term ' alcohol tax' yielded no relevant hits. With the search term 'alcohol age limit', the STAD intervention (i.e. STockholm prevents Alcohol and Drug problems) came up – as it includes enforcement of minimum legal age for purchase – but was not considered in our context, as it subsumes to multi-component community interventions. Using the search term 'alcohol advertising' yielded no relevant hits. The search term 'sales hours' yielded no hits, and the search term 'alcohol enforce' yielded the STAD intervention and local environmental alcohol licensing strategies, as described above. Finally, a search employing the term 'sales hours' yielded no hits.

Thus, the Xchange Registry included two alcohol policy interventions of relevance in our context: regulating hours of sales and local environmental licensing strategies.

Description of method employed for the assessment of the referenced literature

Our point of departure and governing principles lay primarily in the set of criteria for evidence of efficacy or effectiveness as presented by Flay and colleagues [26] (see Appendix I) and later updated by Godtfredson et al. [27]. In International Standards for Drug Use Prevention (ISDUP), criteria for assessment of evidence (see Appendix II) do mainly resemble those of Flay et al. [26] and Gottfredson et al. [27]. That is, overall, there is a resemblance regarding the guiding principles pertaining to: - outcome of interest; - study design and causal inference; - duration of effect; - replication and consistency of findings; - assessment of any adverse effects; and - distinction between efficacy and effectiveness studies.

However, there are some notable differences. The following is an account of what these differences are and what we have chosen in our assessment. First, while the outcome of interest in both Flay et al. [26]/Gottfredson et al. [27] and ISDUP is elimination or reduction of substance use or/and substance related harms (i.e. our interpretation of [26, 27] in this specific context), ISDUP includes also mediating outcomes for interventions targeting young children. In our assessment, we include only studies of substance use/and or substance use related harms as outcome. Second, Flay et al. [26] and Gottfredson et al. [27] included practical value (i.e. practical significance in terms of public health impact) as a criterion for efficacy/effectiveness, which is not evident in ISDUP. In our assessment, we will also include an assessment of practical value. Third, the criteria presented in Flay et a. [26]/Gottfredson et al. [27] are generally at a more detailed level than in ISDUP, although one exception in this regard pertains to specification of study design and causal inference. Here, ISDUP is more detailed and notes specifically RCTs, controlled studies and interrupted time series analysis as adequate designs for assessment of intervention effect. In our assessment, we will apply this specification of adequate study design. Fourth, ISDUP provides a rating of the strength of the

evidence of efficacy or effectiveness (e.g. strong evidence, good evidence, promising evidence), which is not found in Flay et al. [26]/Gottfredson et al. [27].

While the latter [26, 27] implicitly covers primary studies only, ISDUP includes assessment of systematic reviews and meta-analysis, which is essential to the rating of evidence (see Flowchart 1 in ISDUPs Annex on description of the methodology utilised for the collection, assessment and utilization of the scientific literature)). In our assessment, we will refer to this ranking and assess its validity based on the overall criteria for assessment of evidence.

Ideally, a thorough assessment of the literature would include detailed descriptions of all criteria for each study, whether primary study or review study, whereupon an overall assessment of each study is made. However, for pragmatic reasons and due to limited resources, we have restricted the assessment of the literature as follows:

For literature reviews, we have considered those that followed the standards of Guide to Community Preventive Services [28] as a good quality review, without making any further quality assessments. This corresponds to assessments in ISDUP where reviews of 'good quality' were systematic reviews that followed the standards of Guide to Community Preventive Services [28] [6]. For systematic reviews (i.e. systematic literature searches and inclusion criteria) not following the Guide to Community Preventive services, we have also assessed the included primary studies with regard to adequate methodology (for inferring causal effects) and relevant outcome measures (i.e. substance use and/or related harms). Literature reviews that were not systematic, were considered as providing supportive evidence.

For primary studies, we have assessed those that at least fulfilled the following main criteria as providing some evidence of efficacy/effectiveness: i) relevant outcome measure(s) (i.e. substance use and/or related harm(s)), ii) adequate methodology for inferring causal effect, and iii) meaningful magnitude of effect. Primary studies that did not fulfil one or several of these criteria were assessed as not providing sufficient evidence of efficacy or effectiveness.

In addition, we have – to some extent – taken into consideration possible conflict of interest (CoI) in our assessments of studies. For studies where CoI is acknowledged, we assessed whether CoI likely led to biased conclusions. CoI may for example pertain to research funding by a commercial actor or

[6] In brief, the *Community Guide* process involves forming a systematic review development team (review team), consisting of subject matter and methodology experts from other parts of the CDC, other federal agencies, and academia, and the Task Force on Community Preventive Services (Task Force); developing a conceptual approach for organizing, grouping, and selecting interventions; selecting interventions to evaluate; searching for and retrieving available research evidence on the effects of those interventions; assessing the quality of and abstracting information from each study that meets inclusion criteria; assessing the quality of and drawing conclusions about the body of evidence on intervention effectiveness; and translating the evidence on effectiveness into recommendations. Evidence is collected and summarized on (1) the effectiveness of reviewed interventions in altering selected health-related outcomes and (2) positive or negative effects of the intervention on other health and non-health outcomes. When an intervention is shown to be effective, information is also included about (3) the applicability of evidence (i.e., the extent to which available effectiveness data might generalize to diverse population segments and settings); (4) barriers to implementation; and (5) the economic impact of the intervention. To help ensure objectivity, the review process is typically led by scientists who are not employed by a program that might be responsible for overseeing the implementation of the intervention being evaluated (citation from Hahn et al., 2010).

researchers' own financial interests in dissemination of a prevention program. Information about possible CoI is mainly obtained from the referenced publications and is therefore incomplete.

Moreover, ISDUP rates the level of efficacy for interventions and policies found to yield positive results in preventing substance use in five categories: 'Limited' (one star), 'Adequate' (two stars), 'Good' (three stars), 'Very good' (four stars) and 'Excellent' (five stars). We will also refer to this rating in our assessment.

The European Prevention Curriculum (EUPC) is based on the ISDUP but also offers substantial additional guidance to policymakers beyond scientific evidence for efficacy or effectiveness of prevention strategies or intervention measures. The EUPC provides an overview of evidence-based programs and interventions in two data bases: i) the Best practice portal – evidence database, and ii) the Xchange Registry. In the Best practice portal each included program or prevention strategy is given a rating of the evidence (of efficacy or effectiveness), in one of the five following categories: 'Beneficial', 'Likely to be beneficial', 'Trade-off between benefits and harms', 'Evidence of ineffectiveness' and 'Unknown effectiveness' (Description of rating categories is presented in Appendix III). In the Xchange prevention registry, which includes only programs/prevention measures evaluated in at least one European country, the programs/interventions are rated in one of the following six categories: 'Beneficial', 'Likely to be beneficial', 'Possibly beneficial', 'Additional studies recommended', 'Unlikely to be beneficial' and 'Possibly harmful'. These ratings will also be referred to in our assessments of the literature.

Assessment of the referenced literature for alcohol policy measures

In the first edition of International Standards on Drug Use Prevention (ISDUP) [1], 'alcohol policies' were rated as having excellent efficacy. Altogether 12 references were given for this claim. The referenced studies are all reviews, six were deemed as 'good reviews' (i.e. they applied inclusion and assessment criteria of the Guide to Community Preventive Services [28]) and six were deemed as 'acceptable reviews' in an Annex to ISDUP 1st ed. In the second edition of ISDUP [2] no new reviews were identified. In the following, these 12 reviews [4-15] are assessed with regard to efficacy or effectiveness of regulatory alcohol policies as they apply to various specific alcohol policy measures. Moreover, referenced literature identified in the Best Practice portal and in the Xchange registry is also assessed in the following.

a) Pricing policies and taxation:

In ISDUP (1st ed.), it is stated that "raising the price of alcohol and tobacco reduces their consumption in the general population. With regard to alcohol, the impact appears to affect both moderate and heavy drinkers and an increase of 10 per cent has been found to be associated with a 7.7 per cent decrease in in alcohol consumption."

There is good supportive evidence in ISDUP's referenced literature for claiming that raising the price (or taxation) will reduce consumption. The systematic review by Elder et al. (2010) [7] is of good quality (i.e. applied inclusion and assessment criteria of the Guide to Community Preventive Services [28]) and 38 of the included primary studies of good quality reported price elasticities for consumption. Among these, 36 reported negative price elasticities (i.e. higher prices were associated with lower consumption and vice versa). Furthermore, there is supportive evidence in the Bühler and

Kröger umbrella review [5]: the authors concluded that higher alcohol prices have effects on alcohol consumption, with reference to an unsystematic review by Chaloupka et al. [29].

The statement that higher prices affect both moderate and heavy drinkers could be supported by the Bühler and Kröger [5] umbrella review, in the sense that Bühler and Kröger gave a very similar statement with reference to an unsystematic review by Chaloupka et al. [29]. In the latter review [29], three primary studies, all from the USA and based on survey data from samples of young people collected in the 1980's, found that higher beer prices/beer taxes were associated with reduced alcohol consumption or drinking frequency among those with initial modest or moderate consumption and those with higher or more frequent consumption. As these three primary studies were not included in the review referenced in the ISDUP, they were not subject to a more detailed assessment here. Moreover, in the systematic and somewhat more recent review by Elder et al. [7], the authors noted that while theory suggests that the price effect depends on income and hence those with low income are expected to be more price sensitive than other consumers, included studies did not assess whether price elasticities differed by age or income. They further noted that "the available data were not adequate to assess potential differences in price elasticities based on drinking pattern (i.e., between excessive and non-excessive drinkers).» Thus, the referenced literature in the ISDUP does not provide strong evidence for the above noted claim that both moderate and heavy drinkers are affected by higher prices.

The ISDUP statement that a 10 per cent increase in alcohol price is associated with 7.7 per cent decrease in consumption is well supported by evidence in the Elder et al. publication [7]. In their review, Elder et al. presented median price elasticities by study characteristics (Table 2), and for primary studies with the greatest suitability (n=16), the median price elasticity was – 0.76. This corresponds to a 7.6 per cent reduction in alcohol consumption given a 10 per cent increase in price. The umbrella review by Bühler and Kröger also provide figures for the magnitude of price elasticities, however, these are based on the unsystematic review by Chaloupka et al. [29] and give the range of elasticities. This range (-0.65 - -0.29) is below the median price elasticity estimate (-0.76) in Elder et al. [7], and the latter provides a more accurate and reliable estimate considering that it stems from a systematic review and quality assessment of included studies.

In the EUPC, statements from ISDUP regarding price policies are repeated: "*Specifically, the review of research on environment-based substance use interventions, conducted during the development of the International Standards, found that raising the price of alcohol had several positive outcomes. It had an impact on both moderate and heavy drinkers, including heavy drinking among college-age young people. Furthermore, it found that increasing the price of alcohol by only 10 % was associated with a 7.7 % decrease in alcohol consumption in the general population and that increased prices for alcohol were also associated with decreases in violence*."

The statement that raising the price had an impact on heavy drinking among college-age young people could be supported by the above-mentioned three studies included in the Chaloupka et al. review [29], which was included in the referenced umbrella review by Bühler and Kröger [5]. But, as noted above, these three primary studies were not subject to a more detailed assessment here. Thus, considering that there are three (quite dated) primary studies in an unsystematic review which is briefly noted in Bühler and Kröger [5], this referenced literature in the ISDUP does not provide strong evidence for the claim that heavy drinking among college-age young people are affected by higher prices.

The statement that increased prices were associated with decreases in violence is supported by evidence in the review by Elder et al. (2010) [7]. Three primary studies, all from the USA and published between 1993 and 2000, found that higher alcohol (i.e. beer) taxes were associated with decreased violence of some form. Various violence outcomes were examined in these studies: homicide rates, assault rates, rape rates, robbery rates and violence toward children. Statistically significant associations in the expected direction (i.e. less violence with higher taxes) were found for rape rates, robbery rates and violence towards children. However, associations between alcohol taxes and homicide rates and assault rates, which are frequently used indicators of violence, were not statistically significant. Thus, while there is some support for the claim that increased prices are associated with decreases in violence in the reference to Elder et al. [7], the referenced review does not provide strong evidence. It may in this regard be noted also that the Elder at al review [7] included altogether 22 studies evaluating effects of changes in alcohol price/tax on alcohol-related harms. Half of these, i.e. 11 studies, examined the impact of change in alcohol price or alcohol tax on motor vehicle crashes. Most estimates of the tax – crash rate association (7/12) were negative and statistically significant; i.e. higher taxes/prices were associated with lower crash rates. Three of these pertained to crash rates among young people. None of these studies found a statistically significant positive association. Elder et al. [7] summarised their findings as follows: "The reviewed studies provide consistent evidence that higher alcohol prices and alcohol taxes are associated with reductions in both excessive alcohol consumption and related, subsequent harms. Results were robust across different countries, time periods, study designs and analytic approaches, and outcomes. According to *Community Guide* rules of evidence, these studies provide strong evidence that raising alcohol taxes is an effective strategy for reducing excessive alcohol consumption and related harms.» Thus, while Elder et al.'s review provides an overall picture that higher prices or taxes reduce alcohol related harms, violence does not stand out as the type of alcohol-related harm with strong evidence in this regard.

Thus, to sum up: the statements regarding efficacy of raising alcohol prices /taxes are to varying extent supported by evidence in the referenced literature. Statements regarding the direction and magnitude of overall effect on alcohol consumption are well supported by the referenced review by Elder et al., whereas the other statements are less well supported. Moreover, it should be noted that the referenced literature in the ISDUP is dated, the most recent review is from 2010 and the primary studies showing impact of price on young people's consumption and impact on moderate and heavy drinkers, date back to the 1980s. Moreover, no additional literature on price policies were found in the EUPC and the Best Practice Portal and Exchange registry.

b) Raising minimum legal age for purchase of alcohol

In the ISDUP, there is no mentioning of raising minimum legal age for purchase (or drinking) in the text. In much of the literature, this policy measure is often referred to as 'minimum legal drinking age', shortened MLDA, which will also be used in the following. Among the 12 referenced review studies to support the efficacy of alcohol policies in general, three reviews pertained in part (i.e. Bühler and Kröger [5]; Spoth et al. [14]) or exclusively (Wagenaar and Toomey [15]) to this policy measure.

The latter is a systematic review which was assessed in the ISDUP Annex as having acceptable quality. Wagenaar and Toomey [15] examined effects of changing (either lowering or raising) the minimum legal drinking age (MLDA) (or minimum legal purchase age). Studies that compared jurisdictions with

high and low MLDA were also included. All included studies stemmed from the USA or Canada and most of these from the USA, examining effects of lowering or raising the age limit in the range between 18 and 21 years.

The review included 48 studies that assessed the effects of changes in MLDA on alcohol consumption (sales figures or alcohol consumption), providing a total of 78 effect estimates. Among these, 33 were deemed to be of high methodological quality and a third of these (n=11) found a statistically significant negative effect (i.e. raising the MLDA led to reduced consumption), whereas one found a statistically significant positive effect (i.e. lowering the MLDA led to reduced beer sales).

The review included 57 studies examining the effect of MLDA on traffic crash outcomes (e.g. fatal crashes, drink-driving crashes, self-reported driving after drinking), including a total 102 estimates/analyses. Among these, 79 estimates were from studies of higher methodological quality, and out of the 79, 46 (58%) found a statistically significant negative effect of legal age on traffic crashes (i.e. raising the MLDA led to few traffic crashes), whereas none found the opposite effect. Moreover, among the 61 analyses of other health and social problems as outcomes, 36 were considered high quality and among these 9 analyses (25 %) found a significant inverse relationship and none found the opposite. Wagenaar and Toomey [15] concluded that: "Compared with a wide range of other programs and efforts to reduce drinking among teenagers, increasing the legal age for purchase and consumption of alcohol to 21 appears to have been the most successful effort to date."

In their umbrella review, Bühler and Kröger [5] stated that raising minimum legal age for alcohol consumption reduces alcohol consumption and this claim was supported by the above-mentioned systematic review by Wagenaar and Toomey [15]. Bühler and Kröger [5] further stated that raising minimum legal age reduces negative consequences from alcohol consumption in terms of alcohol-related accidents. This statement was also supported by referring to two reviews; the Wagenaar and Toomey's review [15] and the Shults et al. [30]review , published one year earlier. The latter reported on 15 studies which consistently showed a decrease in traffic crashes involving alcohol when raising the MLDA (median decrease 15% - 17 %), and 9 studies that consistently showed increase in alcohol-related traffic crashes (median increase 5 % - 8 %) when lowering the MLDA.

The umbrella review by Spoth and colleagues [14] is also of some relevance here and the review was rated as having acceptable quality in the ISDUP Annex. This review differs from the other reviews referenced regarding alcohol policies. The review focussed on a broad range of interventions addressing alcohol use and for which evidence was established for three developmental periods (i.e. children < 10 years, 10 – 15 years, and 16 - $\geq$ 20 years of age). The review included primary studies, systematic reviews and literature summaries. Initially, more than 400 interventions were identified and screened, and among these, 127 interventions seemed to show at least some evidence concerning the desired outcomes (i.e. prevention of alcohol use and/or alcohol related harms among those 10 years or older, and prevention of early aggressive behaviour among those under 10 years of age). Among the 127 interventions reviewed, 41 met a set of evaluation criteria and were included in the report. The number of studies providing evidence for each of these 41 interventions varied considerably; for many interventions only 1 publication or 2-3 publications from the same study (reporting on different follow-up points) were provided, whereas for the one intervention with most studies providing evidence (i.e. raising minimum drinking age law), 10 studies were cited.

The authors summarized the findings in 6 Tables, two for each of the three developmental periods. For each period, interventions were assessed as: i) having most promising evidence on alcohol outcomes, and ii) having mixed or emerging evidence on alcohol outcome.

Among the 41 interventions, only one is an alcohol policy measure in terms of laws or regulations: raising minimum drinking age law at the state level. (Spoth et al. assessed also raising minimum drinking age law at the school level, which is beyond our focus here). Spoth et al. [14] stated that: "Although there has been much discussion of policy- and environment-level interventions, we were not able to locate any effective policy inventions for children below 16 or 17 years of age; no evidence based policy interventions that have been shown to delay the initiation of alcohol use or to reduce its early use before the age of high school graduation seem to exist. " (p. S326)[14]. The authors assessed raising minimum drinking age law at the state level as an intervention with mixed or emerging evidence on alcohol or alcohol-related outcomes, providing the following arguments: "Concerning the effects of laws raising the minimum drinking age and zero-tolerance laws, the evidence from studies with quasi-experimental designs suggests that minimum legal drinking age laws can reduce rates of underage drinking, single-vehicle nighttime car accidents, and fatalities. The preventive effects from studies examining the minimum drinking age laws were not completely consistent, however. For example, some studies noted that drinking levels among 18- to 19-year old students on college campuses remained high after enactment of underage drinking laws; in other cases, rates of accidents and fatalities remained the same after the change in law. In addition, the issue of whether drinking was not reduced as a result of these laws but there was a change in where teens drank and how they obtained alcohol has been raised. Although our conclusions are consistent with those of other reviews, that the minimum legal drinking age laws seem to have a preventive effect, these interventions were included in the review as having mixed or emerging evidence, considering the criteria discussed above." An overview of the cited literature in Spoth et al., 2008 on effectiveness of raising minimum drinking age and some comments on their assessment of the literature at hand are provided in an endnote[i].

Thus, the review by Wagenaar and Toomey [15] reported compelling evidence for an association between MLDA and alcohol consumption and related harms, that is: raising MLDA was associated with reduced alcohol consumption and reduced harm rates. The umbrella review by Bühler and Kröger [5] concluded that MLDA reduces alcohol consumption based on their assessment of the Wagenaar and Toomey review [15] and furthermore that raising MLDA reduces alcohol-related accidents. The latter was supported by referring to evidence provided in the Wagenaar and Toomey review [15] and the review by Shults et al. [47]. In the umbrella review by Spoth et al. (2008)[14], the authors concluded that, consistent with other reviews, raising MLDA seems to have a preventive effect. However, they also assessed the intervention as having mixed or emerging evidence on alcohol or alcohol-related outcomes. The latter assessment does not seem well-founded and is disregarded in the further review of the literature.

In the EUPC, raising MLDA is assessed as follows: *"Other strategies that have been shown to reduce use include increasing the minimum purchase age for [tobacco and] alcohol products and enforcing this rule".* No further references regarding MLDA were provided in the EUPC or the Best Practice Portal or Xchange Registry. As for enforcement of MLDA, one relevant specific intervention was found in the Best Practice Portal; Electronic Age Verification (EAV) devices. The reference given was the umbrella review by Calafat et al. [18], which included one primary study of effect of EAV on

requesting ID before offering to sell alcohol. This study [48] was from the USA and evaluated effect of EAV on frequency of requesting an ID before offering to sell alcoholic beverages to mystery shoppers. The study was a controlled experiment and found no effect on frequency of ID checks.

To sum up: there is good evidence that raising MLDA reduces alcohol consumption and alcohol-related injuries among young people in the referenced literature in the ISDUP, however, this literature stems only from studies conducted in North America.

The statement that enforcement of minimum purchase age reduces use, is not supported by evidence with regard to alcohol.

Also with regard to this alcohol policy measure, it should be noted that the referenced literature is somewhat dated and that no additional literature on MLDA was found in the EUPC and the Best Practice Portal and Exchange registry.

c) Regulating alcohol advertising and promotion:

This regulatory policy intervention is among the alcohol policy measures that are addressed explicitly in the EUPC, where it is stated:

> *"Restricting and banning advertising and other forms of marketing of [tobacco and] alcohol to young people have also been shown to be effective [to reduce use] "*
>
> *"Banning the advertising of tobacco and restricting the advertising of alcohol products targeting young people have also been shown to reduce use."*

Two publications in the referenced literature in ISDUP pertain to alcohol advertising. These are: i) the Anderson et al. [4] review of prospective studies of commercial exposure to alcohol and subsequent alcohol use among young people and ii) the quite similar review by Smith and Foxcroft [13]. The review by Anderson et al. [4] included 16 publications from 13 studies, while the review by Smith and Foxcroft [13] included nine publications from seven unique studies, the latter nine publications were all among the 16 included in the review by Anderson et al. [4]. A main difference in selection of included studies/publications in these reviews, is that Smith and Foxcroft [13] assessed threats to internal and external validity for each study using the Newcastle-Ottawa Quality Assessment Scale for cohort studies, which likely explains their smaller study selection. Thus, findings and conclusions from the review by Smith and Foxcroft [13] form the basis for the following assessment of the literature. This systematic review addressed effects of alcohol advertising, marketing and portrayals on drinking behaviour among young people. The review was rated as having acceptable quality in the ISDUP Annex and included 7 primary studies (reported in 9 publications) employing prospective cohort data from young people (i.e. of school or college age). The studies used disparate measures of exposure to advertising (e.g. direct advertising in using broadcast and print media) or marketing (e.g. instore promotions) or portrayals of alcohol drinking (e.g. in films) and all used self-reported measures of exposure and outcomes. Three studies found that onset of drinking in adolescent non-drinkers at baseline were significantly associated with exposure. Two studies found that increased exposure to TV or music video viewing was associated with increased alcohol consumption. Two additional studies also found that increased exposure was associated with increased likelihood of initiating alcohol use. Overall, this review showed that baseline non-drinkers were significantly more likely to have become a drinker at follow-up with greater exposure to alcohol advertisements, whereas there was little difference in drinking frequency at follow-up in baseline drinkers. In studies that included

drinkers and non-drinkers, increased exposure at baseline led to significant increased risk of drinking at follow-up. The authors reported that effect sizes were generally modest.

As noted by Smith and Foxcroft [13], the prospective study design employed in the included studies, is probably the closest one gets to a evaluating effects of advertising exposure on drinking behaviour. Yet, control for important confounding factors is essential. While some potential confounding factors were adjusted for in the analyses in the included studies, important risk factors such as peer drinking and parental attitudes and behaviour were not adequately accounted for in some studies. Notably, the authors concluded that this systematic review did not provide direct evidence that limiting alcohol advertising will have an impact on alcohol consumption among young people. While the second author of this study acknowledged funding from the alcohol industry for another (unrelated) project, there is no indication that this possible conflict of interest led to biased conclusions in this study.

To sum up: while the literature shows consistent findings of associations between commercial exposure to alcohol and subsequent drinking behaviour among young people, which may suggest a causal effect, this literature may only serve as indirect evidence for likely beneficial effects of restricting or banning advertising and promotion. The claim that 'restricting the advertising of alcohol products targeting young people have also been shown to reduce use' is thus not supported by the referenced literature.

Also in this regard, the referenced literature is somewhat dated and no evidence on efficacy of regulating advertising or alcohol promotion was found in the Best Practice Portal or in the Xchange Registry.

d) Restricting days or hours of sale

Restricting days of sale is not stated explicitly as an effective policy measure in the ISDUP or in the EUPC and related evidence data bases. However, the systematic review by Middleton et al.[10] solely pertains to effects of restricting days of sale, and the fact that this review is included among the referenced literature in support of effectiveness of alcohol policy measures, suggests that this alcohol policy measure is among those with excellent evidence of beneficial effects. Correspondingly, the importance of restricting hours of sale is not stated explicitly in the ISDUP or in the EUPC. However, effectiveness of regulating hours of sale was the sole focus in one (Hahn et al. (2010) [8]) of the 12 referenced reviews of alcohol policy measures and it may thus be inferred that restricting hours of sale is also among the effective alcohol policy measures referred to in the ISDUP and EUPC.

The systematic review by Middleton et al [10] addressed effects of extending or maintaining days of sale and applied inclusion and assessment criteria of the Guide to Community Preventive Services [28]. A total of 14 primary studies met inclusion criteria. Effects of changes in days of sales were evaluated for both on-premise and off-premise settings. Effects of adding days of sale were assessed in 11 studies, and effects of restricting days of sales (i.e. imposing a ban on sales on a given day) were assessed in 3 studies. The studies assessed changes that took place in cities, in states, or in countries or large regions of countries and the changes occurred in Australia, European countries and the USA.

Seven studies examined the effect of increased days of sale at on-premise outlets. One study (of weak design) examined effects on individual consumption and reported no statistically significant effect. Five studies examined the effect of allowing Sunday on-premise sales on outcomes related to alcohol-impaired driving and all five reported increased harm rates subsequent to lifting the Sunday ban on

alcohol sales at on-premise outlets. One additional study examined the effect of allowing Sunday alcohol sales in restaurants only (not in bars) and this study reported an increase in arrests for driving under the influence, however, 95 % confidence interval was not calculable.

Four studies evaluated the effect of increasing days of sales at off-premise outlet. Two of these pertained to the reinstatement of Saturday sales (in monopoly outlets) in Sweden and involved an experimental design in two phases [49, 50]. In the first phase, the ban on Saturday sales was lifted only in experimental areas (while the ban was upheld in control areas); in the second phase the ban was lifted for the remaining areas in Sweden. During the first phase, repealing the ban was associated with an increase in alcohol sales and arrests for drink driving in experimental areas, whereas there was no association with assault rates. During the second phase, alcohol sales increased in the former control or buffer regions, whereas no significant effect on harm rates (weekend drunk driving or assault) was observed[ii]. Two other studies examined effects of Sunday off-premise sales in US states, one found that this increase in number of sales days led to an increase in per capita alcohol sales (i.e. beer sales and spirits sales), the other found an increase in the relative risk of alcohol-related crash-fatalities on Sundays.

Three studies examined the effect of introducing a ban on sales of alcohol on a specific weekday. One of these occurred in Sweden in terms of a ban on Saturday sales, and when lifted 20 years later, the effects were examined as described above. Effects of the ban were examined with regard to various harm rates, and the study found that harm rates on Saturdays (i.e. assaults, domestic disturbances, and police interventions against intoxicated people) decreased relative to harms for the rest of the week. Another study examined an experimental ban on Saturday closing of wine and spirits sales in Norway. Middleton et al [10] reported that: i) this study found that while wine and spirits consumption decreased, beer consumption increased and total consumption increased by 0.7 %, and ii) some harm rates decreased (arrests for drunkenness and domestic trouble), whereas another harm indicator (reports of violence) increased in experimental communities relative to control communities. However, a summary conclusion of the Norwegian report [51] is reported in a Nordic review [52] and stated that " … there was hardly any overall effect in terms of changes in total consumption or alcohol-related harm in the general population, but an effect among the more marginal group with heavy drinking problems. " A more detailed account of findings from the Norwegian experimental study is provided in an end note and demonstrates some minor inaccuracies in Middleton et al's review [10] in this instance[iii]. The third study is from New Mexico, USA, where some counties - shortly after the state's repeal of Sunday ban on alcohol sales – reinstated the ban. In these counties, the relative risk of an alcohol-related crash on Sundays was smaller than in other counties.

Middleton et al. [10] concluded that maintaining a ban on Saturday or Sunday sales of alcohol can prevent alcohol-related harms that would be associated with increased days of sale and that there is some evidence suggesting that imposing limits on days of sale will reduce alcohol-related harms. According to the Community Guide rules of evidence, there is strong evidence for the effectiveness of maintaining limits on days of sale for the reduction of alcohol-related harms. The authors also concluded that further scientific evidence is needed to fully assess the symmetry between maintaining existing limits and implementing new restrictions on days of sale.

The systematic review by Hahn et al. [8] focussed on effects of changes in hours of sale. The systematic review applied inclusion and assessment criteria of the Guide to Community Preventive

Services [28]. The review included 16 studies that met inclusion criteria. The 16 included studies (reported in 20 publications) all examined changes in hours of sale at on-premise outlets; 10 pertained to relatively large changes (i.e. of two hours or more) and six to smaller changes (i.e. less than two hours).

Studies examining changes of 2 or more hours reported on 6 events, 4 of these occurred in Australia, 1 in London, UK and 1 in Reykjavik, Iceland. Among these 10 studies, 2 found that an increase in trading hours led to a reduction in alcohol-related harms, 6 studies found that an increase led to an increase in alcohol related harm, 1 study found no effect and 1 study found a positive but non-significant effect. Four studies reported on effects of extended trading hours in London, UK. Two of these reported more alcohol-related harms and two other studies reported less harms subsequent to extended trading hours. The insignificant effects were found for events in Australia, where permit was not required.

Studies examining changes of less than 2 hours (n=6) reported on 4 events. From this small body of evidence there was no indication of consistent effects of small changes on alcohol-related outcomes. One study (from Perth, Australia) reported substantial increases in wholesale alcohol purchases, assaults, and motor vehicle crashes subsequent to extended trading hours. Two studies (of events in England and Wales and in Ontario, Canada) reported small and inconsistent changes in alcohol-related outcomes. Two studies of extended sales hours in Scotland also reported small and inconsistent changes in alcohol-related outcomes (i.e. sales and consumption).

Taken together, there was evidence of harmful effects of extending on-premise sales hours by two hours or more, whereas there was not sufficient evidence on effectiveness of smaller changes in sales hours. This review did not include any studies of effect of restricting trading hours and hence inferences of effectiveness of restricting hours of sale are based on indirect evidence. Nor did the review include any study on effect of changes in off-premise trading hours. The review did not make any mentioning of possible effects on specific population groups, including young people or heavy drinkers.

The review by Popova et al. [11] covered a broader set of alcohol policy measures, including both restrictions on days of sale and hours of sale. Their review of studies evaluating changes in days of sale included three publications, all of which were also included in the Hahn et al. (2010) review. Popova et al. [11] included 10 publications on quantitative evaluations of changes in sales hours and seven of these were included also in the Hahn et al (2010) review. The conclusions drawn in Popova et al. [11] that controls on hours and days of sale are shown to be effective are thus in line with, yet less nuanced than those in the reviews published one year later by Middleton et al. [10] and Hahn et al. [8].

Finally, evidence on efficacy of regulating sales hours was obtained also from the Best Practice Portal and the Xchange Registry. In the former, 'restricting opening hours to reduce alcohol related injuries' was assessed as having 'unknown effectiveness' and the reference was the umbrella review by Calafat et al. [18]. This umbrella review assessed efficacy of 11 types or broad categories of interventions of relevance to prevent harmful drinking or alcohol harms in nightlife settings, including opening hours. The review was pragmatic and there was no mentioning of systematic literature searches or assessment of study quality. With regard to opening hours, 4 primary studies and a review study were briefly assessed in Calafat et al.'s umbrella review [18]. Two primary studies reported increase in

harms subsequent to extended on-premise trading hours [53, 54], one study [55] reported decrease in harms subsequent to restricted on-premise trading hours and one study reported no change in harms after restricted trading hours [56]. Only one of these studies employed a time series analysis approach of adequate quality for assessment of intervention effect [55], the other 3 studies employed a simple before – after design, or a kind thereof. The latter three are therefore not considered any further. The included review study [57] was based on systematic literature searches and additional material identified from the 'grey literature'. Among 49 unique studies, only 14 included baseline and control measures and were peer-reviewed publications. Among the 14, 11 reported at least one significant outcome indicating adverse effects of extended hours or beneficial effects of restricted hours, and controlled studies with fewer methodological problems were most likely to report such effects. Thus, among the studies employing adequate methods, findings were generally in support of effectiveness of regulating hours of sale.

In the Xchange registry, two additional primary studies were referenced regarding effects of extending opening hours [19, 20]. Rossow and Norström [19] evaluated the effect of changes in on-premise closing hours on night-time violence rates in city centres on weekends employing an adequate study design (i.e. interrupted time series analyses with adequate control). Altogether 23 events of small changes (extensions or restrictions by $\leq$ 2 hours) in bar closing hours in 18 Norwegian cities over a 10 year period were evaluated employing time series analyses and control for potential confounders. The findings were consistent across different modelling techniques and showed that the impact of changes in closing hours was symmetrical; an extension of trading hours by one hour was associated with an increase in violence rate and a restriction was associated with a decrease in violence rate, and the effects were of the same magnitude, that is 1 hour change in on-premise sales hours was associated with a 16 % change in violence rates.

de Goeij et al. [20] evaluated the effect of extended closing hours by one hour for alcohol outlets on alcohol-related injuries. The extension of trading hours at nighttime applied to outlets with a daytime or nighttime license (presumably both off-premise and on-premise outlets) in one part of a Dutch city (Amsterdam) and the effect of this policy change was examined using an adequate method (i.e. a controlled before-after design). The study found a substantial increase (+ 34 %) in alcohol-related injury rate at nighttime on weekends. Thus, these two studies hours [19, 20] added to those reported by Hahn et al. [8] as the former found also substantial effects of small extensions of on-premise sales hours on rates of violence or injuries (i.e. increased harm rates with extended sales hours). Moreover the study from Norwegian cities [19] also found symmetrical effects of restricted sales hours (i.e. reduced harm rates with restricted sales hours).

To sum up: While there are no explicit statements in the ISDUP or EUPC regarding restricting days or hours of sale, the referenced literature provides evidence that these are also among the regulatory alcohol policy measures that, by limiting physical availability, are effective in curbing consumption and related harms. In particular, the evidence suggests that extending on-premise days or hours of sale is associated with increased consumption or harm rates and hence maintaining existing limits is an effective strategy for preventing excessive consumption and related harms [10].

It is noteworthy that there seems to be different assessments of the efficacy of regulating hours of sale in the ISDUP and in the Best practice portal. In the former, this policy measure has – implicitly – excellent evidence of effectiveness, whereas in the Best practice portal regulating hours of sale is assessed as having 'unknown effectiveness'. This contradiction in assessment of the evidence base

probably reflects that they build on different approaches to identification of the evidence base and different referenced studies.

Also for these policy interventions, the referenced literature is somewhat dated, and many of the primary studies included in the referenced reviews date back to the 1970s, 1980s or 1990s.

e) Restricting outlet density

Restricting outlet density is not stated explicitly as an effective policy measure in the ISDUP or in the EUPC. Three review studies referenced in the ISDUP provided evidence of effectiveness for restrictions on outlet density and were among the 12 reviews referenced with regard to effective alcohol policies. Two of these reviews pertained solely to effects of regulating outlet density and hence it assumed that this alcohol policy measure is also among those with excellent evidence of beneficial effects.

First, Campbell et al. (2009) [6] reviewed impact of outlet density employing different kinds of approaches and types of changes in outlet density.

The systematic review applied inclusion and assessment criteria of the Guide to Community Preventive Services [28] and thus a good quality review. Altogether 88 publications (articles or books) met inclusion criteria. The authors noted that "studies directly assessing the control of outlet density as a means of controlling excessive alcohol consumption and related harms do not exist." Thus, to assess the effects of outlet density alcohol-related harms, they used primary evidence from four kinds of studies: - time series studies of outlet density, studies of the privatization of alcohol sales, studies of alcohol bans, and studies of changes in license arrangements – all of which affected outlet density. Primary evidence was supported by secondary evidence from correlational studies.

Effect of changes in outlet density:

Primary studies of effect of outlet density on alcohol consumption (n=5) all found a positive association; that is: increased density was associated with increased consumption, and vice versa.

Primary studies of effect of outlet density on alcohol related harms (n=4) found mixed results. Two studies (both from California) assessing the relationship between alcohol outlet density and car crashes had inconsistent findings; one study found a negative association between off-premise density and fatal/non-fatal crashes and a positive association between on-premise density and crashes. The other study examined associations between outlet density and crashes among people 60 years or older and found a negative association with nonfatal crashes and a positive association with fatal crashes. Two studies examined associations between outlet density and violent assaults; both reporting positive associations between the two. However, while one study reported a positive association with on-premise outlet density, the other reported positive associations for off-premise outlet density and for density of bars, whereas the association was negative for density of restaurants.

Effect of changes in privatization:

The review included 11 events of privatization (in 8 US states and 2 Canadian provinces) and 1 event of re-monopolization (in Sweden). The events occurred between 1978 and 1993. In all areas assessed, the number of outlets increased dramatically following privatization. The authors summarised the findings as follows: the studies indicate that privatization increases the sales of privatized beverages (median effect + 42 %) and has little effect on sales of non-privatized beverages. The one study of re-

monopolization found that the reintroduction of government monopoly on medium-strength beer led to a significant decrease in car crashes for most age groups and a significant decrease among youth for several, but not all, alcohol-related harms.

Effect of bans on alcohol sales or consumption:

The review included seven studies examining effects of bans on local on-premise or off—premise alcohol sales or consumption. Five of these studies examined bans in native settings in the USA or Canada. All events occurred between 1970 and 1997. The authors summarize the findings as follows: The effectiveness of bans in reducing alcohol-related harms appears to be highly dependent on the availability of alcohol in the surrounding area. In isolated communities, bans can substantially reduce alcohol-related harms. However, where alcohol is available in areas nearby those with bans, travel between these areas may lead to substantial harms. The conclusion from these seven studies suggests that potential effect of local bans is likely of little relevance in most societies today.

Effects of licensing policy changes affecting outlet density:

The review included four such studies (from Finland, Iceland, New Zealand and the USA). These licensing policy changes differed for the four studies. The study from USA examined the 'liquor by the drink' (LBD) licensing change in North Carolina, which led to the opening of many bars adjacent to restaurants, and found that spirits sales and night-time crashes among men above legal drinking age (21 + years) increased significantly in LBD counties. The study from Finland examined effects of the 1969 enactment which, first and foremost, implied a substantial increase in off-premise outlets with license for beer sales (17 400 outlets) and an increase (46 %) in on-premise licenses. From 1968 to 1969 total alcohol sales increased by 46 % (most of which were attributed to increased availability of beer. Consumption increased significantly at all levels of consumption. In Iceland, a 1989 policy change implied an increase in off-premise monopoly outlets and on-premise outlets. Over the subsequent 4 years, consumption increased, mainly among men with an initially high consumption level. Also in New Zealand a policy change occurred in 1989, allowing for off-premise sales of table wine also in grocery stores (in addition to warehouse stores and specialised wine stores or liquor stores) [58]. Over a two-year period, the number of wine outlets increased by about 25 % and an increase in wine sales by 17 % and no change in sales of other alcoholic beverages, implying an overall increase in total alcohol consumption. These four studies of other changes in licensing policy add to the overall picture of an association between increase in outlet density and increase in alcohol sales/consumption.

Overall, the review by Campbell et al. [6] showed that most studies found an association between outlet density and alcohol consumption or related harms; greater outlet density was associated with increased alcohol consumption and related harms, including medical harms, injuries, crime and violence.

Among the studies included in Campbell et al.'s review [6], most studies stemmed from North America and Scandinavian countries and most studies examined effects of increased outlet density. Campbell et al. [6] noted that reductions in outlet density with resulting reductions in consumption will have substantial consequences for the industry, which may oppose such policy change. Moreover, while little data on program implementation costs were available, it was assumed that costs would likely be small compared to savings from alcohol related harms.

The referenced review by Hahn et al. (2012)[9] pertained exclusively to effects of alcohol retail privatization. This systematic review applied inclusion and assessment criteria of the Guide to

Community Preventive Services [28]. The review included 17 primary studies that evaluated the impact of privatizing alcohol sales on per capita alcohol consumption and met inclusion criteria. The studies evaluated 12 distinct events of privatization that occurred in 7 US states, in 2 Canadian provinces and in Finland. Moreover, one additional study evaluated the effect of re-monopolization in Sweden. In the US states, sales of wine or spirits were privatized, in Canada, sales of beer, wine and/or spirits were privatized, whereas in Finland and Sweden sales of beer was privatized or re-monopolized.

The review examined the effects of: i) privatization on consumption of privatized beverages; ii) privatization on consumption of non-privatized beverages and total consumption; iii) privatization on and alcohol-related harms; and iv) re-monopolization on alcohol-related outcomes.

Regarding effects on consumption of privatized beverages, the median effect was a 44 % increase in sales. Inconsistent findings were reported for the privatization of wine sales (in 1985) and spirits sales (in 1987) in the state of Iowa, USA. Specifically, some reports found no statistically significant effect of privatization on sales, whereas other reports found that privatization led to increased sales of the privatized beverages. Hahn and co-authors [9] noted that this inconsistency may be the consequence of different modelling strategies, different time periods covered, and that different forms of alcoholic beverages were included (i.e. 'wine coolers' were included in measures of wine sales in two reports). One study from Finland employed survey data and reported that the greatest increase in consumption after privatization occurred among heavier drinkers.

Several studies reported effects of privatization on sales of other non-privatized beverages and found that these decreased slightly (median decrease of 2.2 %), which was not sufficient to off-set the overall increase in sales of privatized beverages. The review included two studies that examined possible effects of privatization on alcohol-related harms. In both studies non-significant associations were reported and for both studies, important methodological weaknesses were noted. Only one study (from Sweden) examined effects of a re-monopolization of medium strength beer. This led to a reduction in number of outlets for medium strength beer (from 11 500 to 300). Alcohol related harm rates were examined for 4 age groups, and the associations were mainly negative but statistically insignificant. However, hospital admissions for treatment of AUD and alcohol psychosis decreased by 20 % in the youngest age group (10-19 years) and motor vehicle crashes decreased significantly (by 14 %) for 3 age categories.

The authors of the review further assessed 20 other studies (cross-sectional and panel studies) and a review of studies from Nordic countries not published in English translation [52]. The latter study findings corroborated those reported from included primary studies, showing that privatization (of beer sales) was associated with increased alcohol consumption and increase in alcohol-related harms.

Overall, most of these studies found that greater outlet density is associated with increased alcohol consumption and related harms. Another good review by Hahn et al. (2012) [9] examined effects of privatization of alcohol sales (i.e. lifting a state monopoly on retail sales). Such privatization leads to substantial increase in alcohol outlet density and is also found to increase consumption and related harms.

Popova et al. (2009) [11] also examined effects of outlet density. However, this review did not distinguish between correlational studies and studies with more adequate design for inferring intervention effect. Consequently, the findings, which generally showed that higher density was

associated with higher consumption and related harms, are only considered here as supportive evidence for the abovementioned studies.

Finally, in the Xchange registry 'Local environmental alcohol licensing strategies' were rated as possibly beneficial and four studies from the UK were referenced [21-24]. Three of these studies pertained to effects of local regulations of availability in terms of 'cumulative impact zone' and increased licensing enforcement [21-23]. de Vocht et al. (2017) [21] examined whether restrictions in alcohol availability at the local level (i.e. cumulative impact zone and increased licensing enforcement) was associated with alcohol-related hospital admissions and crime rates in England. The study compared local areas with maximum extent of intervention (i.e. cumulative impact zone and increased licensing enforcement) post intervention (n=5) and employed postintervention synthetic time series based on weighted time series in control areas (n=86). Intervention was associated with moderate reductions in alcohol-related hospital admissions and violent crime rates, however estimates were not statistically significant at 5 % level. Another study by de Vocht et al. [22] relates to that above and examined effect of restrictions in alcohol availability (a graded measure of cumulative impact zone and decline of licenses for new premises over a nine-year period) on alcohol-related hospital admission rates. Growth models were employed with adjustment for confounders. The study found a greater reduction in alcohol-related hospital admissions in areas with the most intense local licensing policies (i.e. presence of cumulative impact zone and more intense scrutiny of alcohol license applications) ($p < .05$). A third study by the same author group [23] pertained to the same intervention and used the same graded measure and similar methods as those in the 2016 study [22], but here the outcome measure was reported crime rates. The study found that in local areas with more intense alcohol licensing policies, rates of violent crimes, sexual crimes and public disorder declined more strongly compared with areas where these policies were not in place.

Finally, one study [24] examined the impact of individual alcohol licensing decisions on health and crime rates. Specifically, the authors evaluated the effect of closure of a single on-premise outlet (2 events: closure of a night club and closure of a restaurant and cocktail bar) on health and crime outcomes in England. Outcome measures included emergency department admissions, ambulance call-outs and reported incidents of anti-social behaviour and crime rates. Time series analyses were employed, and counterfactual time series were used as control. The study found an immediate short-term effect of the closure of the nightclub on one outcome measure (reported antisocial behaviour) but not on the other outcomes examined. The study found no measurable effect of the closure of the restaurant and cocktail bar. (This study examined also the impact of implementation – and thereafter defunding/restructuring – of new local licensing guidance, however, this intervention was not considered relevant in our context.). Thus, this study adds to the abovementioned literature by addressing possible effects of quite small restrictions (closures of single outlets), and while such effects were detected to little extent, this was probably in line with what could be expected, given the modest magnitude of the intervention.

To sum up: Overall, changes in outlet density are associated with changes in alcohol consumption and alcohol-related harms; the higher density, the higher consumption and harm rates, and vice versa.

f) Introducing dram shop liability

This intervention is the main focus in one of the 12 referenced reviews in support of effective alcohol policy measures, and it is therefore assumed that introducing dram shop liability is among the alcohol policy measures with excellent evidence of efficacy. The review study by Rammohan et al. [12] is a

systematic review which applied inclusion and assessment criteria of the Guide to Community Preventive Services [28]. The review assessed evidence of effectiveness for two different types of interventions; i) dram shop liability and ii) enhanced enforcement of overservice laws. The review of the latter included two primary studies and was not a regulatory alcohol policy measure and hence not considered any further.

Dram shop liability holds the owner or the server(s) at an on-premise outlet (e.g. bar, restaurant) where a patron consumed his/her last alcoholic beverage responsible for harms inflicted by the patron on others. Liability in a state can be established by case law or statute [12]. In the USA, most states have enacted dram shop statutes or dram shop policies by case law/precedent, only seven states have neither [12]. The review included 11 primary studies examining the effectiveness of dram shop liability in preventing excessive alcohol consumption or alcohol-related harms. All studies stemmed from the USA and 10 of these were panel studies of US states that covered overlapping time periods and states and were thus not entirely independent. On the other hand, the models in these studies assessed effects on different outcomes and included different co-variates. Two studies reported on alcohol consumption (i.e. self-reported binge drinking) as an outcome and both found small non-significant decreases associated with dram shop liability. Eight studies assessed effects of dram shop liability on fatal car crashes and six of these examined alcohol-related motor vehicle fatalities as outcome. All eight studies found that dram shop liability was associated with a reduction in car crash fatalities. The four studies that examined motor vehicle fatalities among underage drinkers, all found significant reductions associated with dram shop liability. One panel study examined effects of two lawsuits brought against on-premises outlets filed by families of people killed in alcohol-related car crashes and both lawsuits were associated with decreases in single-vehicle nighttime crashes. Other outcomes were assessed in one study, which reported significant associations with reductions in homicide rates and alcohol-related medical conditions, whereas no significant association with suicide rates.

While the review by Rammohan et al. [12] provided good evidence that dram shop liability is an effective intervention for reducing alcohol-related harms, it should be noted that the studies are exclusively from the USA, and it remains unclear whether such liability is a feasible intervention in other jurisdictions.

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Appendix I:

Criteria for assessment of evidence of efficacy or effectiveness (from Flay et al., 2005)

1. **Efficacy – criteria**
    a. Efficacy statement: (*Program X is efficacious for producing Y outcomes for Z population at time T in setting S*)
    b. Intervention description: that allow others to implement/replicate. Desirable to measure implementation.
    c. Outcomes: measured in line with stated public health outcome, measurement of established quality, long-term follow-up when outcome may decay over time. Desirable to measure potential side-effects. Desirable to use multiple measures/sources
    d. Clarity of causal inference:
    i. Design must allow for strongest possible causal statement: at least one control condition (RCT or controlled design) or interrupted time series
    e. Generalizability of findings: specify sample and how obtained
    f. Precision of outcome: statistical analysis must be based on design (e.g. analyse at level of randomisation (e.g. school class, not individuals) and intent-to-treat analysis). Reports from 'high-fidelity samples' should be reported as such. Adjust for pre-test differences if necessary. Adjustment for multiple comparisons.
    g. Statistically significant effects: results must be reported for every measured outcome. Reporting only statistically significant results is misleading. Efficacy can be claimed only for constructs with a consistent pattern of statistically significant positive effects. For an efficacy claim there must be no serious negative effects on important outcomes.
    h. Practical value: Necessary to demonstrate practical significance in terms of public health impact.
    i. Duration of effect: For outcomes that may decay over time, there must be a report of significant effects for at leas tone long-term follow-up at an appropriate interval beyond end of intervention.
    j. Replication: consistent findings are required from at least two different high-quality studies meeting all above criteria and each has adequate statistical power. When more than two efficacy/effectiveness studies are available, preponderance of evidence must be consistent with that from the two studies of highest quality.
2. **Effectiveness – criteria** (from Flay, 2005)
    a. Meet all efficacy criteria
    b. Program definition: information available must be sufficient such that practitioners could implement program/policy.
    c. Intervention delivery: under the same types of conditions as expected in the real world.
    d. A clear theory of causal mechanisms should be stated. Also 'for whom' and 'under what conditions' the intervention is expected to be effective
    e. Measures: Essential to measure integrity and level of implementation/delivery of intervention. Essential to measure acceptance, compliance, adherence, involvement of target audience
    f. Generalizability of findings: degree of generalizability should be evaluated.
    g. Practical value: the effects of an intervention should be practically important and evaluation reports should report some evidence of practical importance.
    h. Replication: Consistent findings are required from at least two different high-quality trials that meet all of the above criteria and each of which has adequate statistical power. It is desirable to have more than two replications.

Endnotes:

Endnote 1[i]

Spoth et al (2008) provided 10 referenced studies on Raising minimum drinking age laws as presented in their Table 6:

31. Yu, J. and R.W. Shacket, *Long-term change in underage drinking and impaired driving after the establishment of drinking age laws in New York State.* Alcoholism: Clinical and Experimental Research, 1998. **22**(7): p. 1443-1449. 54

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40. Hingson, R.W., et al., *Impact of legislation raising the legal drinking age in Massachusetts from 18 to 20.* American Journal of Public Health, 1983. **73**(2): p. 163-170.

In addition, the following studies are cited in the text but not in Table 6:

41. O'Malley, P.M. and A.C. Wagenaar, *Effects of minimum drinking age laws on alcohol use, related behaviors and traffic crash involvement among American youth: 1976-1987.* Journal of studies on Alcohol, 1991. **52**(5): p. 478-491.

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47. Shults, R.A., et al., *Reviews of evidence regarding interventions to reduce alcohol-impaired driving.* American Journal of Preventive Medicine, 2001. **21**(4, Supplement 1): p. 66-88.

When Spoth et al. (2008) argued that the evidence for a preventive effect of raising minimum legal drinking age is mixed or emerging, this argument seems to build on the observations that the literature is not entirely consistent in the sense that all studies at hand show (statistically significant) preventive effects. Specifically, Spoth et al (2008) gave two examples of studies which found that drinking among college students remained high after enactment of underage drinking laws (at least one of these studies employed a simple before – after comparison design). Moreover, two examples of studies that reported no significant effect of this intervention on rates of traffic accidents or fatalities were also provided. Spoth et al.'s (2008) assessment of the literature differs from that in the two systematic literature reviews that they cite. Thus, Wagenaar and Toomey (2002) summarized the findings of their systematic review of effects of minimum drinking age laws (MLDA) as follows: "Of the 33 higher quality studies of MLDA and alcohol consumption, 11 (33%) found an inverse relationship; only 1 found the opposite. Similarly, of the 79 higher quality analyses of MLDA and traffic crashes, 46 (58%) found a higher MLDA related to decreased traffic crashes; none found the opposite. Eight of the 23 analyses of other problems found a higher MLDA associated with reduced problems; none found the opposite. Only 6 of the 64 college-specific studies (9%) were of high quality; none found a significant relationship between the MLDA and outcome measures. Conclusions: The preponderance of evidence indicates there is an inverse relationship between the MLDA and two outcome measures: alcohol consumption and traffic crashes. The quality of the studies of specific populations such as college students is poor, preventing any conclusions that the effects of MLDA might differ for such special populations." Correspondingly, Shults et al. (47. Ibid.) found that there was strong evidence for the effectiveness of raising minimum drinking age laws for preventing impaired driving.

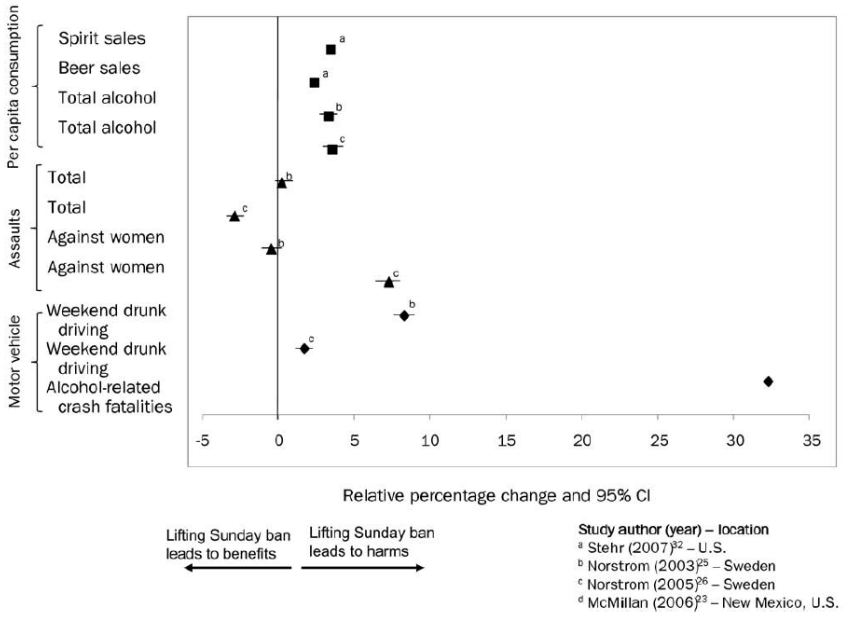
[ii] Endnote 2

Comment on Middleton et al. on Phase 1 and 2 of the Swedish experiment study on effects of repealing ban on Saturday sales in monopoly outlets:

The review states as follows:

> "During Phase I, alcohol sales in the experimental area increased 3.6% (95% CI_2.6%, 4.6%) and incidents of drunk driving arrests increased by 11.3% (95% CI_4.2%, 18.4%) compared with that in the control areas. Both findings were significant. However, the researchers noted that along with repeal of the ban, there was increased police surveillance for alcohol-related motor vehicle incidents in the experimental region, which may have contributed to the increase in the number of drunk driving incidents reported. Assaults against women indoors (a proxy for domestic violence) increased 0.6% (95% CI__6.5%, 7.7%) and total assaults declined by 1.3% (95% CI__5.6%, 3.0%); neither result was significant. During Phase II, the repeal of the ban on Saturday sales was extended to the whole country (26). Alcohol sales increased by 3.5% (95% CI_3.0%, 4.0%) in what had been the control and buffer regions in Phase I—an increase similar to that which had occurred in experimental counties in Phase I.

The 1.7% (95% CI__7.0%, 10.0%) in-crease in drunk driving arrests in the rest of the country was not significant in Phase II (unlike in Phase I)."



igure 3. Relative percentage change in three categories of alcohol-linked effects attributble to an increase in days of alcohol sale in off-premises establishments

In their text Middleton et al., provide an accurate report of the findings from the Swedish studies of effects of Saturday opening of monopoly outlets 49. Norström, T. and O.-J. Skog, *Saturday opening of alcohol retail shops in Sweden: an impact analysis.* Journal of Studies on Alcohol, 2003. **64**(3): p. 393-401.; 50. Norström, T. and O.J. Skog, *Saturday opening of alcohol retail shops in Sweden: an experiment in two phases.* Addiction, 2005. **100**(6): p. 767-776.. One may add that the primary studies reported no statistically significant effect on assault rates (total or domestic) in Phase 1 or Phase 2.

However, Figure 3 (copied in above) shows a statistically significantly negative association between Saturday opening and total assault rates in Phase 2 of the experiment and statistically significant effects on weekend drunk-driving in both Phase 1 and Phase 2. Hence, the Figure 3 provides several incorrect data points (measures of association).

Endnote 3

[iii] **Comment on Middleton et al on the Norwegian experiment of Saturday closing in monopoly outlets:**

The authors give reference to Nordlund, 1985 (i.e. stated reference is: Nordlund S. Effects of Saturday closing of wine and spirits shops in Norway. Oslo, Norway: Statens institutt for alkoholforskning, 1985.). However, this study was only published in Norwegian (i.e. Effekten av lørdagsstengningen ved Vinmonopolets butikker), but it was reviewed by Mäkelä, Rossow & Tryggevesson (2002) 52. Mäkelä, P., I. Rossow, and K. Tryggvesson, *Who drinks more or less when policies change? The evidence from 50 years of Nordic studies.*, in *The effects of Nordic alcohol policies? What happens to drinking and harm when alcohol controls change?*, R. Room, Editor. 2002, Nordic Council for Alcohol and Drug Research: Helsinki. p. 17-70. in a literature review of Nordic alcohol policy studies, published in English. This literature review was also included in another systematic review by the same author group 9. Hahn, R.A., et al., *Effects of alcohol retail privatization on excessive alcohol consumption and related harms: a community guide systematic review.* American journal of preventive medicine, 2012. **42**(4): p. 418-427. In the review by Mäkelä et al. the following was reported with regard to effects of Saturday closing in Norway:

> *"Sales figures from the monopoly stores showed that 1) the total number of buying visits decreased significantly in the experimental towns and a significant proportion of the Saturday sales took place on Fridays instead. 2) The sales in terms of value decreased somewhat, but far less than the number of buying visits, implying that people would purchase less often but for a larger amount each time. The effect on sales in terms of value was mostly*

immediate, and after the first 4 months the sales were only 3% lower than before Saturday closing; 3) the sales in litres of alcohol decreased only very slightly, and only with respect to sales of liquor. No increase in beer sales was observed. An evaluation of admissions to a detoxification center in Oslo (mainly serving skid-row alcoholics) showed a significant decrease in the number of admissions on Saturdays and Sundays in 1984 compared to 1983 (Krogh & Ihlen 1984). Among those who were admitted in the first period after Saturday closing in Oslo, a larger proportion reported drinking non-beverage alcohol, illegal spirits, etc. than in the previous year. Moreover, police records showed that reports of drunkenness decreased significantly in the experimental towns, particularly during Saturdays and nights before Sundays (Hauge & Nordlie 1984). The total number of arrests for drunkenness did not change significantly, but there was a shift from Saturdays to the other weekdays. Reports of domestic disturbance decreased significantly in the experimental towns, yet this decrease was larger on other weekdays than on Saturdays and nights before Sundays, and thus the decrease seems less likely to be attributable to the Saturday closing. Possible effects on drunken driving were also assessed (Irgens-Jensen 1984), but no significant change in drunken driving in the experimental towns could be observed. Thus, a summary conclusion from the various assessments of possible effects of Saturday closing was that there was hardly any overall effect in terms of changes in total consumption or alcohol-related harm in the general population, but an effect among the more marginal group with heavy drinking problems (Nordlund 1984b). The experiment of Saturday closing was not continued beyond 1984."

In the research report by Nordlund (1984) [51] Nordlund, S., *Effekten av lørdagsstengningen ved Vinmonopolets butikker (the effect of Saturday closing of alcohol monopoly outlets).* Alkoholpolitik, 1984. **1**(4): p. 221-229., which is the only primary study reporting on the Norwegian experiment of ban on Saturday sales in 1984, the author reported that sales of wine and spirits (calculated into litres of pure alcohol) decreased by 1.3 % from 1983 to 1984 (the first four months in each year) in the intervention cities and these sales increased by 1.5 % in the control cities, hence relative to the control condition wine and spirits sales decreased by 2.8 % in intervention cities. Beer sales (calculated into litres of pure alcohol) increased in both intervention cities (+ 4.4 %) and control cities (+ 4.1 %) and hence beer sales increased by 0.3 % in intervention cities relative to control cities. Thus, when considering total alcohol sales, the increase in beer sales more than outweighed the decrease in wine and spirits sales in intervention cities (+ 1.4 %), however this increase in total sales was lower than that in the control cities (+ 2.6 %), and hence the effect of Saturday closing was a decrease in total alcohol sales by 1.2 % (Table 6, p 225 in Nordlund, 1984). This is in contrast to Middleton et al.'s report that total alcohol sales increased by 0.7 %. Moreover, in the English summary of Nordlund's (1984) research report it is noted that

"The effect on total alcohol turnover (beer, wines and spirits) seems rather uncertain. The difference between the test town and the control town was only 1.2 percentage points. Despite the insignificant drop in total record ed alcohol turnover, effects could be discerned within certain alcohol problem groups. In Oslo, for instance, a more marked decline in the number of admissions to sobering-up stations was recorded on Saturdays and Sundays than on other days. Saturday closing also seems to have brought about a reduction in the number of reports to the police of drunkenness. Records of arrests for drunkenness showed a shift from Saturday to other days of the week, though no decrease in the total number of arrests for drunkenness was perceptible. The number of reports of violence in the home declined more sharply in the test towns than in the control towns, but the difference was greatest from Sunday to Thursday, that is to say the days one would have least expected to be affected by Saturday closing, and smallest on Saturday. A slight shift from Saturday to other days of the week could be discerned in the number of violence reports, particularly to Friday. Totally, however, there was a definite increase in the number of reports of violence in the test towns, while the figures were approximately unchanged in the control towns."