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Dr. Dai Lloyd, AM
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National Assembly for Wales
Cardiff Bay, Cardiff, CF99 1NA
[REDACTED]

cc: Ms. Sarah Sargent
Policy and Legislative Committee Service
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Re: Evaluation of Minimum Price for Alcohol (MPA) Bill

Dear Dr. Lloyd:

Thank you for your request of 26 October for my views on the MPA Bill, presently before the National Assembly for Wales. As I understand from your letter and HSCSC webpage, Stage 1 scrutiny of the Bill allows for continued consideration of general principles of MPA and extent to which this Bill will improve the health and well-being of the population of Wales. This letter contains my independent review and skepticism regarding the MPA Bill and supporting documents on minimum unit pricing (MUP).

Some details on my background; since 1990 I have conducted academic research on economic aspects of alcohol use and alcohol-related harms. I have published over 40 articles and book chapters related to this research, with an emphasis on marketing and pricing of beverage alcohol. Collectively, this body of research has been cited more than 750 times. A listing of my work published since 2006 appears at the end of the evaluation. Most of these publications have undergone the anonymous peer-review process employed by academic journals and compendiums.¹ My recent research has focused on systematic reviews of empirical work on alcohol use and misuse, which are highly pertinent to the MPA Bill. I have published 10 meta-analyses and systematic reviews focused on alcohol marketing, alcohol pricing and alcohol-related harms, including drinking by youth and young adults. All 10 articles were peer reviewed. Some of my past research was supported financially by US public agencies; some was supported by industry-associated groups; and some was conducted independently as a normal part of academic employment. In the Economics Discipline, most economists take the position that scientific research reports are evaluated on their merits, absent innuendos and claims of personal bias. I resent crude *ad hominem* attempts to discredit my research as somehow tainted by industry support, without arguments that detail the scientific basis for such claims or errors on my part. Several recent attempts to do so have in my opinion failed (see Nelson 2008a, 2014e, 2016a). This letter presents my independent views, and received no financial support, input, or consultation of any kind or manner from individuals associated with the alcohol industry. As in all my publications, it is my work alone, and does not necessarily represent the opinion or position of other groups or institutions.

¹ EM (p. 80) argues that peer-review is strong evidence of support for a research approach, but fails to address other research-related issues such as publication bias and general issues of statistical hypothesis testing. I have written extensively on issues of *publication bias* in alcohol-related research (Nelson 2010a, 2011, 2013d, 2013e, 2014a).

A summary of my recent research is available as: “Economic evidence regarding alcohol price elasticities and price responses by heavy drinkers,” **Public Health Open J**, 1 (2), August 2016, 36-39. Open Access at: <http://dx.doi.org/10.17140/PHOJ-1-108>

Much of the literature on MUP is focused on hypothetical price changes, including the Sheffield Model, and not real-world price changes. Actual policy-induced price changes have been the focus of my recent research, including effects on heavy drinkers and alcohol-related harms for adults and youth. As demonstrated below, the Evaluation Memorandum (EM) and report of the Advisory Panel on Substance Abuse (APoSM) are incomplete, misleading, and deeply flawed as scientific documents. An assessment of the MPA Bill indicates that it is unlikely to achieve its objectives. The main reason is that harmful and hazardous drinkers are relatively insensitive to price changes contrary to claims in the Bill’s supporting documents (EM, p. 84; APoSM, p. 58). As shown below, evidence that alcohol-related harms will be lessened by price increases or minimum prices is not extensive or convincing. However, this letter is not a complete appraisal as several issues deserve more attention, such as methods used to determine benefits and costs of MUP and the major deficiencies in empirical studies of MUP in Canada and elsewhere.

The focus of the Bill is a reduction in alcohol-related harms that are a consequence of harmful or hazardous use alcohol. However, heavy drinkers, including youth, are not responsive to alcohol prices as depicted by the Sheffield Model or other claims in the public health literature. Many of these claims concern population-level drinking, including moderate drinkers (e.g., APoSM, p. 54). Review of changes in alcohol prices from survey data and natural experiments reveals that price effects on heavy drinking and alcohol-related harms are more nuanced than earlier studies suggest, including the Sheffield Study for Wales. My detailed evaluation, attached to this letter, sets out reasons for my conclusions. It is my opinion that other policy actions besides MPA need to be considered:

1. Maintain the existing policy banning below-cost sales of alcohol at off-premise outlets.
2. Adopt policies proposed under EM Option Two (p. 92), especially education programs targeting children and young people, that strengthen the focus on alcohol misuse. Implement laws and regulations to reduce alcohol sales and drinking in conjunction with athletic sporting events and other youth-oriented events, such as public concerts. Consider limits on beverages that combine alcohol and caffeine. Consider an increase in the legal age to 19 or 20 years. Better enforce existing laws on underage consumption.
3. Adopt additional policy actions that better target harmful and hazardous drinkers of all ages, including strengthening of laws and penalties for drink-driving, public intoxication, underage drinking, and other actions that are closely related to harmful or hazardous consumption. Penalties such as drink-driving fines are more salient than broad population-level policies such as advertising bans and minimum prices. Better enforce existing laws on hazardous consumption.

Thank you for this opportunity to comment on the MPA Bill. Alcohol-use harms are a serious problem in Wales and other countries. Such problems deserve serious assessment and evaluation, including scientific reviews that do not meet the current public health view of political correctness. Evidence-based policies should be based on consideration of all scientific evidence, and not a selective slice thereof.

Respectfully submitted:

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J o n P . N e l s o n

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Evaluation of Minimum Price for Alcohol (MPA) Bill in Wales (dated November 26, 2017)

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Introduction

1. I reviewed the following MPA reports:

Public Health (Minimum Price for Alcohol) (Wales) Bill – Explanatory Memorandum incorporating the Regulatory Impact Assessment and Explanatory Notes, October 2017 [EM].

Model-based Appraisal of Minimum Unit Pricing for Alcohol in Wales – An Adaptation of the Sheffield Alcohol Policy Model version 3, September 2014 [Sheffield Study or SS].

Advisory Panel on Substance Misuse, Minimum Unit Pricing: A Review of Its Potential in a Welsh Context, July 2014 [APoSM].

2. EM (p. 124) states that:

The objective of the minimum pricing legislation is to tackle alcohol-related harm, including reducing alcohol-attributable hospital admissions and alcohol-related deaths, by reducing alcohol consumption among harmful and hazardous consumers, including among young people in Wales.

3. While the above reports contain evidence regarding possible effects of higher prices on alcohol consumption and alcohol-related harms, evidence for harmful and hazardous drinking is incomplete or presented in a misleading manner. Additional evidence reviewed here indicates that MPA will not be as effective as claimed for consumption or harms. Major errors and problems are detailed below.

3.1 EM and APoSM also employ rhetorical language in attempting to make a case for MPA. Examples of misleading language from EM include: “strong evidence” (p. 10); “growing body of evidence and research that shows a strong link” (p. 20); “there is clear evidence” (p. 47); “there is a strong evidence base” (p. 65); “strong and consistent link” (p. 78); “robust process, using conservative assumptions” (p. 80); “robust evidence base” (p. 84); and “strong and consistent evidence” (p. 121). APoSM employs similar rhetorical language, including: “the evidence base is extensive” (p. 10); “modelling is ... well-founded and robust” (p. 10); “strong evidence” (p. 55); and “evidence consistently indicates” (p. 55).

4. As shown below, these bold claims ignore conflicting evidence, and often are presented prior to an actual examination of evidence, so the accompanying review is presented, *ipso facto*, as obvious truth. This language is misleading and in the current context, deliberately so. It should be removed or modified.

Economic modelling by Sheffield

5. In principle, minimum unit pricing operates by placing a floor under prices for alcohol beverages, with an intended effect of limiting choice for all drinkers, especially harmful or hazardous drinkers who tend to consume greater quantities of low-cost and/or higher-alcohol content beverages. Evidence for beneficial effects of minimum pricing are based largely on the University of Sheffield Model as adapted for Wales. The Sheffield Study is deficient:

- 5.1 Many details of the Sheffield Model are largely unknown or underreported for Wales and other countries (e.g., Canada, Scotland, UK), including forecast error intervals for policy simulations. Confidence intervals for forecasts increase in size as model inputs or parameters are changed importantly relative to current values.² Sheffield regressions and simulations lack standard measures of forecasting accuracy. The estimates are always presented as “precise” *point estimates* (e.g., reduction of 53 deaths and 1,400 fewer hospital admissions, SS, p. 10), which hides the range of uncertainty for effects and possible benefits of MPA. Although a sensitivity analysis is presented for some parameters (p. 77), this does not fully capture uncertainty surrounding forecasts for a 20-year period. Statistical forecast intervals and forecast statistics should be presented for all Sheffield Model estimates. Point estimates are insufficient as the sole scientific basis for MPA. All estimates should include standard errors, and confidence intervals for parameter estimates should be reflected in the sensitivity analysis.
- 5.2 Estimated own-price elasticities for the UK (SS, p. 25) are not representative of values obtained elsewhere for beverage alcohol, and most cross-price elasticity estimates do not meet a widely-accepted test for statistical significance ($p < 0.05$). For example, a meta-analysis by Nelson (2014a) reports a consensus beer price elasticity that is only -0.20 compared to values of -0.98 (off-premise) and -0.79 (on-premise) used by Sheffield (p. 25).³ Even if the Model can be shown to be internally valid, its external validity is in doubt. Further, large elasticities by beverage reported by Sheffield do not coincide arithmetically with an overall average elasticity of -0.50 used elsewhere in MPA reports (APoSM, p. 54; EM, p. 28; and SS, p. 79). Price elasticity estimates for Wales for harmful and hazardous drinkers are required to formulate an evidence-based MPA policy.
- 5.3 Various other Sheffield estimates show low overall levels of statistical reliability. As one example, the Sheffield Study (p. 29) in Table 4.5 reports statistical models for risks of binge drinking and mean daily consumption. Values for R-squared statistics range from only 0.19 to 0.45, indicating that less than 50% of variation in the data are explained by the regressions. Uncertainty in estimates in

² As explained by P. Kennedy, *A Guide to Econometrics*, 6th ed. (2008, p. 332), an error interval is smallest at average values of a data set and expands away from the means. See also J.S. Armstrong, *Standards and Practices for Forecasting* (2001), available at <http://forecastingprinciples.com>. One possible way to demonstrate legitimacy of a forecast or simulation is to save part of a data sample for model validation. This is referred to as *cross-validation* (Kennedy, p. 102), and helps to internally validate a given model. To the best of my knowledge, Sheffield has not done this modelling exercise. Hence, robustness of the Model is largely unknown, contrary to EM (p. 80).

³ The consensus value of -0.20 is incorrectly reported as -20 in EM (p. 32). The full citation for Nelson (2014a) is given below, which is incomplete in EM (p. 32). EM also incorrectly reports the year of publication. For additional evidence regarding own-price elasticity values, see the reviews in Nelson (2013d, 2013e, 2014a, 2016c), which correct for “*publication bias*.”

Table 4.5 is not reflected in model simulations.⁴ This is important because some alcohol-related harms are linked to “peak day consumption.” In many cases, parameters are reported without standard errors or other indicators of significance. For example, Table 4.13 (p. 48) simply reports slope estimates for work absence, without regressions or associated standard errors. This is incomplete reporting of key results. All statistical estimates and simulations using the Sheffield Model need to report standard errors, complete regression results, and forecast error intervals and other pertinent statistics.⁵ Only if model simulations reflect statistical (in)accuracy can policymakers accurately judge the possible range of impacts of MPA.

- 5.4 A major omission in the Sheffield Study is its failure to model outcomes if there is a shift in major parts of the price distribution when a MUP is imposed on the lower end of that distribution (EM, p. 135). An across-the-board shift in prices imposes costs on all consumers, regardless of drinking level, which are not accounted for in current benefit-cost calculations.⁶ Sheffield Model simulations need to be modified to account for this possibility, but more importantly benefit-cost analysis for MPA needs to reflect costs that will be imposed on moderate consumers due to higher prices generally.
- 5.5 EM (p. 64) argues erroneously that it is not possible to quantify “. . . a possible reduction in consumer utility” [due to higher prices]. Monetary measurement of loss of consumer surplus from price increases is a standard exercise in benefit-cost analysis, and it is inconceivable that this statement was written by a trained economist. Hence, it is not known accurately that “. . . MUP policies would have only a small impact on moderate drinkers” (EM, p. 65). The benefit-cost analysis should be modified to account for loss of consumer surplus arising due reduced consumption of alcohol by all consumers, under a variety of assumptions concerning the final distribution of prices.
- 5.6 In addition to MUP, the Sheffield Model is used to evaluate other possible policies, including a general 10% price increase and a ban on below-cost selling. For the latter, the Sheffield Study (p. 79) argues that “a policy to ban below-cost selling has virtually no impact on consumption and alcohol-related harms.” This conclusion is premature without statistical confidence intervals for all Model estimates. The statement is based on point estimates, which might have wide ranges. Simulations of other policies require confidence intervals for the forecasts involved. The estimates also fail to account for a general shift in the distribution of alcohol prices, following imposition of a MUP.
- 5.7 Simulation modelling is not a perfect substitute for evidence of actual real-world price differences and changes. The Sheffield Model is based on general population data. The scientific evidence assessed, however, should be focused on harmful and hazardous drinkers. Population-level econometric studies incorporate all manners of drinking levels and patterns, including in many instances individuals who abstain from consumption of alcohol.

⁴ The Sheffield Study (p. 28) incorrectly reports that these estimates are contained in Table 4.6.

⁵ Coefficient of determination or R-squared is the standard measure of the amount of variation explained by a regression model. It is subject to possible misuse if investigators simply miss-specify the model to increase R-squared. However, the Sheffield Study for Wales fails to include specification tests of any kind.

⁶ For example, Stockwell et al. (2011, p. 916) reports actual price-distribution increases for moderate- and higher-priced products of about 1% to 2.6% compared to average real price increases of 3.6% for lower-end products.

Evidence relating to heavy drinking, alcohol prices, and alcohol harms

6. EM (pp. 27-35) provides a highly selective survey of the evidence base relating to impacts of price on alcohol consumption and associated harms. The survey is neither comprehensive nor systematic, but it is used to arrive at a sweeping conclusion that “. . . in the *majority of cases*, this evidence demonstrates that in response to an alcohol price increase, there is a decrease in alcohol consumption and – crucially – a decrease in alcohol-related harm and mortality. Likewise, when there is a decrease in price, alcohol-related harm increases” (EM, p. 32, emphasis added). These claims are erroneous and misleading.

7. A comprehensive review of alcohol policies related to taxes and prices is important for several reasons. First, the Sheffield Model is not a study of real-world policy changes relating to prices, alcohol taxes, minimum prices, below-cost selling, or other policy changes. The Sheffield Model might be best described as a “correlational study,” which leaves issues of causality unresolved. Second, MUP is targeted toward heavy and hazardous drinking, and many economic studies are based on population-level data. Third, as noted by EM (p. 32, citing Boniface et al. 2017), the evidence-base for MUP has been produced by a small number of research teams and “the quantitative uncertainty in many estimates or forecasts is often poorly communicated outside the academic literature.” This comment echoes my concern above that the Sheffield Study has failed to provide sufficient statistical uncertainty measures.⁷

8. I next provide several examples of the incompleteness of evidence cited by EM for alcohol and price as it relates to both heavy drinkers and alcohol-related harms. My critique is not complete, but is provided to indicate that there are important scientific omissions in EM and APoSM, which need to be rectified before decisions are made on the MPA Bill. The additional evidence considered here pertains to real-world price changes, and not those produced by model simulations. This is important as external validation of Sheffield Model estimates for Wales.

8.1 **Switzerland** – EM (p. 33) cites one study for Switzerland (Heeb et al. 2003) for an actual policy-induced change in alcohol prices.⁸ However, there have been five studies of this policy change for alcohol consumption, with conflicting results (Nelson and McNall 2017, p. 430). Overall, Swiss results indicate that spirits consumption rose *modestly and temporarily* for heavy drinkers following a price reduction. One study also is available for Switzerland for changes in alcohol dependency among younger drinkers, with *null results* (Nelson and McNall 2016b, p. 268).

8.2 **Finland** – there has been extensive study of policy-induced changes in Finnish prices, but EM (p. 33) reports results for only three studies for alcohol-related harms. This is selective reporting. For consumption changes, Nelson and McNall (2017, p. 424) report results for nine studies. We conclude that “overall, consumption results for Finland are mixed” (p. 425), with possible *short-term effects* on heavy drinkers and little effect on lower-income younger persons and youth. For alcohol-related harms, Nelson and McNall (2016b, p. 267) review 28 studies covering multiple harms, including

⁷ Similar issues exist in other Sheffield studies cited in support of MUP, such as Meir et al. (2009) that reports off-premise and on-premise average prices paid by UK drinkers with different average consumption levels. Results in Meir et al. do not include standard errors, so it is uncertain if any of the reported differences in average prices are statistically significant.

⁸ Studies of actual policy-induced changes are often characterized as “natural experiments” in contrast to more abstract modelling approach used by Sheffield. Natural experiments address some causality issues left uncertain in the abstract modelling approach.

studies relating to mortality and hospitalizations (15 studies); assaults and other crimes (5 studies); drink-driving (3 studies); intoxication (4 studies); and alcohol-dependency (2 studies). Results are generally mixed for most harms, but lower prices in Finland had some effects on mortality and hospitalizations, especially for liver disease among older persons (p. 270). This result is indicative of nuanced or selective effects of alcohol price changes on distinct subpopulations and/or distinct alcohol-related harms.

8.3 **Sweden** – EM (p. 34) ignores the large body of evidence relating to price reductions in Sweden, and chooses to focus exclusively on one study of quality substitution due to a price increase. Nelson and McNall (2017, p. 427) examine 13 studies of Swedish alcohol consumption following actual policy-induced changes in prices. Numerous null or negative results are reported in these studies, and again any increases in consumption appear to be *short-term* in nature. In addition, Nelson and McNall (2016b, p. 267) review 20 studies for alcohol-related harms in Sweden following actual price changes. We conclude (p. 270) that any effects on mortality and hospitalizations were *short-term* in nature, and other effects were weak or non-existent.

9. Summary of alcohol consumption changes following actual price changes in five countries – In a peer-reviewed study, Nelson and McNall (2017) examined 29 primary studies containing 35 sets of results for alcohol consumption, including results for binge drinking (18 studies), young adult and youth alcohol consumption (18 studies), and older adults and heavy-drinking adults (16 studies). Results are reported for five countries, and some studies cover multiple countries or outcomes. Our general conclusion from a comprehensive review is as follows:

Overall, we find a general lack of consistent results that can provide a sound evidence-base for development of alcohol tax policy. In all countries there is a lack of robust results for major segments of the population, following interventions that reduced prices and relaxed import quotas . . . In many cases, positive policy effects are short-term in nature or apply to particular groups of individuals or subpopulations . . . what we learn from this review is that alcohol tax and price changes are likely to have selective effects on drinking and drinking patterns (Nelson and McNall 2017, p. 431).

10. Summary of alcohol harm changes following actual price changes in nine countries – In a peer-reviewed study, Nelson and McNall (2016b) examined 45 studies for nine countries for five harmful outcomes: mortality and hospitalizations; assaults and other crime; drink-driving; intoxication; and alcohol-dependency.⁹ We reviewed 69 outcomes as some studies covered more than one harm or country. Our results and conclusion are summarized as follows:

Findings indicate that changes in taxes and prices have selective effects on harms. Mortality outcomes are positive for liver disease and older persons, especially in Finland and Russia. Mostly null results for assaults and drink-driving are found for five countries. Intoxication results

⁹ It is worth noting that EM fails to address issues of social unrest, public nuisance, and other anti-social behaviors that are often associated with drinking by youth and young adults and sporting events. Results in Nelson and McNall (2016b) for intoxication may capture this type of harm. There is not strong evidence that intoxication is increased by lower prices. Some indirect evidence on this issue might also be found in so-called field studies that examine several on-premise pricing practices such as happy hours, pitcher specials, drinking games, and buying rounds. Boniface et al. (2017, p. 10) review only one laboratory study in their “systematic” review of minimum pricing. In contrast, Nelson (2015a, p. 9) reviews six field studies, with mixed results for binge drinking outcomes.

for Nordic countries are mixed for selective subpopulations. Results for survey [dependency] indexes are mixed, with no strong pattern of outcomes within or across countries. Prior reviews stress taxes [and pricing] as a comprehensive and cost-effective intervention for addressing alcohol-related harms. A review of natural experiments indicates the confidence placed on this measure is too high, and natural experiments in alcohol policy had selective effects on various subpopulations (Nelson and McNall 2016b, p. 264).

11. While policy changes studied by Nelson and McNall (2016b, 2017) do not include minimum unit prices as such, they do provide information on the kinds of evidence cited by EM in support of a MUP policy for Wales. Further, these policy changes entail across-the-board price reductions. Heavy drinkers account for a substantial share of total alcohol consumption. If harmful and hazardous drinkers are as sensitive to prices as claimed by supporters of MUP, then one might expect to see dramatic effects of these natural experiments on both alcohol consumption and alcohol-related harms. Dramatic effects are not apparent, especially over the longer-run.

12. Three other systematic reviews by Nelson (2013c, 2014d, 2015a) – all peer-reviewed – provide evidence on price-sensitivity of individuals who engage in heavy or hazardous consumption of alcohol. Only one study – Nelson (2013c) – was incorporated in EM (p. 82) and APoSM (p. 54), which again is indicative of uninformed or selective reporting. A summary of each review follows:

12.1 **Nelson (2013c)** examined 19 individual-based studies (survey sample methods) that examine price responses by heavy-drinking adults and nine studies of prices and cirrhosis mortality. A total of 573 studies relating to alcohol prices and taxes were retrieved as a first step in the review process, with final selection based on further examination of studies and their content. The 19 studies for consumption excludes population-level empirical studies. The 19 studies include results from five countries, while the nine studies cover multiple countries, including an international OECD panel.¹⁰ This peer-reviewed study concluded the following:

The review finds only two studies [out of 19] of heavy drinking with a significant and substantial negative price response. For cirrhosis mortality, only two studies [out of 9] find a significant negative price response. Overall, the role of price and taxes as a significant deterrent to heavy drinking by adults is uncertain (Nelson 2013c, p. 265).¹¹

12.2 **Nelson (2014d)** provides a review of alcohol prices and gender differences for drinking and heavy drinking by adults and young adults. Starting again with a broad database, relevant studies were narrowed to 15 studies of adult drinking and eight studies of drinking by young adults, aged 18-26 years. As in Nelson (2013c), this review included discussion of samples, measurement and econometric issues, and key empirical results in each primary study. I attempt to review all relevant

¹⁰ Small samples of studies in systematic reviews result from a tight focus on closely-related studies. This contrasts with the “kitchen-sink” approach used in some studies; e.g., a MUP review by Boniface et al. (2017) combines MUP studies with several other studies are not closely-related to MUP; see EM (p. 32). The same problem occurs in the sample of “heavy drinking” studies reviewed in Wagenaar et al. (2009); see EM (p. 29).

¹¹ Since publication of this survey, I have re-examined EU data on cirrhosis mortality with a focus on statistical outliers in the data sample; see Nelson (2015b). My study of “affordability” of alcohol is contained in Nelson (2014b), which demonstrates that increased “affordability” of alcohol in most countries of the OECD and EU is due to rising real personal incomes and not falling real prices.

evidence on the issue at hand, and not a selective slice thereof. Results of the peer-reviewed study are as follows:

First, adult men have less [price] elastic demands compared to women. Second, there is little or no price response by heavy-drinking adults, regardless of gender. Third, although the sample is small, price might be important for drinking participation for young adults. Fourth, the results strongly suggest that heavy drinking by young adults, regardless of gender, is not easily dissuaded by higher prices (Nelson 2014d, p. 1260).

12.3 **Nelson (2015a)** conducted a systematic review – again peer-reviewed – for effects of alcohol prices (or tax surrogates) on binge drinking for three age groups: youth, young adults, and adults. Outcomes examined include binge participation, intensity and frequency. Criteria for data collection and potential sources of bias are discussed, including adequacy of price data. Fifty-six relevant studies were found, with studies and results distributed equally among three age groups. Also found were five natural experiments for tax reductions and six field-based studies examining price-promotions in bars and pubs. This is a much larger sample of results compared to reviews used in EM (p. 29, citing Wagenaar et al. 2009; Elder et al. 2010). My review included results for four countries. An innovation in Nelson (2015a, p. 4) is a demonstration of under-reporting of econometric studies that occurs in earlier reviews by public health researchers, such as Wagenaar et al. (2009, 2010). My systematic review of binge drinking studies concludes that:

Null results or mixed results are found in more than half of the studies. The body of evidence indicates that binge drinkers are not highly-responsive to increased prices. Non-responsiveness holds generally for younger and older drinkers and for male and female binge drinkers alike. Increased alcohol taxes or prices are unlikely to be effective as a means to reduce binge drinking, regardless of gender or age group (Nelson 2015a, p. 1).

13. As MUP is specifically targeted at harmful and hazardous drinking, it is particularly important that the evidence-base focus on that element of drinking and on real-world price changes as opposed to simulations of price changes or evidence for population-level drinking. Evidence for alcohol harms does suggest that there might be some positive benefits for highly-selective subpopulations, but selective effects are better dealt with through more targeted policies, rather than a population-level MUP policy.

14. Overall, these five reviews cover numerous studies, countries, sub-populations, drinking patterns, and outcomes. My systematic reviews provide virtually no support for the notion that MUP will be effective over the long-term in reducing heavy use of alcohol or alcohol-related harms. The extensive nature of these reviews is in stark contrast to the limited and selective summaries contained in EM and APoSM.

15. Based on a limited review, APoSM (p. 54) argues that “. . . taken as a whole, there are far more estimates demonstrating a strong relationship between alcohol and price compared to a handful that do not [citing only studies by Nelson 2013c and Ayyagari et al. 2013]. As such there is strong evidence to support a connection between the price of alcohol and demand for alcohol.”

15.1 **This statement is a red herring** in my opinion, and unfortunately appears repeatedly in the public health research literature (e.g., Babor et al. 2010, p. 125). The issue is not overall “demand for alcohol,” but price responses of those drinkers who are targets of the MPA Bill, viz., *harmful and*

hazardous drinkers. APoSM is correct that numerous studies demonstrate a relationship between price and alcohol demand (see Nelson 2013e, 2013d, 2014a), but most of these studies are not relevant for evaluation of the MPA Bill – they are concerned with all manners of drinking levels and patterns, including light, moderate, and heavy drinkers combined in population-level studies. It is my professional opinion that only studies in my five systematic reviews – and similar focused studies – are relevant to the MPA Bill and MUP.¹² These reviews and primary studies should be given careful assessment prior to final consideration of the Bill. APoSM fails to provide sufficient appraisal of the evidence-base for heavy drinkers and real-world price changes. The APoSM report is incomplete and misleading.

16. EM (pp. 31-32) also selectively cites literature pertaining to alcohol prices and heavy drinking. The EM report appears to be suggesting that only one or two studies have reported a weak link between prices and heavy drinking. **This suggestion is false and misleading.** The EM (p. 32) also incorrectly cites Nelson (2014a) as a study of “harmful and hazardous drinkers.” It is not; rather it is a study of population-level drinking.

17. As an indication of results in other studies relevant for MPA, I have appended quotations and references from 16 selected studies. The appendix provides a summary of studies that report null or negative statistical results for alcohol prices and heavy drinking or alcohol-related harms. None of these 16 studies are cited in EM and only one of the studies is cited in APoSM. More complete and detailed results are contained in tables in my five reviews:

- Nelson (2013c) – Table 3 for 19 primary studies for heavy-drinking adults (p. 274); Table 4 for 9 primary studies cirrhosis mortality (p. 277).
- Nelson (2014d) – Table 2 for 15 primary studies for adults (p. 1267); Table 3 for eight primary studies for young adults (p. 1270).
- Nelson (2015a) – Table 2 (p. 7), with binge-drinking studies divided according to youth (18 studies); young adults (20 studies); adults (19 studies); and studies using natural experiments (5 studies) and field methods (6 studies).
- Nelson and McNall (2016b) – Table 3 (p. 268) for a summary of five categories of alcohol-related harms, divided by positive vs. null evidence, for 69 outcomes (45 primary studies).
- Nelson and McNall (2017) – Table 2 for alcohol consumption in Denmark (6 studies); Table 3 for Finland (9 studies); Table 4 for Hong Kong (2 studies); Table 5 for Sweden (13 studies); and Table 6 for Switzerland (5 studies).

18. A summary of these five systematic reviews is Nelson (2016c), “Economic evidence regarding alcohol price elasticities and price responses by heavy drinkers,” **Public Health Open J**, 1 (2), Aug 2016, 36-39. Open Access at: <http://dx.doi.org/10.17140/PHOJ-1-108>

¹² Empirical results in Stockwell et al. (2001, 2012) for minimum pricing in Canada do not address heavy, harmful or hazardous drinkers, and are therefore largely irrelevant for evaluation of the MPA Bill. As noted by EM (p. 31) “. . . an MUP is more targeted towards the heaviest drinkers,” but alcohol measures in the Stockwell studies concern entire provincial populations of drinkers. There are numerous other flaws in Stockwell’s empirical studies, including: omission of “adding-up” constraints; omission of cross-price effects; omission of measures of goodness-of-fit; and inconsistent results. Results also are not robust to a first-differencing of data, which likely renders the data stationary. Numerous other MUP studies in the alcohol literature are based on hypothetical changes in prices, and not actual real-world price changes as in the natural experiments reviewed in Nelson and McNall (2016b, 2017).

Anticipating unintended consequences

19. Governments around the globe have experimented with population-level market interventions to limit undesired activities or promote those activities that are currently politically popular or desired.

Unintended consequences often result since it is impossible to close-off all forms of innovation by consumers and producers. The EM report, MPA Bill, and several commentaries speak to issues of unintended consequences, such as cross-border shopping (EM, p. 84, p. 128); product substitution and/or innovations (p. 132, p. 140); rent-seeking activity (p. 136); non-price competition (p. 139); mixed or joint sales (Bill, sections 5-7); illicit and illegal alcohol use (EM, p. 83; Duffy and Snowdon 2012; O'May et al. 2015, 2016); and general effects on consumer spending patterns (EM, p. 83; Snowdon 2014). This is a long list. The MPA Bill also incorporates a “sunset provision” in Section 21-22, which provides for a report on operation and effects of MPA. This is at least tacit recognition that MPA could be ineffective or have unintended consequences that may not be desired.

19.1 Another unintended consequence of alcohol policy is that beneficial effects are often short-lived. As discussed above, this is apparent in many studies examined by Nelson and McNall (2016b, 2017).

Other recent econometric studies also indicate that alcohol policy interventions can have short-run effects for alcohol-related harms that do not carry-over in the long run.¹³

19.2 The possibility exists for positive short-run effects if a MUP is instituted, but null or negative effects in the long run. Hence, provisions should be made that anticipate this consequence.¹⁴ First, political and governmental organizations should refrain from self-congratulatory speeches and notices regarding MUP, since effects may be short-lived. Second, for appraisal of effects of MUP (if implemented), methodologies should be adopted that recognize the potential for short-run effects only.¹⁵ Third, considering the highly political nature of alcohol policy in Wales and the UK, those groups who are closely associated with the Bill should not be major participants in the “sunset review.” This includes individuals responsible for the Sheffield Model as well as other prominent members of the public health research community, who have been at the heart of MUP debate in Wales, UK, Ireland, and Scotland. Frankly, it is ludicrous to suggest that such vested interests do not exist.

19.3 As discussed by Craven et al. (2013), there are a wide range of issues that proponents of minimum prices must first resolve, including such concerns as substitution of marijuana and other illegal or illicit drugs for low-price alcohol.¹⁶

¹³ See, e.g., R. McClelland and J. Iselin, Do alcohol excise taxes reduce motor vehicle fatalities? Evidence from two Illinois tax increases, Tax Policy Center, Urban Institute and Brookings Institution, October 2017. This study is consistent with long-term results reported in Nelson and McNall (2016b, 2017), and contrary to results in Wagenaar et al. (2015) and Wagenaar et al. (2010). The latter study is cited favorably in EM (p. 29).

¹⁴ In financial markets, short-run effects are referred to as “announcement effects.” For a recent study of this economic phenomenon, see H. Allcott and T. Rodgers, The short-run and long-run effects of behavioral interventions: Experimental evidence from energy conservation, *American Economic Review* 104, 2014, 3003-37.

¹⁵ What I have in mind here are assessment methods that address issues of causality and selection, such as employed for assessment of US labor supply programs. For an introduction, see J.D. Angrist and J-S Pischke, *Mostly Harmless Econometrics: An Empiricist’s Companion* (Princeton University Press: 2009).

¹⁶ B.M. Craven, et al., The economics of minimum pricing of alcohol, *Economic Affairs* 32, 2013, 174-89.

What can be done

20. A brief review of the MPA Bill indicates that it is unlikely to achieve its objectives. The EU and APoSM reports are incomplete as scientific evidence in support of MPA. My evaluation indicates the scientific errors and omissions in these reports. However, this letter is not a complete evaluation as several additional issues deserve attention, such as methods used to determine benefits and costs of MPA and major shortcomings in empirical studies of existing MUP policies. The focus of the MPA Bill is harmful and hazardous consumption of alcohol and alcohol-related harms that are a consequence of this level or pattern of alcohol use. Harmful and hazardous drinkers, including youth, are not responsive to alcohol prices as depicted by the Sheffield Model or many other claims in the public health literature. **Population-level studies do not reveal this insensitivity.** Review of real-world changes in alcohol prices indicates that effects on alcohol-related harms are likely to be nuanced or selective.¹⁷ These effects also may be short-term in nature. It is important to remember that the objective of the Bill goes beyond just increasing prices at the low-end, and entails a desired long-term reduction in harms.

21. In conclusion, it is my opinion that other policy actions besides MPA need to be considered:

1. Maintain the existing policy banning below-cost sales of alcohol at off-premise outlets.
2. Adopt policies proposed under EM Option Two (p. 92), especially education programs targeting children and young people, that strengthen the focus on alcohol misuse. Implement laws and regulations to reduce alcohol sales and drinking in conjunction with athletic sporting events and other youth-oriented events, such as public concerts. Consider limits on beverages that combine alcohol and caffeine. Consider an increase in the legal age to 19 or 20 years. Better enforce existing laws on underage consumption.
3. Consider additional policy actions that better target harmful and hazardous drinkers of all ages, including strengthening of laws and penalties for drink-driving, public intoxication, underage drinking, and other actions that are closely related to harmful or hazardous consumption. Penalties such as drink-driving fines are more salient than broad population-level policies such as advertising bans and minimum prices. Better enforce existing laws on hazardous consumption.

22. Thank you for this opportunity to comment on the MPA Bill. Alcohol-use harms are a serious problem in Wales and other countries. Such problems deserve serious review and evaluation, including scientific reviews that do not meet current public health views on political correctness. Evidence-based policies should be based on consideration of all scientific evidence, and not a selective slice thereof.

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¹⁷ See, e.g., A. Allamani, et al., A commentary on the limits of alcoholic beverage policies, *Alcohol and Alcoholism* 52, 2017, 706-14 [suggesting that alcohol policy must consider social and cultural context].

Appendix – 16 representative studies reporting negative/null results for heavy drinking:

- The estimated tax effects among the whole population . . . are relatively large and significant among light drinkers but shrink substantially for moderate and heavy drinkers. We cannot reject that alcohol consumption of the latter types is unresponsive to tax changes (An and Sturm 2011, p. 19).
- The more [price] responsive group is more likely to be non-white, female, married, and older and to consume less alcohol . . . Our results indicate that the heavier drinking group is insensitive to price; thus higher taxes would be unlikely to reduce negative externalities for older drinkers (Ayyagari et al. 2013, p. 102).
- Taxation policies which increase the price of alcohol, and are very efficient at decreasing harms associated with reduced average consumption, may be relatively inefficient at decreasing alcohol harms associated with high-intensity drinking (Brynes et al. 2012, p. 2).
- The beer tax estimate for heavy episodic drinking is negative in sign but not statistically significant (Carpenter et al. 2007, p. 9).
- Increases in beer prices . . . [for] both binge drinking and underage drinking . . . indicate that male college students are virtually unresponsive to price (Chaloupka and Wechsler 1996, p. 122).
- The liquor [tax] responsiveness of self-reported drinks per month is large among women and those aged 25 to 55. . . results based on the full sample had suggested that binge drinking was unresponsive to all of the tax measures. This pattern is repeated in models based solely on these particular groups. More specifically, none of the results in Table 5 indicates that increased alcohol taxation reduces binge drinking (Dee 1999, p. 15).
- For heavier drinking . . . the overall price elasticity of increased salience of drinking is negative (-0.411) but not statistically significant . . . the qualitative pattern in the price elasticities is largely insensitive to our inclusion of these [state alcohol] policy variables (Farrell et al. 2003, pp.129-30).
- According to the results, alcoholic beverage taxes have no effect on alcohol consumption. For the general population, taxes have no effect on neither the number of drinks consumed nor binge drinking (Gius 2002, p. 80).
- The negative own price effect for occasional and moderate drinkers is consistent with the consumer behaviour but the insignificant effect for the heavy drinkers is against intuition . . . the results are not inconsistent with those found in Manning et al. (Harris 2006, p. 794).
- Both the frequency and intensity of moderate drinking are sensitive to price . . . At the extremes, heavy drinking by the most-informed consumers is much more price elastic than moderate drinking, while the estimated price elasticities of heavy drinking for the least-informed consumers are not statistically significantly different from zero (Kenkel 1996, pp. 306-07).
- The results indicate that both light and heavy drinkers are much less price elastic than moderate drinkers. Further, we cannot reject the hypothesis that the very heaviest drinkers have perfectly price inelastic demands (Manning et al. 1995, p. 123).
- Alcohol prices do not affect mortality rates due to chronic liver diseases. Empirical results in the study do not lend support to broad price-based approaches to alcohol policy (Nelson 2015, p. 1).
- All three models show a negative impact of current distilled-spirits taxes on log cirrhosis mortality rates, although the effect is not significant in Model 2 . . . Wine and beer tax rates . . . are never significant predictors of cirrhosis mortality (Ponicki and Gruenewald 2006, p. 936).
- Logistic regression analyses were conducted to identify predictors of heavy drinking . . . Neither degree of crowding nor the discounting of drinks significantly contributed to the model (Stockwell et al. 1993, pp. 1522-23).

- Price had negative effects on the probability of heavy episodic drinking and drinking and driving among heavy drinkers, but the effects were not statistically significant (Stout et al. 2000, p. 408).
- However, after adjustment for adult binge drinking, the association between tax and youth drinking was attenuated and no longer statistically significant [i.e., no direct effect] . . . We observed similar findings when assessing the effect of adult binge drinking on the relationship between tax and youth binge drinking (Xuan et al. 2013, p. 1717).

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