

## **A public response to the Adam Smith Institute's critique of the Sheffield Alcohol Policy Model**



### **Introduction**

As the research team responsible for the development and dissemination of the Sheffield Alcohol Policy Model, we welcome the recent contribution of [REDACTED] and [REDACTED] to the debate over the effectiveness of minimum unit pricing (MUP) for alcohol published by the Adam Smith Institute.

Duffy & Snowdon raise a series of points regarding the detail of our research; however, we believe at heart their critique is a broad rejection of the use of mathematical models to estimate the potential impact of social policy options. In the response below we address this point of principle before responding to the more specific criticisms. First though, it is important to recognise that the Sheffield Alcohol Policy Model is far from an isolated piece of work on the relationship between alcohol prices and alcohol-related harms and so we begin with a brief summary of this substantial set of further evidence.

### **Further evidence on the relationship between alcohol prices, alcohol consumption and alcohol-related harms**

By examining the relationship between changes in alcohol prices or taxation and changes in alcohol consumption, two separate reviews of over 100 studies have demonstrated that: (1) it is consistently the case that when prices go up, consumption goes down; (2) this relationship is statistically robust; and (3) this relationship holds true for different kinds of heavy drinking as well as average consumption [1, 2].

Turning to the relationship between alcohol prices or taxes and alcohol-related harm, a review of over 50 studies similarly concluded that: (1) it is consistently the case that when prices go up, overall levels of harm go down; (2) this relationship is statistically robust; (3) the relationship holds true for alcohol-related disease, violence, other crime or disorder and road traffic accidents [3].

If this is the general relationship, then what of the specifics of minimum unit pricing? A form of this policy exists in Canada and recent studies have begun to evaluate its effects [4-6]. These studies have shown that increases in minimum prices are associated with falls in alcohol consumption and alcohol-related deaths.

In sum, there is a substantial body of national and international scientific evidence on this subject. That evidence is robust, compelling and accepted by respected academics and a range of expert bodies including the World Health Organisation and, more locally, the National Institute for Health and Clinical Excellence, the Academy of Medical Royal Colleges, each of the Royal Colleges of Physicians, General Practitioners, Nursing, Psychiatrists and Surgeons, the Royal Society for Public Health and the Faculty of Public Health. Any critique of our model and its limitations should be interpreted alongside an understanding that the conclusions policymakers are drawing are not based solely on our modelling work but more fundamentally from this much wider body of scientific evidence.

### **Mathematical modelling for the appraisal of social policies**

Mathematical models are acknowledged to have two primary roles in supporting evidence-based policymaking. These are documented in the UK Treasury's *Green Book* of best practice as being: (1) to **appraise** the potential impact of policies yet to be implemented; (2) to **evaluate** the historical impact of policies that have been already implemented [7]. The Sheffield Alcohol Policy Model (SAPM) is used for policy appraisal and, whilst the scope of our model is quite ambitious, its methods are largely orthodox and models of this kind are frequently used to inform decision-making.

Duffy & Snowdon appear firmly of the opinion that appraisal of MUP is impossible. They ignore the strong evidence cited above on the relationship between changes in alcohol prices, alcohol consumption and alcohol-related harm and conclude that "there is no shame in saying that we simply do not know", "minimum pricing might reduce alcohol harm or increase it" and "the evidence base is, to all intents and purposes, non-existent" (p12).

We disagree with these assertions. Yes, the character of drinking in the UK is complex; it has a history, it is influenced by a number of factors (of which price is one) and it is made up of multiple components which vary across population subgroups and over time. However all areas of social policy, from welfare and employment to criminal justice, confront similar complexity but still seek evidence to inform decision-making. Model-based appraisal is a particularly useful form of evidence as it helps to pick apart this complexity by bringing together existing research on different aspects of the policy question.

In conducting such appraisals, we are forced to: (1) be transparent regarding our theories of the steps which link a policy intervention to changes in the outcomes of interest; and (2) identify the evidence which underpins our modelling of each step. This is advantageous in enforcing presentation of a clear causal process linking policy to outcome, allowing the theory and evidence behind each step in that process to be subject to scrutiny and enabling identification of the key strengths and weaknesses of the underlying evidence. Indeed, Duffy & Snowdon engage in this process by pointing to alternative research suggesting heavier drinkers are less responsive to

price changes than moderate drinkers. However, they do not, as might be expected, critique the strengths or weaknesses of that alternative evidence or use it to calculate alternative estimates of policy impact. Instead, having presented this evidence, they then curiously conclude that such evidence is “non-existent” (p12) and that the whole exercise is worthless. Users of our research know that we actually account for this alternative evidence (along with many others) by testing the impact of using it in our model and demonstrate that it does not change the conclusion that a MUP would reduce the consumption of heavy drinkers by more than that of moderate drinkers. We document all of this testing of alternative evidence in our reports [8].

### **Duffy & Snowdon’s claim to empirically disprove the modelling**

Duffy & Snowdon claim to be “in the unusual position of being able to empirically disprove a prediction [from SAPM] about a policy [MUP] which has not yet been introduced” (p13). This is a strong but unsubstantiated claim as Duffy and Snowdon do not actually present the evidence they claim disproves our estimates. Instead they simply sketch a method by which such evidence might be obtained. Our belief is their method is naive and we explain our reasoning below.

The basis of their claim is that the 17.5% reduction in alcohol consumption estimated by SAPM for a 70p MUP is similar to the actual reduction in consumption that has occurred since 2006. However, the change in alcohol-related deaths since 2006 does not match the SAPM estimates of a 1,273 reduction in the first year after MUP implementation, rising to 7,263 after 10 years.

For this evidence to “disprove” the SAPM estimates requires Duffy & Snowdon to make a number of assumptions to ensure the comparison they are making is like-for-like. None of these assumptions are made clear to the reader. We will focus on three of them to demonstrate that, rather than empirically disproving a prediction, Duffy and Snowdon are making a comparison as inappropriate as comparing apples and oranges. The three assumptions concern ignoring differential effects between population subgroups, ignoring time lags between changes in drinking and changes in harm and ignoring other causes of mortality. All of these considerations are included in our modelling.

- The first of Duffy & Snowdon’s unstated assumptions is that the 17.5% recent reduction is the same 17.5% reduction that would arise from MUP. In reality, similarly sized overall consumption changes, will have different implications for alcohol-related mortality depending on which groups within the population have changed their behaviour. This is because different groups have different risk of mortality (e.g. harmful drinkers’ risks of mortality are much higher than those of moderate drinkers so a consumption change in harmful drinkers has different implications for rates of harm). Minimum unit pricing has been considered attractive by policymakers precisely because resultant consumption reductions are composed of larger reductions in high-

risk groups and smaller reductions in low-risk groups. Two separate policies or processes leading to equal overall consumption reductions can lead to very different changes in rates of alcohol-related harm.

- The second of Duffy & Snowdon's unstated assumptions appears to be that a 17.5% reduction in consumption occurring over one year would have the same effect on mortality as a 17.5% reduction arising over 6 years (2006-2012). The epidemiological evidence suggests that there is a time lag between change in consumption and change in mortality, and that the size of the lag varies for different diseases. We account for this evidence in the model, which is why we report health effects over a number of years rather than just a single year. Duffy & Snowdon do not account for any time lags in their argument, choosing instead to ignore the substantial evidence base showing that a 17.5% reduction in one year would not have the same effect as a 17.5% reduction over a number of years.
- The third of Duffy & Snowdon's unstated assumptions concerns other risk factors and causes of mortality. Their proposed method implicitly assumes any change in mortality for causes partly related to alcohol consumption must be wholly due to changes in alcohol consumption. Therefore, they are either assuming that alcohol is the only risk factor for these causes of mortality or that no other risk factors change over the comparison period. Whichever assumption our critics are making, there is a huge weight of epidemiological evidence to show that they are wrong. For many diseases (such as colorectal, breast and oesophageal cancers), alcohol is one of several risk factors (such as obesity, diet and smoking) that can affect the mortality rate. So, for example, if there were an increase in obesity then there might be an increase in the number of deaths from colorectal cancer even if alcohol consumption had decreased.

### **Some specific errors and misunderstandings made by Duffy & Snowdon**

Having discussed the issues of principle that Duffy & Snowdon appear to have with the appraisal of social policies, we now turn to their detailed criticisms of our model. In examining the Duffy & Snowdon critique we have found some basic errors and misunderstandings. We believe that these would likely have been identified and remedied prior to publication if they had submitted their work for independent peer-review. We set out six of these problems here and suggest that their existence raises questions regarding the extent to which the authors have understood the reports and publications they are endeavouring to criticise.

- Duffy & Snowdon are wrong when they assert and discuss over numerous pages that "at the heart of SAPM's projections is the 'single distribution' model, a theory first advanced by...Lederman in 1956" (p8). Our research is not based on this theory, which assumes a direct relationship between average alcohol consumption in a population and rates of alcohol-related

harm; rather we specifically emphasise the importance of policy appraisals looking at changes in behaviour and outcomes within subgroups of the population. This aspect of Duffy & Snowdon's critique is bemusing both because they dedicate so much space to an incorrect assertion and because we have devoted a whole journal article to this very issue of subgroup-specific impact [9].

- Duffy & Snowdon are also wrong in stating that the model assumes “there are no health benefits to be derived from moderate alcohol consumption” and that “one searches the Sheffield research in vain” for such evidence (p12). On the contrary, we include evidence of protective effects for ischaemic heart disease, ischaemic stroke, type 2 diabetes, and gallstones and document all of this fully in our research reports and our paper in *The Lancet* [10].
- Since we first published our findings in 2008, we have observed that a common tactic used by those wishing to misinterpret the alcohol policy evidence base is to begin a sentence with one subject before subtly shifting to another subject. We observe a classic case when Duffy & Snowdon say “it is heavy drinkers who cause and suffer the most alcohol-related harm, but can we really assume that someone with an alcohol dependency is more likely to be deterred by price rises than a more casual consumer?” (p11). Note here the conflation of heavy drinkers (i.e. those drinking above NHS guidelines) with dependent drinkers (those who are addicted to alcohol). Whilst our model certainly seeks to consider effects for the 20-30% of the adult population who drink above recommended limits and are thus at significantly elevated risk of suffering or causing harmful outcomes, we do not explicitly consider the policy's potential impact on alcohol dependency in our modelling. There is strong evidence that alcohol dependency is most effectively handled by specialist treatment services and this is already well-understood by policymakers and practitioners.
- When discussing price elasticities, Duffy & Snowdon also state “minimum pricing will raise the cost of every type of drink”, and link this assertion to claims in a blog (rather than a peer-reviewed article) that “heavier drinkers are *least* responsive to aggregate changes in price” of this kind because they simply substitute their previous purchase for a cheaper option (p11). Again, Duffy & Snowdon appear to misunderstand both the policy and the modelling they are attempting to critique. The proposed minimum pricing policy would not enforce increases in the cost of every type of drink; it only directly affects alcohol sold below a given price per unit. Further, by preventing the sale of ‘cheap’ alcohol, minimum pricing is specifically acting to minimise opportunities for substitution behaviour by heavy drinkers. Duffy & Snowdon further ignore the fact that our modelling actually uses differential price elasticities for moderate and heavy drinkers, undertakes further sensitivity analyses on these estimates (including analyses where heavier drinkers are assumed to be less responsive to price changes) and explicitly models how

the impact on different groups depends on how much alcohol they buy below the MUP threshold.

- Duffy & Snowdon assert that there is “oddly enough not enough information for a third party to rerun bits of the model” (p18). We are unsure if our critics have actually tried to reproduce any of our work, but we reject the view that this is not possible. Reproducibility of findings is a key criterion for publication in high quality peer-reviewed journals and we have published full details of the SAPM structure and parameters to facilitate this. The reports and publications describing the methods used run into a combined total of over 500 pages.
- Duffy and Snowdon contend that the effects of MUP on “people on low incomes” (p12) are important and we would agree that it would be useful to undertake further research to examine this. Of course this would only be possible by undertaking a model-based appraisal. Duffy & Snowdon do not mention that some research has already been done on this issue by Ludbrook [11, 12] and the Institute for Fiscal Studies [13], both of whom suggest that any regressive effects on expenditure are likely to be small. Further, we are clear that a considered analysis of policy impact across the income distribution should not simply focus on consumption and expenditure; it should also account for evidence that the risk of harm for a given level of alcohol consumption is actually substantially higher for lower socioeconomic groups relative to higher socioeconomic counterparts [14]. Thus it is conceivable that low income groups may in fact experience disproportionate benefits from the proposed minimum price policy. To examine the balance all of these effects and incorporate such evidence would of course require undertaking further model-based appraisal.

## Conclusions

In summary, we welcome the opportunity to respond to the critique of Duffy & Snowdon and to clarify the following:

- There is a strong and substantial international evidence base regarding the effects of changes in alcohol prices on alcohol consumption and related crime and health harms which Duffy & Snowdon do not even acknowledge.
- The UK government has substantial guidance on appraisal of the potential impact of policies yet to be implemented. The Sheffield Alcohol Policy Model follows good practice in making the fullest possible use of evidence to inform decision-making.
- Duffy & Snowdon’s claim to be able to empirically disprove Sheffield modelling estimates is flawed as it does not account for subgroup behaviour changes, time lags or other causes of mortality.

- Duffy and Snowdon make assertions about the modelling which are factually incorrect and here we have highlighted some particularly surprising errors and misunderstandings.

To conclude, we restate that our purpose in undertaking the modelling work has been to generate for policy makers the best understanding and estimates of the potential effects of MUP given the scientific evidence available. The judgment as to whether the wider evidence base and the modelling is reliable enough to enable policy makers to take the next step and implement MUP falls within a complex public process of debate involving academic peer review, political judgment and scrutiny, and commentary and consultation with the public and stakeholders holding a range of worldviews and vested interests. Duffy & Snowdon have a right to contribute to that debate from their particular standpoint and interest, and as academic researchers we are pleased to respond to the points they have made.

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