



Changing the way we drink



Turning Point
Alcohol & Drug Centre

A ONE-SIDED AND OFTEN MISTAKEN REVIEW:

The authors of *The Range and Magnitude of Alcohol's Harm to Others* respond to an Access Economics critique commissioned by the National Alcohol Beverage Industries Council

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A one-sided and often mistaken review:

The authors of *The Range and Magnitude of Alcohol's Harm to Others* respond to an Access Economics critique commissioned by the National Alcohol Beverage Industries Council

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In August, 2010, the Alcohol Education & Rehabilitation Foundation (AER Foundation) released a report from a study which it had funded, *The Range and Magnitude of Alcohol's Harm to Others* (Laslett et al., 2010). On 8 October, Access Economics issued a critique by Lynne Pezzullo and Henry Cutler (2010). The Access Economics report also put forward its own “economic framework for policy development” and included a summary of an earlier critique of cost-of-alcohol studies by Collins and Lapsley (2008) that has not been made publicly available.

This paper responds in particular to the parts of the Access Economics report which critique our report, *The Range and Magnitude of Alcohol's Harm to Others*, and its underlying study materials and methods. We show, contrary to the assertions in the Access Economics critique, that the conduct and methods of the study are fully up to contemporary standards of research, and that if our study errs, it is probably in the direction of underestimating the harms from drinking to other Australians.

Some general comments

As indicated in its Executive Summary, the primary thrust of the Access Economics report is to cast doubt on any cost estimates from alcohol derived from our report's data and any estimates which combined such estimates with those of Collins and Lapsley. Cost estimates for harms to others, mostly for harm to particular others (e.g. spouses, family members), were included in our report, but they were not its main thrust.

Our aim was to conceptualise and measure the extent and seriousness of harms caused by drinkers to those around them. We used data from a wide variety of sources that included a population survey conducted for the study. Pezzullo and Cutler's single-minded focus on the implications of the issues they raise for cost estimation thus leaves out of consideration substantial parts of the study's agenda.

The report did not attempt to combine the cost estimates which were developed in the study with Collins and Lapsley's. It explicitly acknowledged that double-counting issues would need to be dealt with before that could be done.

The Access Economics critique ignored the technical report on the commissioned survey which the study team prepared, and which was referenced in the report, and was then and is still available on the web (Wilkinson et al., 2009). Rather, the Access Economics critique relied only on the technical report prepared by the fieldwork agency staff (Challice and Van Dyke, 2009). The results, as we note below, were errors in the critique.

Critiques of the survey methodology and sample

Non-response bias

The fieldwork results of the *Alcohol's harm to others* survey are discussed on p. 15-20 of the technical report (Wilkinson et al., 2009). The response rate in the *Alcohol's harm to others* survey was in line with that achieved in other current general population CATI (computer-assisted telephone interview) studies.

The study's technical report gives a response rate of 35.2%. This response rate is higher than the figure of 23.6% calculated by Access Economics because, as

explained in the technical report, a proportion of the numbers which were not reached would have had no eligible respondent. It is accepted practice to remove an estimate of this proportion from the denominator (AAPOR, 2008). As the technical report documents, the cooperation rate (the proportion of completed interviews among households reached with an eligible respondent) for the *Alcohol's harm to others* survey (49.7%) was greater than for the CATI portion (39.3%) of the 2007 National Drug Strategy Household Survey (AIHW, 2008).

Population surveys in Australia and other developed countries have a general problem with falling response rates. The response to this has been an active tradition of conducting methodological studies to estimate how those not interviewed may have differed in their experiences and opinions from those who were interviewed. These include, for instance, studies of variations in amount and pattern of drinking. The findings have been mixed. For instance, Wild et al. (2001) found that heavier drinkers were less likely to return a follow-up questionnaire. On the other hand, Hill et al. (1997) found that those more difficult to interview reported lower rates of risky drinking. Viviënne et al. (2002) found higher rates of abstainers among non-respondents. An Australian study of university students found that hazardous drinkers were more likely to be late responders. Nonetheless, excluding late responders would have lowered rates of hazardous drinking only marginally (Kypri et al., 2004).

In the present study, Challice and Van Dyke's (2009) tabulation (Table 5.3c) by the difficulty of obtaining an interview found a somewhat higher rate of heavier drinking by the respondents who were *more* difficult to reach. However, the general finding in the methodological literature is that the extent of differential loss of heavier drinkers in survey samples does not have a great effect on estimated rates of drinking or alcohol-related harms in the population (e.g., Mäkelä and Huhtanen, 2010).

Differential sample losses for reports of others' heavy drinking may well diverge from the effects of sample losses on those for the respondent's own drinking. While this issue has been less studied, it should be noted that Challice and Van Dyke's Table 5.3c did not find differences by ease of interviewing in rates of reporting that there was a heavy drinker in the household.

The Access Economics report notes that the survey was introduced to potential respondents as a “national study on alcohol issues in the community”. They hypothesise that because of this those who had experienced no alcohol-related harms from others may be more likely to have refused to be interviewed. This hypothesis is an unlikely stretch: the phrasing of the statement about the survey’s purpose was intentionally broad and no mention was made of alcohol-related harm to anyone.

The evidence offered in support of this claim is weak: that 40% of those answering the phone and refusing to be interviewed gave an answer that was coded as “not interested”. This is a non-committal answer with a wide diversity of possible meanings (e.g. “I’m busy” or “I do not do market research telephone surveys”). It is more directly relevant to the hypothesis that only 2% gave an answer that was coded as “don’t like the subject matter”.

The Access Economics critique notes a discrepancy on age distribution between two tables in Challice and Van Dyke (2009). The age figures in Table 5.4b of that report are indeed misstated.

Drawing on a table from the Challice and Van Dyke (2009) report, the critique complains that the *Alcohol’s harm to others* sample is unrepresentative on various dimensions compared to other surveys. A general problem with the critique’s comparisons is that the data Challice and Van Dyke report is based on is unweighted data. The surveys from which comparators were derived used data weighted to be representative of the population. The Access Economics comparisons are therefore generally invalid.

On a weighted basis, the interviewed sample was closely comparable with Australian Bureau of Statistics (ABS) figures on gender and age: 48.7% male, vs. 49.3% in ABS; 13.3% vs. 13.2% for those aged 18-24; 32.1% vs. 32.0% for those aged 55+. The “employed” figure was a little lower (62.3% vs. 67.0%), but definitions of the category vary slightly between the two studies.

Pezzullo and Cutler show a figure derived from the 2007-2008 National Health Survey of 61.4% for “drinking frequency once a week or more” in the “population aged 18+”. This misstates what the figure represents: it is in fact the proportion of those aged 15 and older who reported drinking within the last week. This is a very

different question from the question with which it is being compared, namely, “in the last 12 months, how often did you have an alcoholic drink of any kind?”

A study using more comparable survey items, the National Drug Strategy Household Survey (NDSHS), conducted by the Australian Institute for Health and Welfare, produces results similar to ours. In the 2007 NDSHS, 52.2% of those aged 18+ reported consuming alcohol at least weekly (AIHW 2009). The equivalent figure was 50.1% in our study (using weighted data).

The item from the National Health Survey that was used for comparison with the proportion not anxious or depressed is not comparable to the one in our survey. The National Health Survey estimated the prevalence of diagnosable anxiety and depression-related disorders, while the EQ-5D used in our study measures less severe levels of mental distress. We could not locate any other national surveys that have used the EQ-5D for comparison, but a large-scale study in regional New South Wales produced similar findings to our study using the same items. We found 21.8% of respondents reported moderate anxiety/depression and 1.4% reported extreme anxiety/depression, which is very similar to the findings from Petrie et al. (2008), 19.9% and 1.8% respectively.

The exclusion of those too old and frail to be interviewed might indeed have resulted in some overestimation of alcohol’s harm to others, since younger adults were more likely to report such harms. However, this is unlikely to have had any overall net effect, because those aged 65 and older were slightly overrepresented among those interviewed in the study (Wilkinson et al., 2009, Table 3).

The proportion excluded because of language difficulties was a relatively small fraction of the population (7% of those contacted in the *Alcohol’s harm to others* survey - Challice and Van Dyke, 2009:10). It is hard to predict whether those not fluent in English would be more or less adversely affected by others’ drinking than other Australians, but the effect on results for the total population would be small, in any case.

In the critique, the claim is made (p. 3, 6, i) that respondents who never consume alcohol will over-report problems from others’ drinking. But to the contrary, in our study, drinkers (30%) were *more* likely than abstainers (21%) to report having

a heavy-drinking family member or friend whose drinking had adversely affected them in the last year.

More generally, the Access Economics claim that defects in the representativeness of the survey will overestimate alcohol's harm to others is ill-founded. There is a strong age effect in reported harm from others' drinking, with younger adults reporting more adverse effects. There are also some gender effects with more family problems reported by women and more friendship and work problems by men. However, since the sample was weighted by age and gender in the analyses of Laslett et al. (2010), any age and gender effects are largely neutralised. And contrary to the critique's claims, heavier drinkers are more likely than abstainers to report adverse effects of others' drinking. Any underrepresentation of heavier drinkers in the sample would most likely mean that we underestimate rather than overestimate alcohol's harm to others.

Proxy interviewing

The Access Economics critique mistakenly uses the term "proxy interviewing" to describe the survey's questions on harm from others' drinking. It claims (without any supporting reference) that this "is not looked upon favourably in surveys because it has a high potential for response errors". The critique is misdirected with regard to the Laslett et al. study.

In the first place, harm from others' drinking is not simply a matter of a drinker's behaviour. It also intrinsically involves the impact of the behaviour on the other – the person who is the respondent in this survey. A response from the other concerning how much s/he was adversely affected by the others' behaviour is inherently more valid than a response to this question from the drinker.

In the second place, the critique assumes that estimates of drinking behaviours will be higher by others than by the drinker him/herself. This is not always true. For instance, comparisons of amounts of drinking reported by the drinker and by a drinker's spouse generally find that more drinking is reported by the drinker (Room, 1989).

The critique cites an older U.S. study by Bard and Zacker (1974) of police reports on marital violence calls and asserts that in this study "police found only half of

people alleged to be assault victims and who were claimed to have been drinking, in fact had been drinking”. We note, first, that Bard and Zacker present their study as being about “police perceptions”, recognising that their data, drawn from the police, are themselves proxy information. Second, Pezzullo and Cutler appear to be referring to the finding that the police reported that the person complained against had been drinking in 30% of the cases, but that the police perceived the drinking “to be primary in the origin of the disputes” in only 14% of the cases. Pezzullo and Cutler misrepresent a finding which in fact compares two proxy responses.

A related issue they raise is the possibility that those reporting that they had been “kept awake at night or disturbed because of someone’s drinking” might be mistaken about whether the person has been drinking. This is indeed a possibility but the common experience is that noisy, sober persons are rarely on the streets late at night. But in any case, in our report, this item is treated as one of the less serious harms arising from strangers’ drinking. Rates were reported excluding four less-serious items,¹ as well as with them included.

These minor items were not used in any cost estimations. Behaviours by strangers which did figure in the cost estimations were those that involved out-of-pocket expenses from damage to property, or belongings arising from another person’s drinking.

In short, “proxy interviewing” is not in our view a valid description of our study methods, and in any case the critique provides no valid evidence that the study’s approach overestimates the range and magnitude of the harm that alcohol use causes to others.

Order bias

The critique claims that “questions that evaluate the impact of alcohol on others start to list the options with the most serious outcomes first”. This is by no means always the case in the questionnaire.

¹ The four less serious items are: In the last 12 months, how many times have you: been kept awake at night or disturbed because of someone’s drinking; been annoyed by people vomiting, urinating or littering when they have been drinking; gone out of your way to avoid drunk people or places where drinkers are known to hang out; experienced trouble or noise because of drinkers at a licensed venue.

For instance, in the items concerning negative events because of the drinking of a family member or friend, a serious item like “were you forced or pressured into sex or something sexual because of their drinking?” (Q. E1g) was more than halfway down the list. In the case of the behaviour of strangers, the equivalent item (Q. I3l) was 12th on a list of 14 items. And the most serious items on adverse effects on children for whom the respondent was responsible were at the end of that list (Q. G3).

The critique’s proposal to randomise items or response choices reflects a social psychological literature primarily concerned with measuring attitudes. These were not the focus of our study. When respondents are asked about concrete behaviours, there is often a logical ordering of response choices which randomisation would disturb, and so randomisation is rarely used.

The only specific items mentioned in the critique’s discussion of order bias that figure in calculations of costs in the *Alcohol’s harm to others* study are those in Section J of the questionnaire (service use). It is not clear how order bias could raise the costs estimated from this section. Yes, “one or more” is recorded above “none” in the responses to each introductory question, but the question is not phrased in any particular answer order – and in the follow-up questions “time given in hours” is recorded before “time given in days”.

Recall bias

The critique correctly notes that “there is generally an inverse relationship between the length of time over which respondents are asked to recall and the accuracy of their response”. However, Pezzullo and Cutler neglect to add anything about the predominant effect of recall biases, which is to omit to report events that occurred, resulting in underestimations of the frequency of events or behaviours.

Studies into people’s report of amount and occasions of drinking, for instance, find that the amount reported drops with the time-distance from the time of reporting (e.g. Mäkelä, 1971; Stockwell et al., 2004; Gmel & Daepfen, 2007). The effect of this phenomenon is stronger than the effect of the telescoping phenomenon that the critique mentions. The claim that “the survey may

overstate the number and extent of harms due to recall bias” is the opposite of what the relevant survey research literature finds.

Other problems with the survey

The critique includes a further list of relatively minor points about the survey. Some arise from misunderstandings of Challice and Van Dyke (2009) and of routine survey fieldwork procedures. Others reflect debatable opinions about what is good survey practice, e.g. the critique’s preference for Likert scales over more abbreviated but substantive choices.

The critique complains that the response categories concerning the duration of time spent should have included a category of minutes. But these response categories were not read to respondents, and answers in terms of shorter times were simply recorded in decimals of an hour.

The critique is also critical of the survey question on whether the respondent would consider any member of his/her household “to be a fairly heavy drinker, or someone who drinks a lot sometimes”. This question was used to open up the subject of others’ drinking, to be followed by many other more concrete questions with more direct implications for policy. The critique’s assertion that there should be a difference in policy responses to “fairly heavy drinkers” and responses to people “who drink a lot sometimes”, is open to debate, but in any case it is irrelevant to the methods of the survey or any conclusions drawn in Laslett et al. (2010).

For instance, answers to this opening question are not used in the cost estimates derived from the survey and neither are the items cited (from p. 149 of Laslett et al., 2010) in terms of worries about whether outliers should be excluded. The only two items used in the cost estimates from the tables on p. 149 were the last two, and for these the maximum answer given was 12 times in the last year, i.e. once a month, which is clearly within the range of plausible answers.

Lastly, Pezzullo and Cutler worry that the interviewers might have been recruited because “their views aligned with those from the anti-alcohol lobby”. This is not the case. The interviewers were employees of the survey research company which was subcontracted to conduct the survey.

Summary concerning the criticisms of the Alcohol's harm to others survey

The survey on which the Access Economics critique focuses is just one part of the empirical material presented in Laslett et al. (2010) on alcohol's harm to others in Australia. The survey was commissioned as a first effort to fill numerous gaps in the available material for quantitative estimates of alcohol's harm to others. It was a relatively large general population sample, so that the sampling variability of estimates was relatively small. Its fieldwork was carried out in accordance with good contemporary standards by an experienced survey fieldwork organisation.

The critical comments about the survey in the critique do not challenge its fundamental characteristics. The results from the survey represent a current best estimate of the rates of events and behaviour in Australia about which it asked. In many places in Laslett et al. (2010), confidence intervals are given for the estimates which are made, which give the reader the opportunity to see how great the probability envelope of answers is around the best estimate given.

The primary object of the critique, as evidenced in its Executive Summary, is to raise doubts about estimates of the cost of alcohol in Australian society, whether based on Laslett et al. (2010), on Collins and Lapsley, or some combination of the two studies. Concerning this, it is notable that few of the criticisms of the *Alcohol's harm to others* survey concern items in the survey which contributed to the costings in Laslett et al. (2010).

Critiques of the costing methodology

Attributable fractions used in Laslett et al. (2010)

The Access Economics critique challenges the validity of the whole international public health research literature on attributable fractions, as implemented for instance in the World Health Organization's estimates of the Global Burden of Disease (WHO, 2002). The basic issue is how to deal with conditional causation, which is commonly involved in many of the calamities of life.

For instance, if a passenger who was not wearing a seat-belt dies from a road crash when a drink-driver skids on a curve on a wet night, a number of factors may be judged to be involved in the death. These include not only the fact that

the driver had been drinking, but also the wet weather, the passenger's lack of a fastened seat-belt and perhaps also the condition of the road.

How is the causal role of alcohol to be measured in this circumstance? Pezzullo and Cutler's answer is to use "regression analysis to separate out the impact of different causal factors". This would lead to a result that each of the factors named would be assigned a fraction of the causation for such events. But such an algorithm ignores the conditional nature of the causation. In the example we have given, if the passenger had been wearing a seat-belt, the death might not have happened. The same may be true if it had not been wet that night -- and also if the driver had not been drinking.

In this circumstance, the public health research answer on causation has been in terms of the answer to the question "if this factor was removed, would the adverse event or condition have happened?" On a societal level, the comparison is with a "counterfactual" situation in which that risk factor was removed (Begg et al., 2007:27-29). This is the basis used for the attributable fractions drawn on and calculated in Laslett et al. (2010).

It is curious that Access Economics objects to this approach in our study when it has itself used the same approach in calculating attributable fractions. For instance it used the calculations by Begg et al. (2007) in its study of the costs of smoking in mental illness (Access Economics, 2007, p. 43). It was appropriate for Access Economics to have done so in investigating costs in a society, as it was also in our study, since it measures the extent of improvement which could be expected if the risk factor was removed.

Pezzullo and Cutler (2010) erred in claiming that Laslett et al. (2010) count "any involvement of alcohol in a vehicle 'operator' as causing the crash". Appendix A gives as two alternatives the estimated aetiological fractions on the basis of drivers with two levels of blood alcohol, both of which are above the legal limit in Australia: a 0.05% level and a 0.10% level (Tables A3 and 4). In the further analysis in the report (Chapter 3 and Appendix D), the more conservative figure of 0.10% was used. This is common, current practice in the alcohol epidemiology literature to arrive at a balance on issues of causality: on the one hand, some crashes with drivers above 0.10% would have occurred even if alcohol was not present, and on

the other hand some crashes with drivers below 0.10% would not have occurred without alcohol being present.

Pezzullo and Cutler note (p. 12) that the car crash dataset used in our analysis excludes those with no measured blood-alcohol level (BAC), and assert without citing any source that this will result in an overestimate of the alcohol-attributable fraction, since “police do not routinely take BAC readings if the cause of the crash is not related to alcohol”. This statement is not accurate, as the data underlying analysis of fatal BACs are drawn from coronial records. In 1999, the then Federal Office of Road Safety (FORS, 1999) noted that unknown BACs among fatalities in these data largely occur as a result of administrative or technical difficulties, and not subjective judgements by coroners, who are required to test for alcohol involvement. Some states also require mandatory testing by police of all seriously injured persons involved in road crashes.

Concerning the point that all injured passengers were included as ‘victims’, and would have been counted in Collins and Lapsley, this point is not directed at Laslett et al. (2010), since Laslett et al. point out the issues of double-counting which have to be dealt with in case of an effort to combine figures from the two studies (p. 176-177), and do not attempt to do so.

Pezzullo and Cutler note that attributable fractions presented in Tables A.4a and A.4b are lower for 0.10 mg/ml BAC than for 0.05 mg/ml. However, they draw a mistaken conclusion from this fact. The finding is as expected for the “estimated proportion of all road crash injuries attributable to alcohol impaired operators”: if the BAC threshold was set higher, those above the threshold would account for a smaller proportion of the crashes and consequent injuries. For the last row in the tables, the “estimated proportion of all road crash injuries attributable to victims of alcohol impaired operators”, the results are less predictable, since raising the threshold to 0.10 results in some drivers with BACs of 0.05 – 0.09 becoming defined as victims.

Pezzullo and Cutler’s point about attributable fractions for child abuse is covered in our discussion above of attributable fractions generally. As clearly stated in Laslett et al. (2010), both the Victorian and NSW data include cases where other drugs along with alcohol may be risk factors. In NSW, the proportion of child abuse reports (the most inclusive level of system data) that involved alcohol alone

was 10.9%, as noted in Pezzullo and Cutler. However, an additional 20.4% of the cases involved both alcohol and drugs, as specified. The Victorian and NSW data are thus in rough agreement. The figure of 31.4% of system costs attributable to alcohol was based the more conservative of the two sources.

From the point of view of costing, the NSW data is more appropriate, since the data on alcohol involvement is collected earlier in the child protection process than in Victoria (all reported cases, whether or not later substantiated). A substantial proportion of child protection costs are spent investigating the majority of claims that do not result in substantiations. However, Laslett et al. (2010, p. 102) also show that as clients progress further through the system, alcohol involvement increases.

Future costing studies should investigate whether there are commensurate increases in costs in more serious cases. The proportion of child abuse cases attributable to alcohol nationally is unlikely to be below the 31.4% used in Laslett et al. (2010), as the data from the two largest states have already been examined. Indeed rates of alcohol-related child abuse cases in other jurisdictions (e.g. the Northern Territory) are likely to be even higher (Wild and Anderson, 2007).

Methodologies used to estimate costs

Impact of others' drinking on health and wellbeing (intangible costs)

The re-analysis of the HILDA study discussed on pages 13 and 14 of the Access Economics critique found limited impacts on health and satisfaction due to spousal drinking. This was counter to our expectation and to other studies in the area (which consistently point to heavy drinkers' detrimental effects on their spouses). Despite these results not matching our expectations, they were included in full and two reasons were offered as potential explanations of these unusual findings.

Firstly, the use of panel data from the long-running HILDA study may have left us with a slightly biased sample. It has been repeatedly shown that respondents who do not participate in follow up interviews in panel studies differ in substantive ways from those who are successfully reinterviewed (Lillard and Panis, 1998). For example, a study of substance use amongst young people found that respondents lost to follow-up were heavier drinkers than those who were successfully retained

in the study (Tebes, Snow et al., 1992). Thus, the sample included in the fifth wave of a longitudinal study is likely to differ substantially from a sample selected at random. Early work on the HILDA study showed, for example, that young, less educated, Indigenous respondents who were single or in de facto relationships were more likely to be lost to follow-up than their counterparts (Watson and Wooden 2004).

Thus, it is clear that the HILDA sample after four follow-ups (with a total attrition of more than 25% from Wave 1) will be a more stable and less harmed sample than a random sample of the population. Similarly, the sample contained more lighter drinkers, with 7.6% drinking at long-term risky or high-risk levels, compared with more than 11% in the National Drug Strategy Household Survey in 2007 (Australian Institute of Health and Welfare 2009).

Secondly, the use of the NHMRC cut-offs for long-term risky drinking was considered conservative, since a majority of those above the cut-offs would be drinking not much more than the cut-off levels. While drinking at the levels specified by these guidelines increases your risk of health problems, it is clear that the problematic drinkers among relatives or friends identified in the *Alcohol's harm to others* survey were drinking at levels far in excess of the NHMRC cut-offs. On average, the person whose drinking had affected the respondent the most was reported to be drinking 13 standard drinks when drinking heavily, and drinking five or more standard drinks on four days per week.

The Access Economics report criticises a number of Laslett et al.'s assumptions and caveats in estimating intangible costs (2010, p. 15). The main concern seems to be that these caveats were not also provided in the report's executive summary. But clearly the summary document cannot cover everything that is in the main report – the assumptions behind our costing methods were laid out clearly in the full report, and are thus available for review.

The critique also notes discrepancies between the figures presented for intangible costs in Chapter 3 and those provided in the final cost tables. To clarify, intangible costs were estimated in a number of different ways. In Chapter 3, costs were estimated using the number of drinkers identified by the respondent, resulting in a total intangible cost estimate of \$8.5bn. To focus more strictly on the costs related to problematic drinkers, more refined intangible cost estimates were

provided in Chapters 9 and 11, based on the level of harm reported by respondents from the known drinker (\$6.4bn) and from strangers (\$5.3bn). Because of the methods used to calculate them, estimates of intangible cost contain substantial overlaps. Thus, as a conservative approach, only the larger of the two was included as the intangible cost figure in the summation of costs given on p. 178.

Costing adverse effects from drinkers well known to the respondent

Pezzullo and Cutler (2010, p. 16) note that Laslett et al. (2010) list \$845.8bn in out-of-pocket costs incurred by respondents from known drinkers (this is indeed the figure listed in the summary costs table on p. 173, discussed on p. 172). However, they claim that this includes “money spent on drinking, arguing it leaves less money for household expenses”. This is not true. We reported how much respondents said was “less money available for household expenses” because of a nominated household member’s heavy drinking (\$437.7m as a national projection) on p. 135 and discussed it on p. 134 because it was a relevant finding of general interest. But we did not include this amount in any cumulation or summary of costs, for the reasons Pezzullo and Cutler raise.

Pezzullo and Cutler are also critical of including estimates of time spent as a result of someone else’s drinking. They argue that there are “uncertainties” about the “reliability” of this information, since caring for someone because of their drinking may overlap with “normal activities”. This is not a credible objection. In all the questions related to this issue, respondents were asked specifically “how much time did this take out of your normal routine?” Access Economics has included similar estimates of time spent caring for family members in its own cost of illness estimates (Access Economics, 2007, p. 52-55; Access Economics, 2006, p. 78-80).

Pezzullo and Cutler also complain at this point (p. 16) that intangible costs have been double counted. As noted above, intangible costs were estimated by several methods in Laslett et al. (2010); but they were not double counted. This was discussed specifically on p. 177 of the report, where it was noted that the figures in the intangible cost column in Table 13.2 “should not be added up”. Pezzullo and Cutler are incorrect in stating (summary on p. 17) that the \$6.4bn figure cited on p. 177 of our book includes double counting.

Costing alcohol-related harm from strangers

In their summary concerning this section (p. 18), Pezzullo and Cutler speak of “severe error” concerning one issue and regard a second even “more seriously”. These conclusions are unwarranted.

The first issue raised is the estimate of \$1.6m for out-of-pocket costs arising from damage caused by the drinking of persons not well known to the respondent. Pezzullo and Cutler assert that respondents may answer the underlying questions inaccurately. However, the questions asked were quite specific about the drinking attribution in their wording. Respondents were answering whether “your house, car or property [was] damaged because of their drinking” and whether a “person affected by alcohol damaged your clothes or other belongings”. The claim that such concrete questions may have been systematically misinterpreted is speculation.

Pezzullo and Cutler also return at this point to the issue of intangible costs, claiming that “it seems that Laslett et al. (2010) have triple counted the same cost”. As noted above, this is untrue. In the report, in fact, readers were specifically warned against doing so on p. 177.

Problems with estimating productivity loss

Pezzullo and Cutler mistakenly claim (p. 18 and 19) that Laslett et al. calculated all time lost by part-time workers and those not employed as if they had been working fulltime. This is not true. Laslett et al. actually used an average weekly earnings computed across both full-time and part-time workers. Amongst those who are not employed, there indeed will be a proportion of people who will be self-employed or who do sell part of their output, as in the case of some housewives. Information is not available on the marginal production of this group. It would be incorrect to assume that it is zero, and in fact in particular cases it could be much higher than the average wage rate used.

Likewise, a segment of those not employed do engage in quasi-market activity such as provision of care for the drinker, an activity available in the market and for which market prices exist. There is also plenty of precedent for estimating the value of a housewife’s time. Should a housewife cease to do household chores, her role may need to be filled with a paid worker or her output (e.g. food) may need to be purchased instead. Market prices also exist for these. Hence, even

though she may not have a direct value in the market, there is an indirect value to her output.

The average wage rate is used as a proxy for the marginal product of these groups, in the absence of required information. In the distribution of the sample, there will be indeed a small proportion of time for which opportunity cost cannot be represented as any form of direct or indirect lost output. However, since there is quite a substantial underestimation in the overall value of the rest of the time lost, the inclusion of this does not violate the condition that the total value of the productivity loss of the sample is plausibly representative.

Approaches in terms of “opportunity cost”, the usual terms for the method employed in Laslett et al., have commonly been used in Access Economics’ own cost estimations. Thus, a study of the *Economic Value of Informal Care* (Access Economics, 2005) used the average weekly earnings of *all* Australian workers to estimate the opportunity cost of time spent caring for others.

To estimate the true productivity loss of time spent caring for others, information is required on the marginal production of each person. Amongst those employed in the workforce, this will vary by gender, between high-skilled, low-skilled and seasonal or part-time workers, and between private and public sector workers. As in Access Economics estimates in this area, the Laslett et al. estimates are made at a broader population level. In the absence of detailed data, the aim is to provide the best estimate possible with the available data.

Pezzullo and Cutler claim that “it is incorrect to include the cost associated with extra hours worked due to other people’s drinking” (p. 18). In their view, such extra work would not represent a cost to the economy, presumably because the drinker would not be paid for the time lost because of the drinking. In many work situations, this is unlikely to be true. In any case, the “inclusion” they refer to is presumably in cumulations of costs, which as we have noted was not a main focus of Laslett et al. Our report in fact warned (p. 174) of the potential for double-counting in any combination with workplace costs of drinking counted in Collins and Lapsley.

Other problems with estimating costs

Pezzullo and Cutler complain that insufficient information is given in Laslett et al. to validate calculations. The specific example they cite is lack of a data source reference in Table D1 in Appendix D. However, Appendix D is clearly related to Chapter 3 of the book, and the sources for the mortality data and morbidity data used in this Appendix are clearly identified in that Chapter (p. 20).

The critique also complains that “well-established practices are not used” in the book (p. 20). It gives as an example that the value assigned to a year of life in calculating intangible costs “is different by a factor of three from the value ... recommended by the Department of Finance and Regulation”. This is true. What Pezzullo and Cutler do not disclose is that the value used in Laslett et al. is *one-third* of the DFR value, i.e. that Laslett et al. deliberately used a conservative approach in this valuation. If we had used the DFR value the estimated intangible costs would have trebled. Disclosing the direction of the discrepancy would have undercut Pezzullo and Cutler’s claims that any estimates based on Laslett et al. are substantially overestimating costs.

Pezzullo and Cutler also claim that Laslett et al. “do not transparently specify which year costs relate to”. But in the 2½ pages of the report which are devoted to costing analysis methods and sources” (p. 23-25), the references given are consistently to data for 2008. This is indeed the year for which costs were estimated.

Problems with report presentation

In a final section on Laslett et al. (p. 20-21), Pezzullo and Cutler present a variety of complaints about the presentation style and details in the report. Some of these complaints are rather minor. For instance, it is complained that no reference is given for an example on page 4 in a discussion about the diverse categories of strangers who may be affected by others’ drinking, including “owners of ‘lemon’ automobiles supposed to be more commonly built when the weekend drinkers were missing from the assembly line”. Those interested in pursuing this folk belief, identified as such with the word “supposed”, can find various discussions of it on the internet: e.g. http://msgboard.snopes.com/cgi-bin/ultimatebb.cgi?ubb=get_topic;f=20;t=000639;p=0 or <http://www.city-data.com/forum/automotive/567293-built-monday-friday-its-junk-tuesday.html>.

We fail to see how giving a reference about a folk belief, mentioned in passing in a conceptual discussion, would have improved the report.

We do not claim perfection or omniscience for the report, and apologise to any reader who finds it “unnecessarily verbose and tediously repetitive”, in Pezzullo and Cutler’s words. But we vigorously contest one claim by Pezzullo and Cutler that “much of what is written seems to be emotive assertion from a single (anti-alcohol) perspective”.

Writing from a single perspective in fact characterises their critique, which is written from a biased perspective concerned only to find any arguments which minimise the extent of problems associated with drinking. It is telling, for instance, that Pezzullo and Cutler criticise the report for using an estimate of the value of a year of life that differs “by a factor of three” from an official recommendation without disclosing that the effect of using the official recommendation would have been to triple the estimated costs attributed to alcohol.

In the section on “problems with the report presentation”, Pezzullo and Cutler (p. 20) object to our use of the phrase “alcohol harms” rather than “alcohol abuse harms”. There are good reasons not to use the phrase Pezzullo and Cutler suggest. “Alcohol abuse” is a diagnostic category in a major diagnostic classification (DSM-IV), and many of the harms arising from drinking occur without the drinker qualifying for this diagnosis. Pezzullo and Cutler explain their point with a parenthetical statement: “there are only *benefits* from healthy alcohol use”. This only makes sense as an ideological statement; anyone familiar with the epidemiological literature on consequences of drinking would be aware that there is no apparent lower threshold of drinking for risk of many adverse consequences, e.g. cancer (Rehm et al., 2010). Thus, however “healthy” a particular pattern of drinking may be considered to be, it is not without any risk of harm, however small it may be.

A review which is one-sided, often mistaken, and tendentious

Pezzullo and Cutler’s critique reads like a document written to serve the interests of the industry which commissioned it. Their brief seems to have been: find any arguments that you can, whether relevant or not, that can be used to argue that

the report by Laslett et al. has used incorrect methods that bias its results to “grossly overestimate” the costs to others of drinking in Australia.

The main text of the critique is a laundry list of complaints about the methodology and analyses in Laslett et al., presented only in terms of how they could lead to possible overestimates. No reference is made to the potential underestimates. In our response above, we have addressed Pezzullo and Cutler’s criticisms, and shown that on many of their claims, they are in fact mistaken. On others, they take a position which is not in accordance with accepted practice in epidemiology and in costing studies.

While the discussion in the main part of the critique’s text is one-sided, it is not as unbalanced as the statements to be found in the grey boxes of conclusions which are inset in the text. Perhaps to be sure a superficial reader will not miss the message of the critique, it is repeated over and over: “estimates of the amount and cost of alcohol harm to others ... in Laslett et al. are likely to be overestimated” (p. 6); “estimates of the cost of alcohol harm to others are likely to be overestimated” (p. 7); “this approach will overstate the impact and costs associated with alcohol harms” (p. 11); estimates “are unreliable” (p. 12); the estimate “is not valid” (p. 16); the estimated cost “is not valid” (p. 18).

The rhetorical tone is raised a further notch in the Executive Summary (p. i), which speaks particularly to the policy concerns of those funding the critique: “cost estimates presented in Laslett et al. (2010) appear to be grossly overestimated.... Consequently, the total cost and individual cost components should not be used to inform policy decisions regarding the consumption of alcohol”.

There are undoubtedly ways in which our report can be improved. There is also plenty to be debated and much work to be done in establishing the range and magnitude of alcohol’s harm to others, as well as in measuring and analysing other aspects of the Australian experience with alcohol. However, Pezzullo and Cutler’s critique fails to offer a credible or useful guide to producing better estimates.

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