Annex 7

W. KIP VISCUSI

EXPERT REPORT ON STANDARDISED PACKAGING REGULATIONS IN THE UK: SUPPLEMENTAL REPLY

10 NOVEMBER 2015

1. I am the University Distinguished Professor of Law and Economics at Vanderbilt University. The qualifications and experiences described in my report for these proceedings, dated 26 October 2015, (my "Second Report") remain the same. I have now reviewed the "Expert Report on Standardised Packaging Regulations in the UK," Second Report, prepared by David Hammond, Ph.D. (the "Hammond Second Report") and the Reply Report of Frank J. Chaloupka IV (the "Chaloupka Second Report"), both of which comment on my Second Report.
2. Even though I understand that the timetable for providing expert reports in the proceedings is complete, after reviewing the comments on my Second Report by Professors Hammond and Chaloupka, I feel compelled as an expert to the Court to make a short supplemental reply so that the Court will not be misled. The Hammond Second Report, in particular, falsely represents the evidence that I have presented.
3. My Second Report made two principal points using the CITTS and the NTPPTS data. First, the Hammond analysis in his First Report of the post-implementation evidence from Australia presented no new empirical analysis but relied on a literature that did not consider many of the most pertinent questions in the surveys' underlying the studies, such as those pertaining to cigarette consumption and attitudes toward the warning message. Second, the Dunlop et al. article not only ignored these pertinent questions but also selectively reported the results based on only the most extreme responses in favour of particular points of view, i.e., the "strongly agree" or "strongly disagree responses." Consideration of the full range of responses, including as well the "agree" and "disagree" responses, leads to a different conclusion based on the CITTS data. Both of these

comments still stand and are unaffected by Hammond's various concerns discussed below.

4. Hammond complains that he received scant information regarding the statistical procedures that I undertook. However, I provided all the computer code that was used to create all of the calculations in my Second Report. The statistical program software in which I provided the code was Stata, which is the same software used for the NTPPTS data file that I received and which is commonly used by tobacco control researchers (including Cancer Council Victoria) and economists throughout the world. Anyone using Stata and with an understanding of that software would have been able to replicate my calculations.

5. Hammond claims that my analysis of the average cigarettes per day, which was based on the entire CITTS sample, errs by its inclusion of both smokers and recent quitters. He suggests instead that the more pertinent measure is the average number of cigarettes per day restricted to the smoking population. Hammond claims that the inclusion of recent quitters in my analysis undermines the credibility of my Second Report. This is incorrect and Hammond's presentation of this issue is misleading. I explain below the reasons for including recent quitters in my analysis. However, at the outset, I note that excluding recent quitters from my analysis strengthens my results contrary to Hammond's claim. Even in an analysis that excludes recent quitters and includes the age variable as Hammond suggests, in addition to mobile phone use and the time trend I used, the average number of cigarettes smoked per day after the advent of plain packaging rises by 1.327 cigarettes (significant at the 0.01 level).

6. Furthermore, examining the total number of cigarettes smoked across the entire survey population as I did in my Second Report is useful, as it also takes into account the effect of plain packaging on recent quitting and those who are smokers but smoke less often than once per week. Moreover, the measure of total cigarettes smoked across the entire survey population is more properly related to the overall public health risks of cigarettes. One way that anti-smoking policies could be effective is by moving people from being smokers to being quitters. Failure to take a comprehensive population perspective and focusing on average cigarettes smoked only for smokers will create distortions to the extent that the smokers who continue to smoke are the very heavy smokers. Indeed, Hammond raises this problem later in his Second Report at paragraph 6.17 where he opines that the smokers who are not discouraged by anti-smoking policies will be more likely to be the very heavy smokers, thus distorting the cigarette consumption measure. But he does not recognize that this comment undermines his earlier criticism of my interest in examining the sample-wide cigarette smoking figures. By including recent quitters and those who are smokers but smoke less often than weekly, my measure addresses the problem that Hammond raises in his paragraph 6.17
7. Hammond also claims that my failure to account for demographic factors distorts my results (and Professor Chaloupka makes a similar claim at paragraph 65 of his Second Report). Again this is incorrect. To make his point, in the only statistical evidence presented in his Second Report, Hammond presents regression analysis of cigarettes smoked per day and finds that the positive effect of PP on cigarettes per day is not statistically significant after controlling for mobile phone use and age. Although he presents his results as if they directly parallel mine, except for the inclusion of the age

variable, they do not and it is misleading to suggest that they do. What he did differs in a number of ways to my analysis, including by excluding recent quitters from his sample and excluding a time trend which I used in my analysis. In fact, if the age variable is added to my model, as Hammond suggests, the increased number of cigarettes per day after the advent of plain packs is 0.844 (significant at the 0.05 level). Accordingly, including the age variable for my earlier full sample analysis does not undercut my results.

8. As I indicate above, excluding recent quitters from my analysis strengthens my results contrary to Hammond's claim, even in an analysis that includes the age variable as Hammond suggests. Although Hammond's report does not indicate how he constructed his population sample for his analysis of cigarettes per day, other than stating in his Appendix that his analysis is of cigarettes per day among smokers, the only set of results for cigarettes smoked per day for which I was able to find that the increase in cigarettes per day after the advent of plain packaging was not statistically significant required that I also exclude from the sample both recent quitters as well as respondents who designate themselves as current smokers but who smoke less often than weekly. There is no sound rationale for eliminating these smokers who smoke less frequently than weekly, as they are in fact self-identified current smokers. Hammond also does not explain why his analysis fails to include a temporal trend variable so that the role of long run trends in smoking behaviour are not recognized even though the role of long run trends in smoking rates is a well-established empirical aspect of smoking behavior that is generally considered in empirical studies of smoking behavior.

9. In paragraph 6.8 of his Second Report Hammond alleges that he found a number of errors when he tried to replicate the "pre-implementation" point estimates in my figures. However the only issue that he points to is the calculation of the values for the appropriate sub-sample for cigarettes per day, daily smokers, and graphic health warnings encouraged the respondent to stop smoking. For the reasons indicated above in my discussion of the cigarettes per day (in para 6), my analysis is of the full population results which I consider is a meaningful perspective taking into account the net pertinence of these measures to both smokers and recent quitters. Accordingly, I haven't used any sub-samples in my analysis, so Hammond's point is irrelevant. I consider Hammond's allegation that there are other "basic errors" in my analysis without stating what those errors are to be quite unprofessional, and he has not demonstrated that my analysis is invalid.
10. In addition, the inclusion of the time trend variable and the mobile phone variable in my analysis captures many of the demographic changes that Hammond says should have been included. The time trend variable controls for long-run changes in cigarette consumption or sample composition, and the mobile phone variable controls for the change in the sampling procedure to contact respondents who are mobile phone users. My results pertain to the average population-wide effects controlling for these factors. Indeed, for matters such as pack appearance and related questions pertaining to pack appeal that were the focus of the published studies using the NTPPTS data, Chaloupka (para 61) observes that my results track the demographic-adjusted findings in the literature quite closely. In short, Hammond has never demonstrated that controlling for demographic factors beyond what I have done is influential, and the only example he

provides generates a difference not because of any demographic controls but because he focuses on a different measure of cigarette consumption.

11. Hammond also criticized my analysis for not doing sample weighting. Even without weights, my results are pertinent to the analysis of the sampled New South Wales population. The role of weighting would be to project the results to the population at large. However, there are only minor differences between the sampled population and the New South Wales population, as Dunlop el al. note that there is "a slight overrepresentation of females, older respondents and regional residents (living outside the capital city) in the CITTS sample compared to the NSW population." It is noteworthy, that Dunlop et al. report both the sample weighted and the unweighted results at various junctures in the article. Sample weighting is simply a non-issue in relation to these data, particularly in light of Hammond's failure to identify how the lack of weighting in my analysis affected the results.

12. Hammond also claims that my work on warnings for the U.S. Environmental Protection Agency was not peer reviewed. However, it actually received three separate peer reviews. As part of the government cooperative agreement on this topic, there was a formal peer review process. Second, many of the chapters in the book were published separately as peer reviewed articles. Third, the book was published by MIT Press, which undertook the peer review process for the book. MIT Press is one of the leading university presses in the world.

13. Based on my review of Hammond's report and my comments above, I conclude that his criticism of my report is unfounded and invalid. His comments do no lead me to change any of the conclusions in my Second Report.

DECLARATIONS

14. I understand that my duty is to the court and I confirm that I have complied and will continue to comply with that duty. I am also aware of and have complied with the requirements of Part 35 of the Civil Procedure Rules, Practice Direction 35, and the Guidance for the Instructions of Experts in Civil Claims 2014.

15. I confirm that (i) I have no conflict of interest of any kind; and (ii) I will advise the Court if between the date of this report and the trial, there is any change in circumstances which affects my answer to (i) above.

16. I confirm that I have made clear which facts and matters referred to in this report are within my own knowledge and which are not. Those that are within my own knowledge I confirm to be true. The opinions I have expressed represent my true and complete professional opinion on the matters to which they refer.

17. In undertaking my analysis, I received computer programming assistance from Jason Bell, a statistical consultant, who worked under my direction. Mr. Bell has worked on my research grants for over two decades and has been employed by Duke University or under contract to Vanderbilt University throughout that period. The opinions in this report are mine alone.

STATEMENT OF TRUTH

18. I confirm that I have made clear which facts and matters referred to in this report are within my own knowledge and which are not. Those that are within my own knowledge I confirm to be true. The opinions I have expressed represent my true and complete professional opinions on the matters to which they refer.

W. Kip Viscusi

10 November 2015

Appendix: Regression results, predicting number of cigarettes smoked per day, CITTS data

	Full Sample	Quitters Excluded
Plain packaging	0.8442**	1.3272***
	(0.3456)	(0.3581)
Mobile phone	-0.9146***	-1.4853***
	(0.2985)	(0.3114)
Age	0.1282***	0.1342***
	(0.0059)	(0.0062)
Year trend	-0.1488*	-0.1218
	(0.0874)	(0.0906)
Constant	6.4040***	8.1529***
	(0.3448)	(0.3615)

Significance levels: *0.10, **0.05, ***0.01.