

# Modelling of tobacco endgame interventions: a response

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We are writing in response to the letter published in the *Journal* by Associate Professor Laugesen and Professor Grace on the impact of tobacco tax, denicotinisation of cigarettes and e-cigarettes on smoking prevalence in New Zealand.<sup>1</sup> We have several points to make about this letter and the findings it presents about the estimated impact of these interventions on achieving the endgame target of a smoking prevalence below 5% by 2025.

Firstly, the letter lacks context. There is no mention of the previous modelling work on forecasting trends and on tobacco tax interventions in New Zealand,<sup>2-6</sup> nor any comparisons with the findings from this work. Previous US work on modelling the potential impact of denicotinisation is also not referenced.<sup>7</sup> The New Zealand tax modelling work carried out by BODE<sup>3</sup> in particular includes a sophisticated simulation study to estimate future smoking prevalence trends before and after tax increases, and through to health gains, costs and inequality impacts.<sup>2,4,6</sup> This work includes price elasticities at the heart of the modelling (as is the international norm), and allows for changing demographic structure, competing mortality risks, and so on.

Secondly, detail on the methods and model assumptions is limited. Providing information on methods is a fundamental principle for communication in scientific journals, and perhaps even more crucial when (complex) simulation modelling techniques are applied. We understand that the length of letters is inevitably constrained, making it difficult to include all necessary methodological information. This means that letters may not be the appropriate mechanism for communicating the findings of complex modelling studies. However, if

this approach is used, the authors could link the letter to an online report that explains the methods and assumptions fully (an action that would still be good to do, given the importance of the tobacco endgame for New Zealand).

Thirdly, based on the information that is provided about the methods, we have several concerns about the approach used and assumptions made. For example, it appears that Laugesen and Grace assume that all of the change in smoking prevalence from 2010 to 2014 was due to the annual tobacco tax increases. There are three main concerns about this assumption. First, other policies were in place or introduced over this time period (eg, point-of-sale display ban, smokefree prisons and extension of smokefree areas in many jurisdictions). These policies plus further denormalisation of smoking, resulting from the adoption of the Smokefree 2025 may account for some of the observed decline in prevalence from 2010-14. Second, the modelled effect of tax should be that over and above business-as-usual trends in smoking prevalence. A third problem is overreliance on two points in time (2010 and 2014). If the 2010 estimate was by chance high, and the 2014 by chance low, then impact of tax will be overestimated. In addition, Laugesen and Grace seem to have simply extrapolated a change in tobacco consumption into an equivalent reduction in smoking prevalence. However, reductions in tobacco consumption are made up of a combination of falls in prevalence and reduced consumption among continuing smokers. Hence, tobacco tax consumption elasticities (how much consumption reduces with price increases) are substantially greater than prevalence elasticities (ie, how much prevalence reduces due to a price increase).

Estimates for falls in tobacco prevalence by way of annual tax increases of 10% or larger in previous New Zealand modeling work were lower than those reported by Laugesen and Grace, presumably reflecting more realistic assumptions about impact. For example, Laugesen and Grace estimate an annual 15% increase in tax will result in a prevalence of 5.1% by 2025, whereas Cobiac et al estimate 8.2% prevalence for the same scenario.<sup>2</sup> Further work by the BODE<sup>3</sup> group is underway to examine how price elasticities may change with very high tobacco prices, and the estimates may subsequently be amended.

However, at this point in time, the published projections are the best (we believe) that can be done for New Zealand.

Similarly, the estimates of the population-level impact of e-cigarettes may be optimistic for several reasons. Firstly, their assumed annual quit rate through e-cigarettes in New Zealand (if they were made more widely available) seems to assume that all US and UK ex-smokers who are currently regular users of e-cigarettes gave up during the last year, gave up using e-cigarettes and, importantly, would not have given up otherwise. This assumes, unrealistically, that an equal number of new ex-smokers as the total current number of ex-smoker, e-cigarette users will quit using e-cigarettes every year subsequently. Furthermore, it assumes none of this group of ex-smokers only started using e-cigarettes subsequent to quitting (and hence did not quit through e-cigarette use). Finally, it assumes that all of these ex-smokers who quit using e-cigarettes only quit because of their use of e-cigarettes—ie, none would have quit anyway using other means (NRT, Quitline support etc) if e-cigarettes had not been available. An example of the importance of taking the latter into account is a recent estimate of the impact of e-cigarettes on prevalence (authored by a supporter of e-cigarettes in tobacco control) which used the marginal effect of quitting using e-cigarettes over and above quit rates from unassisted quit attempts.<sup>8</sup>

The estimates of the impact of e-cigarettes also ignore potential (but unproven) negative impacts of e-cigarettes on reducing smoking prevalence, such

as through gateway effects for youth or reduced quitting among dual-using smokers. The latter could occur if the ability to use e-cigarettes where smoking is not allowed, or the perception that cutting down smoking with the help of e-cigarettes is sufficient, resulted in reduced motivation to quit. These effects are plausible. For example, in the UK, whilst 41% of dual-users report using e-cigarettes to quit, another 43% report using e-cigarettes to cut down, but not stop completely, and 25% report using them “because I want to continue smoking tobacco, and need something to deal with situations where I cannot smoke (eg, workplaces, bars or restaurants).”<sup>9</sup>

The uncertainty about the net population impact of e-cigarettes on smoking prevalence is illustrated by the finding in some, but not all, longitudinal population-based studies that e-cigarette users do not have higher quit rates than non-users,<sup>10</sup> and the lack of evidence of a substantial increase in the rate of decline in smoking prevalence or change in quit rates among smokers in jurisdictions where e-cigarette use is very common like the UK.<sup>11</sup>

There are also uncertainties about the population-level impact of denicotinisation—such as how interventions studied in experimental settings (eg, controlled trials using denicotinised cigarettes) may impact on smoking prevalence in real life.

Given this degree of uncertainty, the lack of any sensitivity analyses, discussion of alternative scenarios or at least some information on the degree of uncertainty in the predicted prevalences is concerning, and contrasts with previous modelling work.<sup>2,4</sup>

Finally, we note that the Smokefree 2025 goal was derived from a recommendation of the Māori Affairs Select Committee.<sup>12</sup> Given the much higher smoking prevalence among Māori in New Zealand, there are concerns that the Smokefree 2025 goal may be achieved for the overall population, whilst Māori smoking prevalence remains well above 5%. We suggest, therefore, that all modelling work should present estimates by ethnicity, as is the case in other recent New Zealand modelling studies.

We believe that work like Laugesen and Grace’s modelling the potential impact

of endgame interventions is important and can help inform the development of evidence-based strategies for achieving Smokefree 2025. We are sure that the authors are strongly committed to enhancing constructive discussion around achieving that goal.

However, we are concerned that the estimates presented provide unjustifiably optimistic and misleadingly definitive estimates of intervention effects, and hence skew debates about how best to achieve the Smokefree 2025 goal.

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