tiveness will become apparent because of crucial changes in vaccination coverage and epidemiology that quickly overrule variations due to discounting.

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Practical Implications of Differential Discounting of Costs and Health Effects in Cost-Effectiveness Analysis

We welcome Westra et al.’s comments on our recent article that addressed the influence of the number of future cohorts on cost-effectiveness estimates under differential discounting. Their comments usefully illustrate some of the unresolved questions regarding the correct implementation of differential discounting.

Differential discounting is already recommended practice in The Netherlands and Belgium and is also being used in a number of other countries. The National Institute for Health and Clinical Excellence in England and Wales recently recommended that differential discounting be applied in a sensitivity analysis in certain circumstances [1]. Therefore, understanding the practical implications of differential discounting is important. Our article intended to further develop that understanding, not oppose differential discounting.

Westra et al. contend that variation in cost-effectiveness estimates due to the differences between studies in the numbers of cohorts modeled is not arbitrary because the numbers of cohorts modeled are not arbitrary but determined by the time horizon for the implementation of the intervention. We did not claim in our article that the number of cohorts modeled is arbitrary in any general sense. We contend that if there is no clear and consistently applied understanding of the appropriate number of cohorts to include in CEAs then the actual number of cohorts modeled may vary arbitrarily between studies. This is the situation as we see it at present, evidenced by the large variation in the numbers of
cohorts applied in published CEAs. For example, from a sample of CEAs of human papillomavirus (HPV) vaccination included in a recent review [2], all the static models include only one recipient birth-year cohort (evidently shorter than the likely lifetime of use of the vaccine), while the dynamic HPV transmission models include 50 or 100 vaccinated cohorts. It appears, in the case of HPV vaccination, that modelers are not currently specifying their CEAs according to the expected lifetime of the intervention, but either according to the principle of parsimony in the case of static models or by allowing the model sufficient time for disease incidence to reach a steady state in the case of dynamic models. Similar variation in the number of cohorts modeled can be found in the broader CEA literature beyond the specific example of HPV vaccination.

Westra et al. suggest that an intervention should be modeled for the minimum period it will be in place. If this minimum period, however, is different from the expected actual implementation lifetime, then the resulting cost-effectiveness estimate will be wrong, assuming that we want to estimate the intervention’s cost-effectiveness over its entire lifetime.

A further question remains even if the numbers of future recipient cohorts are known with certainty and CEA models specified accordingly. Consider two interventions of identical cost-effectiveness when assessed on a per-cohort basis, but with unequal expected lifetimes of use. The intervention with the longer lifetime will be more cost-effective when the two interventions are compared over their respective lifetimes. How this difference between the per-cohort and lifetime perspectives for assessing cost-effectiveness relates to decision making still needs further attention.

Westra et al. then describe how costs and effects could be discounted not to a single discount year, but to the year in which the intervention is started by each recipient cohort and the results compared with the prevailing cost-effectiveness threshold at that time. Essentially the suggestion is that cost-effectiveness be judged on a per-cohort basis. We agree that assessing cohorts separately might bring some useful clarity in certain cases. Considering cohorts separately, however, is not an adequate solution in all cases, as multiple cohorts are required where effectiveness is shared between cohorts over time, such as in the case of infectious diseases, or where a technology is shared by successive cohorts, such as a diagnostic imaging machine. Furthermore, the results could be difficult to interpret if an intervention that has benefits shared by many cohorts is found to be cost-effective in some cohorts but not others: We still need some sort of aggregate cost-effectiveness estimate to inform us whether the intervention is worth undertaking.

We agree with Westra et al. that a dynamic model would be more suitable for assessing HPV vaccination, especially when the vaccination uptake is not very high. Our model, however, was to illustrate an important methodological point, for which we consider it entirely appropriate. We firmly disagree with their statement that other factors such as herd immunity “overrule” variations due to discounting. Our article estimated a 26% reduction in the incremental cost-effectiveness ratio of HPV vaccination with the inclusion of future cohorts over 30 years, demonstrating the very significant influence that discounting can have on cost-effectiveness ratios. This influence of discounting will be in addition to other factors such as herd immunity, not subordinate to them.

Finally, we would like to reemphasize that our article is not opposed to differential discounting but calls for both greater understanding of its impact under different model structures and clearer guidance to ensure models adequately correspond to the policy questions they are to inform.

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