

Peer Review of Cost Benefit Analysis of options to restructure the New Zealand grocery sector

John Davies, Independent consulting economist, 9 October 2025

1. I have been asked by the Ministry of Business, Innovation and Employment (MBIE) to conduct a peer review of the above report ('the Report') and underlying analysis ('the Study'), from Sense Partners, focusing on the analytical approach that has been taken, and the assumptions that underpin the analysis.

Summary

- a. Overall, I think the study provides a sound basis for the Government's decision on policy, although it necessarily does not cover all topics of relevance to that decision. It assesses the direct effects of divestment options, using tools that are considered best-practice among competition economists, for example in the assessment of mergers. It is an impressive piece of work, given the limited time available.
- b. As with any economic modelling, a lot of assumptions and simplifications of reality are required. In general, the modelling choices and assumptions made seem to me sensible and similar to those made when competition authorities model mergers. The Study includes the factors I would expect to see in such models and identifies and discusses the most relevant gaps and limitations. Those limitations especially relate to longer-term effects: supermarkets could reposition stores and their offerings under competition and longer-term the viability of chains and individual stores needs to be considered. These are more relevant to the Government's proposed policy than they would be to a merger, but in my view they do not require extensions to the Study (because reliable models of longer-term effects do not exist); rather MBIE should consult widely with industry experts, as it has been doing.
- c. I have not reviewed the data used in detail, as I have no independent knowledge that could usefully be brought to bear. The sources and data categories used generally seem to me to be sound. However, I understand that ComCom considers that the margin data from different supermarkets may not be consistent, for reasons I find convincing having encountered similar problems elsewhere. Demerger model results are likely to be sensitive to this so it should be discussed more in the report. If time permits, an extension to allow modelling of prices, as discussed in footnote 38, would be a valuable cross-check on this and on other assumptions.
- d. I have three main criticisms of the modelling framework and the way it is reported, none of which seems to me to invalidate the overall findings:
 - i. I find the Report's treatment of 'total welfare' to be confusing. I agree with the need to consider producer profits and the viability of chains post-divestment, but focusing on this measure does not seem to me a good way to do this. Rather, I suggest viability should be assessed separately (presumably not as part of this Study). I think the Report should simply report both consumer and total welfare effects (as mostly it does), without seeking to come to a conclusion as to which is 'right' – both are relevant to the MBIE's decision.
 - ii. The treatment of increases in the Cost of Goods Sold (COGS) is hard to follow. It is not clear from the Report to what extent these cost scenarios reflect increased inefficiency as opposed to increased transfers to suppliers. The two would lead to very different conclusions on total welfare effects. More substantively, the Report implies that higher prices paid to upstream suppliers would be welfare-reducing, which will not be the case if supermarkets are presently exerting buyer power to distort prices below efficient levels.
 - iii. The modelling framework at present does not consider regional or local changes in prices, except at the island level. I agree with the Report, when it argues it should not assess

store-by-store 'optimised' pricing. However, at present the model assumes that to respond to increased competition in a high-demand area, chains would have to cut prices island-wide, which seems to me unrealistic (and in effect dampens the modelled firms' likely price cuts in response to competition). The regional effects presented in the Report seem likely to be driven by this assumption and MBIE should be cautious in relying upon them. I suggest that, if there is scope for further modelling work, considering regional price effects (especially for the cities) should be a priority.

- e. I am concerned that the Report presents high level statements about the benefits of concentrated markets that are not at all supported by the literature. When discussing in more detail, the text is usually more nuanced and this does not affect the modelling itself. However, these statements make unnecessary and unsupported concessions to opponents of the policy.
- f. The Report itself is not always as clear as it should be about the underlying Study, data and assumptions. Although many of the ambiguities identified in the first draft have been fixed, in a few ways more have been introduced in the second draft Report, as some text now relates to an earlier version of the Study and is not so relevant to the analysis presented.

Background

- 2. The Government of New Zealand is considering restructuring one or both of the main supermarket chains to provide one or two additional national competitors, while also encouraging new entrants.
- 3. Sense Partners carried out a Cost Benefit Analysis ('the Study') of two policy options specified by MBIE: one to split the Foodstuffs Group (after combining the groups on the two islands), the other to require Woolworths to split into two roughly equal businesses. The Study uses three quantitative models to assess effects of both options, over ten years on consumer welfare and total welfare (taking account of producers' profits – as I discuss further below) and considers sensitivities to some of the assumptions made, as well as scenarios in which significant additional entry occurs.
- 4. I am a competition economist, having worked in the area for about thirty years, including as Chief Economist of the UK Competition Commission and Head of Competition Policy at the OECD. I have worked in merger and market investigations of supermarkets in the UK and I am familiar with competition policy developments towards supermarkets elsewhere but I am not a supermarkets specialist. Nor am I an econometrician or specialist in quantitative models, although I have frequently used and assessed such models in my career.
- 5. I have read the Report, as well as an earlier draft on which I and others provided comments. I have also been provided with underlying data, model outputs and model code. I have looked at the underlying data and outputs, particularly for details not provided in the Report, but I have not checked that data, nor any of the logic in the coding (which I have had no cause to look at).
- 6. I set out the findings of my review below, under four headings: data, welfare framework and costs, modelling and reporting. I concentrate on the points where I have concerns and suggestions for change, so I emphasise here my main point: that I find the overall approach and findings of the Study to be convincing and agree with the approach. I have specifically expressed endorsement on some big-picture points, such as choice of models, but in matters of detail I only record those items about which I have concerns. It should be taken that I support the approach in anything I do not mention.

Data

- 7. I have not assessed the data used in the modelling in detail. I comment on some specific aspects of the data in the modelling sections. Overall, the data used seem to me to be consistent with academic and competition agency modelling work in this area. In particular, it is recognised that assessing supermarket competition requires analysis of specific stores in specific locations, as the

Study does: taking store level data supplied by the chains to ComCom. These store level data then need to be related to local consumer, demand data. The Study defines a representative consumer for each "SA2" suburb area of New Zealand, based on demographic data. There are 2283 such areas, which leads to a very detailed national data set compared to supermarkets analysis I have seen in the UK.

8. Some gaps in the data have been filled using proxies, for example a regression using store area where store revenues were missing. I have not assessed these changes in detail but they seem sensible and fairly minor. The end result is nonetheless a more detailed, granular and evidenced data set than competition authorities often have to deal with for similar purposes.

Margin data

9. Data on margins are of critical importance in the demerger model, as they are in all merger models used in competition analysis (because they determine prices by considering how supermarket chains value the loss of sales to a rival, which depends on the existing margin). There is often dispute over which is the 'right' margin to use, as the same data can imply different margins depending on the timeframe of analysis and how costs and revenues are allocated across different activities with a business or a group of businesses.
10. The Study uses aggregate chain-level gross margins derived from profit and loss data supplied to ComCom. I have not verified these data but I assume they correctly report the chains' own accounts. However, data produced for accounting purposes will not necessarily be the right ones for a given economic analysis.
11. Although I do not criticise Sense Partners' use of these numbers, as they were the data supplied, I would be cautious about reliance on them and I would like to see more sensitivity checks for the following reasons.
 - a. First, as a matter of principle, although 'gross margins' (as opposed to profits after depreciation and tax etc) as reported in accounting are in principle the right level to take, a detailed investigation might throw up differences in definitions between chains, the inclusion or exclusion of items inappropriate for this modelling purpose and so on and no such detailed investigation has been conducted or any adjustments made.
 - b. Second, an email exchange made available to me shows that ComCom itself has concerns about the comparability of COGS data, an important component of the margins calculation. Understand ComCom regards them as reliable only for comparison over time (so it might conclude, say, that WW's GOCS are going down or up, but cannot reliably conclude as to whether they are higher or lower than FS's COGS).
 - c. There are some specific oddities in the data, for example **Commercial Information**
[REDACTED]
[REDACTED]. These are presumably correct reports of the accounts, but this does not mean these are the right numbers to use in the modelling. It is possible these numbers are determined by some cross-subsidy between brands. Even if not, comparison to the others suggests the margins presently earned by these brands seem unsustainable, in which case they should not be used in a ten-year analysis but should be assumed to revert to the mean, at least as a sensitivity.
 - d. The margins are chain-wide but the modelling allows regional price-setting, at least at the level of each island. In principle, when considering the prices set in a region, only the margins in that region should be considered. I suspect this makes little difference for regions as large as each island, but if some future analysis goes further into regional or even local prices, as I recommend, regional, local and store-level margins should be used. Given the large differences in costs of supplying different areas in NZ, store level margins are likely to vary very significantly.

12. Ideally, one would work with the supermarkets to check consistency and definition of these margins. However, as ComCom has not been able to resolve this, I suspect this is not possible (and I emphasise I make no criticism of Sense Partners for using the margin data supplied). However, I would recommend further sensitivity analysis. In any event, the Report needs to set out and discuss the margin data more, as results are likely to be sensitive to them and this is not reflected in the text.
13. I note that Sense Partners do have price data at SKU level and with more time could use this to obtain comparable prices between chains (footnote 38 page 53). I believe it would be worth extending the analysis to do this, (albeit I think the existing results are reliable enough for MBIE to move forward on policy) as pricing data would allow less reliance on the margin data and also a cross-check on the LA-AIDS demand assumption in the demerger model, as with pricing data alternative options can be checked using the *antitrust* model.

Welfare framework and costs

Choice of total welfare as opposed to consumer welfare

14. The modelling framework allows the calculation of consumer welfare changes resulting from divestments, from both the demerger model and a separate 'consumer welfare' model, and also changes in 'total welfare', which is here consumer welfare plus changes in producer (supermarket and upstream supplier) profit. The Report explains (Section 1.3, page 2) that it considers total welfare the right measure, essentially because it is important that divested brands be viable, so their profitability matters.
15. I agree with this objective, but I disagree that focusing on total welfare is the right way to achieve it. Viability matters but should be assessed separately. It is perfectly possible for a model to show total welfare increasing, when modelling firms that are not viable. Conversely, total welfare falling does not necessarily imply unviability, although it might well do. Total welfare simply is not an effective measure for this concern.
16. I do think that total welfare should be reported, and taken into account by MBIE, but for a different reason. Competition authorities typically only take account of consumer welfare in their decisions and I believe MBIE should consider consumer welfare too. However, in a merger competition authorities are considering a presumably profit-enhancing deal, while in enforcement work profits are derived from illegal conduct, so in both cases there are reasons to disregard profits. This does not fully apply to MBIE's proposed policy. In any case, competition authorities recognise that the relevant measure is properly-speaking *long run* consumer welfare, which could be harmed by opportunistic short-term interventions even if they lower prices.
17. However, unlike the Report, in this case I believe that decision-makers should pay *more* attention to consumer welfare than total welfare. I agree that it is essential to ensure that any market structure resulting from divestment is sustainable, but assessing total welfare is not an effective way to achieve this. If the Government is concerned that supermarkets might not be sustainable post-divestment it should try to assess this directly (in a different analysis), looking at total welfare does not guarantee this.
18. I therefore suggest that the Report should simply present both consumer and total welfare effects throughout, without seeking to decide which is more important and MBIE should consider both when taking its policy decision.

Total welfare and supply chain costs

19. I have concerns about how the Study treats the component of total welfare relating to increases in the cost of goods supplied (COGS) and upstream supplier profits. NB: this is unrelated to the modelling of increased overhead costs, which are fixed, capital costs
20. An increase in COGS is for supermarkets a higher marginal cost, so likely to be passed on through higher prices to consumers, particularly if the increase is industry-wide and particularly if the grocery

retail market is competitive. Upstream, an increase in COGS may represent supply chain inefficiency or higher profits by suppliers. The former is a loss of total welfare, the latter is to a first approximation welfare-neutral: it is a transfer from supermarkets and perhaps also consumers, to the suppliers as the Report notes at page 21.

21. I found the Report's presentation of changes in COGs resulting from divestments hard to follow; this may in part be because of feedback on the first draft report, that has resulted in some changes. The 1% increase in COGs assumed in the central scenario is considered to represent 0.5% increase to suppliers and 0.5% lost efficiency. In contrast, the 'additional' 1% increase in the high cost scenario in which COGs increase by 2% (page 28) is entirely lost efficiency (not stated in the Report but confirmed in an email to me), so three-quarters of the 2% COGS increase is lost efficiency. I understand that all of the COGS increases modelled in the sensitivities shown on page 29-30 is inefficiency.
22. This matters because total welfare should be largely unaffected by reduced buyer power (indeed, in principle I believe it should improve as I discuss below). For example, the Report notes (table 9 page 28) that the high costs (2% increase in COGS) sensitivity results in total welfare gains of only \$0.9bn and this is small over a ten year period. However, this result is partly driven by the assumption that $\frac{3}{4}$ of the 2% COGS increase is inefficiency. If instead half of the COGS increase in the high cost scenario were higher prices to suppliers, I expect the total welfare gain from the policy would be around \$1.8 bn, not so far off the central scenario's \$2.9 bn. Similarly, the conclusion stated on page 289 about the full range of COGS sensitivities, that "We have tested a wide range of assumptions and find that cost increases above 2% will cause negative net benefits from a Foodstuffs restructuring." would not be the same, if these sensitivities included some price rises for suppliers, as the central scenario does.
23. I am concerned that this approach does not compare like with like, when the central scenario is assessed against higher-cost variants and this is not explained clearly in the report. At a very minimum, this needs clearer explanation
24. More substantively, however, I am a little sceptical in any case of the Report's focus on costs increasing as a result of demerger, although I do understand the intention to provide a note of caution¹. In my comments on the first report, I noted that I thought buyer power concerns were overstated. In the new report, there is less emphasis on a loss of buyer power, but instead of this leading to a more positive result (as I had intended), the Report clarifies that it is concerned with COGS increases that do not benefit suppliers. However, I do not see that the literature cited by the Report justifies an expectation that the majority of any higher COGS would be lost in inefficiency. By both focusing on increased COGS and increasing them in a manner that does not benefit upstream suppliers, the Study seems to me distinctly pessimistic, although I recognise that this is a judgement call as the evidence is ambiguous and the authors are entitled to their views on this.

A more technical point: reduced supermarket buyer power may benefit total welfare

25. Finally on COGS, (with apologies, as this is a little esoteric) I have a technical objection to the way the Report describes higher prices paid to upstream suppliers that increase those suppliers' profits. To a first approximation, these are total-welfare neutral, and they reduce consumer welfare if passed on in higher prices and this is reflected in the Report. However, to the extent they do affect total welfare, I believe the Study treats them as reducing total welfare (higher deadweight losses): in any event the Report presents reduced buyer power as a possible bad outcome of the policy (albeit less frequently than in the first draft).
26. I disagree. Buyer power is a form of market power, albeit one exercised 'upwards' to suppliers rather than 'down' to consumers. Such buyer power *can* be welfare enhancing, when it 'pushes back'

¹ In the first draft of the report, there was much more emphasis on buyer power, but it was not clear how this was modelled.

against upstream monopoly power. In these circumstances, a loss of buyer power would result in inefficiently high pricing by suppliers, leading to higher deadweight losses in the familiar monopoly framework. Implicitly, this is what the Report presents as the result of weakened buyer power post-demerger. However, a more common concern is that supermarkets themselves have monopoly power *on the buyer side* ('monopsony') and exercise this against a fragmented and competitive upstream supplier market. In this context, buyer power can reduce welfare, so a demerger weakening buyer power would increase total welfare. I note that ComCom grocery market reports seem to treat supermarkets' buyer power in this spirit: as a matter for concern rather than celebration.

27. I do not suggest that the Study should specifically seek to model the bad effects of monopsony buyer power, but I think the Report should not imply reduced supermarket buyer power is a bad outcome of the demergers: it can increase total welfare and efficiency, while perhaps reducing consumer welfare (another reason for reporting both measures).

Cost modelling: costs of restructuring

28. For completeness, I will briefly comment on the other main cost increase component considered by the Study: additional costs which are mainly capital costs for construction of duplicate facilities. I have no means of assessing the accuracy of the specific numbers, but I agree with the approach taken here. Because these are capital costs, they do not feed through to consumer prices in the short term and the model rightly reflects this, so they reduce total welfare but not consumer welfare (and I agree with that). As I discuss briefly later, in a longer-term perspective all costs need to be covered so there could be an effect on consumer prices, but this is something to bear in mind rather than to model.
29. In considering these costs to be capital costs incurred as a result of demerger, the Study is entirely mainstream. Indeed, when merging firms do claim 'efficiencies' in mergers, they are often the elimination of such duplicated overheads (and therefore often not taken into account by competition authorities, operating a consumer welfare standard, as there is no benefit to consumers).

Welfare reporting: recommendations

30. This is all quite complex and nuanced and I do not suggest the modelling should further be complicated but I have some practical, simplifying suggestions for the Report:
- Present both consumer and total welfare effects throughout (as largely it does) without choosing one over the other.
 - Clearly distinguish between assumed increases in COGS from weakened buyer power from any increases resulting from supply chain inefficiency. These are separate matters and need separate reporting.
 - Increased transfers to suppliers resulting from reduced buyer power should be considered at worst total welfare neutral, while reducing lower consumer welfare to the extent they are passed through. This is an important trade-off that needs to be presented clearly.

Modelling

31. The Study makes use of three quantitative models:
- The spatial model takes store and chain-level data on supermarkets and local data on households to model how the supermarket chains compete. The main purpose is to produce diversion ratios between supermarkets, which measure how any sales lost by one chain divide amongst its rivals.
 - The demerger model takes these diversion ratios, along with data on supermarket margins and assumptions about demand, to model price-setting behaviour with different ownership patterns among the chains. A smaller chain has more to gain by reducing

prices, because it can attract business from more rivals (whereas pre-demerger, a large chain reducing prices would to a greater extent be attracting business from itself). The output effectively measures overall changes in prices and thus consumer welfare and (given assumptions about costs) supermarket profits and thus total welfare. This model is one option from a publicly-available tool created by economists at the US competition authority and widely used by competition authorities. Results are reported in Tables 2 and 9.

- c. Separately, a household welfare model takes these results and considers the effects on consumer welfare in a more sophisticated framework. This does not provide the main results for consumer welfare reported in Tables 2 and 9 but provides the distributional and regional impacts reported in 4.2 and 4.3. I am less familiar with this modelling framework, which is not used in competition economics (and this is not a criticism – it simply goes beyond what competition economists typically require in sophistication).

The spatial model

- 32. The spatial model is an adaptation to New Zealand of Ellickson et al (2020) an accepted academic paper, in a highly-respected journal in the field of industrial organisation, the branch of economics most relevant to competition policy. I had not previously encountered this paper, but it is in a mainstream tradition of modelling industries, particularly those with local elements of competition, with which I am familiar. I have reviewed the paper and I have no concern about the choice of this model and I see no reason why it should not be applied to New Zealand.
- 33. The model takes specific data on the location of supermarkets and demographic data about local consumers, represented by a 'representative consumer' located at the centre of each SA2 area. By looking at store level sales and relating this to chain and consumer characteristics, it seeks to identify how 'close' the rival supermarket chains offers are to one another, in consumers' eyes, represented as 'diversion ratios' in the model outputs. Two chains which have very similar offerings (by range or whose stores are often physically close to one another) would have a high diversion ratio between them, for example. Modelling using this spatial model to assess diversion ratios is a more sophisticated and comprehensive approach than most competition authorities are able to apply in a merger investigation; often the diversion ratios they use are based on extrapolating data from surveys of only a small part of the market.
- 34. Not all of the relevant diversion ratios are presented in the Report, as diversion ratios are reported only between existing entities. It would be useful to present the equivalents for the post-divestment entities in each of the two options.
- 35. I have not seen any sense-checking or cross-checks of the diversion ratios resulting from the models. As they are a crucial input to the demerger model, it would be reassuring to see them checked against historical data, for example by looking at whether the changing patterns of demand when new stores open or stores close are consistent with these findings.

The demerger model

- 36. The demerger model is a mathematical model which is 'solved' to determine optimal prices (or other competitive aspects of the offer, such as product range) after a change in ownership, splitting Foodstuffs in the first option and Woolworths too in the second. Such models are commonly used by competition authorities in merger cases (in which the change goes in the opposite direction). The Study makes use of one option – the LA-AIDS version of a suite of several models packaged together under the name 'antitrust', created and published by economists working at the US Federal Trade Commission for economists to use in merger assessment.

Use of a merger model

- 37. I support the Study's use of a 'merger assessment' model for assessing this demerger, as it seems to me the policy is sufficiently similar (in reverse) to a merger for this to be appropriate cases are

sufficiently similar. I agree too with the caveats in the report, particularly that such merger models are quite short-term in that they consider the immediate effects of the change in market structure, and consequential price changes, not how the industry might then evolve. As the Report notes, under competitive pressures, chains might change their offerings (perhaps simplifying range, perhaps moving up or downmarket to find a niche) and also physically reposition, closing less profitable stores and entering other areas.

38. This short-term focus of merger models should be more of a concern for New Zealand's proposed policy, looking to the long term (ten years in the Study), than it is for a competition authority which typically considers a three year time horizon to assess mergers. However, this is not a criticism of the Sense Partners model and the solution is not to try to find a long-term model. The long term is fundamentally different in many ways: stores opening and closing, brands repositioning their range and other aspects of their offer to reposition themselves and so on. As part of this entry and exit, I would also expect prices to consumers eventually to adjust to reflect the capital costs incurred by the newly-divested firms (the existing model rightly does not reflect them in prices as they are not variable costs in the short term but in the long term all costs should be considered variable). Such a long-term equivalent to the demerger model does not really exist and I suspect that an attempt to model range and store repositioning over ten years would do no more than reflect whatever assumptions the model creators imposed, presumably on the basis of expert industry advice. Better simply to understand these matters qualitatively, from those advisors, I suggest.
39. As the Report notes, competition authorities do not determine their decisions solely upon the basis of such modelling; it is one input along with much qualitative information. MBIE should do the same.

Choice of LA-AIDS demand system

40. The antitrust package that Sense Partners use has about ten different options for how to model demand, from which they have chosen one called LA-AIDS. Demand modelling necessarily requires assumptions to be made. Although in principle, we would like just to measure how consumers behave, in practice, there is never enough data – or enough variation in the data – to observe how consumers will react to any given change. The choice of 'demand system' fills this gap.
41. Sense Partners' choice of the LA-AIDS system was dictated by data availability: it is one of the few in the package that does not require pricing data. However, the AIDS system is well-known and accepted; it is a more sophisticated treatment than many competition authorities use in simple screening tools, in which demand is often assumed to lie upon a simple line. The AIDS system has been used in modelling grocery demand quite frequently, in academic studies. It therefore seems a reasonable assumption and in any case, results should not be too sensitive to the choice of demand system for the fairly small (~5%) price changes modelled.
42. However, as I note above when discussing margins data: if the work were extended to make use of pricing data this would allow for testing robustness to the use of different demand systems.

Ten year time horizon

43. The model considers outcomes over ten years. As the modelling does not seek to forecast any specific changes over ten years, just steady growth, the main effect of this assumption is the period and market size over which to 'amortize' the one-off restructuring costs. A shorter period would reduce the assessed benefits of the policy, because these costs would be spread over a shorter period and the overall scale of the market each year will be smaller on average, so the benefits proportionately less. It might be worth exploring a sensitivity to a five year horizon; I would not consider longer than ten years.

Entities are assumed to optimise pricing only island-wide.

44. I am concerned that the modelling is too crude in that it assumes island-wide price setting. The Report discusses this, in particular considering whether it should go to the opposite extreme and model chains maximising profits by setting prices differently at each store. It rejects such an

approach, essentially because current pricing is indeed largely chain-wide. It also notes economic studies finding that supermarkets do not flex price locally as much as economic theory would predict. As a minor quibble, it describes this as 'not maximising profits' – I disagree with this phrasing, I suspect it is more that there are profit considerations supermarkets take account of that are not included in simple short-term models. However, I agree with the approach of not modelling pricing store-by-store, first because it would be an assumed huge change from now, second because it would be hard to model – because there is no baseline 'locally optimised' price today.

45. That said, however, I do see benefit in extending the model to consider some limited local price flexing, particularly in cities that might experience significantly more competition both as a result of the demergers and through entry. As the model stands, a chain faced with more competition in a city can only respond by lowering prices across the island, including in places where it faces unchanged competition. Consequently, responses to increased competition will be muted, as benefits of lowering prices to meet competition in the cities can be obtained only by lowering prices also to rural consumers for no competitive gain to the chain. This seems implausible. Even if chains want to avoid 'headline' price differences in different areas, because of public opposition, it is well-documented that there many ways of lowering effective prices only locally, for example through promotions and discounts, or just making more budget brands and sizes available.
46. I therefore recommend that if there is scope to extend the modelling, it considers price setting in a few more regions – for example, treating the cities separately for a total of 4-5 price zones instead of the current two. This is particularly important for the scenarios modelling additional entry but applies to the central case too.

Some other assumptions in the demerger modelling

47. Like any demand model, the demerger model requires an assumption about the market elasticity of demand for groceries as opposed to the 'outside good' (all other items consumers might buy). This is set at 0.95 on the basis of one earlier study. It would be more reassuring to see this checked against wider literature. The figure is flexed essentially for technical reasons to 'nudge' the model into converging, as set out on pages 63 and 64. This is unfortunate, at least presentationally, as a key variable is changed without empirical support just to make the model work. The Report discusses this fairly and notes small sensitivity to flexing as far as 1.1, implying this is the maximum figure used (but this could be stated more clearly).
48. The use of an 8% discount rate seems high in international comparison, as I would not define the demerger policy as 'commercial' so would favour a social discount rate. However, I have looked at NZ Treasury Guidance and it seems to be consistent with that.

The household welfare model

49. Competition economists generally do not make use of models such as Sense Partners' Household Welfare Model, although I can see the value in calculating distributional effects by household type and regionally, for a national policy intervention of this sort. I have not really assessed the modelling, but it seems to me essentially to illustrate the consequences of the demerger analysis.
50. The distributional analysis in Section 4.2 seemed interesting if unsurprising. It is well-established that poorer households spend a larger proportion of their income on groceries (and may obtain a still higher proportion of their utility from such purchases). However, this serves very effectively to confirm the value of a policy intervention that on total welfare grounds may appear rather marginal. The findings also depend crucially on the assumption of chain-wide island-wide pricing, as I discuss for the regional analysis below.
51. I have no concerns about the regional analysis presented in 4.3, except to reiterate that I think the island-wide pricing assumption in the demerger model is too restrictive. The results in 4.3 are to a great extent determined by this one assumption, so the added value from the modelling here is low.

I would caution against MBIE claiming ‘modelling shows no adverse regional effects’ as opponents of the policy could easily demonstrate this is largely driven by modelling assumptions.

Expansion of alternative formats

52. The Study has also modelled the demerger options against a backdrop of scenarios of additional entry, as reported in Appendix A to the Report. This seems a sensible extension of the modelling, as the beneficial additional effect of demerger will be reduced somewhat, if there is more competition from new entrants. Indeed, this is what the modelling finds.
53. The Study does not consider causation in the opposite direction: whether demerger would make the market less attractive for entrants. This could not be modelled in the existing framework, as the entry is an assumption, not a modelling outcome. However, MBIE could consider in its wider discussions and consultation whether this effect might arise and the Report’s findings (particularly estimating the price effects of demerger) will be helpful background for that.
54. I do note that the analysis of entry in Appendix A seems particularly likely to be affected by the restrictive assumption of island-wide pricing. As the Report sets out, entrants are likely to target the cities. In the modelling framework, existing and demerged chains cannot respond to that with localised price or other strategies: if they respond at all they must do so in all local markets island-side, which diminishes their modelled competitive response by increasing the profits they lose by responding to the entrants. This does not seem to me to be realistic – even if supermarkets want to avoid local reductions in headline prices, they can use discounting and many other tools to respond to a local challenge.
55. This reinforces my conclusion that it would be useful, as an extension to the analysis, to consider a few more pricing regions, without going to the granularity of store-level optimised pricing.

Reporting

56. In general, the Report presents its assumptions clearly and illustrates sensitivities to many of the key assumptions, particularly on cost changes. Some assumptions drive fundamental modelling choices (such as island-wide pricing, or the demand system used in the demerger model), so numerical sensitivities to these could not be presented without significant additional work and although that would be helpful, it does not seem essential. I do, however, recommend that the Report should discuss and present sensitivities to the margin data used in the demerger model.

Effects of market concentration on costs, quality and variety

57. Headlines and other summary text in the report frequently states that demerger is likely to result in cost rises and quality and variety losses (for example, when discussing merger policy on page 6). When the report comes to consider these matters in more detail, it generally recognises the effects are ambiguous (rightly in my view). The modelling, too, reflects a more balanced view of the effects of demerger, for example including cost efficiencies from competition. It is right to warn against possible downsides but I think it is unnecessary and incorrect to frame the problem so starkly as the Report does now: of a trade-off between improved competition at the expense of cost, quality and variety. Competition can provide a spur to improve all of these.
58. There is far too much evidence to summarise here, but there is a huge literature (not from competition agencies and specialists) debating whether mergers generally have any productivity benefits at all, for example.
59. I would not go that far, but I do not believe the wording of the trade-off presented on page 6 is correct and, later on page 20 the statement “increased concentration consistently results in efficiency gains.” is simply not true (there is a huge and established literature finding the opposite – see survey here for example: <https://www.gov.uk/government/publications/productivity-and-competition-a-summary-of-the-evidence>). I suggest amending these bold statements.

60. I also suggest adding the word “Assumption:” to heading 2.3.3. I appreciate this is in a section titled ‘Assumptions’ but the other headings are not worded so starkly and I think a casual reader could easily think this to be a conclusion of the modelling.

Minor reporting points

61. In this section, I briefly list a few gaps and ambiguities in the reporting that should be sorted out, including some I have discussed and explained above.

- a. The way COGS increases are modelled in different scenarios needs to be set out more clearly.
- b. The tables of diversion ratios only cover the existing entities, it would be useful to present the post-demerger diversion ratios too, including new entities.
- c. My slightly pedantic concern from the first draft about the language used to describe the diversion ratios in Table 3 on page 11 (repeated on page 62) remains: I am fairly sure that “a 1% reduction in sales at New World is shown to increase sales at PAKn’SAVE by 0.45%” is incorrect. Instead it would increase sales at PAKn’SAVE by 0.45% *of New World sales*. Better to say \$100 loss of sales at NW would lead to \$45 increase at P&S.
- d. Margin data needs more explanation and discussion, subject of course to confidentiality.
- e. The sensitivities to COGS changes are presented at different points as being -3%/+3% and -1%/+3%. The Report does make clear that Sense Partners *modelled* -3/+3 but only *present* the more limited -1/+3 but this seems unduly fiddly and confusing. I understand this reflects a change since the first draft and I suggest the change should be followed through so only the final range appears.
- f. There is a surprising focus on product variety effects, given that the Report concludes this is ambiguous and the model does not feature any change to variety post-demerger. Again, I think this is in effect a relic of a previous draft and it could confuse a reader only seeing the final draft. No objection to the discussion, but it could be shortened.
- g. Page 8: ‘Pirides’ should be ‘Pire’?
- h. Stephenson 2021 reference missing from bibliography.