IN THE MATTER OF the Resource Management Act 1991

AND

IN THE MATTER OF applications by Central Plains Water Trust to:

Canterbury Regional Council for resource consents to take and use water from the Waimakariri and Rakaia Rivers and for all associated consents required for the construction and operation of the Central Plains Water Enhancement Scheme

Selwyn District Council for resource consents to construct and operate the Central Plains Water Enhancement Scheme

AND

IN THE MATTER OF a notice of requirement by Central Plains Water Limited to:

Selwyn District Council for the designation of land for works associated with the construction and operation of the Central Plains Water Enhancement Scheme

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BRIEF OF EVIDENCE OF CLIFFORD JOHN MAXWELL TIPLER

29 February 2008

BUDDLE FINDLAY
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Qualifications and experience

1. My full name is Clifford John Maxwell Tipler.

2. My qualifications and experience have been presented to this Committee in my evidence-in-chief and has not been repeated here.

3. I have read the code of conduct for expert witnesses set out in Environment Court practice note, and confirm that I have complied with the code in the preparation of my evidence.

Scope of Evidence

4. I will provide in this section of my evidence, my response to the matters raised in the Section 42A report compiled by Mr L Fietje. This will include:

    - Comment on the groundwater assessment by Mr C Hanson
    - Comment on the hydrology assessment by Mr M Duncan
    - Comment on the groundwater assessment by Mr D Scott
    - Conclusions

REPORT OF CARL R HANSON

5. Mr Hanson provides a summary of his conclusions in paragraphs 13 – 16 and I concur with his conclusions in paragraphs 13 and 14, but I do not concur with his conclusions in paragraphs 15 and 16, both of which go to the heart of the issue of nitrate contamination of groundwater in the Central Plains.

6. At the outset, it is fair to state that I agree with most of what has been presented by Mr Hanson, but our differences arise in relation to the base data used and the subjective assessment of the significance of the changes in nitrate concentrations that I have predicted in the groundwater.

7. Mr Hanson has referenced the URS report dated 27 November 2007. The same material as contained in that report is presented in my Brief of Evidence – Section 2, however the modelling has been updated since that time to correct an error in the drainage data from the distribution system. Mr Hanson has been supplied with the updated modelling and results prior to the issue of the s42A report. There are no material differences between the results in the November report and my evidence and I have assumed that Mr Hanson's comments apply equally to the modelling as presented in my evidence as in the November report.

8. The conclusions of Mr Hanson differ from my own because he concludes that:

- There is considerable uncertainty in the inputs to the model,
- The input data are not conservative, and
- The interpretation of the results is not correct.

9. I shall by reference to his individual paragraphs explain how I reached my conclusions as stated in my Brief of Evidence - Section 2.

10. I agree with Mr Hanson in paragraphs 45 – 50 that intensification of land use will result in an increased mass of nitrate entering the groundwater.

11. In paragraph 57, Mr Hanson states that *"the URS report concludes that there will be increases in both the mass of nitrate and the volume of water that enter the groundwater beneath the CPW scheme area. According to the report, however, these two increases will balance each other, so that concentrations in the groundwater beneath the scheme area will not change"*. This is an incorrect representation of the report and what is contained within my Brief of Evidence – Section 2.

12. I have stated that "any increase in the mass of nitrate from land use intensification is accompanied by an increase in clean water dilution" in my paragraph 77. I do not state that the two increases will balance each other, so that the concentrations will not change. In fact my evidence clearly states and shows that the nitrate concentrations with the groundwater of the Central Plains will increase. It is my opinion that this increase is not significant.

13. Similarly in Mr Hanson's paragraph 60 he states that elevated nitrate concentrations will extend to deeper levels within the groundwater. This is consistent with my paragraph 95, and again I discuss the significance of this more closely in this brief.

14. In paragraph 119, Mr Hanson states that the URS model provides one result, a single distribution of nitrate concentrations with very little discussion of the potential error associated with it. I do not agree with this statement.

15. The modelling I have undertaken does not provide "one result". In fact each run of the model produces approximately 75,000 results, from which a distribution is created, that is representative of the range of the results calculated. Further there has been a sensitivity analysis undertaken looking at the effects of the major variables and assumptions within the model. Therefore for the five scenarios I have presented, there have been approximately 375,000 results which have been

analysed and presented as cumulative frequency distributions. This in my opinion enhances the ability to interpret the results.

16. It is my experience that this approach contrasts starkly with the assessment of the effects from nitrate leaching from irrigated pasture presented at other hearings before ECan. For example at the Ngai Tahu Properties Ltd hearing to take and use water from the Waimakariri River, Mr Brough acting for the applicant presented a single figure assessment of nitrate concentration in the drainage water of 7.3 mg/L (7.3 g/m^3). This was used by Mr Callander also acting for the applicant, in a single calculation assessment of the increase in nitrate concentration beneath the scheme area of 3 g/m^3 and in the order of 1-2 g/m^3 downgradient of the property. These applications were granted by the Regional Council and the Environment Court.

17. It was my dissatisfaction of single figure assessments of dynamic natural systems that lead to the use of the Monte Carlo simulations in my assessment. I believe that my analysis is more scientifically robust than single figure assessments, and to my knowledge this is the only assessment of nitrate contamination within the Canterbury groundwater system that has so openly addressed the issue of variability with input data and the interpretation of the results.

18. Mr Hanson mentions “uncertainty” with this approach (paragraph 61). I fully accept that there is uncertainty and this is why I have brought a statistical approach to my assessment. For example, one could assume that the existing data of nitrate concentrations in groundwater accurately reflects the current state of that groundwater. However at best these data provide an indication of the state of the groundwater. Because there is a large number of data (682) upon which I can form my opinion, I can say that I am reasonably confident that these data provide a fair representation of the current state. I cannot say that it accurately represents the true state. It could show things to be worse or better than the true state, I simply do not know which. I would suggest that in all environmental systems there is this difficulty and therefore we need to accept there is uncertainty and not lose the value obtained through the interpretation of these data. Mr Hanson (paragraph 26) acknowledges that these data give a fair picture of nitrate concentrations in the area. I agree with this.

19. Therefore in paragraph 42 where Mr Hanson states that *“it is not possible to predict the magnitude of nitrate concentration increases with any certainty”*, he belies the usefulness of an assessment that incorporates uncertainty as my assessment does. I would phrase this statement slightly differently, by saying ‘it is possible to predict the magnitude of nitrate concentration increases with some uncertainty”. The modelling I have presented recognises that there is uncertainty in any assessment and directly

addresses this matter. This therefore invites the discussion on how uncertainty has been dealt with in my assessment, and I shall discuss this further.

20. In Mr Hanson's paragraphs 61, 67, 121 – 127, he discusses the uncertainties in the inputs to the models and that the range of potential effects on nitrate concentrations is much greater than what is suggested in the URS report. Further in paragraph 91 he states that the distributions have some important shortcomings. My paragraphs 40 – 52 outline how the distributions have been assembled and importantly how they have been modified for pastoral farming practice to address concerns expressed by Mr Hanson about the use of data provided by Crop and Food (refer evidence of Dr Glyn Francis). I believe the connection between the Crop and Food data and my distributions is transparent. The model has used the same leaching distributions for dry and irrigated crops, because it has made the assumption and that all cropping land is irrigated and therefore we have not needed different distributions. I have not provided justification for the dryland pasture distribution because it is a low value and of little significance. However I understand that the central point is a conservative estimate for leaching from dryland pasture.

21. In paragraph 93 Mr Hanson claims that the distributions are too narrow. It is easy to make such a claim, but the basis for this claim is not provided. Most importantly, Mr Hanson has not stated that the point of central tendency is wrong. I use the term "point of central tendency" in a general sense as it could be the mean value, the median value, or simply the highest point on the distribution graph. It is the value that will have the largest influence in the numerical processes followed. If the distributions were wider then the extreme events would be more extreme, but nevertheless just as unlikely, as it would involve the combination of the higher leaching losses with the lower drainage and mixing volumes. For this reason I do not consider this to be significant. Further on the advice of Dr Francis, I believe my distributions are fit for their purpose.

22. Mr Hanson correctly notes in paragraph 94 that the drainage volume and nitrate loading have been treated as independent variables. It is likely that they are not independent, however including a relationship between the two brings a level of sophistication and complexity to the model that in my opinion is not necessary and may be beyond the present state of knowledge in terms of nitrate leaching. I base this conclusion on the problems I have had getting corresponding data between drainage and leaching loss through the use of OVERSEER. I do not consider that this detracts from the usefulness of the modelling.

23. In paragraphs 95 – 104 Mr Hanson outlines why he considers that the distribution of nitrate loss from irrigated pasture is too narrow. Implicit in the assumption that the

distribution is too narrow, is that the top end of the range is too low. The distribution can be found in my Figure 3. Mr Hanson references Ledgard et al (1999) which reports leaching losses from 20 – 204 kgN/ha/yr. The 204 value is outside the range of my distribution, however this relates to a system where 400 kgN/ha/yr has been applied as urea. I have been advised[1] that the common practice in Canterbury is to apply less than 200 kgN/ha/yr and under the farm management plans proposed by CPWL 200 kgN/ha/yr would be a pseudo-limit, unless a nutrient budget can demonstrate that the effects will be minor. I therefore do not consider this to be a realistic number for the upper end of my distribution.

24. Mr Hanson also refers to a study by Di and Cameron 2002 which reports nitrate losses of between 112 – 162 kgN/ha/yr. However there is a major difficulty in using these data to reflect what might happen for CPWES. This study was based on flood irrigation and specifically cautions the use of these results for spray irrigated conditions. The CPWES is totally reliant upon spray irrigation. The paper refers to improved irrigation practices to apply less water more frequently to reduce the significant drainage that follows flood irrigation. High drainage fluxes will remove more nitrate from the soil profile as the nitrate spends less time within the soil profile. Further, the upper end of the range reported relates to land that has 400 kgN/ha/yr applied as dairy shed effluent, plus a contribution from urine from grazing cattle. Again I do not consider that these data justify a change to the distribution I have presented.

25. For the reasons outlined above, I do not agree with the statement in Mr Hanson's paragraph 104 where he states that the nitrate losses from the CPW scheme area could exceed 100 kgN/ha/yr much more frequently than the distribution in my report.

26. In paragraph 106, Mr Hanson states that the graph as Figure 2.5 in my report, which is reproduced as Figure 4 in my Brief of Evidence – Section 2 has been misinterpreted. I accept that my statement regarding these data is ambiguous, however my understanding is the same as Mr Hanson's. This graph and the associated data is of interest for two reasons, firstly that as drainage increases through the year the concentrations of nitrate decrease, the second aspect is that the numerical values of the drainage concentrations are very low. If leaching concentrations are typically in the 4 – 8 g/m^3 range, then the effects as assessed by me will overstate the impact. However I accept that this is only one set of data, but nevertheless it is useful.

27. Mr Hanson notes the flow-on effect of dairy cattle moving to dryland pasture during the winter and as a consequence the leaching from that dryland will be greater. He

[1] Mr Geoff Stevenson – Dairy Farmer and Ritso Society member applies between 160 – 170 kgN/ha/yr.

notes that this has not been allowed for in my assessment, and in this regard he is correct. The studies from which I based my data for leaching rates includes both wintering off and wintering on of stock. The papers by Monaghan et al are based in Southland, where it is stated that stock are wintered off The papers by Ledgard, Selvarajah and Thorrold are from the Waikato where the predominant practice is to winter on. In Canterbury the normal practice is that dairy farmers send their cows to a dryland property for 8 – 9 weeks to enable the pasture to recover and to protect the soils from pugging[2]. There is a mix of wintering off and wintering on practices within the data I have presented in my Table 5. The potential issue is the extent to which wintering off of dairy cattle affect the nitrate leaching assumptions I have used for dryland farming.

28. In my Appendix A Figure A-9 shows the distribution of nitrate leaching from dryland. It ranges from 0 – 30 kgN/ha/yr with a point of central tendency of 24 kgN/ha/yr. In a report prepared for CPWL by Dr Francis[3] he estimates the average loss from a sheep and beef farm (dryland) to be 6 kgN/ha/yr with a range of 5 – 16 kgN/ha/yr depending upon the development stage of the pasture. These are significantly lower than the leaching loss assumed in my dryland option.

29. Dr Francis states in his report that under average conditions the nitrogen leaching from a dairy farm was 32 kg/ha/yr, however with "winter grazing off" the annual nitrogen leaching would reduce to 18 kg/ha/yr. This represents a reduction of some 14 kgN/ha/yr. Further if we assume that wintering off is for 8 – 9 weeks (15% - 17% of the year) and that would contribute a proportionate contribution to dryland leaching (say 17% of 55 kgN/ha/yr = 9 kgN/ha/yr) then between 9 – 14 kgN/ha/yr additional could be lost from a dryland farm wintering dairy stock. Adding this to Dr Francis's dryland losses of ~ 6 kgN/ha/yr, still gives a value less than that assumed for my dryland option. Therefore the effect of wintering off does not need to be explicitly allowed for, as it already is accommodated within the areal losses I have assumed.

30. In paragraph 114, Mr Hanson states that the comparison between the distributions as shown in my Figure 5 have been used to defend the reliability of the model, and that the distributions are not comparable and therefore my argument is not valid.

31. In paragraph 115 Mr Hanson notes that the ECan data set is a series of instantaneous readings at various locations in different years and different times and that in contrast my results represent annual average concentrations. I agree with the statement about the ECan data, but do not agree entirely with the claim that my results represent annual average concentrations.

[2] Pers comm.. Mr Geoff Stevenson – Ritso Society and Dairy Farmer.
[3] FY Li, PD Jamieson, GS Francis, (2007) Estimates of nitrogen and phosphorus leaching under various land uses in Canterbury – an update. Crop & Food Research Confidential Report No. 2042.

32. This is a difficult aspect of the modelling to understand. I have used an annual time increment for the modelling, therefore all water and all nitrate is added together to produce an average annual value for each of the 15 individual sectors. These 15 values are not combined to form a single "annual" value for the central plains for that year. If I had done so, the results would have been "damped" – the spread in the values would have been reduced, in exactly the same way that if all ECan data for each year was combined to a single average value, the spread of the data would have been reduced. Therefore my data conceptually reflects an annual average value for each particular land use activity and therefore will include a combination of a high nitrate loss rate, low drainage rate and low mixing volume to produce the maximum increase in nitrate concentration in the groundwater. If I did average all my data, the maximum increases would have been reduced.

33. While the above clarifies what the model actually does, this is somewhat immaterial, as I infer from Mr Hanson's comments that he believes that I have used the co-relation of the data as shown in my Figure 5 to show that my conceptual representation of the existing land use and groundwater system has been validated. I have not done this. As stated in my paragraph 64, I believe this shows that there is a good basis upon which to conduct my evaluation. As stated in my paragraph 65 the modelling is more appropriately considering the changes between before and after CPWES. Thus I have attempted to define a starting point that is very similar to the existing environmental state, so that when I predict an increase, the magnitude of the increase is realistic. By examination of the shift in the before and after lines on my graphs, it is possible to ascertain the magnitude of the potential effects, rather than focusing on the absolute results. Further the shift at the top end of the data range is less than at the lower end of the data range and this is consistent with my understanding of the mixing processes involved.

34. To summarise these points, I have not used the comparison of the data to "validate" the model in the manner that modellers refer to validation (as for Mr Weir's modelling). My "validation" is really a justification that the starting point for my comparative assessment is a reasonable position to start. I stand by my conclusions in this regard.

35. In paragraph 122 Mr Hanson discusses the leaching rates used and states that he is not convinced that the rates are particularly conservative. He does not state that the rates I have used are unrealistic and I do not believe that I necessarily need to be conservative, provided my assumptions are clear and my base data realistic. I believe I have met these criteria, and therefore my results do provide a realistic assessment of what might happen. It might be better than this if other mitigation

measures as referred to by Mr Hanson are adopted. I discuss this further later in this brief.

36. In paragraph 123, Mr Hanson notes that I have assumed there will be 36,000 ha of newly irrigated land within the 60,000 ha scheme area. In my paragraph 31 I provide a discussion over the uncertainty of the area of currently irrigated land that will convert to scheme water. I could have included this as another variable in my modelling by providing a range of areas from 15,000 – 30,000 ha being converted. By selecting a single value in the middle of the range, I may not have fully included the variability around this assumption in the model, but I have selected a value that realistically represents the most likely outcome. There is no guarantee that 24,000 ha will convert from groundwater supply to surface water supply as sought by Mr Hanson.

37. In paragraphs 124 – 126, Mr Hanson notes some of the aspects of the Canterbury groundwater system that are not specifically provided for in my methodology. I have explained my reasons for this in paragraphs 27 – 29 and 53 - 54 in my Brief of Evidence - Section 2. However I do not follow Mr Hanson's line of reasoning in paragraph 127, where he says that these aspects are difficult to predict and therefore implies that the range of effects will be much greater than what is suggested. In particular my assessment shows that the existing high concentrations are unlikely to change significantly because the nitrate concentration in the drainage water is not dissimilar to the existing nitrate concentration in the groundwater, but at the lower existing nitrate concentrations in the groundwater, the magnitude of the change will be greater. What Mr Hanson is referring to in terms of a greater range of effects, does therefore not follow from the aspects he refers to as being uncertain.

38. In paragraph 131 Mr Hanson states that it is his opinion that *"the scheme is likely to cause nitrate concentrations in at least some parts of the Central Plains to increase by more than 2 mg/L, the limit set by Objective WQL2"* in the PNRRP. Objective WQL2 (2) (b) (i) states that "*for nitrate-nitrogen, the maximum concentration shall not increase by more than two milligrams per litre above the maximum concentration measured between 1996 and 2001, and reported in 2002, and the maximum concentration shall not exceed 11.3 milligrams per litre*". It is important to note that the increase refers to maximum concentrations and it is my assessment that the maximum concentrations are unlikely to increase by 2 mg/L. In my paragraph 81, I perform a similar calculation to Mr Hanson's in his paragraph 75, which is a gross single number assessment of the potential increase in nitrate across the plains. My assessment shows an increase of ~ 1.2 g/m^3 around the point of central tendency. This is greater than that I have calculated from the results of Mr Hanson in his paragraph 75. Based on his estimates of nitrate increasing by 50% and recharge

increasing by 35%, the concentration would increase by 11% (1.50/1.35=1.11) which, if taken at the median point of 3.7 g/m^3, would indicate an increase of ~ 0.4 g/m^3. My figure is therefore more conservative than Mr Hanson's. Further if the increase at the central point is of this order, the increase at points of higher natural concentration will be less, therefore I would expect increases of maximum existing values "on average" to increase by something less than 1 g/m^3. To take account of variability and uncertainty, it is necessary to look at the results of my predictions in Figures 6 - 10. These show that the biggest increase can be expected if the distribution network is piped and even then the increase is still likely to be less than 2 g/m^3 over the full range of concentrations.

39. In the piped reticulation scenario as shown in my Figure 10, there is an indication that more groundwater would be above the MAV of 11.3 g/m^3, however this is the only scenario that shows this effect. This is directly a consequence of less dilution water available. If the scheme was piped, I could not conclude that there would no instances where already high nitrate concentration groundwater was tipped over the 11.3 g/m^3 MAV. I do not consider this to be a significant adverse effect, as it is likely to be transient and in any event CPWL has given an undertaking that for any well that as a consequence of its irrigation activities, results in a water quality that does not meet the drinking water standards for New Zealand, an alternate water supply will be provided. In a practical sense, this will mean that the depth of the well would be increased. It is my opinion that even though this is the worst case scenario I have considered, it is still better than that which could occur if pasture was irrigated from a groundwater source, and there are many current examples of consent being granted for groundwater abstraction.

40. In Mr Hanson's paragraphs 68 – 82, he discusses the aspect of the mixing depth increasing, such that existing wells that access "deep" groundwater that at present may have low nitrate concentrations, may become affected by the surface drainage effects and see an increase in nitrate concentration. I agree with the basic premise put forward by Mr Hanson, but I do not agree with his assessment of the significance of this. These wells will at present have low concentrations of nitrate. Given this, they are likely to experience an increase to a level that will still be relatively low and in my opinion will not have any adverse health impacts. I base this conclusion on two facts. The first is through observation of my Figures 6 – 10 that show that low concentrations (less than 2 g/m^3) will increase by about 1 g/m^3. However as I have stated, care is needed when interpreting these figures at the low concentrations and I would accept that an increase greater than this could be possible. Secondly my Figure 11 shows the spatial differences in groundwater concentrations across the plains. This shows how deeper groundwater has lower nitrate concentrations and

therefore I would expect this trend to continue. Therefore the wells affected by the increased mixing depth would still have lower nitrate concentrations, as they are deeper.

41. However if the groundwater nitrate concentration was to increase from 2 g/m^3 to 4 g/m^3, this would have no adverse health impacts and therefore I conclude the effect would be minor. The only other aspect I need to discuss in relation to this, is whether there is any consequential effect on low land stream water quality.

42. I discuss lowland stream water quality in relation to groundwater quality in my paragraphs 96 – 101. The basic premise I propose is that the water quality of the lowland springs and streams is representative of the shallow groundwater aquifer 1 and that the contribution from deeper groundwater is not significant. Therefore while I agree with Mr Hanson that the nitrate plume will mix to a greater depth, I do not agree that this will necessarily increase nitrate concentration in the lowland streams.

43. I note that in paragraph 133 Mr Hanson qualifies his concerns by stating that *"it is possible that overall, increases in nitrate concentrations will be within acceptable limits. The chances for this outcome will be greatest if nutrients on farms are managed to minimise nitrate leaching*". Consequently Mr Hanson recommends conditions should be placed on the consent to ensure proper management. The conditions I have proposed in my Brief of Evidence – Section 1, include conditions to ensure appropriate management of nutrients on farm. In particular I refer to Schedule 2: Administrative Conditions, condition 6 that requires through the farm plan that;

g) That, for each property, for each 12 month period ending 30 June:

i) either, it is demonstrated, via the nutrient budget required in (c) above, that the average total nitrogen (fertiliser and effluent) application has been less than 200 kgN/ha/yr;

ii) or, approved methods are used to undertake calculations or measurements of the average annual concentration of nitrate nitrogen in the soil drainage below the plant root zone and the actions in (iii), (iv) or (v) below are implemented depending on the calculated or measured nitrate concentration. For the purposes of this rule, approved methods shall be:

- *Calculations using either the most recent version of the OVERSEER® model or the most recent version of the Soil Plant Atmosphere Model (SPASMO); or*
- *Any other method of calculation or measurement approved by the Canterbury Regional Council.*

iii) where the average annual concentration of nitrate nitrogen in the soil drainage water below the plant root zone as calculated in accordance with clause (ii) or measured, for the property exceeds 8 grams per cubic metre, management practices are implemented to reduce the loss of nitrate nitrogen to soil drainage water. These may include but not be limited to:

- *Split applications of fertiliser*
- *Timing of fertiliser application to plant growth*
- *Avoiding application of fertiliser to saturated soil*
- *Avoiding applying fertilizer when the soil temperature at 10 cm depth is less than 10°C*

iv) where the average annual concentration of nitrate nitrogen in the soil drainage water below the plant root zone calculated in accordance with clause (ii), exceeds 12 grams per cubic metre of nitrate nitrogen:

- *Nitrification inhibitors, winter cover crops, or appropriate technology or management practice, implemented to reduce the loss of nitrate nitrogen to soil drainage water.*

v) where the average annual concentration of nitrate nitrogen in the soil drainage water below the plant root zone calculated in accordance with clause (ii) or measured, exceeds 16 grams per cubic metre of nitrate nitrogen:

- *The average total nitrogen (fertiliser and effluent) application to that property is limited to 200 kgN/ha/yr.*

h) That the following records are kept for each property and made available to the consent holder, in a form that is suitable to be made available to Canterbury Regional Council on request:

i) Timing and rate of inorganic fertiliser applications;

ii) Timing and rate of nitrification inhibitor applications;

iii) Stocking rates (number and type of animals) on an annual basis; and

iv) Land uses, including timing and type of cultivation activities.

44. Given the above, I believe the impact on groundwater quality from nitrate contamination will not be significant.

45. I will not address the matters raised by Mr Hanson in his section on the Effects on the Christchurch Groundwater System, as I believe this is adequately covered in my Brief of Evidence – Section 2.

REPORT OF MAURICE J DUNCAN

46. In general I have little to comment on in relation to Mr Duncan's hydrological assessment, in that his results and mine are very similar.

47. In paragraph 11 Mr Duncan states he has found it difficult to determine the exact regime proposed by CPW for the takes from the Waimakariri River. I have explained this in detail in my Brief of Evidence – Section 1 and will not repeat that here. In paragraph 14, Mr Duncan has confused the definition of summer as used in the Golder Associates report with the irrigation season. Mr Duncan is correct in noting that irrigation commences during September or October, however the use of the summer period by Golder Associates refers to the season, rather than the period over which irrigation occurs. I have supplied my full irrigation take data to Golder Associates and therefore they have not assumed irrigation only occurs during the summer period as defined in their reports.

48. Mr Duncan has reached the same conclusion as me in his paragraph 20, that the larger the take, the shorter the time of the take as expressed in my paragraph 158. In Mr Duncan's paragraph 22 he presents data showing that flat-lining of the Waimakariri River increases. He states that it is clear that there is a significant increase in the flat-lining of the river flows. Mr Duncan's data shows the total length of time that the river will be at minimum flow will increase from 10% to between 17 – 19%. However this data does not present any information on the length of each individual event as discussed in my paragraph 140. My analysis shows that there will be a substantial increase in the number of short duration events but not a significant increase in the longer term events. Therefore even though the river will be on average at the minimum flow for longer, the individual events are not dissimilar to those that occur now without the CPWES take.

49. Mr Duncan in his assessment of the effects on the Rakaia River has made a number of assumptions that differ slightly from mine. He has modelled the period December 1978 – April 2007 compared to my period of June 1967 – May 2001. This may have resulted in slight differences, but I do not consider these to be significant.

50. Mr Duncan has also assumed that if water is available to CPWES and ACWT, then it will be taken. I have used a demand for CPWES based upon what the scheme requires for irrigation and storage purposes, although in my base case scenario, no winter water is taken from the Rakaia to replenish storage, as all that water comes

from the Waimakariri River. In my modelling, if water is not used by CPWES, then it will be used by ACWT. This will result in a slight difference between the flow duration curves presented by Mr Duncan in Figures 12 and 13 for the CPW and CPW+ACWT curves. I also note that ACWT's water if used for hydro is returned to the river and therefore Mr Duncan's assumptions that all takes apply to that reach of the river between the ACWT intake and the hydro discharge, is not strictly correct. However again I do not consider this to be significant.

51. The ecological significance of the data presented by Mr Duncan will be commented on by others, as will the effects of the bywash flows and sediment transportation issues.

REPORT OF DAVID M SCOTT

52. I have only one point to comment on in relation to Mr Scott's report. In his paragraph 56 he refers to the Groundwater Protection Zone proposed in Variation 6 of the Proposed Natural Resources Regional Plan, notified on 28 July 2008, in that it overlaps with the scheme area. The consequence of this is to make the application of water to land for irrigation a non-complying activity. From the effects perspective I have no concerns in relation to the effects of such an application, however from the consenting perspective it could lead to the interpretation that the entire activity of using water for the CPWES becomes non-complying. CPWT and CPWL have therefore decided to remove that portion from the Scheme Area. I will include a definition of the Scheme Area within any revised conditions I present, with reference to the Central Plains Water Enhancement Scheme Overview Map 1.

CONCLUSIONS

53. Mr Hanson's report identifies the "considerable uncertainty" in the modelling approach I have used. I believe that given there is uncertainty in all environmental assessments, this methodology is particularly strong in how this is addressed. The fact that something is uncertain, cannot be used to draw the conclusion that the effects will be greater than predicted. This is flawed logic.

54. Mr Hanson's report suggests that the input data used are not conservative and he cites literature with higher leaching losses from pastoral systems. However the literature cited relates to land use practices that are not applicable to these applications due to flood irrigation and excessive nitrogen fertiliser use, and therefore his conclusion is not valid.

55. I have accepted that there will be increases in the concentration of nitrate in the groundwater of the central plains, including deeper aquifers, however my conclusion that the nitrate concentrations will not change to any significant extent still stands.

56. The fundamental mitigation effect associated with this proposal is that while land use will intensify and nitrate loss from land will increase, so too will the volume of water entering the aquifer systems. This offsets increases in nitrate concentrations.

57. Mr Hanson has not raised any matters that alter my views in relation to Christchurch City's water supplies and there is no significant risk to its groundwater sources.

58. Mr Duncan's hydrological assessment is very similar to mine and I have no significant concerns relating to it.

59. Mr Scott's report has raised the issue of irrigation within a groundwater protection zone that has resulted in a change to the definition of the scheme area.

CJM Tipler