

## REBUTTAL EVIDENCE OF RICHARD SEATON – INDEX

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**BOARD OF INQUIRY**  
**HAUĀURU MĀ RAKI WIND FARM PROPOSAL**

In the matter of the Resource Management Act 1991

And

In the matter of resource consent applications by Contact Wind Limited in respect of the Hauāuru mā raki Wind Farm Proposal

And

In the matter of notices of requirement and a resource consent application by Contact Energy Limited for transmission infrastructure related to the Hauāuru mā raki Wind Farm Proposal

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**REBUTTAL EVIDENCE OF RICHARD SEATON**

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**Counsel Instructed:**  
**Trevor Robinson**  
Wellington

Tel 64-4-472 1755. Fax 64-4-472 1766. PO Box 8018 Wellington  
[trobinson@xtra.co.nz](mailto:trobinson@xtra.co.nz)

**Solicitor Acting:**  
**H Rosemary Dixon**  
Contact Energy Limited

Telephone 64-4-462 1284. PO Box 10742, Wellington  
[rosemary.dixon@contact-energy.co.nz](mailto:rosemary.dixon@contact-energy.co.nz)

## **Introduction**

1. My name is **Richard Seaton**. I am an **ornithologist** currently employed by **Golder Associates (NZ) Limited** based in Palmerston North.
2. I refer the Board of Inquiry to the statement of my qualifications and experience in my evidence in chief. I reaffirm my commitment to comply with the code of conduct for expert witnesses in the Environment Court.
3. The purpose of this brief of evidence is to respond to the evidence of Steve Percival, John Dowding, Phil Batley and Lawrence Barea (on behalf of the Director General of Conservation) relating to avifauna at the proposed Hauāuru mā raki Wind Farm.

## **Response to the evidence in chief of Steve Percival**

4. At paragraph 19 of his evidence Dr Percival states that the lower numbers of birds recorded (in Kessels & Associates 2009) as passing over the southern end of the site, compared to the north, may indicate methodological issues between the sites rather than a real difference in numbers. Although I accept that this is possible, to my knowledge this is not the case and I have seen no evidence to support Dr Percival's proposition.
5. Assuming that the same number of birds passed over the north and south ends of the site has implications for the results of the analyses presented later in Dr Percival's evidence and for the conclusions reached in Dr Dowding's evidence. If, as observed to date, there truly are a lower number of birds passing over the southern end of the site then the number of birds at risk would be lower in that area (and therefore for the overall site). Although I acknowledge that assuming a constant risk over the entire site might be tested on a precautionary basis, in my opinion the results and conclusions need to be treated with caution as they are based on a worse case scenario. Assuming a worse case scenario has the potential to compromise the assessment of whether suitable avoidance, remediation and mitigation strategies are practically possible.
6. At paragraph 24 of his evidence Dr Percival states that "Given Dr Dowding's position, the presence of this species (NZ Dotterel) in the area and its threatened status, I would consider it appropriate to include it in the proposed measures for avoidance and mitigation." Considering the information on movements and the threats to the local and national population of NZ Dotterel provided by Dr Dowding (paragraphs 26, 27 and 28 of his EIC) I agree with this recommendation. I further recommend monitoring the local resident population (currently two pair) using radio transmitters. The results of this monitoring should be used to determine whether mitigation is required, and if so, what action might be appropriate.

7. At paragraph 27 of his evidence, Dr Percival asserts that two years post construction collision monitoring is too short a time period and makes a recommendation that a monitoring program of 6 years spread over 15 years would be more appropriate (in years 1,2,3,5,10 and 15 after commissioning of the wind farm). His recommendations contrast with those of Dr Dowding (paragraph 91 of his EIC) who suggests 10 years of post construction monitoring is required, whereas Dr Barea (paragraph 63 of his EIC) recommends that a minimum of 3 years monitoring is required. Currently the suggested consent conditions provide for 2 years monitoring at each wind farm cluster following its completion, with a further 2 years monitoring of the whole wind farm once complete and commissioned to pick up any cumulative effects. Given that the wind farm is proposed to be constructed and commissioned in stages over a 2-3 year timeframe, this is effectively 4-5 years of collision monitoring.
8. The reasoning put forward by Dr Barea (paragraph 63 of his EIC) for a minimum of 3 years monitoring is “because the second year provides for a replicate to compare with the first year and the third provides for a level of confidence around variation in the data between the first two years.” Due to the need to sample a variety of potential conditions a third year of monitoring may well be required. In my opinion the decision of whether to proceed with a further year, or a number of years monitoring, would be best assessed by the Ecology Peer Review Panel using the results from the first two years of monitoring following commissioning of the final stage of the wind farm.
9. In his evidence (paragraph 30 and further outlined in paragraph 76) Dr Percival states that “a package of measures....and better baseline information to inform these measures” is required to avoid, remedy and mitigate any effects on migratory shorebirds. I concur with Dr Percival's opinion (and as Dr Percival notes I have provided a list of potential options that could be used to achieve this in Section 12 of my EIC). To clarify, it was not my intention to imply that any one option would likely be sufficient or appropriate as a single means of avoiding, remedying or mitigating any effects.
10. In his evidence (paragraph 31), Dr Percival notes that it is important that, should bright white and navigational lights, be used on turbines that where practical they are shielded to avoid birds being attracted to the wind farm at night. I agree with Dr Percival's opinion.
11. At paragraph 48 of his evidence Dr Percival notes that “The principle that I have used in my analysis has been to work from the best available data, but make reasonable worst case assumptions where there are data gaps.” Considering the level of data available this seems a reasonable course of action in principle, but as such it must be noted that the results and consequent conclusions must therefore be viewed as a worst case scenario

which in my opinion is likely to overstate the actual level of effects to some greater or lesser degree.

12. At paragraph 50 of his evidence, Dr Percival states that he used the Band Model to undertake a collision risk assessment of shorebirds at the HMR wind farm. Considering that this approach has been used to assess the risk of collision strike at wind farms around the world, and that it is generally accepted as the standard approach to providing a quantitative analysis of risk of bird strike at wind farms, I agree that this is an appropriate analysis to use, while taking into account the limitations of that model. As Dr Percival identifies (paragraph 61), the parameter the Band Model is most sensitive to is the avoidance rate and without detailed data from operational wind farms the decision of what level of avoidance is appropriate to use in the model is an essentially subjective one. I comment further on the choice of avoidance rates in paragraph 13 of this evidence.
13. At paragraph 57 of his evidence Dr Percival, when describing the parameters used in the collision risks analysis, uses 27% as the proportion of migrating shorebirds likely to pass over the proposed HMR wind farm site during migration and 54% as the proportion of these that cross at the rotor swept height. It should be noted that, in the report by Kessels & Associates 2009 of their monitoring study (relied on by Dr Percival and produced by Mr Kessels with his rebuttal evidence), of the 27% crossing over land, 37% were observed over valleys, not ridgelines. Dr Percival does note this later in this paragraph, but asserts that birds flying over valleys may also fly over ridgelines so he has considered all 27% as potentially at risk. In my opinion, this is a worst case assumption, as a proportion of these birds will not in fact fly over ridgelines and therefore will not be at risk. More detailed analysis of the Kessels & Associates data would enable a clearer picture of what proportion of birds only flew down valleys and is recommended to give a more accurate assessment of collision risk.
14. At paragraph 59 of his evidence Dr Percival notes that the lack of multiple years of monitoring does not account for any slight changes in migration path that may result in larger numbers of shorebirds passing over the site in future years. I agree that without further monitoring it is not possible to say whether the numbers of birds flying over the site recorded in the study by Kessels & Associates 2009 are representative, or indeed, whether the routes they take through the area every year are consistent. However, in my opinion the Kessels & Associates study has provided suitable data from which a preliminary assessment of risk can be undertaken. Dr Percival has used a selection of the data to produce such a preliminary assessment in his EIC. I would emphasise, however, that any quantitative assessment at this point is necessarily preliminary and I do not believe that it is appropriate to seek to derive any assessment of required mitigation from it. Rather (as the proposed consent conditions suggest), this exercise should be

undertaken when the data set has been extended to include a further 3 migration surveys and the complete dataset can be analysed.

15. At paragraph 64 of his evidence Dr Percival outlines the avoidance rates that he used in the collision risk analysis (95% for the northward migration and 90% for the southward migration). Considering the paucity of data available from operational wind farms in New Zealand on avoidance rates and the lack of information on avoidance rates of large numbers of migrating shorebirds these avoidance rates represent, as Dr Percival acknowledges, a precautionary assessment of risk to migratory shorebirds at this site. It is therefore worth noting, that the actual avoidance rates could potentially be higher, (especially considering that empirically established avoidance rates are typically higher than 99 % and the recorded levels of shorebird mortality at wind farms overseas is generally low) and that again this appears to represent a worst case scenario assessment.
16. In his evidence (paragraph 70) Dr Percival notes “The collision risk (from Dr Dowding’s evidence) is of such magnitude that it could result in significantly reduced populations. To manage this risk therefore would need avoidance, remediation, or mitigation measures that reduced that collision risk to an acceptable level.” If the worst case scenario results Dr Percival provides are verified by further pre-construction monitoring and analysis, I agree that the collision risk would need to be reduced to an acceptable level or suitable mitigation measures put in place. I further expand on this in my rebuttal of Dr Dowding’s evidence below.
17. At paragraph 82 of his evidence, Dr Percival concludes by stating that “...on the information currently available one simply cannot say whether the risk of the HMR wind farm to birds can be avoided, remedied or mitigated. It is possible that it may, but this can only be concluded with any certainty once an adequate baseline data set has been obtained.” I recommend a monitoring program (comprising 4 seasons of migration) should be completed before the wind farm commences operation for this reason. The conclusions of this monitoring program should be used to develop avoidance, remediation and mitigation packages that are appropriate to address the specific risks identified from this monitoring. I suggest that the Ecology Peer Review Panel oversees this process.

### **Response to the evidence in chief of John Dowding**

18. In paragraph 60 of his evidence, Dr Dowding states that at paragraph 3.5 of my EIC I am “premature to conclude that significant effects on resident shorebirds are not, however predicted.” In paragraph 9.7 of my EIC I state (in relation to NZ dotterel) that “Currently the data is not available to definitively state that there is no risk, but indications are that overall the risk is low.....” However, in light of the evidence Dr Dowding provides on NZ dotterel (paragraph 27, 28, 45, 54 & 55) I agree that in drawing my conclusion there was a

lack of information on which to make this statement. Clearly the ability to make a definitive conclusion either way remains difficult due to a low level of detailed movement data on this species (as stated by Dr Dowding in paragraph 22 of his EIC) and further monitoring of this species is required. I discussed this point above in the context of paragraph 24 of Dr Percival's evidence.

19. In paragraph 62 of his evidence Dr Dowding asserts that (in relation to the assessment of the risks to migratory shorebirds) "collision risk modelling is essential in this case, rather than useful." In paragraph 71 Dr Dowding further asserts that "it is my firm opinion that some attempt must be made to assess risk before a decision is made on the granting of consents." I agree that before an appropriate avoidance, remediation and mitigation package can be developed further collision risk modelling is required, based on a more robust data set.
20. In paragraph 63 of his evidence Dr Dowding suggests that "there is no guarantee...that the predicted impacts could actually be effectively avoided, remedied or mitigated..." Dr Percival in paragraph 76 of his evidence also suggests that "There remains...a possibility that the level of mitigation required...might not be achievable." These concerns are largely based on the ability to effectively mitigate a worse case scenario (because birds that fly along valleys are included in the numbers of birds at risk, the numbers of birds traversing the site are assumed to be the same at every turbine cluster and because precautionary avoidance rates have been used in the Band model). Such worst case scenarios are not borne out by observations to date. Dr's Dowding, Percival and Battley have commented that further data on birds traversing the site is required in order to validate the results of the risk assessment. I do not disagree with the use of a worse case scenario in order to investigate the potential risk and effects to these species. However, it must be noted that in doing so the predicted mitigation requirements may be larger than are actually necessary. Because of a lack of data on resident and migratory shorebird movements in the area, further investigations are required to more definitively determine the risks, and therefore the appropriateness of any avoidance, remediation or mitigation strategies.
21. In paragraph 76 of his evidence Dr Dowding states that because he is "not convinced that the measures proposed to collect further data will necessarily provide all the information required...I suspect that some of the uncertainties that are inherent in Dr Percival's modelling will still be present in two years, and that many of the assumptions will need to be made when the data are analysed at that time." Although I agree that many assumptions will still need to be made in the future, in my opinion all models require some assumptions to be made, and additional data will reduce the uncertainties attached to such assumptions.

22. It is commonplace for assumptions to be made in order to avoid the need for long-term data sets to make informed decisions; indeed this is the value of modelling. More accurate information on the pathways that migratory birds take over the site will allow a more informed analysis to be undertaken including an assessment of the collision risk posed by each cluster of turbines.
23. In paragraph 77 of his evidence Dr Dowding indicates a requirement for further data to be collected on the risk to resident shorebirds. Considering the paucity of data currently available and the potential risks that Dr Dowding outlines to resident shorebirds throughout his evidence, I agree with Dr Dowding. Although I agree there will be difficulties in collecting this data, I believe with appropriate observer locations, this data can be collected during periods of migratory shorebird monitoring. Further, more intensive monitoring of New Zealand dotterel movements will also realise better data for analysis.
24. In paragraph 90 of his evidence Dr Dowding suggests that collision monitoring during the operational phase of the wind farm would be essential, and I agree with his opinion.
25. In his evidence (paragraph 91) Dr Dowding further notes that collision monitoring for migratory shorebirds is essential. I agree with Dr Dowding and have suggested (paragraph 11.3 of my EIC) that collision mortality monitoring every year should coincide with periods of greatest bird migration.

#### **Response to the evidence in chief of Philip Battley**

26. In paragraph 75 of his evidence Dr Battley highlights that the proposed further monitoring of internal migrants will not coincide with godwit and knot migration and therefore will not provide “new information on migration-related movements of godwits and knots...from which to assess risk of turbine strike.” I agree with Dr Battley’s opinion. He further states in paragraph 91 that “There is inadequate information on which to assess the true risk...in particular, the numbers of birds...and the flight paths (at HMR).” I also agree with Dr Battley’s opinion. And in paragraph 92 Dr Battley, recommends that “winter shorebird monitoring be extended through September and October, specifically to evaluate the numbers of godwits and knots that pass through the wind farm site on southward migration.”
27. However, in paragraph 93, Dr Battley expresses that “there is concern about the ability of any monitoring to adequately detect flocks of godwits and knots making local movements...” Considering this, it may be more useful to carry out further risk assessment of godwits and knots using more detailed data on SIPO as an indicator of the likely routes taken by these species through the site (rather than extending the monitoring of migratory birds to include the godwit and knot migration). Support of a complimentary

research program which aims to describe the internal and international movements of these species would give more information on the routes that these birds take and may be a more appropriate way to further assess the risk to these species. The relative benefits of these options should be further explored by the Ecology Peer Review Panel as further information is accumulated and a decision made accordingly.

28. In paragraphs 83 and 84 of his evidence Dr Battley highlights the fact that the predator control proposed as mitigation for internal migrants is not an option for mitigating any effects on international migrants that breed over vast areas overseas. I concur with this view.
29. In his evidence (paragraph 85 & 97) Dr Battley states that “research aimed at identifying key stopover sites used on migration would probably do more to mitigate any losses in New Zealand in the long term.” Considering this, I agree that support of a research project that provides an understanding of stopover sites would be appropriate for inclusion in any mitigation package that aims to mitigate risks to godwits and knots.
30. In paragraph 96 of his evidence Dr Battley further proposes that the protection and improvement of high tide roosting options for Arctic waders may be an option to aid the mitigation of any effects of the proposed wind farm. I concur that this option should be added to any mitigation package as although it may not be possible to quantify its effect it is likely to improve the overall population fitness of these and other species of shorebird.

#### **Response to the evidence in chief of Laurence Barea**

31. In paragraph 38 of his evidence Dr Barea disagrees with my assessment that the effects of the wind farm on wetland birds is minimal (paragraph 7.9 of my EIC). Considering that the issues surrounding wetland birds are largely concentrated within Punga Punga wetland, and that these will be largely avoided by fitting line deflectors, restricting construction activity during the breeding season, and mitigated by habitat enhancement I remain of the view that overall, any effects on wetland birds at the HMR site will be minimal.
32. In paragraphs 48 and 49 of his evidence Dr Barea discusses the need for collision risk modelling of Tui and Kereru. Due to the relatively small number of individuals involved (in comparison to the numbers of migratory shorebirds), and the well documented threats of habitat loss and introduced predators to these species, it should be possible to come to an agreement that provides a suitable mitigation package that covers a worse case scenario for these species without this. Nevertheless, I do agree that without appropriate collision risk modelling it is difficult to, as Dr Barea states, “objectively consider what measures would be required to appropriately avoid remedy or mitigate”. As a result, I concur that a

further year of monitoring of these species should be completed pre-construction (as recommended in the proposed consent conditions) and collision risk models should be developed from the results of this monitoring to enable a more informed decision as to the appropriateness of any mitigation strategy.

33. In paragraph 51 of his evidence Dr Barea suggests extending the area of bird deflector flagging on transmission lines out over a buffer either side of the bush. I agree that this would be an appropriate precautionary measure to take. Dr Barea makes no suggestion of how long such a buffer should extend beyond the bush edge and recommends that the “ultimate placement including the length of buffer should be made after recommendation by the Ecology Peer Review Panel...” Considering that each line will need to be assessed on a case by case basis, I agree with this recommendation.
34. In paragraph 52 of his evidence Dr Barea recommends “that falcon surveys include all of Te Tehe Bush...because Te Tehe Bush is the largest potential nesting habitat in the vicinity of the HMR site...currently the 2km survey buffer...includes (only) half of Te Tehe Bush.” Considering the information that Dr Barea provides on Te Tehe Bush, in my opinion including this area in the staged approach to monitoring of New Zealand falcon is warranted.
35. In paragraph 54 of his evidence Dr Barea agrees with my recommendation that radio-telemetry should be used to assess collision mortality of New Zealand falcon. He further recommends that this should include any falcon’s located breeding within Te Tehe Bush. I agree with this recommendation.
36. In paragraph 55, 56 and 57 of his evidence Dr Barea agrees that a buffer of construction activity around active New Zealand falcon nests is required to avoid disturbance. However, he disagrees that 200 m is a sufficient buffer, citing a publication in which I stated that falcons can respond to people approaching their nest sites up to 400 m away. Indeed, in some instances the distance that they will respond is even greater than this. However, research that I carried out for my PhD in pine forests showed that if mechanical operations in pine forests were restricted from operating within an area of 200 m from an active nest, no negative effects on falcon breeding success were recorded. As a result, I maintain that 200 m is an appropriate buffer distance to maintain around active falcon nests, should they be located.
37. In paragraph 58 of his evidence Dr Barea recommends that the suggested buffer of activity around Punga Punga wetland be increased from 100 to 200 m. Although I agree with the statement that Dr Barea makes over the uncertainty in the distance required to ensure bittern are not disturbed, it is my opinion that increasing the buffer to 200 m is unlikely to significantly reduce disturbance any further and is therefore unnecessary.

Additionally, considering the temporary nature of the construction activity coupled with the habitat enhancement proposed, it is my opinion that any effects of construction activity should easily be remedied and or mitigated for under the consent conditions already proposed.

38. In paragraphs 59 -61 of his evidence Dr Barea suggests revising the wording of the proposed consent condition (6.9 of Mr Daysh's Exhibits SGD1 and SGD2) from, "A collision carcass retrieval team established to undertake grid searches of a statistically robust and representative sample of turbines..." to "the collision monitoring programme be designed to be statistically robust..." The key difference here is the lack of the inclusion of the phrase "representative sample of turbines". Dr Barea further asserts this in paragraphs 61 and 62 of his evidence stating, "searches of a statistically robust sample of turbines...assumes that the level of risk is equal for all turbines which is unlikely to be correct." I concur that the risk level is unlikely to be equal for all turbines or even between turbine clusters but disagree that every turbine site must be monitored.
39. In Dr Dowding's evidence (paragraph 90) he points out the practical problems with searching all 180 proposed turbines regularly. As a solution, Dr Dowding proposes that as large a sample as possible should be sampled and that the monitoring should be rotated through the site to include all turbines. Dr Barea suggests in paragraphs 63 and 64 that the monitoring and collision risk assessment (discussed above in paragraph 31) could feed into a collision risk program and aid the identification of a representative sample of turbines.
40. As discussed above, currently the suggested consent conditions provide for 2 years monitoring at each wind farm cluster following its completion, with a further 2 years monitoring of the whole wind farm once complete and commissioned to investigate cumulative effects. I suggest that a sample of turbines be monitored within each cluster for the first two years of operation and the results of this monitoring be used to guide the development of a robust monitoring program for the whole site for two years once operational. It is my opinion that the decision on how to achieve collision monitoring of the whole site effectively on the ground, within the limits of what is practical, is a matter of detailed survey design best handled by the Ecology Peer Review Panel after the initial monitoring of each separate turbine cluster have been completed. Dr Barea seems to agree with input from the Ecology Peer Review Panel, in the consent condition he proposes: "the design, implementation and analysis of any collision risk modelling and collision risk monitoring programme be supervised by the Ecology Peer Review Panel" (paragraph 60).

41. In paragraph 59 of his evidence Dr Barea also suggests the inclusion of the phrase “including adjustment for scavenger removal rates and searcher efficiency based on site-specific trials.” It is my understanding is that this is already provided for under 6.9.a.v of the proposed consent conditions (Mr Daysh’s Exhibits SGD1 and SGD2).

**Richard Seaton**