Dear Editor,

Following your decision on our manuscript untitled *"A native beetle fond of exotic plants. Characteristics that contribute to invasive success in Costelytra zealandica (Scarabaeidae: Melolonthinae)*", we have responded to each reviewer's comments and addressed their concerns. The comments were very constructive, and, we believe they have contributed to improve our manuscript. We hope that that these revisions will make this manuscript acceptable for publication in *PeerJ*.

We have sought for some additional help from a colleague expert in GLM to re-analyse of some of our data and we would like her to be a co-author in this manuscript.

Please find below our answers (in blue) to the four reviewers' main comments and suggestions.

Reviewer 1 (Warwick Allen)
Basic reporting

• Line 27: For "feeding preference" and "fitness performance", some idea of the variables measured should be used, especially as feeding preference and actual fitness were not measured, rather surrogate variables. Something like "(olfactometer choice)" and "(third instar weight increase)" would suffice. Details of the variables measured appear in lines 29-32 in the abstract. However, in order to clarify what we did, we have incorporated additional information mentioning the use of an olfactometer device to test the feeding preferences of the insect (see line 31) and the life stage (i.e. third instar) of the larvae we used (see line 32).

• Lines 33-37: I think the results part of the abstract could be stated more clearly and completely. Information is missing regarding the preference experiment. I would modify to something like: "Costelytra zealandica, when sampled from exotic pastures, showed higher preference and performance on exotic plants. In contrast, C. zealandica sampled from native grasslands did not perform significantly better on either host and showed similar feeding preferences to C. brunneum, which exhibited no preference." Done (see lines 34-39).

• Line 79: Like in the abstract, I think the terms feeding preference and fitness response need to be more explicitly outlined by mentioning the actual variables measured. Done (see lines 83-84).

• In general, more detailed and interesting information could be provided in the introduction, especially as the paper is relatively short. Broadly, a greater discussion of the background literature informing the theory/hypotheses the objectives are based on would be useful – consider including your predictions based on the theoretical expectations. In a more narrow sense, quite a lot is

known about the study species, so this information should be included with a bit more detail, perhaps with a particular focus on the authors' prior work (i.e., Lefort et al. 2014, 2015, etc.) and basic biology/ecology (i.e., relationship with pathogens or other microbes). We tried to avoid incorporating general information about the biology of the insect that were not going to be relevant and constructive for the manuscript. We decided to specifically point to the facts that were relevant to our study (i.e. (1) the invasive status of the insect and as such (2) its feeding behavior and (3) distribution in New Zealand (see lines 68-82).

• Lines 89-90: Can you provide references here? This entire sentence is rather clumsy, consider rewriting The sentence has been rewritten which has improved its clarity. Unfortunately, no reference could be given here, since this is a knowledge only shared verbally among scientists studying this species, and nothing has been published so far on the subject. A PhD candidate at Lincoln University (New Zealand) is currently trying to develop a rearing method to successfully produce large numbers of adults' beetles under laboratory conditions.

• A simple table summarizing the details of each collection site/species combination would be useful, appropriate, and would lead to very quick understanding of the experimental design and main results. Consider including: 1) location, 2) dominant plant species at each site, 3) Costelytra species collected, 4) feeding preference, 5) survival rate and 6) mean weight increase on each different food source. Done. We have now included a table in the Material and Method section, which provides information on the location of the sampling sites (including coordinates), a description of the sites and of their dominant groups of plants and the *Costelytra* collected. Survival rates and mean weight increases are provided in Figure 3 and 4, and we thought that it was unnecessary to re-iterate this information in a table.

• Lines 104-105: "given the very low mobility of the earliest larval stages in Costelytra spp." – references must be provided for statements such as this. Done.
• Line 163: Give all appropriate statistical results, including ones which show no difference/preference. Done (lines 164-168).

• Lines 166-169: What were the survival rates for the larvae from population C (native grasslands)? Report these. Provide the statistical results for population C. Also, did the overall survival and performance of larvae differ between sites A and C? As mentioned in the methods section, a two-way ANOVA may be a more appropriate analysis than a t-test. As suggested by reviewer #3 and in order to take into consideration the possible effect of the initial weight of the larvae, we have performed an ANCOVA using this variable as covariate.

• Lines 171-175: Present means, standard errors, and effect sizes to go along with the stats. Since these descriptive statistics are already summarized by Figure 4, we decided not to overload our manuscript with too many tables, and only added one table presenting the results of the ANCOVA analysis (see table 2, line 350).

• Can the C. zealandica host races be identified genetically or morphologically? We would like to insist on the fact that the study presented in this manuscript support the possibility of the existence of host races in *C. zealandica*. The actual existence of the host-races themselves still has to be shown. To confirm this, a larger project, involving genetic analyses, targeting several genes, of a large number of *C. zealandica* populations is currently underway. Differences in size have been observed between the North and the South Island adult beetles, but no morphological differences between *C. zealandica* occurring on different host plants have been observed. Yet, it is too early to say whether the genetic makeup of the different host-races –if any- will matched the host-specific morphological characteristics.

• Using colours in the figures makes things difficult for people who may be colourblind – try using simple patterning or shading for figures 2-4. We submitted our figures to a color blindness simulator (http://www.etre.com) to ensure that the color we choose were distinguishable from one another by colorblind people. The colors we chose are differentiable by people affected by protanopia, deuteranopia or tritanopia, the three colorblindness conditions.

• Figure 2: Further to my previous point, it is very difficult to distinguish the different treatments in the pie charts. There appears to be a transition of some sort between native and invasive choices, rather than showing the exact proportions. This transition in color within the pie chart is used to illustrate the actual statistical investigation performed; were the choice of a plant (either native or exotic without distinction) was first assessed against the non-choice. The exact proportion of native vs exotic is detailed in the bar graph attached to this “plant choice” section of the pie chart.

• Figures 3 and 4: Ideally, the figures would be self-contained and able to be fully interpreted without the caption. Hence, the x-axis label should reflect if the source population was native or invasive, and bar colour should be included as a figure key/legend. Done.

• Figure 3, Line 343: In the methods and results, measurements from week 6 were used. Both sets of data should be included in the analyses and presented in the paper. We do not quite understand the request of the reviewer. The entire raw data set is available (with both information on larval mortality after 15 weeks and their weight at week 0 and 6, used to calculate their average weight gain) as supplementary material. There was no weight measurement after week 6.

• Figure 3: 95% confidence intervals could be bootstrapped to use as standard error bars. We do appreciate the reviewer suggestion, but considering that we used conventional and suitable error bars (SE bar) and that this was only a suggestion by the reviewer and not a requirement, we decided to keep it as it is since it doesn’t affect the correctness of the figure. However, we noticed that instead of indicating 1 SE in the legend as represented on the figure, we only indicated SE. This has now been rectified.

Experimental design

• Line 111: Given temperature is important for Costelytra spp. development and fitness, what were the average environmental conditions in the glasshouse? None of the experiments were performed in the glasshouse (only the plants were grown there – see experimental temperature line 146). However, we realised that we failed at providing this information for the choice test. This has now been rectified (see line 119).

• Lines 121-126: This is hard to visualise. A diagram showing how the olfactometer was set up may be useful. We understand the reviewer's concerns and the fact that, as assumed by the VARK model, some persons are more visual than others. Nevertheless and as much as we would like to meet the needs of every readers, we do not want to unnecessarily overload the manuscript with figures that aren't strictly necessary. We are happy to provide a diagram as supplementary material if needed, but we do not think that such a diagram needs to be included in the manuscript, as it is not the most current practice to display figures of simple experimental settings in papers.

• Line 130: What is the rationale for using 5 larvae at a time? Could this have had an effect on the results? This was the closest representation of the natural clustered distribution of the larvae observed in the field. This was also the best compromise to achieve a large number of larvae tested. Considering that each trial lasted for 24h, the use of only one larva per trial would have ended up in 35x4x24 hours = 140 days of experimentation, which (1) would have been logistically impossible and (2) would have resulted in the use of larvae of different age/size/instar, thus undoubtedly creating an experimental bias. This information now appears in lines 128-129 of the manuscript.

• Lines 142-147: Is it hard to grow Costelytra spp. in the field or in pots? Replicating the experiment in this way would be more similar to field conditions. Alternatively, could you weigh larvae collected at the end of the growing season from areas dominated by native and exotic hosts? This would make the study more robust and allow for greater inference, given the results are no longer restricted to a simplified laboratory experiment. The advantage of performing a lab experiment is that we can easily avoid unwanted confounding effects. In New Zealand, *Costelytra zealandica* has more than 50 known natural enemies, many of which could be virtually impossible to control in an in-situ experiment and could greatly affect its outcome. Weighting the larvae only at the end of the season wouldn't have provided us with the information required, as there is a weight threshold that all third-instar larvae must meet before pupating. Hence, (1) all the larvae will have reached this threshold at the end of the larval growing season, (2) it would be impossible to actually determine the exact "end of the growing season" as it will vary per region/altitude/annual climatic variations, and, (3) this would imply growing the different populations under different conditions (soil, altitude…) which are also likely to effect the length of the larval stage.

• Lines 142-147: Why was the experiment not also replicated with C.

brunneum? It would help demonstrate any differences in C. brunneum development on native or exotic species. Could additional Costelytra species also be used (see Given 1966)? Some further explanation or justification should be given. We do completely agree with the reviewer that it would have been wiser to also incorporate *C. brunneum* in the feeding experiment. However, as in many experimental settings involving elusive species, we were confronted with a shortfall in individuals and chose to use them in the host choice experiment. As suggested by the reviewer, we could also have used another *Costelytra* species, but all the species of this genus apart from and unlike *C. zealandica* are very elusive and difficult to locate at the larval stage.

• Lines 150-152: Was the weight also measured at the end of 15 weeks, which is the usual length of the third instar as mentioned by the authors? It would seem this final fitness measurement (or even dry weight of adults) is more appropriate, especially given this was when larvae survival was quantified. If this data was recorded, then it should be included in the analysis and results. If not, rationale should be provided in the paper as to why it was not measured or used. In its current form, it is very confusing as to what data was used for the analysis. The weight of the larvae were recorded after 6 weeks of treatment, and the rationale for this and as explained in line 142 of the manuscript is that it corresponds to the weeks of most intense feeding for the third instar life stage of this species. There was no weight measurement after week 6. The overall mortality rate was assessed after 15 weeks of treatment, representative of the average length of the third instar stage of *C. zealandica* as stipulated in line 149. We have revised this paragraph in order to make it clearer (see lines 147-152).

It is explained in line
• Line 152: Using a balance? To what level of error? Done.
• Line 154: How was average larval growth calculated? Include equation. We have added some information in lines154-156, however we felt that it was a bit redundant to add an equation as we assumed all the potential readers for this manuscript will know how to calculate a mean.
• Line 155: Looking at raw data, it seems there are a number of larvae which survived but did not increase in weight at all (some even decreased), implying they had completed development upon collection. What would be the biological rationale for this data to be included? We do understand the reviewer concerns, and now discuss this point in lines 189-194 of our manuscript. We would like to indicate that this (survival but no increase in weight) does not necessarily imply that the larvae did complete their development (particularly considering their low weight at week 0). These observations might just be the result of an on-going transition from host-range expansion to host-shift (see Lefort et al. 2014) and the incapability of some larvae to benefit from their host. This is also why it is important to interpret these results in combination with those of the choice test experiment. The observation can also be the result of differential nutritional values in the roots of the plants used, a process that is likely to be observed in heterogeneous soils (Grossman & Rice 2012). Finally, I'm a little concerned that measuring fresh weight is not the same as measuring dry biomass, as fresh weight can be dependent on water content at the time of weighing. Can the authors

alleviate this major concern in some way, such as weighing adult beetles reared on different species, or discussing these limitations in the paper? We understand the reviewer concerns, but the use of such invasive technique (i.e. weighting dry matter) would have prevented us to gather larval mortality data. Given the controlled conditions under which the grubs were grown, the assumption of consistent water content across the population/treatment shall be met.
It is not uncommon to report insect fresh weight and this technique was used previously in *Costelytra* spp.. In 2013, Knapp and Knappova in their manuscript entitled *"Measurement of Body Condition in a Common Carabid Beetle, Poecilus cupreus: A comparison of Fresh Weight, Dry Weight, and Fat Content"* have emphasised that the results obtained by disparate weighting methods are not comparable. Considering that so far in the published literature all the measurements of weight in *Costelytra* larvae in the published literature have been done using fresh weight, it was important to remain consistent so the studies could be compared. We have now highlighted the fact that we have been working with fresh weight in the material and method section (line 147) in order to avoid confusion.
Furthermore, the raw data should be modified to include a column showing the weight increase. We have chosen not to modify our raw data spreadsheet with calculated weight increases as they won't be truly "raw data" anymore.

Validity of the findings

• Line 135: Is a chi-squared test the most appropriate analysis? I believe a better analysis may be using two separate general linear mixed models with 1) choice (plant) vs no choice (control or no choice) and then 2) native vs exotic used as binomial response variables, tested against Costelytra species and source population, with replicate added as a random effect. Done, we have re-analysed our data with GLMs. We did not add in a random effect as there were not enough replicates for the mixed effects model to fit the data. Material and Methods and Result sections have been modified accordingly (Lines 133-138 and 163-168).
Lines 153-154: Again, is a chi-squared test the most appropriate analysis? I believe a better analysis may be a general linear model using a binomial error structure and including source population and diet treatment as interacting fixed factors. Although we respect the reviewer preference for the use of statistical models, the use of the Chi-square is appropriate in this case. Chi-square test can be used to test for the relationship between two categorical variables, in our case host plant and mortality. We choose this statistical test based on published material (see Lefort et al. 2012), where the mortality of the *Costelytra* larvae under different treatments was also assessed. Furthermore, a two-way ANOVA for larval growth would also be more appropriate – a significant interaction would indicate the same result/implications as your separate t-tests, while also controlling for site of origin. As suggested by reviewer #2 and in order to take into consideration the possible effect of the initial weight of the larvae, we have performed an ANCOVA using this variable as a covariate (lines 154-159).
• Data has been made available with submission.
• Conclusions are appropriate but be careful with some implications; the results are not replicated in the field or pots where C. zealandica may feed in

a more natural setting. These limitations should be mentioned as a caveat somewhere in the discussion. Done (lines 259-262)
• Perhaps the authors can expand upon the potential implications of the research a little more. The whole host-race or genotype point is particularly interesting and worthy of further discussion; other species (e.g., Phragmites australis) have shown similar patterns of having invasive and non-invasive genotypes. Done (lines 262-265)

Comments for the author
This interesting study addresses mechanisms underlying invasion success of Costelytra zealandica, a native beetle species which spread extensively and became a pest following the introduction of exotic crop species such as ryegrass and clover. The authors collected third instar larvae of C. zealandica and C. brunneum from areas of native or exotic plants from four field sites to conduct two experiments: 1) an olfactometer choice test (silver tussock, white clover, control, no choice), and 2) a fitness experiment where C. zealandica from native and exotic sites was fed roots of either its native or exotic host species.

In the olfactometer experiment, the authors found that C. zealandica collected from exotic pasture showed a preference for the tube containing the exotic species, whereas C. zealandica and C. brunneum collected from under native plants showed no preference for either native or exotic plants. The second experiment showed that larvae collected from exotic pasture survived and performed better when fed white clover roots than roots of native silver tussock, whereas larvae collected from native sites demonstrated no difference in larval weight increase when fed native or exotic plant roots. These results indicate that there may be distinct host-races of C. zealandica which prefer and perform more strongly on exotic crops, underlying why the species has become invasive on these crops.

Overall, I like the paper. It is clear that a lot of time and hard work has gone into collecting the beetle larvae and running the experiments, which I certainly appreciate. Furthermore, I believe the study is scientifically sound, reasonably well-written, and falls under the broad scope of the journal. Indeed, the use of a native species as an invader alongside a closely-related benign native species is an excellent phylogenetic control, as well as adding to the currently small body of literature on native invaders. Furthermore, evidence for distinct host-races of C. zealandica is an interesting and important result and so should be published. However, I do have a few issues with certain parts of the paper, particularly with completeness of data and results, the statistical approach, and some issues with experimental design (see specific comments). While these revisions will take some time and could be regarded as major, once rectified they will make the paper more thorough and of higher quality. Minor general comments and corrections are as follows:
Title
• Line 15: I would suggest using a colon instead of a fullstop Done
• Line 16: Try using “of” instead of “in” Done
Abstract
• Line 23: “resulted in diet alteration” Done

• Line 26: Consider replacing “new” with “novel” and remove the quotation marks. We replaced new by novel as suggested, but thought it was important to keep the quotation mark, as this was done to point to the fact that somehow these “hosts” aren’t so novel since they have been introduced to NZ quite a long time ago.
• Line 30: “To this end,” and “third instar larvae” Done
• Line 32: Replace “were” with “was” Done
• Line 38: “Intraspecific” and no comma following “variation” Done
Keywords
• Consider adding “grass grub” to keywords Done
Introduction
• Line 55: Consider using novel instead of new, and remove the quotation marks. Done
• Lines 56-57: “Some native species.” Done
• Line 61: “modifications to”. We chose not to revised our text, as it would have modified the meaning of the sentence, which would make the citation of Munoz and Ackerman incorrect.
• Line 64: Why is a native invader “even more so” useful than an introduced invader? Provide a rationale or remove. Done (see line 65-67).
• Line 67: Give common name also, especially seeing as it is a well-known species Done
• Lines 67-69: The sentence is a bit clumsy, try rewriting it Done
• Lines 68-69: Give the scientific names for these crop species Done
• Lines 69-70: The formatting of the references is inconsistent. Here a comma is used before the date of publication, where previously no comma was included. Please check the manuscript for formatting consistency. Done
• Line 73: “are considered to be univoltine (Atkinson…” Done
• Line 81: This is a little broad, modify to just “the invasion process of C. zealanadica.” Done
Materials and Methods
• Line 91: “used for the experiments” Done
• Line 91: Replace “produce” with “rear” Done
• Lines 92-93: “the second best option” Done
• Line 94: “the South Island of New Zealand” Done
• Line 94: Use a colon instead of a comma, i.e. “Costelytra spp.:” n.a. (in-text information have been removed and now appear in a table)
• Lines 95-97: Try “Lincoln (43°64’04’’S 172°47’82’’E), Hororata (43°32’17”S 171°57’16”E), Cass (43°02’10”S 171°45’40”E) and Castle Hill (43°12’20”S 171°42’16”E), as sites A, B, C and D, respectively (Figure 1).” n.a. (in-text information have been removed and now appear in a table)
• Line 101: Remove the word “being” n.a. (in-text information have been removed and now appear in a table)
• Line 103: Remove comma n.a. (in-text information have been removed and now appear in a table)
• Line 107: Insert a space into 15 °C Done
• Line 108: Give scientific name (Serratia spp.) Done
• Line 120: “preferences” Done
• Line 127: Try changing to something more concise like “populations B (exotic pasture, n = 35) and C (native grasslands, n = 35) Done
• Line 132: “contents” Done

• Line 138: “post-hoc” Done
• Line 145: Be consistent with spacing between numbers and units, i.e., “50 g” and “n = 64” Done
• Line 147: Insert a space into 15 °C Done
• Line 154: “Welch’s t-test” n.a. (we replaced Welch’s t-test with an ANCOVA)
• Line 154: delete extra space between “t-test for” Done
Results
• Line 161: Give actual p-value unless < 0.001. Done (GLM results)
• Lines 162-163: Try “C. zealandica (population C) and C. brunneum (Population D) collected from native grassland did not show a preference for either plant species”. We did not perform this change, as we thought is was important to highlight the origin of the larvae.
• Line 165: Use the same header as in the methods section Done
• Line 169: Give actual p-value unless < 0.001 Done (GLM results)
Discussion
• Line 178: “variation” Done
• Line 179: Try “existence of strong intraspecific variation in diet breadth” Done
• Line 181: Delete “the” Done
• Lines 183-185: “such as …”? Be specific about the insights. Explanations come in the following paragraph.
• Line 187: Higher than? Be specific and don’t leave anything up to interpretation Done
• Line 191: “’mother knows best hypothesis’” Done
• Line 193: “could also be a possible explanation, although…” Done
• Line 193-194: “evidence supporting this hypothesis” Done
• Line 196-199: This is a clumsy sentence, try rewriting Done
• Lines 199-200: Try “collected from native grassland species” The sentence has been modified otherwise.
• Line 213: This long paragraph can probably be broken in two when you switch to talking about defences Done
• Lines 222-224: Not clear what the question is here, try rewriting this sentence Done
• Lines 225-226: “do not master or totally overcome…” Done
• Line 230: “resulted” Done
• Line 230-231: Try something more concise and clear like “Here, fitness of C. zealandica may have been less affected by the defences of white clover compared to those of the other hosts, or recent evidence suggests larvae may also benefit from the defences of their host (see Lefort et al. 2015)” The sentence has been simplified.
• Line 240: Replace “that” with “which” Done
• Line 241: Delete comma Done
References
• Line 259: Fullstop at end of reference. Check all references for consistency Done
• Line 260: Species name should be italicized Done
• Line 268: Inconsistent formatting (capitalization) Done
• Line 277: Insert Costelytra in appropriate place Done
• Line 279: “Glare TR, 1993” – check references throughout manuscript Done
• Line 281 and line 309: Species name should be italicized Done

Figures, Tables, and Captions
• Figure 2: Sub-figures are not labelled (a), (b), or (c) in the figure itself Done
• Figure 2, Line 337: Delete extra space between “of (a)” Done
• Figure 3, Line 342: Delete extra space between “zealandica from” Done
• Figure 3, Line 343: Delete extra space between “feeding on” Done
• Figure 4: Error bars should extend above and below mean on these bar plots. The legend of the figure indicates that errors bars represent 1 SE (instead of SE as it was indicated originally). We kept the figure as it was since it doesn’t affect its correctness. It is indeed, current practice to display errors bars only above the mean and not to extend it on both side.
• Figure 4, line 350: “Welch’s t-test” n.a. (test replaced by an ANCOVA).

Reviewer 2 (Anonymous)
Basic reporting
The paper is generally well written and meets the standards required of the journal. There are some odd usages in the text (especially the title) and a couple of typographic errors (e.g. extra word in line 86, use of hyphens in line98) but overall it is sound. I don’t think the use of the word “fond” in the title is appropriate, it is too anthropomorphic. I think it should be changed. We changed the original title to *“Preference of a native beetle for "exoticism", characteristics that contribute to invasive success of* Costelytra zealandica *(Scarabaeidae: Melolonthinae)*”.
I found the pie charts in Figure 2 to be confusing. I feel it would be more useful to present these as stacked bar graphs. They statistics attached to those figures are also too precise. I think Figs 3 and 4 may also need to be modified to accommodate a different analysis. Since both pie-charts and stacked bars aim at illustrating the same thing (i.e. component parts or percentage as a whole), we preferred to keep our original figure. However, we clarified the information regarding the statistics related to this figure.
Experimental design
The choice experiments are interesting and the use of the 3-armed olfactometer for these experiments seems to be similar enough to how larvae find food in the field. Are larvae normally found together? The decision to use groups of 5 larvae in the trials needs to be justified as the patterns observed may also be a function of a single larva’s choice rather than each individual choosing the ultimate host. As explained in our answer to reviewer #1, this was the closest representation of the natural clustered distribution of the larvae observed in the field. This was also the best compromise to achieve a large number of larvae tested. Considering that each trial lasted for 24h, the use of only one larva per trial would have ended up in 35x4x24 hours = 140 days of experimentation, which (1) would have been logistically impossible and (2) would have resulted in the use of larvae of different age/size/instar, thus undoubtedly creating an experimental bias. This information now appears in lines 128-129 of the manuscript.

Some of the analyses could be improved. Could survivorship curves with Meir-Kaplan analyses be performed with the data obtained? That would be more informative than just using the final mortality rates. In this study, survival wasn’t recorded regularly over the 15 weeks of experiment, which prevented us to carry out a Kaplan-Meier analysis. However, other studies where the

same *Costelytra* populations were submitted to different feeding trials using tussock and/or clover as control host plants, and where Kaplan-Meir plots could be provided always show a steady decrease of the survival (see Lefort et al. 2014, Lefort et al. 2015).
The analysis of the feeding trials should also be improved. Using the raw mass gain isn't really appropriate to compare performance on the different diets, it would be better to use something like ANCOVA using initial size as a covariate. Done (lines 154-159, 175-179 and table 2 in line 350)

Validity of the findings
The data collected are interesting but the statistics used to analyse them are not the best options for these data. The work still seems to show that the two species have a different affinity for different host plants and how this is reflected in host choice and use. The speculation on host race bit seems a bit out of place here but is grounded in the paper (and the work associated with it) so it's reasonable to include it.
Comments for the author
This is a really interesting system and the questions raised in the paper are of broad significance to a number of biologists. It's difficult to put the host shifts in the context of how the two species use different hosts naturally, as little is known of the their breadth in unmodified environments (short of the comments in line 73) and this is something that would really advance the questions raised here. I think the work would be made much better with some reanalyses and some rejigging of how the data are presented. Done.

Reviewer 3 (Anonymous)
Basic reporting
No Comments
Experimental design
The experimental design is appropriate for the stated objectives. It might have been more appropriate to make the comparison between C. zealandica populations feeding on native and exotic plant species if they had chosen individuals feeding on ryegrass as an exotic instead of white clover. In that way they would be comparing fitness of the populations on a native grass and an exotic grass rather than between a grass and a legume with all the attendant symbiotic associations that could affect the results. We agree with the reviewer, and this is why we emphasise that some results might be partly explained by the differences in nutritional quality of roots and bacterial symbiosis in clover (see lines 214-217).
Validity of the findings
The conclusions leave out a possible alternative explanation that should be addressed. That is, it may be possible that the authors are dealing with two different cryptic species when comparing individuals from two different populations. I am not necessarily suggesting that they conduct a genetic analysis, but I do think they need to address this possibility, particularly since they do raise the issue of the exotic plants being a selective force in the potential fitness of the herbivore. All these populations were used in other experiments (see Lefort et al. 2012a), Lefort et al. 2012b, Lefort et al. 2014 and Lefort et al. 2015), and barcoded (COI gene). These analyses confirmed

that all populations we used in our experiments belong to the same species (less than 2% variation between the sequences).

Comments for the author

I encourage the authors to carefully edit the text to eliminate cases of awkward or convoluted phrasing (e.g., lines 54-59, lines 64-68, lines 217-220)

Done.