Response to Reviewers

Reviewer 1

*L49-50. Ok but did you address this issue in the present study?*

We did not explicitly document the life-history characteristics of the predatory arthropods in our study, so we removed this sentence.

*L72. "Canola" At this stage, there is no justification of the choice of canola as a focus crop model. The rational for this choice should appear here or before in the text*

Canola is one of the most commonly-planted crops in southern Alberta, and was one of the focal crops for an earlier study (Vickruck et al. 2019), of which this study is a continuation. We have added a few sentences to justify the use of canola crops.

*L72. In the local effect, you only accounted for the type of cover, without consideration for management practices that could affect arthropod population. Could you give information on the types of practices (treatments, soil operations, crop succession for crop fields, mowing frequencies for other fields, etc…)?*

The reviewer is correct that many other management practices could influence arthropod abundances, but we have no information on surrounding fertilizer treatments or mowing practices, and the effect of crop rotation was beyond the scope of the study. No-till practices are common in Alberta (82% of cropped areas in Vulcan County used no-till, Statistics Canada 2016), but again, we did not aim to gather specific information in and around the sampling areas. As mentioned in the text (LINE X IN DISCUSSION), there is some evidence of crop rotations causing increased carabid abundance (Bertrand et al. 2016, Busch 2016), and crop rotation information would be accessible from previous years’ classified landscape data, but this was beyond the scope of our study, and we would likely not have enough data to estimate the effects of both crop rotation history and the spatio-temporal effects of the surrounding landscape.

*L73. "grassy field edges" Is in-field an appropriate name for the category? Is a grassy field margin not more similar to a road margin than to an agricultural field?*

The grassy field edges we describe here are actually the grassy corners of central-pivot irrigated fields. While road margins are managed by mowing alone, pivot corners are variously grazed, mowed, or even cultivated. They also differ in their topology, as pivot corners have a core region (centre of the corner region) while road margins are essentially “all edge”.

*L73. "remnant prairie grasslands" What is it exactly?*

They are uncultivated grasslands primarily composed of native grasses and herbs. They are typically grazed by cattle, but are not usually used for harvesting hay or fodder.

*L74-75. I did not fully understand the design (fields/transects/other sites). I was expecting that the number of traps in canola fields would be three times the total number of traps in wetland, grassy field edges and remnant prairie, but it is not the case. In the map Fig 1. there are only about 35 in-field traps. It is not clear given the legend if a road trap is always placed next to a in-field trap as the two symbols overlaps in the map.*

We updated the map in Figure 1 to clarify this, and provided two small inset maps to show the sampling design in greater detail.

*L112. "30m annuli (rings)" Most of studies are using circle buffers. I agree that buffers may not be fully relevant with many drawbacks when exploring the scale of effects such as de facto correlation between the same variables calculated for different buffer size. However, to allow for comparison with other studies I suggest that (i) you justify your choice for using rings rather than full buffers - I guess it is more rigorous for the analysis with function regression, given the definition of beta(i), but it is not straightforward; (ii) you give information on the possible change in the results if you were using buffers (did you compare for some analysis?). There is only an implicit justification between brackets in Box 1 (L177), I suggest you make it more explicit.*

*L120. "Local and regional" Please use a consistent terminology through the whole ms (see my comment for Line 197).*

We have changed the terminology. See our comments below on line 197.

*L128. "grain" In my mind, this is the scale of effect that is varying when you change from a ring to another, rather than the relevant grain that refers to the degree of fineness in the description of landscape features*

We rewrote this sentence (and a sentence in the Introduction) to clarify that this refers to scale of landscape relevant to an organism, not the scale of description of landscape features.

*L143 "the average proportion cover across days of the year (temporal eﬀect of cover class)" This is the only unclear point for me in the description of the analysis. I understand this sentence as accounting for the variation of landscape composition during the year (which would require multiple dates for mapping) and I do not think that you did that. As you clearly set L180-182, the temporal effect of landscape land uses is accounted through the sampling dates ("measurements taken across time")*

The reviewer is correct, in that we did not measure landscape composition at multiple times throughout the year. Rather, functional regression asks whether the relationship between abundance and proportion cover changes throughout the year (in Figure 2d, the slope of proportion cover on activity density is positive on day 100 and negative at day 200). “Average” here refers to the average coverage in all annuli surrounding a given location, not the average over time. We have rewritten lines 143 and lines 180-182 to clarify this.

*L160. "strongly positively correlated at all distances" Correlated or concurved? Did you choose on the basis of a correlation threshold too? If it is the case, which threshold? If not, I think it is more appropriate to write "as they were strongly concurved"*

Changed “correlated” to “concurved”, as this is the measure we used to assess concurvity. We added a sentence describing how we visually examined the spatial nature of the concurvity to determine whether classes should be combined or excluded altogether.

*L161. "correlated" idem*

As above, changed “correlated” to “concurved”.

*L169. "both" ?*

Deleted “both”.

*L197-198. This is clear and relevant to separate the processes between scales but the terminology is quite unusual. Many references exist in the literature with landscape accounted for with distances <500m. Local usually refer to the habitat were the trap is (and it is also the case in your article, which may be misleading for the reader), what you indeed included in you model (canola, field edge, grassland, road margin, and wetlands). I suggest you use another terminology, for e.g. "near" and "far" as in Whytock, R.C., Fuentes-Montemayor, E., Watts, K., Macgregor, N.A., Call, E., Mann, J.A., Park, K.J., 2020. Regional land-use and local management create scale-dependent 'landscapes of fear' for a common woodland bird. Landsc. Ecol. doi:10.1007/s10980-019-00965-x. This could be moved to (or linked with) the "local versus regional" dichotomy line 120*

We agree with this, and have changed “local-level” and “landscape-level” to “near” and “far” throughout the text.

*L221. "out of the crop" out of this type of crop*

Changed “out of the crop” to “out of pulses”.

*L223. "local" why local? In my mind s(E, N) explores the whole spatial coverage. It is difficult to extract relevant scale of clustering from Fig S3b.*

The reviewer is correct, as the *s(E,N)* smoother models the entire spatial domain. Changed “local” to “large-scale”.

*L232-233. This is not consistent with Table S4, P. distincta, significant temporal component but ns for spatial; Table S6, P. moesta, significant spatial component but ns for temporal. Again, difficult to have an idea of the strength of spatial patterns from Fig S4b and S5b*

Our statement (“Activity-density of both *Pardosa* species had a strong temporal and spatial component, although the temporal component was dominant for *P. distincta*, whereas the spatial component was dominant for *P. moesta*”) is consistent with both tables, as Table S4 shows s(Day) as significant and s(E,N) as not significant, while Table S6 shows s(Day) as not significant and s(E,N) as significant.

To respond to the reviewer’s comments on spatial patterns being hard to detect in Figures S4b and S5b, we have fixed the limits of the y-axis on Figures S3a-S6a and the limits of the colour scheme on Figures S3b-S6b to the same values across all figures, in order to accurately depict the range of variation captured by the random-effect smoothers. For example, Figure S4a now clearly shows that the temporal smoother captured a large amount of variation, while the spatial smoother shown in S4b did not (colours are similar across the map).

*L236. "late in the season" I do not understand. There was no significant effect of s(Day):Pasture. As far as I understand Fig4 and TableS4, the results are negative effects of pasture and woodlands, both increasing for large landscape scales.*

There is no significant effect of s(Day):Pasture, but there is a significant effect of s(Day):Woodland, which is what the “late in the season” part of this sentence was referring to. Figure 4c is showing that woodlands had a negative effect on *P. distincta* activity density during the later part of the season, and that this effect did not depend on the spatial scale. We have updated the first few sentences in this paragraph to clarify this.

*L236-237. "This suggests that large amounts of pasture are unsuitable habitat for P. distincta, and that they migrate into woodlands later in the season" I do not understand your interpretation. I see the woodland effect similar to the pasture effect (bad quality habitat for both)*

See our comment above.

*L247-248. "Similar to P. distincta" See my comment for Line 232*

See our comments on Line 232.

*L257. "but only early in the season" I did no see a significant interaction of Woodland and the time period in Table S8 (also consistent with Fig 6c where regressions for early and late periods have overlapping confident regions over the whole graph)*

The woodland:time interaction (ti(Distance,Day):Woodland) was weak (p=0.07), so we have removed the interaction and only present the main effect of s(Distance):Woodland.

*In the whole ms: I think dispersal (resp. dispersion) is more appropriate than migrate (resp. Migration).*

Agreed; we have changed “migrate” to “disperse” throughout the text.

*In Figs3a, 4a, 5a, 6a, is it possible to add letters for groups as a result of a pariwise comparison (rather than only significant level of estimates in Tables S1, S3, S5 and S7)?*

We added pairwise letter groupings (after correcting for multiple comparisons).

*In tables, S2, S4, S6 and S8, "Roads" rather than "Urban" would be more consistent with the text*

Reviewer 2

*1. Data sampling - From the manuscript, it is unclear how the pitfall traps were distributed. From the text and Figure 2, pitfall traps were installed in road margins and in-field locations. Although the authors provide the number of pitfalls installed, the number of sites or fields sampled is not mentioned. From the text (L66, section 2.1), each field (canola) had four traps, set at 0, 25, 75 and 200m from the field margin, with 45 traps set at 0m being outside the cultivated field (16 in wetlands, 11 in grassy field edges and 18 in remnant prairies). With 68 pitfall traps assigned to in-field canola, it appears that not all distances were sampled in each of the 45 fields. It is also unclear if the surrounding landscape was characterized around each trap or each site (4 traps), although L175 suggests each trap.*

*Overall, this section requires some clarification and would benefit from an explanation for the unbalanced distribution of trap across local covers. Also, because several traps are nested within the same field, the lack of independence between nearby traps (25-50m) should be included in the model. Does the spatial effect (easting-northing) capture the spatial effect at multiple scales, including the one between nearby pitfall traps?*

Reviewer 1 also identified that the sampling design was unclear, so we updated our map to reflect this. Some traps were indeed nested within the same field, so we included a spatial smoother (*s(E,N)*)to account for spatial non-independence, as a classic random-intercept model accounts for nesting but does not account for spatial relations between random intercepts. In essence, our approach accounts for spatial and temporal non-independence while allowing

*2. Modelling and analysis - In the method, I was confused with the analyses and how the authors specified their statistical models. What confused me was the use of the terms "scalar-on-function" and "functional linear regression", the latter used in conjunction with the phrase "where both the independent variable is predicted by a functional dependent variable" (L169). At first reading, I understood that the authors used a function-on-function regression model, where both the dependant and the independent variables are modelled functions. After further reading, I assume that this was not the case and that all models were of the scalar-on-function class, with the response being the activity- density and the smoothers applied on the right-hand side (predictors). This needs to be clarified in the text, and I would suggest providing the formula used to fit the models.*

*In the analysis, location and temporal effect are defined as fixed and smooth effects (L136), but in the results, these are presented as random effects (L238).*

On the first point, the reviewer is correct in stating that all terms used a scalar-on-function regression, with the activity density being the response. We have the *mgcv* model formula to the Supplemental along with a list of terms in order to explain this better.

On the reviewer’s second point, all penalized smoothers are technically “random effects”, as the coefficients (β) for each basis function are drawn from multivariate normal prior (β ~ *N*(0, σ2S-/λ), where σ2 is the variance, S is the smoothing penalty matrix and λ is the smoothing parameter), and are estimated in exactly the same way as traditional random-intercepts coefficients (see section 4.2 of Wood 2017).

*3. A temporal effect of "cover class" was included in the model (L143). However, it is unclear how this term was informed. Was it derived from known crops' phenology and/or sowing dates?*

Reviewer 1 also identified that this was unclear, so we have re-written parts of this paragraph (as well as Box 1) to clarify this.

*4. Activity-density - Because pitfall traps measure activity-density, change in the number of arthropods captured can vary during the season, not only because of movement, but due to specific life-cycle events and change in foraging activity. This signal (S3-6) is likely to be confounded with the one expected from the movement and thereby will influence the interpretation of the positive of negative slopes (L191). These potential confounding effects should be discussed, and inference about movement and spill-over (L267) should be cautious as these rely on correlations.*

The reviewer makes three related points here: a) our study is observational, not manipulative, b) that foraging activity and abundance are confounded within the term “activity density”, and c) that changes in activity density are confounded with movement (“migration”).

The overall spatial and temporal smoothers (S3-6) were somewhat concurved with the individual land cover smoothers (see Figure S1, columns 2 + 3), but this is difficult to remove because certain land cover types are more common in some parts of the spatial domain. For example, Figure 1 shows that Forest/Shrub cover is common in the western portion of our sampling areas (near Okotoks), but is uncommon in the eastern portion (near Duchess). This means that any effect of Forest/Shrub will always be tied up with

*5. Because the sampling scheme and the models are complex, it is challenging to disentangle the landscape and local effects on activity-density, partly because the effect of the landscape was modelled over several local habitat types. I am not clear how to interpret grassland's landscape effect on a species in canola when a species was found mainly in grassland or along road margin sites (e.g., Fig 4-a). If the model suggests that individuals are moving from one habitat to another during the season, we should also detect a temporal effect in the specific habitats. The evidence would be more convincing if supported by such local trends (e.g., change in activity density over time).*

As detailed above, we added the *mgcv* model formula to the supplementary to explain our approach more thoroughly. Our model assumes that a given trap location type has a fixed level of “inherent” activity density (similar to detection probability in mark-recapture models), and that the surrounding landscape influences activity density depending on the distance and time of the year. We also included an overall temporal smoother and distance smoother to account for the spatial and temporal non-independence in the data. To relate this to the example given by the Reviewer (Figure 4a), *P. distincta* activity density is much lower in canola crops than other trap locations, overall. Additionally, activity density was lower if there was Pasture present in the annuli at 500-1500m (4b), and was also lower during late July and August if woodland was present.

The reviewer mentions “local trends (e.g., change in activity density over time)”, which we assume means showing trends in abundance over time at a given location.

*6. L278-280. If the influence was limited to a radius of about 500 m, why is this an indication of "large-scale" effect when in the method, large-scale (landscape scales L198) refers to > 500 m radius?*

*7. L199. The early and late-season effects are defined by two dates. Are these specific dates, threshold dates (before and after, until) or median date of the period?*

These dates (June 20 and August 20) were chosen to show the change in spatial patterns over time. The first and last days of sampling were May 23 and August 22.

*8. L89 Although it has been shown and often repeated that the contribution of common species are more important to pollination services, several studies show that this is not true for every aspect of service provision, as diversity (including less common/rare species) is associated with increased stability and resilience of service provision. I would rephrase this part of the text.*

The debate surrounding the relationship between biodiversity and stability of ecosystem services is highly contentious and even political in nature (deLaplante and Picasso 2011). Since the reviewer has not suggested any references on this point, we have added two references to attempt to demonstrate that this topic is not as simple as stated in the manuscript, but we refrain from joining in this debate at this time as it is beyond the scope of this paper.

References

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