To Dr. Sandrine Petit,

We thank the reviewers for their feedback, particularly in the area of the Methods section. Given that this introduces a relatively new technique, the reviews were helpful in that they pointed to precisely where additional explanation was needed. As a result, the revised manuscript is more lucid, and (we believe) more compelling as a result. Our main changes to the manuscript are:

- Clarification of the sampling design in the Methods section

- Clarification of the temporal aspect of functional regression in the Methods section

- A short piece of additional analysis to outline the utility of our technique: comparison between function regression and “classical” radial regression

- Updates to Figures: scaling of figures in Supplemental, and addition of inset maps on Figure 1 to show sampling design

Thank you for your consideration, and we look forward to hearing from you.

Sincerely,

Samuel Robinson

Diane Edwards

Jess Vickruck

Lincoln Best

Paul Galpern

Response to Reviewer 1

*In this article, you present an analysis of arthropod count data in different types of habitat, exploring spatio-temporal variation of the landscape effect of diverse land uses on local abundances, with the goal to infer population dispersal patterns. In addition with the nice spatio-temporal dataset, one of the originality of this work is the use of functional regression to analyze landscape effects at different scales and to account for the repetition of measures in time.*

*The ms is particularly clear (except for some specific points, see below), well written and structured. The figures are very clear and informative. I appreciated the nice didactic presentation of the statistical method (including a specific box and fig), a method which is quite new in the field of landscape ecology. Please find my comments below.*

We thank Reviewer 1 for their comments, in particular for their close attention to our statistical analysis. We also hope that this manuscript will highlight the potential of this new technique within the field of landscape ecology (as well as agroecology)!

*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*

We added: “The data were collected as an offshoot of an earlier study that examined pollination services in crops (Vickruck et al. 2019), and here we use pitfall trap data to examine the predatory arthropod community with a focus on canola crops (Brassica napus L.). Canola is one of the most commonly-grown crops in southern Alberta, and represented 15% of the total land cover within the spatial extent of our sampling region during 2017 (Figure 1).” (L65 in new manuscript)

*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 gather specific information in and around the sampling areas. In principle, the effect of crop rotation on predator abundance is an interesting and relevant study area, and could be considered in a future study. As mentioned in the text (L300 in original manuscript), there is some evidence of crop rotations causing increased carabid abundance (Bertrand et al. 2016, Busch 2016), but 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. We added: “We did not collect information on soil fertilization, pesticide application, mowing, or tillage (approximately 82% of planting in Vulcan County was done with zero-till in 2016; (Statistics Canada 2016). Diverse crop rotations may also increase predatory arthropod abundance (Bertrand et al. 2016; Busch 2016), but this was beyond the scope of the study.” (L70 in new manuscript)

*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”. As such, we feel it is important to distinguish between these two different types of land cover. We added: “ ‘Wetlands’ were permanent (non-ephemeral) flooded areas, ‘field edges’ were the grassy corners of central-pivot irrigated fields, and ‘remnant prairie’ were uncultivated grasslands composed of native grasses and herbs.” (L79 in new manuscript)

*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.We added brief descriptions of the other trap location types (see above).

*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.The inset maps show the arrangement of traps away from a non-crop feature. We also added: “Pitfall traps were installed in road margins (minimum of 5 m away from the road edge, 85 traps) and in-field in canola crops (113). Traps in canola were installed in 27 separate fields at 25, 75, and 200 m along a transect heading away from the nearest non-crop feature (wetland, grassy field edges, or remnant prairie), while the trap at 0 m was installed in the non-crop feature itself (see Figure 1 inset maps). The remaining 5 sites had only a single trap.” (L74 in new manuscript)

*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.*

To address this reviewer’s question and to help readers appreciate our use of annuli, we ran an additional set of models using circles buffers of increasing radius, and chose the best-fitting model to compare to the annular regression. We found that functional regression can provide a better fit (in terms of AIC scores) than the “traditional” radial regression. Table 2 in the revised manuscript compares the null model (no landscape terms), our functional regression model, and the traditional radial model. We added an extra paragraph to the Methods section to explain this (L290-301 in new manuscript).

*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 as “This allows for the possibility that the scale of landscape relevant to a given organism may change over the course of its life” as well as L43 (“Second, the scale at which the landscape is relevant to an organism depends on their traits and life history”), 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 (“we used the average proportion cover in all annuli across days of the year”, L151 in new) and lines 180-182 (“it is also possible to use proportion cover as a predictor of measurements taken across time, allowing the effect of a (fixed) proportion cover on activity-density to vary over the course of the season”, L201 in new) 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"*

We 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: “For highly concurved classes, we examined the spatial nature of the concurvity using scatterplots of proportion cover within each annulus to determine whether the cover classes should be merged (if classes were consistently related) or removed (if classes were inversely related to one another).” (L176 in new manuscript)

*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*

This is an excellent suggestion, and one that we agree improves clarity. We 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*

We 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. We 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 spatial and temporal 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 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: (“*P. distincta* activity-density was negatively affected by far pasture (Figure 4b), suggesting that large amounts of pasture are unsuitable habitat for *P. distincta*. There was also a negative effect of woodlands late in the summer (Figure 4c) that did not depend on spatial scale, indicating that *P. distincta* may disperse into woodlands at this time.” L260 in new manuscript)

*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) to Figures 3-6.

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

We changed “Urban” to “Roads” in Tables S2-S8.

Response to Reviewer 2

*This manuscript investigates how surrounding landscape composition relate to arthropods activity-density in canola fields, road margins and adjacent non-crop habitats. I found the study most interesting and applaud the use of a functional approach to model the impact of landscape composition at incremental scales and over time within a growing season. Such modelling techniques can provide valuable insight into our understanding of the impact of landscape on species distribution and potentially on the provision of ecosystem services in agricultural systems. This innovative approach has great potential and allows the authors to better account for the spatial and temporal drivers of arthropods' activity-density in agro-ecosystems.*

*Despite my overall enthusiasm, it was sometime challenging to understand how exactly the sampling and the analyses were computed. The method section is not always clear, lacking details, and on many occasions, I found myself guessing how the study and the modelling were conducted.*

*Although critical for the understanding of the study, I believe that the authors can address these issues by editing and augmenting some parts of their text, and potentially providing additional results to support their interpretation. This study has the potential to contribute to our understanding of the spatiotemporal dynamics and habitat use of arthropods in agroecosystems.*

We thank Reviewer 2 for their comments, and share their enthusiasm about the potential use of functional regression in landscape ecology. Reviewer 1 was similarly concerned about how our sampling and analysis was conducted, so our updates to the manuscript provide further clarity on these points.

*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 the Methods section and map (Figure 1) 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 (*e.g.* y ~ x + (1|Site) using *lme4* syntax from R)accounts for simple nested designs but does not account for spatial relationships between random intercepts. In other words, our model accounts for nested sampling design while also using spatial (and temporal) information.

*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 now added the model formula, using the syntax of the *mgcv* R package, to the Supplemental along with a list of terms in order to explain this better.

On the reviewer’s second point, L136 defines “trapping location” as a fixed effect, not landscape composition. Secondly, all penalized smoothers are technically “random effects”, as the coefficients (β) for the basis functions 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). However, this terminology is not commonly used in ecology and could be confusing, so we have changed “random effect” to “smoother” throughout the text to avoid this confusion.

We changed the beginning of the paragraph (L149 in new manuscript) to clarify what smoothers were used (“For each landscape cover class, we used three scalar-on-function terms First, we used the proportion cover in the annuli surrounding each trap location (spatial effect of cover class - i.e. does the effect of cover differ with distance?). Second, we used the average proportion cover in all annuli across days of the year (temporal effect of cover class - i.e. does the effect of cover differ with time?). Finally, we used a tensor-product interaction of landscape cover and day of year (spatio-temporal interaction of cover class - i.e. does the effect of cover differ with distance and time?)”, and added R code to Appendix B to demonstrate how this was done in *mgcv*.

*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: (“Second, we used the average proportion cover in all annuli across days of the year (temporal effect of cover class - i.e. does the effect of cover differ with time?).” L151 of new manuscript; “As functional regression takes any continuous predictor of y, it is also possible to use proportion cover as a predictor of measurements taken across time, allowing the effect of a (fixed) proportion cover on activity-density to vary over the course of the season (Figure 2d).” L200 of new manuscript)

*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 the point here that foraging activity and abundance are confounded within the term “activity density”. For example, higher counts at a given location could represent increased abundance from emergence or higher foraging activity. On this point, the reviewer is correct, in that there is currently no way to disentangle activity and abundance using data collected from single traps (as we also discussed on L80-87 of the original manuscript). However, we used the term “activity density” in the original manuscript to reflect this, and use tentative verbs such as “suggests” (e.g. L267 of original: “This suggests that these arthropods migrate to these habitats...”) in the text to communicate that these results are *suggestive* of migration between habitats, but should be confirmed by future studies.

We added the following text to justify this: “As mentioned above, abundance and activity are confounded within activity-density, meaning that our method cannot explicitly say whether changes in counts are due to changes in abundance (i.e. migration or reproduction) or changes in activity (i.e. foraging). However, we consider it more likely that changes in activity-density related to landscape components are caused by changes in abundance, not activity, as there is no mechanism by which distant parts of landscape could alter activity.” (L217 of new manuscript)

*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).*

We added the *mgcv* model formula to Appendix B to explain our approach more thoroughly. Our model assumes that a given trap location type (e.g. canola) has a fixed level of 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.

*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?*

We deleted “large-scale” and replaced “local cover” with “nearby landscapes”.

*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 2 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). We added the following text to the relevant figure captions to show this: “Coloured regions represent early- and late-season effects (blue = June 2, red = August 20)”.

*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 contentious (deLaplante and Picasso 2011). To reflect the complexity of this issue we have added two references (Kleijn et al. 2015, Winfree et al. 2018) to attempt to demonstrate additional perspectives in addition to those stated in the manuscript.

References

Craven, D., et al. 2018. Multiple facets of biodiversity drive the diversity–stability relationship. Nature Ecology and Evolution. 2: 1579–1587. https://doi.org/10.1038/s41559-018-0647-7

Statistics Canada. 2016. Table 32-10-0408-01  Tillage practices used to prepare land for seeding. Accessed May 18, 2021.

deLaplante, K., and Picasso,V. The Biodiversity-Ecosystem Function Debate in Ecology. 2011. In: Handbook of The Philosophy of Science: Philosophy of Ecology. Eds: deLaplante, K., Brown, B., and Peacock, K. A. San Diego: North-Holland. Pages 169-200.