Decision by the Subject Editor (Prof. Dries Bonte):  
  
Dear Daniella, after 165 days into the review process (this is my record), i finally received the two needed opinions of expert referees. I want to apologise again for this huge delay. I am quite surprised that it showed to be so hard to find reviewers given the topic. Let me be very clear here, despite my decision to reject, i am very keen to continue with your work to get it published in Oikos, not because i feel guilty about the delay but simply because i like your work a lot. You present an interesting study on temperature control of predator-prey relationships in the wild, which is to my opinion a highly needed next step in this field. However, my less positive message is that reviewers came-up with quite dome comments that you would need to consider. I agree with most of them, in particular the need to convince the readers on the suitability of the data (distribution, collinearity) and the apparent absent of analyses that could make the story more complete. Note that i do agree with rev2 on the need to consider potential causality in the parameter selection, and that such an approach should become mainstream in pattern-based ecology. While such an approach may include new approaches (e.g., SEM and/or path analysis) i leave this open to you whether you would be keen to apply such new methods. In any, case the reviewer is correct that a more nuanced interpretation of the effect sizes is needed.

On the editorial side, i would also request to further open the discussion on predator-prey relationships under different environmental (thermal) conditions to make your work more interesting to a large audience. The climate change narrative can be retained but would need to be toned down a bit, and become more central to the discussion than to the intro. All at all, i am happy to be able to provide you reports of the highest quality that will be of excellent use to further improve your work. I am looking forward to receive your revised manuscript. One reviewer has agreed to see it again.  
  
  
  
  
Reviewer(s)' Comments to Author:  
Reviewer: 1  
  
Comments to the Author  
Review of Ambient temperature affects mammalian predator-prey interactions in an African savanna – OIKOS  
  
The manuscript is generally well written with only minor typos. The authors have collected, collated, and analysed a very impressive dataset over many years and including 3 species, which has allowed a detailed examination of the impact of ambient temperature on predator prey interactions in this system. Given the quality and quantity of the data in hand, I found a few analyses that would have enhanced or illuminated the story to be currently missing, and I found the focus of the discussion and interpretation fairly limited. I have a number of main comments that the authors should consider to improve (in my opinion) the quality and accuracy of the manuscript, and also a number of more minor comments. All are meant to be constructive and helpful, and contribute to improving the manuscript.   
  
Main comments:  
The title is accurate and reasonable, but I felt that large sections of the rest of the text overreached considerably. I understand the implications of elevated high temperatures on the dynamic, and that is interesting enough, but I felt that to tie this so completely into a climate change framework lacks nuance and does not do justice to the results. Indeed, the temperature data in the supplementary materials (S1) show that temperature is stable in the study area across the whole long-term dataset. That is not to say of course that temperatures will not rise, but given that they don’t currently appear to be, it seems much more sensible to frame the paper in terms of how temperature affects these important ecological interactions (as the title suggests), and to relegate the possible implications of the results in a climate change scenario to a few paragraphs in the discussion. I realise that this would result in considerable rewriting, but I don’t think the focus on climate change makes sense for the data being presented. At the very least the authors need to make a much clearer case for doing so, given the above. Relatedly, (Ln 121), you discuss evaluating how climate change might affect the system through predator prey interactions, but there are no real projections around what is expected to happen with regard to temperature at the study site under any climate change scenarios. I suspect that the temperature is expected to increase, but you really need to state this and back it up with a reference to tie in the climate angle (and again, a reduced focus on that would be better in my view).  
  
The discussion too, in its current firm, is focused almost entirely on these species, with very little attempt to broaden out and put these results in the context of the wider field. The paper would really benefit from contextualizing the results beyond the study system, and more into the field of predator prey dynamics and temperature impacts more broadly.  
  
Habitat and shade selection: Do impala select for more open areas? You have a lot of data from the three species, but you only really make qualitative descriptions of their habitat selection and preferences. This seems amiss when the paper relies quite heavily on the patterns seen/described. I think you would ideally look at resource selection functions for the three species in terms of shade and habitat, and show quantitative responses to these factors in response to ambient temperatures. The data you describe having would certainly allow you to do this, and so I am somewhat puzzled by its absence in the current manuscript. The methods described in ln 214-8 make this omission clear. You don’t actually describe what you did with these data at all. RSF? Some measure of selection in relation to availability? I think that is a major thing missing from the current paper that would really add to the story.  
  
You describe an analyses showing a reduction in the number of scats that contain impala DNA when it has been hot lately (Ln 322-4), but there’s no similar analysis presented for dik dik. Why not? Surely this is a really important part of the puzzle, and you would appear to have the data.   
  
The effect sizes for temperature seem very small in a number of these models (~0.001), and I wonder why that is. It does make one question the strength of the relationship, but I also wondered whether the fact that there were huge shifts in temperature in the day while the animals rested in the same place could have constrained these effect sizes. Is that a possibility? Would the effect be bigger if you excluded rest locations for this reason?   
  
More minor comments:  
The authors repeatedly refer to the study system as a three species system. This creates the false impression hat these are the only species in the system. African wild dogs prey on a much wider set of species than this description suggests, and while these two species are undoubtedly the primary prey species in the system, it is oversimplistic to suggest that this is a three-species system. I am sure that this is just the way it is written (they do mention 5 other large carnivores in the methods), but the authors need to clarify this, and perhaps make greater or more prominent reference to dietary studies showing niche breadth of wild dogs in this system.   
  
In several places, the authors write of wild dog activity reducing on days when it is hotter. Is it not simply shifted? This should be clarified.   
  
Prey selection in relation to abundance. I do not understand -and so I think the authors at least need to describe more – why wild dogs don’t just pick off dik-dik as a matter of course normally (on non-hot days). In places, the authors suggest that they shift to this strategy because it is easier, but if that were the case then wouldn’t you expect the wild dogs to adopt this strategy independent of temperature?   
  
Ln 40- your language slips briefly to discuss “weather” rather than climate change. It is my opinion that the paper would be better framed in terms of weather, as discussed above.  
  
~~Ln 40-45. There are some really important and large statements here, but none have citations.~~

Additional citations have been added ~~Ln48. Where you say “Because” you would be better served beginning “in contrast” to show that you are shifting focus.~~

We have changed the text to that suggested by the reviewer  
~~Ln 71. More evaporative = more on evaporative~~

This has been amended  
  
~~Ln72. Than smaller = than do smaller~~

This has been amended

~~Ln 75. Relative to what?~~

We have removed the term relative to avoid confusion  
  
~~ln85. I found “predation risk-averse” to be quite clumsy phrasing. Can you rewrite to clarify?~~

This has been rephrased as “habitat selection, driven by predator avoidance...’  
  
Just a suggestion of course, and this is probably personal preference and (like everything!) could be ignored, but I would rename your hypotheses slightly. 1 hunting (rather than foraging). 3. drop the prey from this, as the description talks about both predators and prey not just prey.   
  
~~The shade-seeking hypothesis left me wondering whether they were more likely to be predated in those areas. I appreciate that these data are difficult to come by, and perhaps you don’t have them available, but if you do then I think it would be important to show that this shift in movement in impala is reflected in a shift in predation locations.~~

Ford et al 2014 showed that impala have higher per capita predation risk in denser vegetation using resource selection functions of both predators and prey, however we do not have data on exact predation location so we would be unable to do the analysis suggested.

~~You don’t note the breakdown of wild dog collars by sex and status- this should be specified if possible, though I appreciate that they are travelling as a pack.~~

This has been added  
  
You only look at female impala, and female dik dik, which is fine and reasonable, but it did make me wonder whether there were important information missing as a result. For impala, don’t rutting males spend time defending a very small area, often in an open patch, where they have a midden and attempt to push off rival males and herd incoming female herds? During this time males may be more vulnerable to predation due to exhaustion, spatial anchoring and predictability, and sexual distraction. Is that a possibility? Also, I wonder whether rutting is seasonal in Kenya such that it might affect your result? This would also have implications in terms of the boon of impala fawns/calves. Also seasonal? While I don’t know enough about impala biology and breeding phenology to make a major comment here, I also didn’t learn enough from the paper in that regard to interpret these results. More detail on the breeding timing and general biology of the two prey species is required for a thorough understanding/interpretation of the results. I would revisit the information provided on the dogs too – patterns of hunting could be expanded; Aseasonal breeding at the site… etc. Neither are really mentioned, unless I missed that.   
  
~~I didn’t find the ethics/permit numbers listed anywhere in the paper. These should be added.~~

These can be found in the ethics statement in the statements document  
  
~~Ln 159. What constitutes an “occasion” here? Ln 275 you say that you don’t know which pack or individual left the scat. Given the other predator species in the system, can you be sure that the scat is from wild dog is you didn’t see it deposited? Some more detail on the methods are missing here.~~

Wild dog scats are easily discerned from those of other predators in the system by their size and consistency

~~Ln174. How much data was lost in this way?~~

This information has been added  
  
ln 182. Hunts can be much shorter than this, and presumably are very much shorter for dik-diks.   
Do they really need to be active for 20 mins to be hunting? I would think you miss quite a few dik dik kills in this way.

Whilst individual hunts can be much shorter than this wild dogs don’t generally only undertake a single chase, instead the pack forages over a period of time until they have eaten enough. Individual short chases outside of this time are both rare and impossible to distinguish from other shot bursts of activity on accelerometers that only give a measure over 5 minute intervals such as those on the collars used in the study.

~~Ln 182- is there any justification for 500 units? In general, why not look at a forays from the den or rest site?~~

The criteria were derived using visual inspections of the activity plots and by determining the characteristics of the activity data at times we knew they were hunting – either through visual observation or GPS locations. The GPS data is too sparse to see the timing of forays from the rest area or den site.  
  
~~Ln 186/7. Here you sou seem to disregard the possibility that the dogs might shift their activity windows in response to temperature by only looking at windows when you except them to be moving. Can you clarify what you mean here, and whether it might introduce bias?~~

We have clarified in the paper that very few bouts spanned multiple time periods. We used very wide intervals for the start and stop times to minimise bias. Leaving in bouts that crossed multiple time periods would be more likely to bias the data as it will include long distance dispersal movements.  
  
~~Ln 200-208. This is a really clear description of how the time periods were divided. However, it is in a dik dik specific section. Presumably this is the same for all species. Can you add a general section for all three species instead?~~

This section covers both impala and dikdik. As these categories were derived using the African wild dog hunt times it would be inappropriate to cover wild dogs hunt times in the same section.  
  
~~Ln 208/9 . “In order” is almost always superfluous.~~

This has been removed  
  
Ln 229. I understand the reasoning, but it does seem strange to exclude 10% of the data because it happened in the heat of the day – especially when the paper is about hunting in high ambient temperatures.   
  
~~Table S3 is good. I don’t think you need to keep saying “we” have already shown this. Maybe just that it is known, but that’s a personal preference (at one point you even do it and cite other authors from another site- Cozzi et al – but not yourselves).~~

This has been amended

~~Ln 245-6. This statement suggests that you always use model averaging, but I presume that you set some clear conditions for when it was necessary (e.g. if multiple candidate models did not differ by more than x AICc units”). These conditions should be noted here.~~

We have clarified that we only used model averaging if more than one model was in the top set

~~Also please state whether you are presented full model averaged estimates, or conditional estimates. The latter are more representative and should be used.~~

We have added that we report conditional effects into the results table descriptions  
  
~~Ln 264-268. You describe how the data you had was not possible to use in the way you hoped. Given that it was historical data, you probably knew this in advance, and either way it seems strange to set up hypotheses that you later reveal you can’t test…~~

We initially thought that the accelerometery data may have been a fine enough scale to derive more information about hunts  
  
Ln 316. Of=or. The result here seems to be saying that impala go to the shade when it is hot. I would expect they all do.   
  
~~In the discussion you describe prey preferences, but you do not measure this (or analyses/present this). What you show currently is that they were more likely to have those remains in their scats, not that they preferred them. This is an example of overreach in terms of the analyses you have done, but I think these are analyses that you could have done.~~

We have mediated the language used around prey preference.  
  
~~I can see from the methods that the collaring of prey species were contiguous and not overlapping, but you would probably have data to show some direct interactions between impala and wild dogs? Could that be incorporated, or was the sample too small?~~

There would be no way to derive this from the data as wild dog GPS data was infrequent and simultaneous points were never within chase distance  
  
Ln 346. The description around the dik dik moving to shade very locally makes good sense, and is a good example of how some parts of the interpretation are really well thought out.   
  
~~Ln357-359. Impala seek shade when it is hot, but this doesn’t make them more likely to appear in the dogs diet – isn’t this just because the dogs are not active when it’s hot and so the impala moving into the shade has no impact on their vulnerability? You don’t really interrogate this result enough I don’t think.~~

We have added an additional sentence discussing this  
  
How important are young impala to wild dogs? They would be hunted in the same way as a dik dik I imagine.   
  
Figure 4b. Is this correct? The y-axis says probability of impala in the scat, but the max value for that is ~30%. The text describes them being present in ~74% I think. Has the variable been transformed in some way, or is the figure wrong?

The text reads “Among 795 wild dog scats 71 (9%) contained impala remains”, however because this was correlated with temperature the model estimate (which is what the line on the graph is) estimates around 30% of scats would contain impala remains at 24 degrees Celsius, hence the discrepancy.  
  
Supplementary tables. Very comprehensive and great to see! Some inconsistencies between then in variable labelling. Sometimes you say “Dennin (yes)” and others just “Denning” or “Denning vs not”. Suggest to standardize.

We have standardised this throughout the SI tables  
  
Looking at table S12- the number of hunts had very little to do with temperature, and much more to do with moonlight. Temperature also only featured in one of the top 2 models for morning hunt duration, with denning and moonlight present in both models and (at least for moonlight) having a much greater impact. Is temperature really as important as is made out here- there is a lot more nuanced interpretation that is missing from the paper in my opinion.   
  
~~Figure S1 is probably superfluous as is covered in a previous figure.~~

Although they appear similar figure 1 shows the raw accelerometery data and figure s1 is a histogram of the start and stop times of hunts.  
  
Reviewer 2

This study explores the effects of ambient temperature on the behavior of an African predator species, the wild dog, and its two primary prey species, the impala and the dik-dik. The overarching goal of the study is to understand how increasing temperatures due to anthropogenic climate change may indirectly affect species interactions and, therefore, populations. I feel this is an important area of inquiry as the effects of climate change are likely to be complex and extend beyond direct impacts on animal species.

The authors present three non-exclusive hypotheses for how the behavior of the three species may change with increasing temperature, and use a multi-model inference approach to assess each.

1. Reduced foraging hypothesis: wild dog will be less active on hot days and therefore spend time less hunting. This will translate into higher rates of predation on dik-diks, as predicted by optimal foraging theory. The authors find support for this hypothesis. They find that wild dogs spend less time hunting and have less intense hunts on hot days, and that wild dog scats are less likely to contain impala remains after periods of high temperatures.

2. Shade-seeking hypothesis: all 3 species will shift to shadier habitats on hot days, leading to more prey overlap. As impala tend to occupy the most open habitats normally, they will exhibit the biggest shift. This increased overlap with wild dogs will result in higher rates of predation on impala. The authors refute this hypothesis. Although they find evidence that impala shift their habitat use as expected, they do not find an increase in impala remains in wild dog scats after periods of high temperatures.

3. Overheating hypothesis: all species will have difficulty thermoregulating during chases, but this will be more pronounced for the larger species. This will result in wild dogs being less able to capture dik-diks and more able to capture impala. The authors refute this hypothesis. They were not able to examine chase speeds or durations, but found that wild dog scats are less likely to contain impala remains after periods of high temperatures.

I like that this study interrogates multiple hypotheses and that the authors synthesize several existing datasets to answer their questions. However, I have a number of concerns about the data and analyses that cause me to question the authors’ conclusions. I will present my main concerns first, followed by more minor and line-by-line comments. I hope these are useful to the authors, and I thank them and the editor for the opportunity to review their work.

Main concerns

1. The authors provide no information on data exploration prior to the modeling analyses, nor on any model validation checks. Thus, is it not possible to assess whether the methods they have chosen are appropriate. Given that the data were originally collected for other purposes, it seems particularly important to verify that the data are suitable for answer the questions posed in this manuscript (e.g. reasonably distributed across the relevant temperature range). There are a number of resources that offer guidance on appropriate data exploration and model validation checks. For example, Zuur et al 2010. A protocol for data exploration to avoid common statistical problems. Methods in Ecology and Evolution 1:3-14, and Zuur & Ieno 2016. A protocol for conducting and presenting results of regression-type analyses. Methods in Ecology and Evolution 7:636-645.

2. Related to the previous comment, I have concerns about the scat data. These data provide evidence for or against all three hypotheses, and are the only evidence used to refute Hypothesis 3, so they are very central to the paper. The authors mention (L273-275) that the scats come from two different land use types (community and private) and that impala consumption by wild dogs is far lower on community lands. Meanwhile, it seems that all of the other data come from, or are at least heavily biased to, private lands since the collaring efforts were centered on Mpala. The known difference in impala predation rates between the two land use types suggests that the prey assemblages (or at least impala abundance) differs between the two land uses; it therefore would seem best to limit the scat data to those samples collected on private lands as these samples would be subject to similar ecological dynamics as the other data. If the scats from community lands are included in the analyses, there needs to be explicit justification for this, and more information provided on the data. The authors do not report how many samples come from each land use type and in Table 4, which gives the results of the model of wild dog predation on impala, land use does not show up as a relevant factor. Woodroffe et al 2007, which details the collection of the scats, suggests that the majority of the samples were collected on community lands. I therefore wonder if there is some collinearity between land use type and temperature that may be masking the effect of land use in this analysis and generating a spurious relationship between impala consumption and temperature. I ask that the authors report how the scat samples break down across land use and temperature, and carry out other data exploration steps recommended in the sources given above.

3. More clarity is needed on the authors’ interpretations of the various models, and in particular, how they assessed that an explanatory variable had an important effect. Is it simply that all variables retained in the top model set were regarded as important? The model results tables give measures of “variable importance” but these values are not interpreted/addressed in the text. For example, in lines 293-294, the authors state that “the total duration of wild dogs’ hunts in a 24 hour period was shorter at high ambient temperatures”. However, in Table 2, the estimate for this effect is given as -0.25 with confidence intervals of -3.07 and 2.57. My interpretation of these numbers is that the estimate is very nearly centered on zero, and could plausibly be as low as -3 or as high as 2.5. In my view, this is not very strong support of the claim that hunting time is lower at high temperatures. 4. In the same vein as the previous comment, the authors should interpret not just the existence of an effect of the various explanatory variables, but also the effect size. They should also translate the effect size into “real world” magnitudes to give the reader an idea of its biological relevance. Returning to the example of the effect of temperature on hunting time over 24 hours: if we accept the estimate of -0.25, my understanding is that this means that (assuming all other variables are fixed) for every 1°C increase in ambient temperature, hunting time decreases, on average, by 15 seconds. This seems like a negligible amount over 24 hours, but it is the job of the authors to assess the biological relevance of this effect. Interpretation of effect magnitude is particularly important in instances where the units of measure are not intuitive, for example the measure of hunt intensity.

Minor comments:

1. Regarding the model construction, the authors have been careful to include only explanatory variables with biologically plausible or documented relationships with the outcome variables (as explained in Tables S3 and S4). However, the authors do not appear to have considered the potential causal relationships between the various explanatory variables. Although it is common practice to include all biologically justified variables in GLM/GLMMs to “control” for their effects, doing so can actually confound the analysis under certain circumstances. I urge the authors to consider the causal relationships between their explanatory variables to determine if including them in the models is helping or hurting their inference. For further information, I recommend the book “Statistical Rethinking” by McElreath. A good introduction to causal inference is also available via his recorded lectures, e.g. <https://youtu.be/KNPYUVmY3NM>.

2. In the Introduction, it would be interesting for the authors to address the possibility that multiple mechanisms are in play simultaneously. The hypotheses are not mutually exclusive, so what happens if more than one (or even all three) mechanisms are at play? Alternatively, this could be a point to address in the Discussion, if the results are consistent with a combination of multiple mechanisms.

3. This paper focuses on a single predator species and its two primary prey species, but these dynamics are playing out in a system with many other potential wild dog prey species as well as other predator species that exploit impala and dik-dik and exert competitive pressures on wild dogs. A thorough treatment of all these possible dynamics is well beyond the scope of this paper, but I think it would be interesting if the authors briefly reflected on this fact in the Discussion as it could provide some productive avenues for future work.

Line-by-line comments: -

L8: should ‘predation risk’ be ‘change in predation risk’? –

L16: ‘in habit’ should be ‘inhabit’ –

L18-19: I think this claim is somewhat misleading since there was no direct investigation of thermoregulation during chases. Rather, the authors checked for one possible result of thermoregulation challenges (higher predation on impala rather than dik-diks), which could arise via a number of mechanisms. –

L60: ‘makes’ should be ‘make’ –

L71: ‘Larger mammals rely more evaporative cooling’ should be ‘Larger mammals rely more on evaporative cooling’ –

L147: How much temperature variation was there over the periods of dik-dik tracking? Is an average of 18 days sufficient to capture substantial variation? Actually, nowhere is there any information given on the variation or range of ambient temperatures experienced across days during the collaring periods. –

L195: Were hunting bouts that spanned multiple time periods excluded form the analyses of hunting behavior over a 24 hour period (lines 224-226)? If so, why? The caption of Figure 2 suggests that because these bouts spanned multiple time periods, they were assumed not to be true hunts. If this is the case, please make this clear in the main text. –

L217: How did you choose the 40 m radius? How does this compare to the error of the GPS collars? Since dik-diks seem to operating on a different scale than the other two species, would it make sense to use a smaller radius for them? –

L229-230: It’s unfortunate that midday hunts were excluded from the analyses, as one might expect midday hunting behavior to be perhaps the most strongly affected by high ambient temperature. Is this because sample size is too small to be used with the chosen approach? If so, is there another analysis that might be able to make use of this data? - L257: Here it says that the explanatory variables for the habitat use models were the same as for the hunting models, but solar radiation is listed here and not in lines 234- 237. –

L270-271: Do scats have only one prey type, or can they contain both impala and dik-dik remains? That is, does the presence of impala in a scat imply the absence of dik-dik (and vice versa)? –

L272: Is this the mean ambient temperature over the 7 days prior to scat collection? –

Figure 1: In part A, is there one line per individual? If so, does each line then represent that individual’s activity averaged over all the days it was collared? Please clarify in the caption and/or text. –

Figure 2: The colors for ‘multiple time periods’ and ‘night’ are indistinguishable in greyscale. –

Figure 2: Please verify that the 10% figure reported in L229 for the percent of hunts occurring during midday is correct, as the corresponding block in Figure 2 seems smaller than 10% (e.g. it does not look 5x bigger than the midday block in the activity column, which is supposed to be 2%). –

L293-294: Here you refer to Figure 4a to illustrate the claim that the total duration of hunts in a 24hr period is shorter at high temps, but Figure 4a shows only evening and morning hunts separately, not the total over 24 hours. –

L304: Should Table S5 here be Table 2? –

L316: ‘of’ should be ‘or’ - L349: should ‘lower’ by ‘higher’? –

L352-355: It looks like in Figure 3 that impalas’ biggest shift occurs during midday (when dogs aren’t really hunting) and evening (when dogs do hunt but hunts tend to be considerably shorter than in the morning [according to Figure 4], thus suggesting they might be targeting dik-diks during this time)? Could the specific timing of impalas’ habitat shift partly explain why it doesn’t translate into more predation of impala? Or, if impala are shifting habitats on hot days, and wild dogs are less likely to hunt in the evenings on hot days, perhaps this is an example of two of the hypothesized methods being in play simultaneously and cancelling one another out. - L396: I find this paragraph and the next very interesting. I hadn’t realized the prey base of this wild dog population is so anomalous. I think the authors do a great job here of explaining the broader implications of their study.