**Editor**

*1)* ***Introduction****. It would benefit readers if some data specific to the study region could be included when making predictions. For example, on what basis did you make the prediction that the abundance and richness of plants and bees would increase as a result of fire? It is a stretch to point to a few examples of studies where 'disturbance' has led to increases in several measures of network structure. This is certainly not the case for studies that have specifically examined the impacts of wildfire. For example, Banza et al. 2019 (Functional Ecology) show the opposite (please also see some relevant references within this manuscript). Why might this be the case? Is it specific to the study sites or the animal groups studied?*

We agree that the introduction would benefit from a more detailed description of the prediction that abundance and richness of plants and bees would increase as a result of fire. We specified that these predictions are primarily based on studies focused on native bees and acknowledged that other studies have found a negative impact of fire. We added citations for Banza et al. 2019, Mola and Williams 2018, and Galbraith et al. 2019. We also broadened our statement that changes in the plant and bee communities are reflected in the architecture of community interactions to include both positive and negative impacts. See lines 64-70.

*2) I am not satisfied that the authors have made the most of their BACI experiment. First, it would appear that unburned sites are at low altitude compared to the burned ones. Is this so and how important might this be for the plant and bee communities studied? Insufficient information regarding some aspects of the methodology was provided for the experiment to be repeated. For example, was the time stopped between catching an insect and storing it? Was the plant species on which a bee was caught identified in the field (and was this recorded as an interaction)? To what extent might 'returning to plant patches attracting the most bees' bias network metrics? How were flowers counted - how did you deal with umbels (See Baldock et al. 2015 for definitions in plant-pollinator construction methods)? Second, there is no attempt to check for sampling completeness either at the plant/bee level or network level. Instead, it would appear raw data is used throughout, but that might mask differences based on sampling effort (see Gotelli & Colwell, 2001 Ecology Letters, Chacoff et al. 2011 JAE and Traveset et al. 2015 Nat. Comm). How complete were the networks prior to comparison?*

We apologize for being unclear about some aspects of our sites and methodology. We agree that based on figure 2 it appears that site elevation may vary. All sites were located in the valley, although some sites were adjacent to the upland. There were no major differences in elevation between sites. We have added this to the materials and methods section. See line 137.

Additionally, we have added more detail about our sampling methodology including information on pausing the clock while processing specimens (see line 152), plant species identification and recording of interaction (see line 153), and flower counting (see line 161).

We calculated sampling completeness for species richness and for interactions using the Chao estimator. See lines 186, 305, and 351.

*3)* ***Analysis****. Instead of multiple testing (which, incidentally, needs to be controlled for when presenting results) and partitioning out data, might the plant, bee and network data be better analysed using separate GLMs and/or GLMMs that retain all data (see Banza et al. as above).*

We have considered analysing our data using separate GLMs or GLMMs in order to retain all data, however have chosen to conduct separate analyses for the two pre-fire years in order to match our sampling dates more closely. Since the sampling schedules differ between 2002, 2003, and 2018 we felt that the differences in plant and bee communities throughout the season would introduce substantial additional variation if the analysis retained all data.

***REFEREES' COMMENTS TO AUTHORS***

*(nb. If there is no comment from a Reviewer listed below, this probably means that they have uploaded a separate 'file for author' to the Central Site. You can see these comments in your Author's Centre by clicking ‘manuscripts with decisions’ and then using the 'files attached' link at the bottom of the decision letter)*

***Reviewer: 1***

*CONFIDENTIAL COMMENTS TO AUTHORS*

*This is an interesting paper worthy of publication. It is let down by some inconsistencies and omissions. I have provided some comments that, when addressed, should improve the manuscript.*

***Introduction***

*Li 97-109 Some repetition of Methods here in the Introduction; suggest moving this material to the Methods section*

We agree that there was unnecessary repetition in this paragraph and have moved some of the material to the methods section.

***Methods***

*Li 122 The authors acknowledge the limitations of the small sample size (n=4) however feel that ‘significant’ testing outcomes somehow validate that. There has been considerable discussion about the pros and cons of hypothesis testing, with some journals now not accepting conclusions based on nominated P values. Given that no discussion of effect size, power, or Type I and Type II errors is included in this manuscript, I suggest the authors are a bit more cautious in their interpretation of P values.*

We agree with this comment and have added language throughout the discussion to clarify which results were significant (and for which pre-fire year) and encourage readers to keep the limitations of this study, including small sample size and a substantial amount of time between initial sampling and the fire, in mind when interpreting the results.

*Li 185 There are references to results (Figures) in the Methods section which should be removed.*

Done

*Li 191 Some information is required to explain how nesting and foraging strategy information were obtained. Figure 1 not referenced in text*

Information has been added. See line 205. Reference to figure 1 has been added to the text. See lines 74 and 267.

*Li 201 I could not find the SIMPER output in the Results section.*

The reviewer is correct that there was an inconsistency between methods and results. The SIMPER analysis was left from a previous version of the manuscript in which we considered the impact of different species on the dissimilarity between communities. This mention of SIMPER has been removed.

***Results***

*Li 222-8 and Table 1 (and 2). I think it would be preferable to incorporate these mean and total values into Table 1, both simplifying the text and making the table easier to interpret. i.e. ‘direction of change’ in Table 1 currently has little meaning without the relevant values to compare.*

Means have been added to table 1 to make the results easier to interpret. We have chosen to leave information about total bee abundance and bee abundance per sampling event in the manuscript to clarify differences between years.

*Li 234 Is the R2 value correct here? If so, although the relationship might be ‘significant’, only a trivial amount of variance is being explained.*

We agree with the reviewer’s comment and have added a sentence stating that this only explains a small amount of the variance. See line 249.

*Li 229-236 Earlier, Li 198 suggested differences in community composition were tested using PERMANOVA, however these don’t appear to be results from that analysis. It would be appropriate to present the PERMANOVA model in full, including interaction terms and any pair-wise comparisons.*

See line 244 and Figure 4, S1. Table 3, S1. Figure 4, and S1. Table 4.

*Li 237-243 It is difficult to interpret these statistics when mean values are not provided. Exactly how different were the various groups (foraging and nesting strategies)?*

Means have been added to Table 1 and S1. Table 1.

*Li 251 I would have expected Figure 3 to be referenced in relation to network structure. It is not referred to in the Results section.*

We have added a reference to Figure 3. See line 280.

*Li 281 Again I think including mean values for the group comparisons would improve these Tables and make comments about the direction of change easier to interpret. There are some R2 values with false precision in both Tables (e.g. 0.0006508). Care should be taken when reporting results of computer output.*

Done

***Reviewer: 2***

*CONFIDENTIAL COMMENTS TO AUTHORS*

*The manuscript of Martin et al. explores the effect of fire on bee communities and plant-pollinator networks and their resistance to fire in the Mediterranean ecosystems of California, USA. Fire is considered a frequent disturbance in Mediterranean ecosystems shaping the post-fire pollinator communities and also the structure of the plant-pollinator networks. Our knowledge about post-fire structure and dynamics of bee communities and pollinator networks derives primarily from studies using space-for-time approach due to lack of pre-fire data. Therefore, this paper is unique in that the same sites have being sampled both pre- and post- a wildfire event, offering a rare opportunity to explore bee communities’ dynamics over time and after a major disturbance. Although I think that the manuscript presents a timely and highly interesting topic, I have many concerns. Overall, I found the manuscript somehow lengthy in the Introduction and the Material and Methods sections, and with inadequate interpretation of the results to some extent, while I have also some concerns about the statistical analysis.*

***Specific Comments***

*Lines 80-92 and lines 93-109: I think that the authors should reverse the order of the two paragraphs. Furthermore, they should give a briefer description of the sites used and of methodology in Introduction, and highlight the uniqueness of their study using the first paragraph of Materials and Methods in the introduction.*

We agree that the introduction contained unnecessary detail and repetition. Some material from the introduction has been moved to the methods section and the order of the final two paragraphs has been reversed. Additionally, we added more information on how unique this study is. See line 95.

*Lines 118-121: The authors compare 2018 post-fire data with two baseline years separately to detect the effects of fire, time period, and interaction term and report different results (please see comments on results). This complicates the results’ interpretation and raises some concerns about the observed cause-effect relationships.*

While we agree that there would be benefits to analyzing all years in a single model we decided to analyze our pre-fire years separately in order to retain more data. We felt it was important to match sampling dates in order to retain the maximum overlap in bee flight season between sampling periods, thereby reducing additional variation. For our analysis of 2002 vs. 2018 we were able to use a total of 70 sampling events (an average of 8.75 sampling events per site) and for 2003 vs. 2018 we used 57 total sampling events (an average of 7.12 sampling events per site). Had we combined 2002 and 2003 into a single analysis we would have reduced the total number of sampling events to 45 (an average of 5.62 sampling events per site). Keeping the 2002 and 2003 analyses separate complicates the analysis, but we felt that it was important to retain as many sampling events as possible in order to comprehensively sample across the flight season.

*Line 125: Do composition data refer to species identities?*

This has been changed to “community composition” to clarify that we are referring to species identities.

*Line 126: Here Figure 2 is referenced, but I did not find a reference to Figure 1.*

References to Figure 1 have been added. See lines 74 and 267.

*Line 129-130: Severity is a fire characteristic that has a significant effect on the ecosystems and pollinator communities (e.g. Ponisio et al. (2016). Pyrodiversity begets plant–pollinator community diversity. Global change biology, 22(5), 1794-1808; Lazarina et al. (2019). Moderate fire severity is best for the diversity of most of the pollinator guilds in Mediterranean pine forests. Ecology, 100(3), e02615; Galbraith et al. (2019). Wild bee diversity increases with local fire severity in a fire‐prone landscape. Ecosphere, 10(4), e02668.), affecting also their resistance to disturbances (e.g. Ponisio (2020). Pyrodiversity promotes interaction complementarity and population resistance. Ecology and Evolution). Therefore, it would be very interesting if the fire severity value of different sites was known. If sites are located in low-severity areas or in unburnt patches, then one should expect smaller changes in species richness, abundance, and species composition due to fire. This is also a possible explanation provided by the authors for the lack of significant differences.*

We agree with this comment and have added to the manuscript that fire severity did not differ between sites (line 332). It would be interesting to analyze the impact of fire severity, however a larger study would be required.

*Furthermore, the pre-fire data were collected in 2002 and 2003, that is 14-15 years before the fire event. In this time period, bee communities would change due to year-to-year variation, but this could not be the case for the architecture properties of network (Alarcón et al. (2008). Year‐to‐year variation in the topology of a plant–pollinator interaction network. Oikos, 117(12), 1796-1807; Petanidou et al. (2008). Long‐term observation of a pollination network: fluctuation in species and interactions, relative invariance of network structure and implications for estimates of specialization. Ecology letters, 11(6), 564-575).*

We have added a citation (CaraDonna et al. 2020) to help explain the low inter-annual variation in the aggregate properties of plant-animal mutualistic networks (lines 313). We have also added an explicit acknowledgement of the substantial amount of time between initial sampling and the fire (line 346).

*Lines 130-132: This is part of Data Analysis*

This information has been moved to the data analysis section.

*Line 189: The authors should provide a short description of the BACI analysis here.*

A description of the BACI analysis has been added. See line 192.

*Line 192: Means by site and year? Furthermore, did the authors perform the same analysis for species richness, the percentage of bees with different nesting and trophic preference?*

Means have been added to Table 1 and S1. Table 1. All results for this analysis can be found in these tables (including bee species richness, plant species richness, and percent below ground nesting bees).

*Lines 192- 194: From a statistical point of view, the small number of sites and how this number could affect t-test results troubled me. The authors acknowledge this caveat in lines 121-123, but still troubled me. Furthermore, I think that p-values should be adjusted by a test (e.g. Bonferroni) to correct for the multiple tests performed within dataset. Do the variables meet the t-test assumptions?*

See response to reviewer 1 Line 122. All variables are normally distributed and meet t-test assumptions. While we recognize that we are completing multiple tests we do not feel like the number of tests requires a Bonferroni test because the use of the Bonferroni corrections can be overly conservative (Gotelli, N.J. and A.M. Ellison. 2004. [A Primer of Ecological Statistics.](http://www.amazon.com/exec/obidos/ASIN/0878932690/qid=1104861582/sr=2-1/ref=pd_ka_b_2_1/102-2078418-1100925) Sinauer Associates, Inc., Sunderland, MA.).

*Lines 195-197: Which dissimilarity index was used to perform NMDS? I suppose Bray-Curtis as abundance data are available.*

The reviewer is correct that the Bray-Curtis dissimilarity index was used. We have added this to the manuscript. See line 213.

*Lines 201-203: I did not find the results of the SIMPER analysis.*

The reviewer is correct that there was an inconsistency between methods and results. The SIMPER analysis was left from a previous version of the manuscript in which we considered the impact of different species on the dissimilarity between communities. This mention of SIMPER has been removed.

*Lines 211-214: The authors here refer that resistance was defined as the change in abundance and species richness, and also change in the network structure. However, in my understanding, the results report the relationship between network properties and change in abundance and richness. But why network properties (at least some of them) should predict changes in the network size? The species composition and the numbers might change widely over time, even in the absence of a disturbance, but the network properties such as nestedness and connectance have been shown to be relatively stable. Therefore, the resistance of the network should be explored by comparing the same network property before and after the fire.*

We have changed the language to “change in network properties” to clarify that we did compare network properties before and after fire as the reviewer suggests. We apologize that this was unclear.

***Results:*** *What would we observe if differences between 2002 and 2003 were explored, given that the two baseline years give different results when compared to 2018? Perhaps a 2002-2003 comparison or including both years in the BACI analysis could shed some light on the post-fire dynamics of bee communities and their resistance to fire.*

See response to reviewer 2 118-121. We have added means to Table 1 and S1. Table 1 to allow comparison between years.

*Line 221: The authors should format the table (this applies to all tables in the manuscript).*

Done

*Lines 222-228: Abundance seems to be quite different in 2018 compared either to 2002 or 2003 (e.g. 639 in 2002-2018 comparison vs. 1918), but no significant difference was detected. Did the authors log-transformed the abundance?*

Abundance data was not log-transformed. This paper does not include an analysis of 2002 or 2003 vs. 2018 that includes period but not impact. In a separate analysis we did find a significant overall decline when comparing 2002 and 2003 to 2018. The BACI analysis controls for this overall decline in abundance.

*Lines 229-236: A significant effect of time period on species composition is reported, but the interaction term between period and impact is non-significant. Fire had a significant effect only after excluding time period in 2002-2018 comparison, but not in 2003-2018 comparison. That is somewhat alarming: Is it the fire the primal driving force of these changes or something else applies?*

We have added language to the discussion to encourage readers to consider certain limitations of our study, including limited number of sites and amount of time between initial sampling and the fire, when interpreting results. We have also clarified which years each conclusion is based on. See line 346.

*Lines 236: This is only marginally significant*

We agree and have added a sentence to clarify this finding. See line 249.

*Lines 237-239: Significantly more generalists were found in 2018 compared to 2003, but not in 2002-2008 comparison. How this could be explained?*

We discuss the change in generalists and its relation to changes in the plant community in the discussion. See line 327. We have added that this increase in generalists is based on the comparison using only one of the pre-fire years. We were surprised that we did not find the increase in annual plants that we expected would increase the resources available to generalists. These unexpected results, and the differences found between 2002 and 2003 are likely a result of floral resource availability in the broader landscape.

*Lines 252-260: Generality and modularity of pollination networks were significantly higher in 2018, but only compared to 2003. The pre- and post-fire network structure seems to be similar in the case of 2002-2018 comparison.*

We have added clarification on this to the discussion. See line 321.

*Lines 262-266: I am not sure that I understand how this analysis was performed.*

We have added to the text to try to clarify that this analysis looked at the impact of fire on the overall structure of the plant-pollinator networks rather than the individual network characteristics (generality, modularity, nestedness, etc.).

*Lines 268-284: The resistance was analyzed by linear modelling. The authors should also provide coefficient of determination.*

The estimate values have been added to Table 2 and S1. Table 2.

*Lines 279-284: I am not convinced that pre-fire network properties should predict the changes in abundance and richness (keeping in mind that no significant differences in abundance or richness were found). The authors should explore the effect of fire, time period and their interaction term on network properties change.*

This prediction is based on many studies finding that fire increases abundance and species richness. Given that stability is associated with certain network metrics we could therefore extrapolate that communities with network metrics indicating stability would experience less change in abundance and richness than those with network metrics indicating less stability.

Thebault and Fontaine 2010 explore the relationship between the structure of ecological networks and community stability. Many of our predictions about the relationship between network properties and stability are based on these findings. The path analysis in figure 2 of Thebault and Fontaine (2010) shows how connectance, species diversity, modularity, and nestedness are related to measures of stability (persistence and resilience). More modular networks are less persistent, connectance is negatively related to modularity, and more connected networks should be more persistent. Modularity is only weakly related to resilience. We have added a reference to Thebault and Fontaine 2010 to predictions about the relationship between diversity and stability to clarify our reasoning (see line 48). We also added a reference to this paper in the results section. See lines 76 and 79.

***Discussion****: Overall, I found the discussion a bit “weak” and with some issues in terms of interpretation. The authors seem to focus only on the significant relationships in the case of different results between baseline years, without interpreting the lack of a significant relationship with the other baseline year. Furthermore, 14- 15 years elapsed between the first sampling and the fire event. I believe that authors should also take this into consideration. Many changes can take place in Mediterranean ecosystems in this time period (even from year to year).*

We agree with the reviewer that more caution should be used when interpreting our results. We have added language to the discussion to clarify the significance behind our conclusions and clearly state that additional variation was introduced to this study due to the 14-15 years that elapsed between the initial sampling and the fire. See line 346.

*Lines 291-293: But also a non-significant relationship was observed in 2003-2018 comparison. Furthermore, I am not convinced that all network properties could offer insights into changes in abundance and species richness. Abundance and richness may vary, but network properties have been shown to tend to be relatively stable (e.g. Dupont et al. (2009). Spatio‐temporal variation in the structure of pollination networks. Oikos, 118(8), 1261-1269.; Petanidou et al. (2008). Long‐term observation of a pollination network: fluctuation in species and interactions, relative invariance of network structure and implications for estimates of specialization. Ecology letters, 11(6), 564-575).*

We have added a citation (CaraDonna et al. 2020) to help explain the low inter-annual variation in the aggregate properties of plant-animal mutualistic networks (line 313). We have also added an explicit acknowledgement of the substantial amount of time between initial sampling and the fire (line 346).

*Lines 295-297: This is rather a bold statement.*

We agree with the reviewer and have removed this statement.

*Lines 304-308: A more concise explanation is required.*

Done. See line 322.

*Lines 313-317: This perhaps explains all the results. Perhaps, sites were located either in low-severity or unburnt areas, thus when significant differences are detected are not due to fire, but to a plethora of factors that could change over the years.*

We have added that fire severity did not differ between our sites, so while the patchy nature of the fire may have impacted our findings differences in fire severity did not. Also see our added language about the years between initial sampling and the fire (line 346).

*Lines 324-326: Gathmann, A., & Tscharntke, T. (2002). Foraging ranges of solitary bees. Journal of animal ecology, 71(5), 757-764.*

This reference has been added.