Dear Editor,

First, we thank the reviewers for their review of our manuscript. Their comments have helped us improve the quality of our manuscript. Please find attached our response to the reviewers' comments on our manuscript, 'A post-wildfire response in cave dripwater chemistry' by Nagra et al.

We have provided a detailed reply to all questions and comments raised by the three reviewers, as requested. We will provide a revised version should the decision be made to accept the manuscript.

If you have any further questions we will be happy to answer them. Thanks again!

Kind regards,

For the authors,

Gurinder Nagra

Reviewer Comments

Reviewer #1

The authors present an excellent multi-year cave monitoring study that uniquely provides insight into the response of cave dripwater isotope and geochemical compositions to POST-fire vegetation dynamics.

We thank the reviewer for these kind remarks.

Overall, the manuscript does not present a rigorous, integrated argument. Many of the interpretations come across as speculative without rigorous constraint or consideration of alternative hypotheses.

We appreciate the reviewers' comments, and agree to address them in our revised manuscript. We will go through the manuscript again to further integrate our argument and assure the manuscript reads more smoothly. And to apply more rigor to our argument, we will consider alternate hypotheses, and place more clear constraints on the claims we make. We constrain our claims by making a more quantitative comparison to differences in solute concentrations as a result of natural heterogeneity as suggested by Reviewer # 2.

Furthermore, the use of groundwater and cave dripwater monitoring in another cave used as a pre-fire baseline is not compelling nor necessary, as the authors should be focusing on the response of dripwater to the recovery of the ecosystem following the fire, not to the fire itself. That is, the authors argue that a progressive decrease elemental concentrations reflects a gradual decrease in transpiration due to tree death - however, a fire would result in a dramatic, instantaneous decrease in

transpiration not a prolonged, multi-year response. It is recommended that the authors switch their perspective from "response to the fire" to "response to the recovery from the fire".

We appreciate and acknowledge these comments. Regarding the use of data from another site as a pre-fire baseline, reviewer 2 and 3 recommend putting our site into context. We will adjust the wording in our revised text to reflect the 'response to the recovery from the fire'.

L72 – is there a way to quantify "intense wildfire" and put it into context of the range of wild fires in the region or historical time interval? This seems pertinent to understanding the magnitude of the event when thinking about its implications for interpreting past events from cave deposits.

The wildfire burnt 1200ha and was of a high enough intensity to calcine and cause fracturing of the limestone at the cave's entrance. We will add these details in the revised draft.

L193 – What is 3 years based on? Another study? A best guess?

The three years is an informed maximum water residence time based on the depth of the cave, the response time observed at a greater depth at Golgotha cave (Mahmud et al., 2016) and the isotope variability. We will make this clearer in the revised text.

L201-205 – It seems that the authors tuned parameters of a forward model to best match modeled and observed drip hydrology. What observational window/interval was used? 2005-2011? I assume that the model was tuned to observations at Yonderup, not Golgotha, correct? Is it possible that multiple parameterization schemes could result in similar comparison between observations and data?

We used the observational time window of 2005 to 2011. However, since our seepage reservoir requires a minimum 10-month residence time for year-round flow, we add monthly average rainfall data from 2003 to 2005 as a 'warm up' period for the model to avoid edge effects. But only the observation time window between 2005 and 2011 is shown. Yes, the model was tuned to observations at the studied cave site (Yonderup). We will make this clearer in the revised text.

The input parameters for the model are few (rainfall isotopic composition and the bedrock flow thresholds). And using numerous flow scenarios we show the full range of possibilities. We find that no hydro-climatic scenarios can explain our observed δ^{18}O. Thus the model vs data offset suggests another factor is affecting δ^{18}O. We attribute the higher δ^{18}O values in our data to increased evaporation conditions post-fire. We will re-write this section in our revised manuscript in and add this detail to make this point clearer.

L240 – Does a bivariate plot of δH vs δ^{18}O support evaporative enrichment of these dripwaters? I.e., trend off the local meteoric water line with lower slope?

Yes. Firstly, we would like to note evaporative enrichment that occurs in a high humidity environment (> 95% relative humidity) will occur with a similar slope to the local meteoric water line (LMWL), and this has been observed in cave environments (e.g. Cuthbert et al 2014). Cuthbert et al (2014) also characterized the scenarios which would result in offset from the LMWL and we have based our interpretation on these. In our case, the bivariate plot (Fig.5) shows that the least squares regression (LSR) for cave dripwater falls within the standard error (± 0.45‰) of the slope for the LMWL (weighted

LSR), but with drip water isotopic composition shifted towards higher $\delta^{18}O$ and $\delta^{2}H$ on the LMWL. This falls under a *type 1* scenario suggested by Cuthbert et al., (2014). In the *type 1* scenario drip $\delta^{18}O$ and $\delta^{2}H$ do not deviate from the LMWL but are relatively enriched. The *type 1 scenario* suggested by Cuthbert et al., supports our case for near-surface evaporation occurring in a humid near-surface environment (> 95% relative humidity).

L270 – What does "it" refer to?

Here 'it' refers to observed drip water composition. We have made the appropriate changes in text.

L329-331, 338-340 – A fire is an abrupt event that likely decimated vegetation instantaneously. How can this be reconciled with a gradual trend of increasing dilution? If the fire resulted in a shut-down of transpiration, a flushing of transpiration-concentrated poor water might be expected followed by relatively dilute concentrations until vegetation reestablished and transpiration lead began to concentration waters again. This might explain a gradual increase in solute concentrations (as seen at Site 2a and noted in L340-342), but not decrease. However, increasing solute concentrations with increased transpiration assumes that vegetative nutrient uptake is reliable relative to concentration due to transpiration.

Yes, a fire would lead to a dramatic reduction in transpiration above a site and we do agree that at Site 2a we see a vegetation recovery response post-fire. However, at site 1a a transpiration-reduction response is not necessarily reflected immediately after the fire. This can be due to a number of reasons; firstly, it simply takes time for water to reach the cave as the soil moisture deficit would have to be overcome. Second, many of the solutes would be released from the ash, so their concentrations depend on the dissolution of the elements from the ash over time. It is likely the decrease in concentrations are reflecting both a return of element concentrations to values without the influence of the tuart tree and the diminishing leaching of elements from the ash as suggested by reviewer 2. We will make the appropriate changes to shift our argument and add this explanation to our discussion in the revised draft.

L332 – Is Golgotha Cave further inland, and therefore have less aerosol Cl deposition?

Both caves are ~5 km from the coast and we have added this detail to the text. So, based on location alone, both are likely to have a similar amount of aerosol Cl deposition. However, variations in vegetation density between the coastline and the site could also influence Cl compositions to some extent. We add this to the revised text so the reader is aware.

L346 – How would an increase in surface evaporation induce PCP?

A higher rate of surface evaporation creates longer water-rock interaction times which is ideal for PCP. This is further detailed in Fairchild et al. 2000, which is referenced in the text. We will re-write this to further clarify the appropriate references in the text.

L349 – Secession of microbial and root respiration should be abrupt and coincident with the fire, not a gradual signal.

We agree and have deleted this.

L364-386 – This discussion seems highly speculative with very limited constraints on the proposed interpretation.

We argue that at Site 1a, elevated concentrations of SO_4 and K are being maintained by the abundance of above-ground biomass ash – sourced from the tuart tree. While, Cl and other solutes such as Mg, Sr and Ca at Site 1a, are reflecting dilution or a decrease in leaching of these elements post-fire, as suggested by reviewer #2 (see comment L333 – 334). Grove et al., 1986 found post-fire soils in this southwest Australia region to contain 23% more S and 16% more K than pre-fire soils, up to 1 year after the fire, as a result of the ash deposition of the overlying biomass. Given the abundance of biomass above our site (the tuart tree), we attribute the high SO_4 and K concentrations in post-fire soils from the burnt tree to be maintaining high SO_4 and K concentrations in dripwater. However, we acknowledge the reviewer's comments and will re-write this section and reduce the amount of speculation by quantifying the difference between SO_4 and K concentrations in comparison to other solutes at our sites, as suggested by reviewer #2 (see comment Lines 430 – 431).

L397-398 – How does model dripwater δ^{18}O agree with observed δ^{18}O? They look to be substantially offset. Additionally, it is not clear how simulation of dripwater δ^{18}O supports the interpretation that tree death gradually reduced transpiration resulting in gradual decrease in solute concentrations.

Yes, we agree with Reviewer 1 that the observed dripwater δ^{18}O and modelled δ^{18}O are distinctly offset after the fire. This offset is underpinning our argument: that there was increased evaporation after the fire due to the lack of shading (see reply to reviewer 1, regarding L201-205). We will make this clearer in the text at L258-269. We agree that the statement in L397-398 is inconsistent with this interpretation and thank the reviewer for pointing this out. We will delete this statement in a revised manuscript.

L399 – A bit of context would be helpful – does this apply to a single tree? A stand of trees? At this (fire-affected) site? In this region?

This is specific to a full grown tuart tree that is native to the coastal dune systems of southwest Australia. Drake et al., (2011) tested the transpiration potential of seedlings and full grown trees from Yalgorup Nationl Park in different seasons to determine the pressure gradient created by their roots. We will add this detail in the revised text.

L400 – it is not clear what "this potential" refers to, nor is it clear what gradient (i.e., from where to where) is being referred to. . .. How does a hydraulic gradient maintain high Cl concentrations?

Our use of 'this potential' refers to the 'potential energy' or ability of tuart tree to extract water (by transpiration). This generates a pressure gradient between the soil and the tuart tree. The gradient transports both water and nutrients towards the tree, but not all are taken up and as a consequence are left behind in the soil solution, in particular salts like Cl. We will re-write and further clarify this in the revised manuscript.

L408-410 – It is not clear how a fire in Feb 2005 results in a sharp increase in evaporation in the Mar 2007.

As mentioned in the text, since the cumulative water balance (CWB) is positive we don't yet observe the effect of increased evaporation. Once CWB becomes negative (P < ET) we see δ^{18}O rise sharply. We believe the decrease in shading post-fire coupled with the P < ET condition was enough to drive this sharp increase in δ^{18}O and eventually exhaust the reservoir feeding the drip.

L419 – δ^{18}O reflecting more evaporation but lower solute concentrations tracking less concentration (more dilution) due to less (evap)transpiration does not make the most compelling argument.

Forward modelling of dripwater δ^{18}O compared with the observations provides firm evidence that the relatively higher observed δ^{18}O at both sites can only be explained by increased evaporation (see reply to reviewer 1, regarding L201-205). With this constraint, the simplest explanation for the declining trend in most of the solutes is a decrease in tree water use at Site 1a and removal from the surface and subsurface (leaching) post-fire (suggested by reviewer #2; see comment L333 – 334). The dominating effect of tree water-use on dripwater solute concentrations, post-fire is reported in Treble et al., (in press - accepted 8/4/16) which, using a mass-balance approach, quantifies the role transpiration from regrowth on solute concentrations at Golgotha Cave. We will rewrite this explanation in the revised text section to make it clearer for the reader.

L243-276 – It is not clear how modeling δ^{18}O contributes to this study. The results are not well integrated into the interpretations, and it is not clear why a model tuned to the drip hydrology does so poorly in accounting for observed dripwater δ^{18}O. Is this meant to support the interpretation that evaporation of infiltrating water occurs somewhere between the surface and drip site, and that evaporation might also play a role in dictating solute compositions? If so, this does not come across clearly.

And

L453 – It is not clear how/why the modeled δ^{18}O represents a control (no fire) scenario.

By forward modelling δ^{18}O we aim to predict the composition of δ^{18}O in dripwater using rainfall isotopic composition and bedrock flow parameters. The model represents hydro-climatic influences only. It simulates the probable drip δ^{18}O based on rainfall δ^{18}O as an input and parameters representing the physical hydrology, as outlined in the m/s. It therefore acts as a control scenario in which only hydro-climatic influences are modifying drip δ^{18}O. Hence, the fact that our drip δ^{18}O are higher than that simulated, supports our argument that a post-fire increase in surface/near-surface evaporation is driving drip δ^{18}O. See reply to Reviewer 1, L201-205. For further clarity, we will re-edit the modelling text with these comments in mind.

L436 – use of groundwater and nearby cave dripwater as pre-fire conditions is not compelling, nor necessary. The argument that the recovery of an ecosystem after a disturbance is potentially reflected in cave dripwater is compelling on its own.

Reviewer #2 suggests it is necessary to compare with regional sites. In our revisions we will try to reconcile comments by both reviewers.

L441-444 – The case supporting these statements is not compelling. L446-447 – How has the vegetation forcing been delineated from the climate (i.e., CWB) signal?

See reply above for L453 with regards to δ^{18}O. Our interpretation that a response to post-fire conditions is the dominating response is further supported by our solute data. The fact that most of our solutes display contrasting behaviour at our two sites demonstrates that highly localised factors, not climate, are dominating. Cl is a conservative ion and hence is primarily driven by dilution/evaporation. We interpret Cl's decline post-fire at Site 1a to reflect dilution of the water store that the dead tree previously exploited, and a decrease in leaching of post-fire as suggested by reviewer #2 (see comment L333 – 334). In contrast, Cl increases at Site 2a, sympathetically with $\delta^{18}O$, consistent with an increased evaporative demand on shallow water stores driven by post-fire reduction in shading and reduced albedo. We will clarify our concluding remarks to strengthen our arguments in a revised manuscript.

L448 – Increasing K and SO4 trends are not obvious from Fig. 3, and why would the degree of leaching increase with time from fire? It might be expected that there would be a pulse of K and SO4 following the fire, then leaching would decline after the initial pulse.

At Site 1a, K and SO_4 do not decrease like other solutes due to dilution and removal from the surface and subsurface through leaching, as above. The fact that K and SO_4 appear to be unaffected suggests that their flux has increased, counteracting the dilution and source decline, observed from other solutes. K and SO_4, have been found to have higher concentrations, in post-fire soils (Grove et al., 1986), up to one-year post-fire, as result of over-lying biomass ash deposition. Thus our interpretation for the sustained, high concentrations of K and SO_4 is that ash from the burnt tuart tree has increased the K and SO_4 flux. Further the difference in dissolution rates could also explain the rate at which these solutes are leached into drip water in comparison to others, as mentioned in our reply to Reviewer #1 comment L329 – 331, 338 – 340.

At Site 2a, we see that SO_4 and K follow the concentration trend driven by the dominant forcing, evaporation, at this site (see previous comment) and do not show an ash signal. This due to the site having less biomass available to be converted to soluble ash, and thus it is a less dominant forcing. We will make sure that we quantify the differences between K and SO_4 vs. other solutes at our sites (see reviewer #2 comment L 430 – L431) and outline and possible scenarios for these differences in our revised manuscript.

L449 – This is vague and not all that helpful of a conclusion.

And

L450 – No evidence to support this was presented in this manuscript so it is a bit odd to present as a conclusion.

Thank you for pointing out the weaknesses in the conclusions. We will re-write the conclusion in a more quantitative fashion summarizing the findings of our study.

Reviewer #2

This manuscript presents a high quality cave monitoring study from 2005-2011. The authors collected a suite of analyses to better understand cave and climate processes, and ultimately how these signals are incorporated into speleothems.

Thank you for your kind comments.

Plot the raw rainfall $\delta^{18}O$ time series in Figure 3c along with the forward model dripwater $\delta^{18}O$. It will be interesting to see how the model alters the above-ground signal.

We were very fortunate to be able to access these data for the purpose of performing the forward model calculations. These rainfall $\delta^{18}O$ data are unpublished data created for the IAEA/GNIP program by ANSTO non-coauthors who are acknowledged in the Acknowledgements. As a separate paper on these data is currently in preparation, the owners of these data, understandably have declined to have these data published here in the time series format that is being requested. However, we do have permission to provide Reviewer 2 with a version of Figure 3 that does contain these data to satisfy their query.

Why is the modelled dripwater $\delta^{18}O$ so much smoother than the dripwater data? Is the temporal resolution of the rainfall collection too low? Or is the rainfall data being smoothed too much by the model?

The rainfall data is monthly, as stated in the manuscript, so it is not the temporal resolution of the rainfall data. The model output had been smoothed to reflect a typical stalagmite sampling resolution. This smoothing has now been removed.

Lines 262-264: The slope calculations are subject to serious edge effects. For example, the modelled dripwater $\delta^{18}O$ has an inflection point early in the record in 2006. This is not observed in the dripwater data. Perhaps you could use a bootstrap to calculate the error on the slope, but given the high density of points in blue curve of Figure 3c leaving out one or two or even three points probably will not change the slope too much. But it is this inflection point early in the modelled dripwater record – that is not in the actual data that is causing the very different slopes. The trend over both look very similar from 2007-2011. Also, the slope at for site1a should be compared with the slope of the modelled δ18O over the same time period: ~2005-2007. Therefore, I'm not convinced that evaporation is driving such a large difference in the dripwater $\delta^{18}O$.

Given the slope calculation are prone to edge effects, we will remove the slope calculations. We believe that the 1-2‰ offset between modelled $\delta^{18}O$ and observed $\delta^{18}O$ is compelling enough to show $\delta^{18}O$ is being driven by increased evaporation. This range in isotopic enrichment is consistent with isotopic enrichment due to evaporation in caves from semi-arid regions (Rutlidge et al., Markowska et al., 2014). Further the shallow depth of this cave (4 m) as well as the reduced shading and change in albedo following the fire, makes it even more prone to evaporative effects.

Plot the Mg/Ca and Sr/Ca time series. I cannot do the calculation in my head using data from Figure 3e, f, and g. Do they co-vary in time? Or is the ln(Mg/Ca) vs. ln(Sr/Ca) relationship driven by changes that are not coeval?

Yes, Mg/Ca and Sr/Ca time series co-vary in time we will add this as a figure in the revised manuscript. This coupled with the ln(Sr/Ca) vs ln(Mg/Ca) slopes (Fig. 6) and the diagnostic range (a slope± 0.88) given by Sinclair et al., (2011) support our case for PCP. We will add these details in the revised manuscript.

Mark on Figure 3 when the fire occurred

We will add this in our next draft.

Line 331: how do pre-fire Cl values compare between caves?

Unfortunately, we do not have pre-fire Cl values for the studied cave site (Yonderup).

Lines 333-334: without values from before the fire, it's hard to discern exactly what is the cave response to the fire. Could it not be that at Site 1a there was a spike of Cl, Mg, Ca, and Sr after the fire due to dissolving ash (Lines 357-358)? Then the downward trend would be the slow removal of those from the surface and sub-surface. Without data from before the fire to establish a baseline, most of the arguments about what the fire did are too speculative and unsupported.

It is possible that at Site 1a there may have been a prior peak in Cl, Mg, Ca and Sr. However, any peak would have been a rapid increase prior to monitoring as they are highly soluble and CWB was positive. Solutes would also be affected by a direct concentration effect created as the soil and vadose zone water was heated by the fire. But as suggested by the reviewer, the trends we see here, post-fire, in these solutes, could be reflecting both the slow removal of these nutrients from the surface and subsurface, and reflecting a reduction in tree water-use. We will add this other possible cause for the decline of solutes at Site 1a to our argument in the revised text.

Lines 430-431: The differences between Yonderup and Golgotha should be quantified. Listing many values in the table does not support the differences quoted in the text. There are 10 differences to calculate 2 sites at one cave, 5 sites at the other. From the population of 10 differences, one may then calculate the median, mean and standard deviation. Then it will be clear 1) how much the drip chemistry differs and 2) how much variability exists just heterogeneous environments, which means to say *not* fire-related.

We will calculate the difference suggested to further quantify our interpretation and pre-empt speculation. We think a better way to describe the natural heterogeneity is by the observed differences between sites at each cave (e.g. Site 1A vs Site 2A). As this will show how each site responds over time.

Reviewer #3

Monitoring studies such as this provide insight into karst and speleothem processes and as such are valuable data sets for the scientific community. Understanding the response of karst systems to fire is potentially a great asset for paleoclimate interpretations, as it may be a new way to track past limitations in water availability. Application of a forward model to aid in the interpretation of the monitoring data is also a valuable aspect of this study.

We thank the reviewer for these kind comments.

However, the manuscript lacks rigor in the presentation and interpretation of the results as well as in the overall presentation. For example, the abstract provides no results that would support any of the conclusions.

We will add more details from our finding in the abstract. This includes specifics on how we utilized the forward model to demonstrate that drip $\delta^{18}O$ was 1-2 per mil higher than predicted by hydro-climatic processes supporting enhanced evaporation post-fire, further supported by the Cl data; that the distinct spatial and temporal differences between site δ^{18}O and solutes rules out that both sites could be climatically controlled. We will also detail the potential role of the death of the tree in explaining the post-fire observations at dripwater site 1a. Further we will also detail the use of ash-derived SO_4 and K that are leached in dripwater.

The application to speleothem studies discussion in the Conclusions should be its own section and come before the Conclusions. To be useful to other researchers, it would be helpful to provide exposition of the subtleties that would be involved in such applications. For example, 1) how many different proxies would be needed to delineate fire influence, given that δ^{18}O in speleothems is affected by many processes, including in-cave processes not related to climate or vegetation disturbance?; 2) How would a researcher delineate between drip sites impacted by fire but without a tree in the recharge zone for the drip, vs. climate processes, vs. in-cave processes?

We thank the reviewer for this constructive comment and will add an 'application to speleothems' section discussing the information that can be used out of our study to find a proxy for fire. We will bolster this section by discussing recently published studies from Golgotha Cave (Treble et al 2015 and Treble et al GCA – accepted) that further strengthen the interpretation presented here.

We will specifically address the suggestions that the reviewer has made above. We believe a multi-proxy approach that uses a suite of soil and bedrock sourced elements and δ^{18}O should be used as proxies to analyse a fire signal.

We recommend multivariate statistical analysis should be used to separate climate/seasonal forcing from a soil/ vegetative forcing. Here, we would expect the local soil/vegetative forcing to preserve the impact of a fire on stalagmite composition, in trace elements like S, K and P. All of which have been found to increase in abundance in post-fire soils, due to burnt biomass-ash. Further, given fire can increase discharge by reducing tree water-use, it is also worth looking at colloid associated metals, such as Al, Fe and Cu which could show immediate spikes after the timing of the fire.

We will also discuss the importance of site selection i.e. the depth of the cave, the overlying vegetation and the climate setting, as not all sites will be affected by fire. Our study helps to place constraints on the search for a paleo-fire signal in caves and also informs other monitoring studies of this nature.

Line 28. How is the analysis unique? What specifically is unique about this analysis?

AND

75. see comment on abstract re 'unique'; 'analysis' is used twice in this sentence.

The analysis is one of the first monitoring studies conducted in a post-fire regime that seeks to identify the nutrient dynamics and effects of wildfire on dripwater composition. We will make this point clearer in the revised copy.

29-31. Run-on and awkward sentence.

We will re-write this sentence.

33. This is the most significant claim of the study. Explain how the δ^{18}O, chemistry, indicate and support this claim. There is nothing in the abstract that provides any hint of what the results of the study are.

AND

35. How so? What are the results that indicate this?

AND

36. 'here we open a new avenue for speleothem science': Without answers to the above questions, this statement is not supported.

These points will be covered by the modifications suggested in the first reply to Reviewer 3.

46. 'local environmental factors' such as. . .?

Evaporation, transpiration and leaching from biomass-sourced ash. This will be inserted in this sentence for clarification in the revised copy.

48-49. Monitoring studies have also focused on controls on calcite growth, the role of CO2, respiration, and other factors and processes (Wong et al. 2011, GCA; Breecker et al. 2012, GCA)

We will add these references in the revised copy.

51. AET – define acronyms upon first use. AET is not defined until figure 3

AND

137. ANSTO: define acronyms on first use.

All acronyms will be explained upon first use in the revised manuscript.

52. Is this 'the exception' or the rule? There are many more monitoring studies in water limited regions, including those cited above

Will remove 'the exception'.

62. Wong reference – this study is not an example of vegetation loss due to fire.

We will more clearly state this study is an example of vegetation loss not vegetation loss due to fire.

74. give cave name and location

We will add the cave name (Golgotha Cave) and location 36.10 deg S, 115.05 deg E to the text and location figure of the revised manuscript.

85-6. Give length of time comprised by temp record; give geologic age of Tamala Limestone. 113. 'heterogeneous' in what way?

We will add the length of time from which we obtained the averages for the temp and rainfall records. We will give the age (Quaternary) of the Tamala limestone. Further we will also add that the soil is heterogeneous in thickness and spatial coverage above our sites.

114-5. make into two sentences

We will separate this into two sentences.

123-5. state the length of the collection interval.

The length of the collection interval varied over time between 2 – 4 times a month we will detail this in the new revised draft. Bi-monthly is the best way to describe it.

129, 132. State location of instruments. Is the ICP-AES a spectroscope or spectrometer? List it as such.

Our cation concentrations were measured using a Thermo-Fischer inductively coupled plasma-atomic emission spectrometer (ICP-AES) at the Australian Nuclear Science and Technology Organization facility in Lucas Heights. We will add this to our revised draft.

145. Was this drip size verified for the site? Why refer to an experimental study if you have both drip speed and volume/time?

When drips were fast enough to be measured we recorded the time intervals between the drips. When the drip was too slow we left a bottle and measured the volume of the discharge over a particular period of time. Thus in order to represent the data in common units we needed to use this drip volume in order to convert all our discharge data into volume data. The calculations have been provided in the supplementary info excel sheet. We will clarify this in the new draft.

151-4. What does 'this' refer to? RMC? P-AET? This is a run-on sentence and hard to follow.

Here 'this' refers to the residual mass curve (RMC). We will detail this in the revised draft.

157 what is 'FWE'? 160-161. define terms upon first use, which is much earlier than here

FWE is the acronym for AET in the Australian Water Availability Project data sheets. We have clarified in this sentence that this is the parameter that we used since there are multiple parameters available to estimate evaporation terms from this dataset.

170. How far away? Yanchep is not on the map FIG 1.

Yonderup Cave which is shown on the map is in Yanchep NP. We will make this clearer in text.

178-9. Only one year different from the study's fire. Was the burn less intense? Less destructive? How did it effect the lower and middle understory growth?

The prescribed burn at Golgotha Cave was much less intense and much more spatially heterogeneous than the fire that is reported in our study. We will make this clearer in the text and refer to the manuscript currently under review for the impact of this fire on Golgotha Cave.

Run-on sentence 188. the 'latter' what? Several different things are developed in the previous sentence.

We use the term 'latter' to refer to seepage and fracture thresholds, we will make this clearer in the text.

192-4. confusing sentence, try restating as: 'Storage time for water that enters the seepage reservoir is modeled as a Gaussian distribution. This time is set as a maximum age of 3 years to reflect the shallow depth of our cave system. The model allows for the mean and standard deviation to be specified for these functions.' Furthermore, what is this based on? It's more the nature of the flowpath through the vadose zone than it is the thickness of the vadose zone that will determine

storage time. It seems that this value is adjusted later based on observations at the cave, but need to have a basis for this starting point.

We will readjust the sentence as suggested. These sites have been found to be predominantly controlled by seepage or matrix flow due to the calcarenite type lithology at our sites (Mahmud et al., 2015). Further, capillary barriers have been found to affect hydrology at these sites which delay downward movement of water. With this knowledge and the shallow depth of the cave we set a 3-year maximum limit.

198. What is 'a karst store'?

By 'karst store' we were referring to water storage in the overlying bedrock. We will make this clearer in our next section

217. Why 'soil water availability' here, and 'water availability' above?

We will change 'soil water availability' to water availability to make it more consistent.

224. 'increases' should be past tense

We will change to 'increased'.

236-9. Run-on sentence 239. Add (DFJ) for northern hemisphere readers

We will add 'DJF' after 'summer' and 'JJA' after 'winter' for northern hemisphere readers and divide the sentence with punctuation.

240-1. when first defining 'thresholds' also define it in context of 'seepage thresholds'

We will re-write our definition of thresholds by defining 'thresholds' in the context of seepage and fracture thresholds.

243. Unclear. Try rewording: 'We attempted to model dripwater δ^{18}O that matched the measured drip water values based on using the rainfall isotopic data set as our input?'

We will reword this sentence, as suggested, to make it clearer.

244. Is 30 mm rainfall? State this. Could other variables in the model cause the shutoff?

Yes, 30 mm is the P – AET threshold. Given P – AET is the only input in the model and the only thing limiting the water from entering the seepage reservoir is the threshold. Thus it is unlikely that other variable in the model would cause the shutoff.

247-251. Run-on. 253. is this the mean or the weighted mean?

It is just the mean. We will make this clearer and fix the Run-on sentence.

265. What makes a given model 'meaningful'?

First, meaningful models referred to modeled scenarios that maintain full year round drip flow to match observed drip flow. This eliminated models with a seepage threshold of 40 mm or greater. And given not all rainfall enters the seepage reservoir, some threshold must exist, so we set a minimum seepage threshold of 10 mm. Second, models that reflect the minimum residence time (10 months) to maintain full year round flow at our t our sites were chosen. Under these bedrock constraints we tested all possible scenarios from predominantly fracture flow (10 – 15 mm) to all seepage flow (10 – 1000 mm). We found that fracture dominated flow showed more variability than seepage dominated flow. However despite the variability, none of the models that we ran fit the observed δ^{18}O at both sites. It is clear, δ^{18}O at both sites is offset from modelled hydro-climatic δ^{18}O We will provide brief clarification on this in the revised manuscript.

273-4. try rewording to: 'This is similar to a type 1 scenario defined by Cuthbert...'

We will reword this sentence as suggested.

288. Interpretation, not Results, belongs in Discussion 291-6. difficult to follow, rewrite. 300. Arguments should be in Discussion, not Results.

We will move L288 and L297-301 to the Discussion.

316. For sure evaporation will affect chloride in water in the same way dilution and mixing will, not potentially.

We will change 'and potentially' to 'as well as'.

348. This figure shows no diagnostic model, even though the caption states that both sites fall within the model, the figure doesn't show it.

We will clarify this sentence by replacing 'shown by the diagnostic model' with 'as evidenced by the agreement of the ln(Sr/Ca) vs ln(Mg/Ca) slopes in our data with the diagnostic range (a correlation co-efficient ± 0.88) given by Sinclair et al., (2011)'.

358. Unclear. If 3 increases Ca in dripwater, why would dripwater not reflect the increase? Is the signal from 1 and 2 so large as to make 3 background? Or is it simply sequence of events? Run-on sentence.

Here we were aiming to put forward a possible number of scenarios as it is hard to constrain. It is likely 1 and 2 are more important than 3 which we will clarify in the revised manuscript.

389. Reference figure earlier in text when describing site and processes.

We will reference this figure earlier in the text as suggested.

450-2, and 455. This is new information, more appropriate to include it prior to Conclusions

We will move this information to the Discussion.

456-7. Further, that a fire signal may be much more subtle in a speleothem if the fire impacted drip sites without trees above them.

Thank you. We will add this point to our Discussion.

458. Growth rate not covered in text, this is more new information that is more appropriate to include prior to conclusions. See general comment above about.

We will move this information to the Discussion.

Fig. 1. Where is Yanchep National Park located? Where does the inset sit on the map of Australia?

Yonderup Cave is situated within Yanchep National Park. We will make this clearer in the figure in our revised manuscript

Fig. 2. The same data are presented in Figs. 2 and 6. Only one of these is needed. If the authors are going to employ the Sinclair graphical model, then it would be Fig. 6. However, the discussion of Fig. 6 in the text and caption claim to show that the Sinclair PCP model holds and can account for the trends, yet there is no text or addition to the figure that supports this claim. In Fig. 2, state that the values plotted are for drip water. The interpretation given that both (each) site has an independent flow path is not explained. What specifically indicates this? Different starting points? Distinct slopes?

We will replace Figure 2 with Figure 6 as it is diagnostic for PCP as suggested. We will quote the regression equations for both the normal and log-transformed data. We will clarify in the text that the difference between regressions calculated for each site (using either method) indicate independent drip paths as suggested by the reviewer. We will also add a ln Mg/Ca and ln Sr/Ca time series to make our case for PCP more clear.

Fig. 3B. It's difficult to see Site 2a measured values. Since 'Est Dis', 'Meas Dis' used, also label sites as 'Calc' for clarity.

We will make this adjustment to Fig 3.

641. concentrations appear to be quadruple, not 'double' that of Site 2a.

The concentrations are x4 for K and x2 to x3 for SO4. We will make this adjustment in the text.

643-4. differentiation of temporal trends between 1a and 2a: I disagree with trying to make a difference here, as the data do not support this. Both sites show nearly the same slope of increase and the trends are obscured by gaps in the time series.

We will delete this sentence.

Fig. 4. Explain which thresholds. Why would the 10-75 mm threshold have a lower response than the 10-15 mm model? Why not present time series for the proposed fire-sensitive ions such as SO4 and P? Show where the fire event occurred in relation to the time series.

We thank the reviewer for picking up on this. We found that one of our input rainfall isotopic data points for the date for 07/2009 was + 5.16 instead of -5.16, this was offsetting the model for 15 – 100, 10-15, 40 – 100 and 10 – 1000 mm thresholds which had the (+) instead of the (–) value while the 10 – 75mm had the correct negative value. We have also eliminated the smoothing in the model as suggested by reviewer #2. This figure is provided below. We will also add the time series of SO_4 and K along with an indicator for when the fire occurred to put the time series into context as requested.

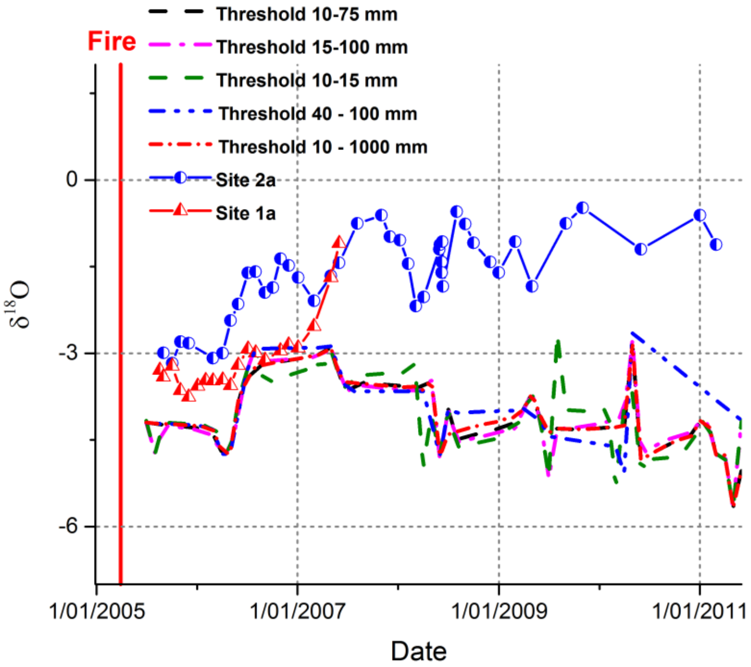

Fig. 5. Move Mean rainfall to below rainfall. Use color other than red since red is for cave drip info. Is the mean rainfall the mean or weighted mean?

We will move the mean rainfall to below the rainfall and use a color other than red.

Fig. 6. Place both sites on same plot with different symbols, in order to help the reader directly compare them. See comments above on Fig. 2. Presentation of time series for these element ratios would aid in their interpretation and how the processes proposed to account for the variation change with seasons, etc.

We will add Mg/Ca and Sr/Ca time series to aid in the interpretation of processes affecting these trace elements such as PCP.

References

Grove, T. S., O'Connell, A. M., Dimmock, G. M.: Nutrient changes in surface soils after an intense fire in jarrah (*Eucalyptus marginata*) forest, *Australian Journal of Ecology*, 11, 303 – 317, 1986.

Mahmud, K., Mariethoz, G., Baker, A., Treble, P. C., Markowska, M., and McGuire, E.: Estimation of deep infiltration in unsaturated limestone environments using cave lidar and drip count data, Hydrol. Earth Syst. Sci., 20, 359-373, doi:10.5194/hess-20-359-2016, 2016.

Markowska, M. et al., 2016. Semi-arid zone caves : Evaporation and hydrological controls on d 18 O drip water composition and implications for speleothem paleoclimate reconstructions. *Quaternerary Science Reviews*, 131, pp.285–301. Available at: http://dx.doi.org/10.1016/j.quascirev.2015.10.024.

Rutlidge, H., Baker, A., Marjo, C.e., Andersen, M.S., Graham, P.W., Cuthbert, M.O., Rau,G.C., Roshan, H., Markowska, M., Marithoz, G., and Jex, C.N.: Dripwater organic matter and trace element geochemistry in a semi-arid karst environment: Implications for speleothem paleoclimatology, Geochimica et Cosmochimica Acta, 135, 217–230, 2014. doi: 10.1016/j.gca.2014.03.036.

Sinclair, D.J., 2011, Two mathematical models of Mg and Sr partitioning into solution during incongruent calcite dissolution, *Chemical Geology*, v. 283, p. 119-133.

Treble, P.C., Fairchild, I.J., Baker, A., Meredith, K.T., Andersen, M.S., Salmon, S.U., Bradley, C., Wynn, P.M., Hankin, S., Wood, A., McGuire, E., (accepted 8/4/16). Roles of forest bioproductivity, transpiration and fire in a nine-year record of cave dripwater chemistry from southwest Australia. *Geochimica Et Cosmochimica Acta*.

Treble, P.C., Fairchild, I.J., Griffiths, A., Baker, A., Meredith, K.T., Wood, A., McGuire, E., 2015. Impacts of cave air ventilation and in-cave prior calcite precipitation on Golgotha Cave dripwater chemistry, southwest Australia. *Quaternary Science Reviews* 127, 61-72.