Dear Dr Blume,

On behalf of the authors I would like to thank the two anonymous reviewers for their time and efforts in providing detailed and thoughtful feedback that I am confident will greatly improve the manuscript.

As requested we have provided a detailed response to each comment and query raised by the reviewers. We will provide a revised version of the manuscript should our response be deemed competent. Please do not hesitate to contact me if you require further information.

Kind regards,

Katie

Anonymous Reviewer #1

The dominance of a particular flow regime changes over time, for example, older limestone tends to have higher secondary porosity (more fractures and enlarged conduits) and a lower primary porosity due to compaction or cementation (Ford and Williams, 1994).

The manuscript presents data of drip counts in a cave in Australia. The data are analysed with a recent signal processing tool which allows to identify the strength and frequency of periodic signals in a time series. A specific feature of the method is that is allows to identify consecutive periods of the time series which show a periodic signal. This is important for this dataset of cave drip water because there are only a few days within almost two years of measurement which show periodic / diurnal signals. However, the signals are not consistent in space that means they are different at other locations in the cave and they are not consistent in time, that means they do no occur at similar periods. Furthermore the phase of the signals is also not consistent. These spurious occurrences of the period signal may render the finding of a period signal as less important. Still its diagnosis is one of the most important and direct results of this manuscript. For the rest of the manuscript the authors try to argue about the origin of the periodic signal. They discuss several earlier proposed causes of the diurnal signal and argue that only a root water uptake could be a reasonable cause. However, there is no direct evidence being presented to undermine this discussion. Therefore I recommend to be much more careful in the wording, e.g. L536 "this is the first volumetric observation of tree water use in cave drip water". I have found a number of other issues, see below, which need clarification. Nevertheless, I think that these issues can be resolved within a thorough revision of the manuscript.

Major remarks

- How representative is the drip measurement? The data shown in Fig. 2 seems to be rather variable and site dependent.

Thank you for this comment. The drip sites were chosen using a stratified sampling method. A transect of the cave was used to select three locations (G, M and LR) that satisfied the following criteria 1) there were actively dripping speleothems, 2) spatially distant from the other locations and 3) different depths within the cave. Individual drips were sampled randomly at each location, with selection guided by practical constraints such as the stalagmite surface being suitable for placement of a logger and the drip falling from high enough to activate pressure sensor on logger etc. We will include a more detailed site description to address this issue.

- In the methods section radiation data is being mentioned, but is not used!

We thank the reviewer for this comment. The radiation data was used to calculate daily evapotranspiration as explained in lines 137-140:

"The climate parameters used were air temperature (mean, maximum and minimum), relative humidity (mean, maximum and minimum), wind speed and solar radiation."

- Abstract L31: unclear what is meant with "trends in drip rate at different timescales"

Thank you for raising this point. We will reword this phrase to make it clearer. This is the first observation of tree water use in cave drip water and has important implications for karst hydrology in regards to developing a new protocol to determine the relative importance of trends in drip rate, such as diurnal oscillations, and how these trends change over timescales of weeks to years. This information can then be used to infer karst architecture.

- Section 2.1: - Is the cave relevant for paleoclimatic proxies? - What is the approximate contributing area to the cave?

Glory Hole Cave is likely to be relevant for paleoclimate proxies as it is well decorated and in close proximity (<100 m) to caves that have been used in multi-proxy speleothem based paleoclimate studies (Webb et al., 2014; Markowska, 2015). The contributing catchment area is ~1 km^2. We will add these details to Section 2.1.

- Methods 2.2 - why do you estimate daily potential ET when the focus is set to diurnal variations?

Thank you for your queries. The daily potential ET was estimated to show the multi-day relationship between the presence of drip cycles and ET, rather than just relying on air temperature. We would like to refer the reviewer to lines 282-285, lines 388-392 and lines 398-404.

- Section 2.3 spectral analysis - clearly describe input and outputs - what is the form of the periodic signal, is it sinusoidal? - By which criteria did you determine the presence of a periodic signal?

Thanks for your queries. We have revised the section describing the synchrosqueezing (SST) procedure in the methodology and it is now much clearer in regards to input and output. The signal does not have to be sinusoidal. If it is, the frequency content will be much stronger. Otherwise, it will be decomposed into signal components and spread across frequency bins. We visually identified the presence of periodic signals from the SST plots, as is clearly described in the manuscript. We clarified many formulations in this section. Hopefully, this has answered the questions.

-Figure 2: - time resolution of drip rates - unit of drip rates

We will change the unit of drip rate in Fig 2, thank you for raising this point.

-Figure 3: - Y-axis labels on left panels are hidden - for the SST panels it is unclear which time series is transformed? - how is the presence of a significant periodic cycle determined from these plots?

Y-axis labels have now been redrawn in a new Figure 3. For SST panels, we have clearly marked the time series in the legend as well as inside the respective panels (labels correspond to the ones in Table 1 and Figure 2). A response to the last question was given previously.

-Section 4.2.1: the p-value of the t-test is very low suggesting a very low probability of the Null hypothesis of no difference. Thus there is a significant difference of pressure in cave and outside. Anyway I doubt if a t-test on the central tendency is the right tool to assess the ventilation effect. Please check this and revise accordingly.

We thank the reviewer for this comment. We agree that a t-test was not the best statistical approach and have instead used Kendall's Tau which shows a strong correlation (0.82) significant at 0.05.

-Section 4.2.5: the authors mix up long wave radiative exchange processes and L403-413 need to be revised.

We thank the reviewer for bringing this to our attention and will revise the text primarily by removing 'long wave' as a source of incoming radiation.

-L433: To my understanding deep root water uptake is only required when the upper soil layers get too dry and have a lower potential than the soil water at deeper levels. See papers discussing hydraulic lift (e.g. Dawson, 1996 Tree Physiology, Zapater et al., 2011, Trees). Therefore I think that in the wetter periods no relevant deep root water uptake occurs.

We thank the reviewer for this information and the suggested references. We agree that deep root water uptake is only required during drought periods, when the shallow soil is dry. We will revise the text to reflect the information in the references provided.

Minor remarks:

- use SI units (L125-L132)

We will correct this so that all presented data is in SI units.

- L272 wrong reference - it should be Fig.3d

Thank you for this comment, this reference will be corrected.

- L300f how are recession times being computed?

The recession times are calculated from the peak of the hydrograph to the point when the drip rate returns to the baseline value. We will clarify this in the text accordingly.

- L342: there is no Fig 4c

Thank you for identifying this mistake, we will correct the reference to Fig 3c.

- L439: What is meant with negative hydraulic lift?

We thank the reviewer for this comment. Negative hydraulic lift refers to the process when the transpiration rate is low and water is transported from the roots back into the soil. We will clarify this sentence along the lines of "Burgess et al (2001) measured sap flow in Eucalypt tap roots, finding hydraulic lift peaked around 1 pm and negative sap flow values indicated reverse (acropetal) flow between 7pm- 7am."

Anonymous reviewer #2

In the submitted manuscript Coleborn at al present a study that deals with the identification and characterization of daily fluctuations of cave drip rates in a karstic cave in New South Wales, Australia. They installed drip counters at 12 locations within the cave and use a method called Synchrosqueezeing to identify periods with stable signals of 1 and 2 drip rate cycles per day (cpds). Such periods could be identified for a subset of the 12 drips, with varying length and signal type (1 or 2 cpds). Comparing the daily signal of those drips with explanatory variables such as air pressure differences between the cave and the atmosphere, the barometric loading due to the daily heating and cooling of air masses, earth tides due to the gravitational influence of the moon, temperature's influence on water viscosity, and solar driven cycles of evapotranspiration activity of the plant cover, they show that evapotranspiration is the most likely reason for daily fluctuations in drip rates. Based on this finding they develop a conceptual model of the impact of vegetation on drip rates under different climatic and structural setups. Generally this is a very valuable study. It reveals understanding of processes that have not been investigated before. The manuscript is well written and the results are plausible and of high relevance.

We thank the reviewer for recognising the value of this manuscript.

However, there are some important revisions to be done before publication. My major point of criticism is the lack of quantification of the relations between diurnal fluctuations of drip rates and their explanatory variables. Some few r2s and p-values are provided but the most important part of the discussion (4.2.5. Solar driven daily cycles of vegetative (phreatophytic) evapotranspiration) could definitely use some more quantification of the identified relationships and their significance.

Thank you for these comments; we will address your concerns point by point below.

Some specific comments:

1. The introduction needs some information of the relevance of this type of research.

We thank the reviewer for bringing this to our attention. We agree that this study has important implications for understanding karst unsaturated flow processes and karstic groundwater recharge. Currently, most karst models use very simplistic representations of unsaturated flow, if it is considered at all (Hartmann et al., 2014a). This study highlights the importance of vegetation dynamics on vadose flow and recharge making it significant to karst modelling research and speleothem-based paleoclimate studies which focus on the impact of vegetation dynamics on proxy records (Treble et al., 2015, accepted for publication 8/4/16).

2. The spectral analysis explained in too little detail (schematic figure could be helpful)

This issue was also raised by reviewer #1, and we have expanded the description in response. The synchrosqueezing transform methodology was developed and tested by other reviewers, and going into detail exceeds the framework of this manuscript. We have referenced all works required by the interested reader to familiarise with SST.

3. It is not clear whether the selection of periods of stable cycle per days was based on a threshold procedure ore done manually and subjectively.

We thank reviewer #2 for their comment which was also raised by reviewer #1. We visually identified the presence of periodic signals from the SST plots, as is clearly described in the manuscript. We clarified many formulations in this section. Hopefully, this has answered the questions.

4. Implications for karst recharge assessment are missing in the discussion.

We thank the reviewer for recognising the wider implications of our research and for providing these excellent references. This study clearly demonstrates the potential for vegetation to impact karst water recharge making this research relevant to karst modelling and karst water resources assessment. Currently, there are no approaches that consider the impacts of vegetation on recharge dynamics in process-based karst models (Hartmann et al., 2014b, 2015) or in empirical recharge estimation approaches (Allocca et al., 2014; Andreo et al., 2006).

Please see the attached commented version of the manuscript for further details.

<u>Comments within text</u>

-Not necessarily → please check and cite standard Karst literature as

Goldscheider, N., Drew, D., 2007. Methods in Karst Hydrogeology. Taylor & Francis Group, Leiden, NL.

Ford, D.C., Williams, P.W., 2013. Karst Hydrogeology and Geomorphology. John Wiley & Sons.

We thank the reviewer for this suggestion. After consulting Ford and Williams (1994) we will reword the sentence “the dominance of a particular flow regime changes over time, for example, older limestone tends to have higher secondary porosity (more fractures and enlarged conduits) and a lower primary porosity due to compaction or cementation (Ford and Williams, 1994).

- also worth citing:

Arbel, Y., Greenbaum, N., Lange, J., Inbar, M., 2010. Infiltration processes and flow rates in developed karst vadose zone using tracers in cave drips. EarthSurf. Process. Landforms 35, 1682–1693. doi:10.1002/esp.2010

Lange, J., Arbel, Y., Grodek, T., Greenbaum, N., 2010. Water percolation process studies in a Mediterranean karst area. Hydrol. Process. 24, 1866–1879.

Sheffer, N.A., Cohen, M., Morin, E., Grodek, T., Gimburg, A., Magal, E., Gvirtzman, H., Nied, M., Isele, D., Frumkin, A., 2011. Integrated cave drip monitoring for epikarst recharge estimation in a dry Mediterranean area, Sif Cave, Israel. Hydrol. Process. 25, 2837–2845. doi:10.1002/hyp.8046

We thank the reviewer for suggesting these references and agree that they are relevant to the manuscript. We will include them in the manuscript citations.

-Please add short paragraph on the relevance of such investigations in terms of understanding Karst unsaturated flow processes and karstic groundwater recharge, paleoclimate reconstructions etc. Most karst models use very strong simplifxations of unsaturated karst flow processes, if they consider unsaturated flow at all.

Hartmann, A., Goldscheider, N., Wagener, T., Lange, J., Weiler, M., 2014. Karst water resources in a changing world: Review of hydrological modelling approaches. Rev. Geophys. 52, 218–242. doi:10.1002/2013rg000443

We thank the reviewer for this comment and for providing a suggested citation. We would like to direct their attention to our response to ‘Specific Comment #2’ above.

-don't want to be picky but the meaning of "AHD" might not be obvious to everybody.

We thank the reviewer for this comment; we will expand the acronym to “Australian Height Datum” for clarity.

-Is this true for all types of drips? Is there some uncertainty involved. Please provide shortly Some more detail why this exact volume per drop ol water is valid.

Thank you for this comment, the cited paper is an experimental study where the effect of spherical stalactite diameter and flow rate on drop volume. There is uncertainty when estimating drop size because the volume changes over time because of variation in flow rate and the changing morphology of the stalactite. For example, the diameter of the tube in a soda straw formation could shrink during a period of low flow rate leading to a smaller drop volume. It is also impossible to quantify the error of these estimates without measuring physical volumes. We argue that the drop volume 0.19 ml is a reasonable assumption as this value was used in a study conducted at the same site in a cave with a similar distribution of stalactite radii and formation (Markowska et al. 2015). We will include a more detailed explanation in the amended manuscript.

-So is it Potential evaporation? Please clarity.

It is potential evaporation; we will clarify this in the text. Thank you for bringing this point to our attention.

-The elaborations of this method are not detailed enough. Also there is too much referencing of secondary literature. Please add some more elaborations, desirably a small sketch that show how the method works schematically.

This issue was also raised by reviewer #1, and we have expanded the description in response. The synchrosqueezing transform methodology was developed and tested by other authors working in the field of signal processing. Going into detail here exceeds the framework of this manuscript. We recognise that SST is a brand new technique, and that many readers would be unfamiliar with it. We have therefore clearly referenced all works so that interested readers can familiarise with this spectral analysis technique.

-Was there a systematic threshold or were stable cpds selected manually. A systematic selection would be desirable.

We used manual visual identification the presence of periodic signals from the SST plots, as is clearly described in the manuscript (lines 160-162).

-Please set y-axis to 0, With the present soaking it is difficult identifying when the drips actually fall dry.

Thank you for this suggestion, we will make the changes to Fig 2 so that y-axis starts at 0.

- Correlation?

We thank the reviewer for this suggestion. We chose the phrase ‘connection’ rather than ‘correlation’ because the statement is not based on a statistical outcome.

-Less permeable ?

Thank you for this suggestion, we will make the suggested change in the text.

-Other studies also show this type of behavior. Please see for example the three studies on cave drip investigations in Israel that I mentioned in a previous comment.

We thank the reviewer for this suggestion, we agree that these references are relevant and we will add the citations in the text.

-What about hydraulic connectivity? When percolating waters passed the regions where erapotranspiration takes place evaporation won't affect its flow percolation rate any more, right?

We thank the reviewer for this comment. If the unsaturated zone above the cave had a homogenous hydraulic connectivity, we would expect there to be no spatial or temporal variability in the occurrence of the daily oscillations. However, this is not the case and we argue the hetereogeneity in karst architecture primarily controls the spatial and temporal variation in the presence of the daily oscillations, however, hydraulic connectivity does not cause the phenomenon. Thus, we argue that it would not be appropriate to include hydraulic connectivity here as suggested because this paragraph discusses possible drivers of the daily oscillations.

-Can you quantify the T amplitude - strenght of cpd signal relationship?

We thank the reviewer for this comment. We found that the T amplitude-strength of cpd signal relationship was quite complex, which we argue is further evidence that this is a biological process rather than a physical one. Unfortunately, we could not quantify this relationship but argue, nonetheless, that it is an interesting outcome from this study. We will reword the text to ensure a statistical relationship is not alluded to.

-How strong is the relationship? Is it significant?

The relationship between 2-day moving average of air temperature and drip rate during the period 1/2/14- 19/2/14 is significant and weak (τ= -0.21, significant at 95%). We will amend the text to reflect this information.

-Can you quantify the negative relationship between cloud cover and strength of cpd signal?

Thank you for this query. We do not use cloud cover data, rather we use daily ET to examine the relationship between radiation and cpd signal on a multi-day timescale and air temperature as a proxy for ET on a sub-daily timescale. We would like to direct the reviewer to our response above regarding the T amplitude-strength of cpd signal relationship.

-This subsection is quite long, moving this statement to the implications in the next subsection could shorten it

Thank you for this suggestion. We argue that this paragraph is needed here so there is a logical flow from discussion of solar radiation to evapotranspiration. However, we agree that this section is too long and will insert a further subsection ‘4.2.5.1 Scenarios for solar driven daily cycles of phreatophytic evapotranspiration’ to reduce the length of this section.

-I really like the conceptual elaborations in Fig 6. They could be improved when adding small graphs of expected daily variation of drip rates in the 9 Figures (similar to Fig 7 in Barbera & Andreo, 2011)

Barberá, J.A., Andreo, B., 2011. Functioning of a karst aquifer from S Spain under highly variable climate conditions, deduced from hydrochemical records. Environ. Earth Sci. 65, 2337–2349. doi:10.1007/s12665-011-1382-4

Thank you for this suggestion, we had similar graphs on the original sketch and we will add them into the Fig 6.

-The outcomes of this study are clearly relevant for paleoclimte reconstructions by speleothems, as well expleined in this subsection. But the implications are also of highest relevance for karst modeling and karst water resources assessment.

There are still no approaches that do consider the impacts of vegetation on recharge dynamics, neither in process-based karst models (Hartmann et al., 2014, 2015) nor in empirical recharge estimation approaches (Allocca et al., 2014; Andreo et al., Andreo et al., 2008)

Allocca, V., Manna, F., De Vita, P., 2014. Estimating annual groundwater recharge coefficient for karst aquifers of the southern Apennines (Italy). Hydrol. Earth Syst. Sci. 18, 803–817. doi:10.5194/hess-18-803-2014

Andreo, B., Vías, J., Durán, J., Jiménez, P., López-Geta, J., Carrasco, F., 2008. Methodology for groundwater recharge assessment in carbonate aquifers: application to pilot sites in southern Spain. Hydrogeol. J. 16, 911–925.

Hartmann, A., Gleeson, T., Rosolem, R., Pianosi, F., Wada, Y., Wagener, T., 2015. A large-scale simulation model to assess karstic groundwater recharge over Europe and the Mediterranean. Geosci. Model Dev. 8, 1729–1746. doi:10.5194/gmd-8-1729-2015

Hartmann, A., Mudarra, M., Andreo, B., Marín, A., Wagener, T., Lange, J., 2014. Modeling spatiotemporal impacts of hydroclimatic extremes on groundwater recharge at a Mediterranean karst aquifer. Water Resour. Res. 50, 6507–6521. doi:10.1002/2014WR015685

We thank the reviewer for their comments; we would like to refer them to “Specific Comment #4” which we believe addresses their queries.