We thank the reviewer for their helpful comments, our responses are below (in blue)

Overall, the paper is well written, and the results are solid. However, the most critical point I see is that the study does not provide a broader context. How do the results impact water management for the study area and for the region? Regarding the uncertainty in the estimated recharge rates and spatial and temporal variability, it is not obvious to me how sustainable water resource management can set up. Perhaps the authors have some thoughts about this problem and might provide some suggestions.

This was also raised by Reviewer #1. Our aim in this study was a better understanding of recharge rates in this area and assessing the uncertainties and limitations of commonly-applied recharge methods in general. As we explained, estimating recharge is important to understand the hydrogeology of semi-arid regions and this study has relevance to other regions where these techniques are used. Groundwater is not extensively used in this catchment, and our comments on sustainability were a more general recognition that groundwater can be a vital resource in semi-arid areas. In terms of the specific need to understand recharge in this catchment, defining whether recharge rates changed with land-use changes are important (especially the potential impacts on waterlogging and streamflow caused by changing water table elevations). We will clarify the aims and implications in the final paper.

Moreover, if one of the objectives of this study is to assess and compare uncertainty in the methods, then this has to be more elaborated and systematically compared. In addition, these results should be compared to similar studies.

We compared our results with other studies (Section 5, lines 439-441); e.g., Dean et al., 2015, Hydrology and Earth System Sciences, 19, 1107–1123; Perveen, 2016, http://hdl.handle.net/1959.9/560005); Cartwright et al., 2007, Journal of Hydrology, 332, 69-92; Crosbie et al., 2010, Hydrology and Earth System Sciences, 14, 2023-2038. These studies specifically mentioned that the WTF method overestimates recharge rates. We will emphasise this more in the discussion and introduction.

Furthermore, I miss a conceptual model which describe the processes. That can be a schematic figure or a cross-section describing the different flow systems and geochemical signatures.

We will add a cross-section to help understand the hydrogeology of the area and the processes.

Some further comments and suggestions are provided below.

Introduction: Personally, I believe that the study objectives should be clearly communicated in 1. Introduction. I found it a bit confusing to get information about the different methods before knowing the target of the study.

Although they normally appear at the end of the introduction, we can move the objectives to earlier in the introduction (before where we discuss the methods). This will help the reader understand how that discussion relates to the study areas. We will also make it clear how this study contributes to a general understanding of recharge in semi-arid areas.

Line 48ff. Not only in semi-arid areas recharge varies in space and time. Also in humid areas, recharge can be considerable spatially and temporally different (see, for example, Moeck et al., 2020 and Mohan et al., 2018, among many others) Line 50ff:

This is certainly the case. Although this paper is not a general review of recharge methods, we will note this in the introduction.

You could add Darcy methods, soil moisture methods, heat tracers, baseflow separation techniques, empirical relationships, etc. for completeness of the provided list (see for instance Healy, 2010, Walker et al., 2019).

While a comprehensive review of recharge processes would be out of place in this paper, we will mention these other methods for completeness.

Section 1.1.1. When residence times are around ~25000 years, how likely is that all Cl is originating from rainfall only and the impact of runoff can be neglected. This is more of a question rather than a critic. You already indicate based Cl/Br ratios that evapotranspiration rather than halite dissolution is the main process in controlling groundwater salinity but would be the error in estimated recharge rates if a small amount of Cl is not only originating from precipitation?

This method does assume that all the Cl is derived from rainfall. These are upland catchments with ephemeral stream systems, located at the top of a major regional catchment divide so the catchments do not receive any Cl input from sources other than precipitation. In the study area, the Cl/Br ratios, and the lack of halite in the soils and bedrock make this the case (regardless of the residence time of the waters), as discussed in Section 4.2. In general, it is also the case for other semi-arid areas in southeast Australia where recharge rates have been calculated (e.g., Cartwright et al., 2007, Journal of Hydrology, 332, 69-92), and similar recharge rates could be estimated using Br rather than Cl. However, it is something that needs to be tested whenever this method is used. We will discuss this assumption further in the introduction to the revised paper.

Line 85ff: In the study area with an actual ET of ~600 mm/a, to what depth can ET impact be observed. I am asking because I am not sure if the observation wells 3008 (depth 1.3, pasture) and 3657 (depth 2.5 m, forest with deeper root zones) can be reliably used by applying the water table fluctuation method, although I have to note that the estimated rates seem to be in the same range as for the other observation points.

The recharge rates using the WTF method were calculated for those bores because they show a clear seasonal variation (lines 222-223). We note that the effect of ET is likely small during winter when radiation and temperatures are lower and rainfall is larger. Additionally, the bores in the plantation are installed near the stream where trees are not planted to create a buffer zone and limit the effect of the plantation on streamflow. Thus, it is reasonable to consider the effect of evapotranspiration on the magnitude of the groundwater fluctuations to be low and possibly negligible. We will clarify this in the revised paper.

Line 295-297: Maybe I misunderstood something here, but did you not indicate that all Cl is delivered by rainfall (e.g. Line 351). Please check the statement and maybe reformulate the second part of the sentence.

Reviewer #1 also commented on this. This sentence was incorrect as written, it should have stated: "The observation that the Cl/Br ratios are significantly lower than those that would result from halite dissolution (10^4 to 10^5: Kloppmann et al., 2001; Cartwright et al., 2004; Cartwright et al., 2006, Chemical Geology, 231, 38-56) and do not increase with increasing salinity indicates that Cl is predominantly derived from rainfall". We will correct it in the revised paper.

Line333ff: Not clear. Please explain why it is not possible.

It is because it would require an initial $a^{14}C$ that is not possible. All the samples with measurable ^3H that lie on the ^3H vs. ^{14}C covariance curves will be less than 200 years old. Over that time span, there has been negligible decay of ^{14}C, and the initial $a^{14}C$ of the sample can be calculated by mass balance (it is the measured $a^{14}C/q$). Consider a sample with a measured $a^{14}C$ of 95 pMC; if there were 10% contribution of DIC from ^{14}C-free calcite dissolution (q = 0.9), it would imply an initial ^{14}C activity of 106 pMC (which is plausible for water recharged during the bomb-pulse period). However, if we were to propose that q = 0.7, then the initial $a^{14}C$ would need to be 136 pMC. This exceeds the highest $a^{14}C$ recorded in soil CO_2 of ~120 pMC (Jenkinson et al., 1992, Soil Biology and Biochemistry, 24, 295-308; Kuc et al., 2004, Geochronometria, 23, 45-50; Tipping et al., 2010, Geoderma, 155, 10-18) and so is implausible. The cited papers explain this; however, we will add a fuller explanation to the text.

Line392ff: Just from Fig.4 it is not possible to identify the samples. Perhaps you can better highlight these samples in Fig. 4 or provide the link to Table 1.

We can better highlight the samples that show the mixing in the figures, and also refer to Table 1 in the caption and text where this information is also shown.

Apart from that, I am wondering that location 3663 do not show mixing with older groundwater, even though it is one of the deepest locations (~25m) and based on the drawn picture with the stratification I was expecting that older groundwater exists.

While that may be expected, the observations from the ^{14}C and ^{3}H imply that little mixing has occurred at this locality. Although bore 3663 is deep, this is largely due to the topography, rather than depth below the water table (the screen is approx. 10-15 m below the water table). Location 3663 is in the regional recharge area and the groundwater in this area is expected to be relatively young. The groundwater flow here is likely to be downwards with little mixing with older laterally flowing groundwater.

Moreover, the decline in the groundwater level for 3663 is uniformly over the monitoring period, which is in contrast to the wells 3657 and 3669. But for all these the TRR is applied and no mixing is assumed. Could you please elaborate more on these differences?

Bore 3663 is the only one that is actually in the forest. The other two (and most others) are in cleared areas between the stands of trees. Since the water levels probably respond to recharge over areas of a few m^2 (Scanlon et al., 2002, Hydrogeology Journal, 10, 18-39), the difference probably reflects the more limited recharge in the forest areas (e.g., lines 473-475). Some recharge will occur at all the sites since the aquifers are unconfined. We will explain the distribution of recharge better in the revised paper.

Line 491ff: Yes, I absolutely agree, and this is an important point. The question of which arises from the results is how we can set up sustainable water resource management than when we have such a spatial and temporal variability as well as uncertainty on ~500 ha. Perhaps the authors have some thoughts about this problem and might provide some suggestions.

As we replied to the other reviewer, our aims in this study was a better understanding of recharge rates in this area and semi-arid areas in general. This is important as we explained to understand the hydrogeology of semi-arid regions. Groundwater is not extensively used in this catchment, and our comments on sustainability were more general recognising that groundwater is a vital resource in semi-arid areas. In terms of the specific need to understand recharge in this catchment, the possibility that recharge rates change with land-use change is important (especially the potential impacts on waterlogging and streamflow). We will clarify both these matters in the final paper.

Line 495: Although I agree that physical-based models are useful tools, the models will likely not represent in every detail the recharge processes because of the lack of observations although a relatively high density of data exists and information about the subsurface heterogeneity are missing (in both, x, y and z-direction). Thus, calibration is required which will also lead to uncertainty. For me, this part sounds like we always should use physical-based models, and then we get the right recharge rates which are obviously not true, even though the models are very powerful. Please reformulate.

We will modify this phrase not to give the impression that models are always correct and can be used to estimate correct recharge rates. This was not the intention of this paragraph, which rather meant to stress the uncertainties associated with the use of measured recharge rates as boundary conditions for groundwater models. Integrated models calculate recharge rates and can support experimental studies by providing an additional estimate of recharge rates.

Figure 1: If available, it would be useful to add the precipitation on top of the graphics (second y-axis).

This relates to Fig. 2. We will add the weekly rainfall to this figure.

Also, why does well 3658 show an increase?

Bore 3658 shows very little response over the monitoring period (head levels vary by <1.4 m). The bore monitors groundwater at ~16 m in the lower part of the catchment and probably does not record the short-term recharge. The actual water level measurements from 3658 are not very reliable because the formation has low permeability so two screens were installed in the completion. The upper screen was placed in what subsequently appears to be a perched zone that periodically dries up and the lower screen in a deeper zone.