Response to comment #C5845 (reviewer 1)

Preamble

We would like to thank the reviewer for the clear, detailed commentary and in-depth review of the manuscript. This has benefitted us greatly.

The reviewer makes a substantial number of comments, divided into specific and technical according to their nature. In the interest of clarity and to facilitate cross-referencing, we have transcribed them into the response and have numbered them according to the original order and type: #1-#20 for the specific comments and #T1-#T16 for the technical ones. The response will address these individually.

We should also state beforehand that an entirely new additional table, providing an overview of the sampling campaigns (parameters, dates) and associated precipitation data (Table 2) has been added, that winter radon mass balance data (2009) illustrating the similarity we had mentioned existed with the summer radon mass balance (2010) and addressed in #T10 has been added to Table 1; and two of the original figures (Fig 1 and 4) where updated to incorporate the suggestions provided in #T15 on the one hand, and help clarify the queries associated with technical comments #T2, #T5, #T9, #T10 as well as further support clarification of queries brought by specific comments #8, #9 and #14 on the other. As such, the redrawn Figure 1 now incorporates two panels, in order to provide a more complete geographical context. It now includes a) the location of all the six inlets mentioned in the text as well as the river Gilão (panel a), and b) additional information pertinent for clarifying pore-water sampling locations for the periods 2007 and 2010-2011, other suggested location references as well as a clear cut definition of the western and eastern sectors of the lagoon area focused upon in this study.

The redrawn Figure 4 on the other hand now includes a fourth panel illustrating the daily precipitation record over the region (2006-2013), taken from public databases, on which we have superimposed the periods of sampling that are relevant to the study to provide a temporal context on the precipitation regime over the region. Hopefully, this will make it easier to follow the discussion and help clarify issues raised in specific comments #8, #9 and #14. We append the redrawn figures, edited Table 1 and new Table 2 to the end of this response letter for consultation.

Responses to review comments

#1. P12436 L9 After *Indeed, [...]* please add *'on a global scale an estimated 6%...'* as the anticipated percentile SGD contribution is different for oceans/ continents etc. as you certainly know.

R: Done.

#2. P12436 L14-19 I suggest rewriting this passage as:
1. I am sure hydrogeologists or any other expert do see SGD, if they are familiar with that term, as any fluid flow regardless of fluid composition or driving as defined by Burnett et al. 2003.
2. In this context, I suggest to simply state the given or any other definition and adapt the following lines accordingly.

R: Done. The segment was streamlined to: 'To understand the contribution of groundwater/seawater interactions to marine biogeochemistry (Moore 1996; Moore and Church 1996; Church 1996, Moore 2006), the definition of SGD needs encompasses any flow of water across the sea floor, regardless of fluid composition or driving force (Burnett et al. 2003). This is because reactivity of solutes when meteoric and sea water mix and travel through porous media significantly alters the composition of the discharging water with respect to both original contributions (Moore 1999; Moore 2010). Submarine Groundwater Discharge is therefore not limited to fresh groundwater discharge but includes seawater recirculation through coastal sediments (Li et al. 1999) and seasonal repositioning of the salt/freshwater interface (Michael et al. 2005; Edmunds 2003; Santos et al. 2009).'

#3. P12437 L 10 The authors may think about exchanging Lee 1977 with one or two of the rather new and partly very interesting publications concerning direct flux measurements.

R: In the absence of any specific suggestions as to what the aim of this addition would be, we assumed that reviewer would want some citations that specifically addressed the temporal or spatial significance of direct flux measurements in the context of basin-wide studies. With this in mind, we opted to add references to Taniguchi et al 2003 and Michael et al 2003 at this point, both of which address the issue of small spatial coverage of direct flux measurements and their up-scaling potential.

Taniguchi M, WC Burnett, CF Smith, RJ Paulsen, D O'Rourke, SL Krupa, JL Christoff, 2003. Spatial and temporal distributions of Submarine Groundwater Discharge rates obtained from various types of seepage meters at a site in the Northeastern Gulf of Mexico. Biogeochemistry 66: 35-53. Doi: 10.1023/B:BIOG.0000006090.25949.8d

Michael HA, JS Lubetsky, CF Harvey, 2003. Characterizing submarine groundwater discharge: A seepage meter study in Waquoit Bay, Massachusetts. Geophysical research Letters 30 (6): 1297, doi: 10.1029/2002GL016000

#4. P12437 L 16 *'fail to include seawater recirculation'* this is not quite correct see e.g. Li, Hu, B., Burnett, W., Santos, I., Chanton, J. (2009) Submarine Ground Water Discharge Driven by Tidal Pumping in a Heterogeneous Aquifer. Groundwater. 47(4): 558-568. Please change.

R: Quite. However, the sentence reads: '**Generally** however, they incorporate assumptions of a steady state inventory and homogeneity of hydraulic conductivity over large scale-lengths and fail to include seawater recirculation.' The '**generally**' is precautionary and here intends to show that this occurs often, rather than always.

Nevertheless, we opted to change the sentence to: 'Frequently however, they incorporate assumptions of a steady state inventory and homogeneity of hydraulic conductivity over large scale-lengths and fail to include seawater recirculation.'

#5. P12441 L12-23 I am not sure why the authors include nitrate contamination at that point. Undoubtedly, it is an important topic, but it does come out of the dark at that point since it is not mentioned in the title nor does anything points at its importance. Since neither ^{14}N nor ^{15}N is used to explain sources later on, do the authors intend to use the contamination aspect for a final assessment as pointed at in the title? If so, it should be better introduced to make the point clear.

R: We use nitrate contamination as a contextual example for the relevance of our points. Within the abstract we already approach the subject (e.g., P12435 L1-11) and on page 12438, L20-28, we introduce the issue in practical terms, as follows: 'The occurrence of SGD comprising significant freshwater contributions was first detected in the Ria Formosa in 2006–2007 and subsequently **described as a prominent source of nutrients into the system** (Leote et al. 2008; Rocha et al. 2009; Ibánhez et al. 2011, 2013). **However**, given the unpredictable nature of freshwater availability in the region, coupled with a mixed-source (i.e., a variable mix of groundwater abstraction and surface water collected in reservoirs) management of public water supply to meet demand (Monteiro and Costa Manuel 2004; Stigter and Monteiro 2008), **it is not yet clear whether meteoric groundwater would be a persistent feature of SGD** into the system.'

This intended to draw attention to two facts: 1) that it was uncertain whether fresh groundwater-inclusive SGD would be persistent, i.e., whether the nutrient input via this source could be considered as a regular feature of the system, hence to be included on annual nutrient budgets as such; Given that GW is contaminated (P12441 L12-24) the input would be allochthonous and as such of concern (see cited references re: SGD in the Ria Formosa), and 2) that even if SGD contributions were detected via tracer studies at the basin scale, these might originate from sources other than the local contaminated aquifer, with the ensuing implications in terms of apportionment of nutrient loads into nutrient budgets. This ties in with the point we make in P12438, L 3-18. We then follow up on this point in the discussion (e.g., P12456 L20-29 and on to P12457 L1-25, especially L7-13) by giving relevant examples, and complete the rationale further on in concluding remarks (P12465 L10-24).

Hence in our opinion, the inclusion of nitrogen contamination at this point is not only relevant, but also most importantly illustrative of the topic that we address in the title as it underpins and contextualizes the significance of our findings.

However, since this is also a comment that has been made by the second reviewer, we opted to try and make the reasons for inclusion more clear. We have now altered the last two paragraphs of the introduction, in order to hopefully improve on this aspect of clarity of purpose. These two paragraphs now read: 'We contribute an answer to this conundrum with a study conducted in a seasonally hypersaline lagoon in southern Portugal where we combine two datasets: radon surveys are used to determine total SGD in the system while stable isotopes in water (^{2}H, ^{18}O) are used to specifically identify SGD sources and characterize active hydrological pathways. We show that stable isotope hydrology is a reliable tool to identify different SGD sources in a very complex coastal system, even though it hasn't been used to this end before. This underuse of the methodology has two main reasons. The first is a disciplinary divide: the technique has been the domain of freshwater hydrologists; correlations between $\delta^{18}O$ and $\delta^{2}H$ are central to research into the effect of evaporation and mixing on surface waters (Gat et al. 1994, Gibson and Edwards 2002) and contribute to the disentanglement of different water sources affecting catchments (Rodgers et al. 2005). The other is the paucity of paired $\delta^{18}O$ – $\delta^{2}H$ data on coastal seawater (e.g., Rohling 2007), even if stable isotope datasets might help constrain the origins of freshwater inputs into the ocean when coupled with salinity data (Munksgaard et al. 2012, Schubert et al 2015), or as part of a methodological arsenal in SGD studies combining physical and chemical measurements with radioactive and stable isotope tracers (e.g., Povinec et al 2008). Hence we also bridge the disciplinary gap between marine chemists and hydrogeologists currently extant in SGD studies by using a combined approach merging techniques from both disciplines.

The occurrence of SGD comprising significant freshwater contributions was first detected in the Ria Formosa in 2006–2007 and subsequently described as a prominent source of nutrients, in particular nitrogen derived from fertilizers, to the lagoon (Leote et al. 2008; Rocha et al. 2009; Ibánhez et al. 2011, 2013). However, the unpredictable nature of freshwater availability in the region, coupled with a mixed-source (i.e., a variable mix of groundwater abstraction and surface water collected in reservoirs) management of public water supply to meet demand (Monteiro and Costa Manuel 2004; Stigter and Monteiro 2008), made it unclear whether meteoric groundwater would be a persistent feature of SGD in the system. This made it difficult to clarify the contribution of SGD to the nitrogen budget of the Ria Formosa, with obvious consequences to environmental management strategies. The overarching aims of the study were therefore to identify the sources of SGD, distinguish its component parts and elucidate the mechanisms of their dispersion throughout the Ria Formosa. The outcomes are then employed to distinguish and quantify nitrogen loads carried into the lagoon by different SGD modes.'

#6. P12442 L4-19 From how I perceive it, so measured ^{222}Rn values do reflect an integral of ebb and tide status and is not corrected/normalized to its specific tide level? Is this correct?

R: Radon activities were measured in-situ, under way (as explained in P12442 L16-19). Following processing, they do ultimately reflect integrals of ebb and flood status as represented in Figure 2. Briefly, a total of 124 data waypoints were obtained for each individual survey, covering all the main channel areas. These were divided according to tidal stage at which they were measured into two groups. Salinity and location as well as depth were recorded in parallel with the radon, under way. As explained subsequently (P12442 L19-22), these were then used to calculate the local inventory (one inventory data point for each measurement). The inventories were normalized to mean tidal height for each location (P12442 L21-25). For mass balance purposes, only the normalized values where used, and those are the ones represented in Figure 2.

#7. P12444 L7 *'Samples'* - Could the authors add some words on how the samples were obtained, from which depth, whether they were stored or directly analysed etc. to provide a comprehensible and reproducible sampling strategy?

R: Done. The sentence now reads: 'Sediment-water diffusive fluxes of radon were measured as described in Corbett et al. (1998) in samples (n=16) collected throughout the lagoon and directly analysed in the laboratory upon collection. To obtain these samples, undisturbed sediment cores (35 cm length) were collected using polycarbonate core liners (∅ 5.5 cm) in both sub-tidal (n=8) and intertidal environments (n=8), with each environment sub-sampled for sandy and muddy sediments in equal proportions. Resulting fluxes from all analyzed cores where then averaged and the latter value, with its associated uncertainty, used in subsequent mass balance calculations.'

#8. P12444 L24f *'which in combination with its location implied very low [...] so we neglected the* - I cannot follow the reasoning of neglecting surface water inputs. Rather intuitive would be to provide a salinity time series at this location that, if there is no surface water contribution, should be rather constant over time. Plus, did the authors check whether or not any floods occurred prior to the campaigns that might have changed the ^{222}Rn signal, if not at the outlet than possibly within the lagoon?

R: We should clarify that we are not neglecting surface water inputs as our statement (P12444 L19-23) indicates: 'Usually, an additional term accounting for the radon influx via river flow is added if the water and particulate flux associated with river discharge is significant. However, the only perennial river in the Ria Formosa is the Gilão, located in the eastern limit of the lagoon'. We then explain why we don't think the input is significant.

Firstly, we clearly stated (Section 2.2. Hydrogeological setting, P 12440) that the average salinity found throughout the year within the lagoon (35) was high – due to low effective precipitation on the catchment, a statement supported in the literature which the reviewer requested be removed (#T3). We nevertheless measured surface salinity (Table S1) during our isotope sampling campaign - it was very high during both tidal stages over the whole lagoon, with the exception of the areas influenced by discharge of the WWTP, where it was slightly above 33 (table S1). Hence surface freshwater inputs, other than the WWTP where generally negligible, something that is consistent with previous studies reported in the literature. In addition, we note that we also compared the annual effective precipitation over the whole catchment with the tidal exchange flux – it makes it clear that the mean volumetric tidal flux is 8 times higher than the annual average effective precipitation – thus compounding the argument above in that surface water inputs are negligible in this lagoon.

Secondly, Newton and Mudge (2003), cited in Mudge et al 2008, find that any freshwater influences caused by the Gilão river (in winter, where the potential to do so would be maximized) are localized to the vicinity of its estuary. Even so, we measured salinity (Table S1) at the Gilão estuary mouth in December 2010 (same month, same tidal conditions as in Dec 2009, same meteorological conditions when the isotope data was collected, see Figure 4 panel d), just to make sure - and it was very high (>29) – this is very common occurrence – the saline influence extends far inland into the river. Freshwater discharge into the sea is negligible except under flooding, which did not occur at any time during sampling or beforehand.

To reiterate the importance of the distance factor, we also clearly state that the location of the estuary is important as is its intermittency of discharge (P12440, L16-18) – it is more than 20 km to the east of the eastern border of the area of study represented in the original Fig 1, as the redrafted figure (top panel) now shows clearly. Combined with a perennial eastward alongshore drift on this coast, the lifetime of radon in surface waters subject to degassing, and the overwhelming contribution of seawater (low Rn) to the discharging mixture at the estuary mouth, the facts are strongly in favour of our contention that the Rn inputs eventually brought into the area of interest by the discharge of the river Gilão are not significant, and certainly, just in terms of freshwater contribution, not even comparable to the WWTP if we go as far in detail as we can and look at our salinity data for the isotopic samples, so we simplify the equation to remove the contribution.

We also verified whether there was any intense precipitation prior to the sampling campaigns that could have led to flooding – see additions to section 3.2, the new Fig 4, panel d, Table 2, and response to comment #9.

#9. P12446 L3 'I am not sure in how far the comparability between samples of different campaigns is given. Please add, at a suitable point in the manuscript, a short passage on the comparability of samples as specifically during the isotope section the authors themselves point out that a variance of up to 50% exist between sampling dates. This questions many interpretations of the presented manuscript and needs to be clarified. Plus, use consistent dates/periods for the isotopic data. Sometimes, the author's use 2007, 2009-2010, sometimes it is 2007 and 2013, sometimes it is 2007 and 2009-2011. This is very confusing and raises questions.'

R: This is a fair comment. We had thought carefully about this issue, albeit tensioned against space constraints since the length of the paper was an important consideration. We originally opted to save some space by providing Table S1 as a way in which the reader could have access to the sampling dates and all the data plotted, but (see also response to #T6, below) the reviewer seemed to be lacking the S1 Table that was available as supplementary material at this point. In addition to the sampling dates in table S1, the sampling periods for groundwater source functions were described in Section 4.2.1 (P 12450 L5-7), where we also drew attention to the temporal similarity in stable isotope signatures of the groundwater end-member (L 7-11).

While revising, we also found some typos – one location (Rio Seco, 08/12/2010, table S1) was mistakenly attributed to the Eastern sector and 2013 is an error.

We have corrected these, tightened up the designations, and provided the discussion as suggested in an update to section 3.2, which as a result was comprehensively revised. We complemented this with a new table (Table 2), where we provide a summary of the precipitation during all the sampling campaigns compared to the historical record average, as well as a new panel, added to Figure 4, comprising the daily precipitation record for 2006-2013 in order to provide a wider temporal context to the stable isotope data plotted there and in subsequent figures.

However, we had addressed the issue of variance mentioned in several ways:

a) With regards to intercomparability of analytical results on P12447 L10-15, where we also refer the reader to Table S1.
b) With regards to the potential role played by the dissimilar sampling strategy on an annual basis with reference to number of samples, frequency (tide – specific) in explaining the variance encountered in section 4.2.2. (P12451 L13-18), where the source of this variance encountered in surface water signatures between 2007 and 2009 is clearly decoupled from the groundwater isotope variability – hence introducing a new surface water source into the lagoon, which we specifically mention on P12458, L10-12 (see also response to #15).

Nevertheless, the new section on inter-comparability and the additional data provided might contribute to clarify the issue. This section now reads:

'Inter-annual comparability of isotopic data

Sampling campaigns where carried out strategically following a field-adaptive protocol. Of primary concern was to capture the extent of temporal end-member variability in isotopic signature under maximum freshwater flow (hi-flow) conditions, in order to a) guarantee coherence of source compositions to feed into mixing models when necessary while assessing the hydrology of the lagoon over wider temporal scales and b) minimizing logistics and costs while guaranteeing inter-comparability. For this purpose, winter season was chosen given that ~61% of the mean annual precipitation falls on the region between November and February (34% in the months of December and January). Stable isotope sampling in winter had the added advantage of minimizing kinetic effects over stable isotope signatures given the lower evaporation potential. Sampling in winter 2007 was exploratory, with two main objectives: firstly, to characterize isotopic signature of M12 groundwater and surface lagoon waters in the western sector, particularly in the area that could be potentially influenced by both SGD and the WWTP outflow under maximum dilution potential (hence high tide), and secondly, conduct an exploratory survey of potential seepage areas along the Ancão peninsula, keeping in mind that the location of at least one of the important SGD seepage sites was known (Leote et al, 2008). Detection of the isotopic signature of groundwater in porewaters at the seepage face at stations Pw_e and Pw_f (Table S1) led to the installation at their location of a nested piezometer transect array in January 2010. This was subsequently used to obtain porewater samples in the 2010/11 winter season (December 2010 and January 2011).

To capture inter-annual variability, the M12 aquifer was sampled twice (winters of 2007 and 2009), with the provision of one common location (Ramalhete) for cross-referencing. Following the same reasoning, the M10 aquifer was sampled in December 2010 while simultaneously sampling Rio Seco (belonging to M12, Table S1). This ensured inter-comparability between groundwater isotopic signatures in 2009 and 2010. Campaigns were planned in advance considering the precipitation over the region to ensure similarity in the hydrological regime and ultimately guaranteeing inter-comparability of results. The sampling itself took place in dry conditions as much as possible, and never after intensive rain that could have promoted flooding (Table 2, Fig 4d). For example, while January 2007 was a dry month (8.8 mm) compared to the historical average (138 mm), the accumulated precipitation during the previous 3 months was 369.7 mm, consistent with the historical average (Table 2). By contrast, both December 2009 and 2010 were relatively wet months (392.2 and 269.6 mm), but followed relatively dry 3-month periods (Table 2). So porewater samples were also taken in January 2011, hence complementing winter 2010/2011. January 2011 followed a wet three-month period (414.7 mm) and was hence comparable with January 2007, also relatively dry but on the back of three wet months (369.7 mm cumulative). The combined dataset therefore contains results from repeated measurements for end-member isotopic composition under hi-flow conditions, across different years. These are in addition compared to historical data (table S1, Figure 4), leading to a temporally coherent quantitative overview of stable isotopic hydrology over the catchment.'

#10. P12447 L19-22 If both, activity range and spatial distribution of ^{222}Rn, are similar I do not understand the neglecting of the winter campaign. I suggest including it, as it may even provide more insights into temporal dynamics despite the associated uncertainties.

R: While this might be a fair comment in other circumstances, we respectfully disagree here. We opted originally to exclude the data and just mention it in the current context for two main reasons:

Firstly, the relative uncertainty associated with the advective radon input to the lagoon derived from the winter data is ~120% of the estimated discharge (7.97±9.62 Bq m^{-2} day^{-1}). Given the variable extremes observed in wind conditions during the survey (see additions to Table 1) and resulting choppy seas (we call attention to the precipitation data on table 2 and panel d in Figure 4, where it is very clear that stormy conditions where fast developing and we where actually very fortunate to have carried out the work in the first place), we accepted the fact that both the uncertainty associated with the evasion term and that linked to in-way radon activity measurements (see additions to Table 1) where indeed too great and not representative of usual conditions in the region – the resulting SGD estimate, while similar to that obtained in June 2010, would then be severely affected as we point out, and now make explicit in the additions to Table 1, for completeness. We then took the option of repeating the radon survey the following summer. Even so, as the reviewer points out in the following comment (#11), 'the representativeness of the given SGD mean value is rather low and associated with a lot of uncertainty. (…)'. This is of course a well-known fact in SGD radon tracer studies and is well documented in the literature – it is associated with the assumptions needed to close whole basin mass balances of radon, and within these, in particular to the limitations associated with fluxes estimated with a parameterisation of gas exchange (k) with the atmosphere, as shown by Gilfedder et al (2015). If data that we present and discuss, obtained under the best possible circumstances and attention to detail in order to minimize uncertainty give rise to this commentary, discussing the extra data in addition would increase the space used (it is already a rather long paper) and probably give rise to many more comments of the same nature, while failing to add anything of note, as:

Secondly, we had actually stated that the data was similar as to activity range and distribution, and explained why we chose not to showcase the extra data – it would be redundant as the derived SGD discharge magnitude and the Rn activity range and distribution was similar (this is now obvious, Table 1). It didn't and still doesn't add anything to our point in the context of the paper. Nevertheless, our calculations, as presented before for the summer (2010), and now on their entirety with the additions made to Table 1, are reinforced by a complete error propagation analysis (hence the high associated uncertainty, since it is accumulated) so that the reader can judge on the merits of our reasoning. We would deem this sufficient, and maintain our discussion focused on the summer radon data – we further note that this approach, that we took, is in fact sadly lacking from the vast majority of published SGD studies involving radon mass-balances, particularly in large systems.

#11. P12449 L11-12 Here and also during the following lines, one SD is almost similar in value as the given mean value. In turn, i.e. the representativeness of the given SGD mean value is rather low and associated with a lot of uncertainty. This even leads to the fact that the resulting advective radon input to the lagoon of 1.36 (±1.28) $x10_6m_3day_{-1}$ may results in an filtering of the entire tidal-averaged volume of the lagoon ($140x10_6$ m_3) through its sandy beaches within 100 days, as given by the authors but, with an almost similar probability, it may also be only 74 or 2450 days if we include the SD.

R: Indeed. See Response to comment #10. Uncertainty is part of the scientific quantification of magnitudes of natural processes. We recall that the error is propagated throughout the entire mass balance calculation process. Given the size and complexity of the lagoon system, which requires substantial averaging assumptions in terms of prevalent environmental conditions (wind speed for one, see response to comment #10) that affect the magnitudes of the flux terms in our radon mass balance, and how they propagate with every single mathematical operation, we would actually challenge anyone to do better.

#12. P12451 L17f How is the strategy influencing the range, specifically the delta18O range?

R: If we had not taken a sufficiently large and representative dataset (see response to comment #9), the question could be raised, and certainly within such a large pool of data and a lengthy discussion, this point might be lost to the casual reader. Questions could be raised, as we thought under this perspective, essentially because one potential reason for the observed difference would be that we didn't take into account natural variability of the end-member isotopic composition, and we would therefore have a diminished confidence in attribution of source functions for the water in the lagoon. So we provided the possibility and discussed it, even briefly, in spite of the confidence we had on the inter-comparability of results.

#13. P12455 L17 I would encourage the authors to double-check the percent-values. Corresponding to my calculation it would be 3.16, 0.97 and 0.04 (based on the mean value and the mean daily flood prism of $140x10^6m^3$).

R: Indeed – many thanks for pointing this out, we mistakenly wrote 1.04×10^8 m^3 (Line 18 in the same page) rather than 1.40×10^8 m^3, leading to the confusion. This is now corrected.

#14. P12451 L1f This comment is similar as one I have given before, but should underline the importance. On the given line, I started getting very confused. Despite stating in the text that isotope samples were taken 2007 and apparently at least twice between 2009 and 2011 (otherwise the authors would have given only one year) Fig. 5 states sampling years 2009-2010 and the supplement even 2012. I strongly encourage the authors, and this does account for all parameter and samplings to give a clear overview, which samples have been taken at what date and to discuss the comparability between parameter (Rn, 18O, 2H) in the context of the intended aims the authors follow within the presented manuscript.

R: We recall that our strategy was to sample for end-member stable isotope compositions under maximum predicted flow conditions (i.e., winter, as explained now in the redrafted section 3.2. – see also response to comment #9), and also accounting for annual and tidal variability – hence the multiple sampling runs (2009, 2010, 2011). The complete isotope data set is presented in table S1 available as a supplement to the discussion paper and it doesn't include 2012 (that might be a typo), but it seems the reviewer didn't have access to this for some reason at this stage of the review (see also response to comment #9). In any case, we have tightened the descriptions up, opting for specifying the years whenever deemed necessary, rather than mentioning the periods, and adding clarifying material – Table 2, for example, now includes all the sampling periods summarized against the backdrop of precipitation regime.

#15. P12458 L11f I assume the authors mean the WWTP. If so please state so, for clarification aspects.

R: Done. The section now reads: 'While this is clear for the eastern sector, within the western sector there is another surface source of water (WWTP) that further complicates the picture. This water joins the lagoon close to Station 2B (Fig. 6a).'

#16. P12463 L16-18 I cannot follow. I agree, rainwater plots at d-excess of ~25‰, but how do the authors derive the point that water for public consumption was mainly withdrawn from a meteoric source? On the other hand, isn't that somewhat logical? I assume this arid region to use shallow GW to large extents, which should have a meteoric origin.

R: Indeed, all freshwater sources are ultimately meteoric. However, we specifically mention a direct meteoric source, to distinguish it from groundwater captions – the d-excess/$\delta^{18}O$ line P4 (Fig. 7b) rules out groundwater captions as the source of the measured isotopic signature in the lagoon in this particular context (see also response to comment #17 below). Surface reservoirs (Odeleite: 37°20'15"N 7°33'1"W, http://wikimapia.org/13820374/pt/Albufeira-da-Barragem-de-Odeleite and Beliche: 37°17'1"N 7°31'49"W, http://wikimapia.org/13820331/pt/Albufeira-da-Barragem-de-Beliche) are used under the multi-municipal water supply system active since 2000 to cater for public water supply in the eastern region of the Algarve, including the city of Faro, with groundwater captions only used in emergency situations (i.e., when the reserve levels in these two are low), as explained in P12464 L10-19 and in more detail within the cited literature references (Monteiro & Costa Manuel, 2004; Stigter & Monteiro, 2008). Given their location Northeast of the region of interest, important sources of precipitation for recharge are originated in Southern France and the eastern Mediterranean with occasional influences from the Magreb, and these have d-excess signatures that are quite high, as explained in Frot et al, 2007 cited in P12451, L2). In addition – we checked, using the GES DISC (NASA) tool (not shown) – the source of this rain collected in winter 2009 was the centre/south of France and the rain clouds travelled southwest over the mentioned reservoirs. This explains why, in this regional context, local rainwater and groundwater water may have origins in different meteoric water sources and thus have distinct stable isotopic signatures.

#17. P12463 L20 Do the authors mean the mixing line P4? If so, the reasoning is not clear to me as I do not understand the distribution of the surface water and porewater samples? How can surface water plot above the porewater samples? Or, are these the sample points, the authors discuss earlier when mentioning the influence of the WWTP?

R: Yes, we do. Mixing line P4 connects surface water in the Ramalhete channel (Fig 1) that is influenced by water coming out of the WWTP with high d-excess and seawater with low d-excess. This line ultimately extends to a high d-excess signature (~25‰), and this is why we state that it originates in surface reservoirs (see also response to comment #16 above). Mixing of a comparatively small volume of WWTP water with a larger volume of surface water, including seawater, greatly depletes this channel's overall signature in d-excess along its course from the WWTP. Porewater has an intermediate isotopic signature and lays in between the two endmembers: this is possible for a number of reasons: one, because surface water infiltrates the unsaturated zone of the beaches, located at the outer limits of the system (and hence at the end point of the water path towards the inlets), and while travelling through the pores in the beach toward the seepage face, becomes initially even more depleted in δ^2H and $\delta^{18}O$ because of the distillation effect within the unsaturated zone, as described in Barnes & Allison 1983 and 1989 (cited in P 12458 L 5-9); two, because its mixing ratio with seawater and more depleted surface water (in d-excess) is necessarily small.

#18. P 12463 L23f Again the question of comparability? Was the tidal status the same during both samplings? Or could it be that during 2007 it was a low tide during which all the porewater was sampled plus a wet period just before the sampling campaign? Both in combination might also explain the differences to the 2009-2011 data that might reflect a rather dry period before. Additionally, S1 shows the different porewater samples but there is no information whether they show different depths, or different close by locations with a constant depth, nor how they were retrieved. Please clarify and state some words to the comparability.

R: The issue of comparability has hopefully been addressed at this stage (see response to comment #10). We reiterate also that there seems to be some contradiction between the perception that the reviewer has access to Table S1 in this comment, and #T6 below, where the existence of Table S1 is challenged. Briefly, January 2007 and 2011 are directly comparable (see new Table 2) – the month was relatively dry, but came on the back of three really wet months. This is why additional sampling was done in 2011 – to make the pore water dataset available under comparable hydrometric conditions in its entirety. Pore water in January 2007 was sampled right along the Ancão peninsula (see redrawn Figure 1), while all subsequent (2010, 2011) porewater samples were taken at a single site through a fixed piezometer profile, but at various depths as explained more carefully in section 3.2 now, as well as catering for different tidal stages. Winters in 2009 and 2010 where directly comparable in terms of hydrometrics (Table 2). The most likely explanation therefore for the stable isotope hydrology of the catchment is the one we put forward – it is anchored on a careful, field-adaptive sampling strategy, and is for extra care compared with historical data (table S1, Fig 4 a, b and c).

#19. P12464 L13-16 '*the activity of the SGD subterranean pathway into the Ria becomes dependent on whether groundwater levels in M12 are sufficient to establish a hydraulic gradient driving the flow as was apparently the case in 2007 (Fig. 7a).*' I agree, but could the authors underpin this aspect with recorded precipitation amounts in the period before the campaigns?

R: This was done – see added table 2, discussion in section 3.2 on inter-comparability, and new panel on Figure 4 showing the daily precipitation during the period 2006-2013 with the sampling periods overlain to provide a historical context.

#20. P12465 L12-27 Instead of referring to N, I would suggest to briefly list the direct findings of the study and to assess the connectivity as the title suggests. From my perspective, this last paragraph, if intended as assessment, is not at all suitable as such as none of the direct results of the manuscript are used except (possibly) a measurement for N in 2006 but here it is unsure whether it is an own measurement or by Leote et al. (2008).

R: Leote et al 2008 was produced by an earlier project carried out by the same team and directed by the same PI, and in many way laid out the groundwork for the project under which this paper was produced. From the authors' point of view, it is thus important to underline the importance of the present findings relating them to direct local productive activities and water management. This is of special relevance if we look at the consequences of not having a supported case for attribution of nitrogen loads to the different SGD modes we distinguish using our approach, as we mention in the introduction and discuss further in the conclusion.

Technical Comments:

#T1. P12435 L21-25 It is a very long sentence that is hard to follow. I suggest to shorten or to rewrite it.

R: Done. This segment now reads: 'In the absence of meteoric SGD inputs, seawater recirculation through beach sediments occurs at a rate of ~1.4×10^6 m^3 day^{-1}. This implies the entire tidal-averaged volume of the lagoon is filtered through local sandy sediments within 100 days (~3.5 times a year), driving an estimated nitrogen (N) load of ~350 Ton N y^{-1} into the system as NO_3^-.'

#T2. P123439 L25 *'six tidal inlets'* – Fig 1 shows only 3 inlets. I assume the other three are east of the region Fig. 1 shows. And this is one point I would encourage the authors to change. Throughout the text, several times locations, rivers, stream (ephemeral and perennial) and inlets are mentioned and not shown on the map. Please include them to provide a comprehensive and complete picture of the area. Plus, please add all sampling sites and possibly indicate the time they were taken (e.g. colour coded).

R: The three remaining inlets are indeed to the east, as explicitly stated on P 12440 L 3. In addition, the figure was not expanded to the east because of the relatively small contribution of these 'missing' inlets to the tidal prism, explicit in P12440 L4-5, and to avoid the risk of overcrowding the panel.

Nevertheless, we have redone Figure 1, which now incorporates two panels. We thought this to be the best solution, catering for the reviewer's request for additional information to be inserted while still maintaining the illustration free of clutter.

#T3. P12440 L12-15 Please change from *'The surrounding watershed covers 740 km. and receives effective precipitation of 152mmyr^{-1} (Salles, 2001). This corresponds to a potential annual rainfall of 1.2 10$_6$ m$_3$, very small compared to the tidal exchange flux – hence the high average salinity of 35 found throughout the year in the lagoon (Mudge et al., 2008).'*

To *'The surrounding watershed covers 740 km2 and receives effective precipitation of 152 mm yr-1 (Salles, 2001) corresponding to an annual rainfall amount of 1.2 106 m3.'*

R: Done.

#T4. P12441 L5 *'The two units'*- It is unclear which two units are meant here. Aforementioned is the M12 as multi-layered aquifer only but no specific units. Please clarify.

R: The previous sentence reads: 'The Campina de Faro (M12, Fig. 1, 86.4 km^2) comprises a **superficial unconfined aquifer (Pleistocene deposits)** with a maximum thickness of 30 m **and an underlying Miocene confined multi-layered aquifer**, which Engelen and van Beers (1986) suggest discharges directly into the Atlantic Ocean bypassing the lagoon.' The two units refer to the superficial Pleistocene aquifer and the underlying Miocene aquifer. To make it clearer, we changed the sentence, which now reads:

'The unconfined Pleistocene aquifer is hydraulically connected to the underlying Miocene aquifer.'

#T5. P12443 L3 '*Faro Channel Fig.1*' - Where exactly are the two fixed stations located, is it station 3 and 4? Please clarify.

R: In Figure 1 provided with the manuscript, the two stations are identified by a symbol depicting a buoy and a flag. These symbols are captioned in the figure as "Tidal stations", with the northwestern-most one named as 'Quatro-Águas', in accordance to Figure 1 (and Table 1) and the Rn mass balance approach, and the other at the entry of the main inlet, Faro-Olhão (or Barra Nova). We therefore don't quite understand the request, but nevertheless, in the expanded Figure 1 we attempt to make them more visible.

#T6. P12444 L23 There is no Table S1, neither at the end of this document nor in MS manuscript overview of HESSD. Please add it as at least from my perspective it is crucial for the understanding of certain processes and samplings and its evaluation.

R: We agree that it is crucial, but are afraid that this statement is incorrect. The S1 (from Supplementary-1) Table, comprising the entire stable isotope dataset, with locations (coordinates), dates of sampling, number of samples taken, and associated uncertainties, as used in the manuscript does exist. It is 4 pages long, and is available for download as supplementary material online at the site of the discussion paper due to size constraints. This table also addresses some of the other comments made with regards to locations.

#T7. P12445 L11 Please explain 'T'

R: We thought this to be clear in the paragraph 'If we then take the mean tide level (MTL) as a reference, it follows that the Rn_{adv} term may be calculated for different **periods**: the **period** at which the tidal height in the lagoon is below MTL (Rn_{adv} $(T<MTL)$), i.e., the trough of the tidal wave or low tide, and the one when it is above MTL (Rn_{adv} $(T>MTL)$), corresponding to the peak of the wave, or high tide.' (P12445 L9-14)

Nevertheless, for enhanced clarity, we opted to edit the sentence and add the 'T' between parenthesis after the second mention of the word 'period', and it now reads: 'If we then take the mean tide level (MTL) as a reference, it follows that the Rn_{adv} term may be calculated for different periods: the period ***(T)*** at which the tidal height in the lagoon is below MTL (Rn_{adv} $(T<MTL)$), i.e., the trough of the tidal wave or low tide, and the one when it is above MTL (Rn_{adv} $(T>MTL)$), corresponding to the peak of the wave, or high tide.'

#T8. P12445 Eq4a/4b Although possibly being pedantic, please move the Rn_{net} to the end of the equation to match Eq. 3

R: Done.

#T9. P12446 L13f '*western sector*' – the 'western sector' is explained later on and may lead to confusion here. Either the author's add the locations in Fig.1 (what I suggest) or they explain the western sector at that point.

R: Changes to Figure 1 have been made.

#T10. P12446 L 13f *'Barra Nova'* –this term is not shown on the map but used several times in the text. Please add it to Fig. 1 in order to allow a clear understanding of the spatial context

R: Done – there is a now a list of all the inlets, their English and their Portuguese names associated with the new panel in Figure 1.

#T11. P12448 L1-4 The spatial distribution is explained at the example of Rn activity but Fig. 2 shows inventories. To make it easier to follow I suggest to use either inventory or activity, but to use the same for the verbal explanation and Figure 2.

R: We respectively disagree here. While in the text the activity values where shown to make them easily comparable for example with the tidal data shown in Figure 3, the most correct way of showing data on a geographical scale that is directly inter-comparable in spite of the different depths due to the bathymetry and to the tide stage is to show inventories referenced to MTL on the map.

#T12. P12450 L22 *'for the Great Barrier Reef'*–_is this relevant? If not, I would suggest to delete it.

R: It is relevant inasmuch as it provides the location of the study referred to in the citation – contextually, it is also important given the importance of freshwater sources to the marine environment there. The study focuses on the input of freshwater into a marine environment and therefore ties in with ours.

#T13. Equations - Is it relevant to show them in the text, especially the coefficient of determination r2 and later the p-value? I would suggest putting them in the plots as it disturbs the text quite a bit.

R: We understand that it might disturb reading flow a bit. However, it would also clutter the figures quite a bit as well. We intend to show the r2 and p values of mixing lines and meteoric lines because it makes them more immediately comparable to the hydrological literature on the first instance and on the second adds strength to the formulations. Torn between cluttering the text or the figures, we opted for the text as a compromise, in order to maintain the figures as clear as possible in spite of the plethora of data they show.

#T14. P12451 L27 'proper' – I assume the word is wrong here. Please delete.

R: Done.

#T15. P12460 L20-25 Very long sentence that I would suggest to split or rewrite.

R: We agree – we have split the original sentence and it now reads as two: "On the other hand, the exchange in position of the isotopic signature of water at Stations 1–5 and 1B–3B with reference to the LMWL in $\delta^{18}O$ – $\delta^{2}H$ space during flood (Fig. 6b) suggests a hydrodynamic connection between the Ramalhete Channel, the Ancão inlet and the water masses on the eastern sector. This connection would occur via the Faro-Olhão inlet and associated channels as ebb progresses onto flood, linking both the stations closest to the city of Olhao (Stations E, F, G) and the ones closer to the coastal ocean (Stations A, B, C), to the water masses originally present in the western sector."

P12476 Fig1 Please,
1. add all names, places, sampling locations etc. the authors mention throughout the manuscript and extend the figure to the east,
2. add a colour legend,
3. add a colour to M10-M12 to be able to differentiate it from water
4. add a coordinate system to be able to locate sampling locations from the supplement

R: Done.

#T16. P12477 Fig2 Is ebb and tide (a and b) erroneously exchanged, as more data points to the west are available for the ebb subset (a) which I would assume for the flood with more water in shallow reaches? Please provide some names in Figure 2 that the authors use during the description of the spatial distribution and, if the authors have it, a bathymetric chart would add some value.

R: No, they have not been confused. The ebb subset contains more points to the west on purpose, as when surveying, we made a point of following the outflow of water westward from the WWTP.

Appendixes: redrafted figures and tables.

Figure 1. Map showing location of the sampling sites within the Ria Formosa and its geographical context. The top panel shows the full geographical extent of the system, with the operational separation of the region of interest into western and eastern lagoon and the names of all the inlets; The lower panel shows an amplified map of the region of interest, including major channels, locations of sampling and tidal stations,

as well as boundaries of the aquifers bordering the lagoon (M10, M11, M12).

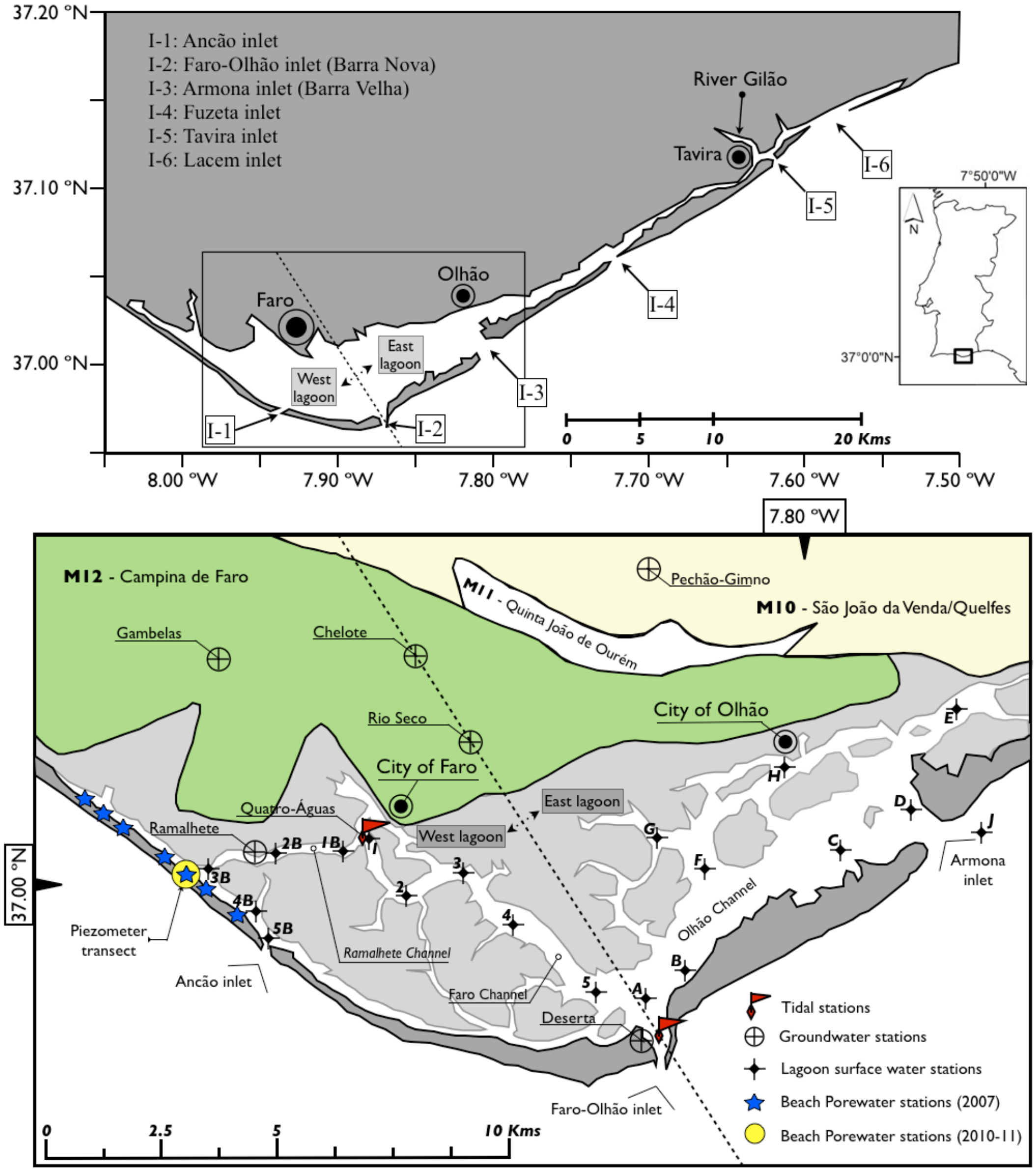

Figure 4. Catchment isotope hydrology. Anticlockwise, from top left: panel a shows the main meteoric water lines framing the isotopic composition of precipitation within the catchment, including the precipitation-seawater mixing line (PP-SW Mix, section 4.2.1.). Panel b plots the isotopic compositional range of water samples taken during 2007, while Panel c plots the isotopic compositional range of water samples taken

during the period 2009–2011; the lagoon surface water samples (inset) are shown in more detail on Fig. 6. Panel d provides the complete record of daily precipitation over the region for the period 2006-2013 for contextual support (see also Table 2 for summarized data). EMMWL: Eastern Mediterranean Meteoric Water Line (Gat and Carmi 1970); WMMWL: Western Mediterranean Meteoric Water Line (Celle-Jeanton et al 2001); GMWL: Global Meteoric Water Line (Clark and Fritz, 1997); LMWL: Local Meteoric Water Line (Carreira et al 2005)

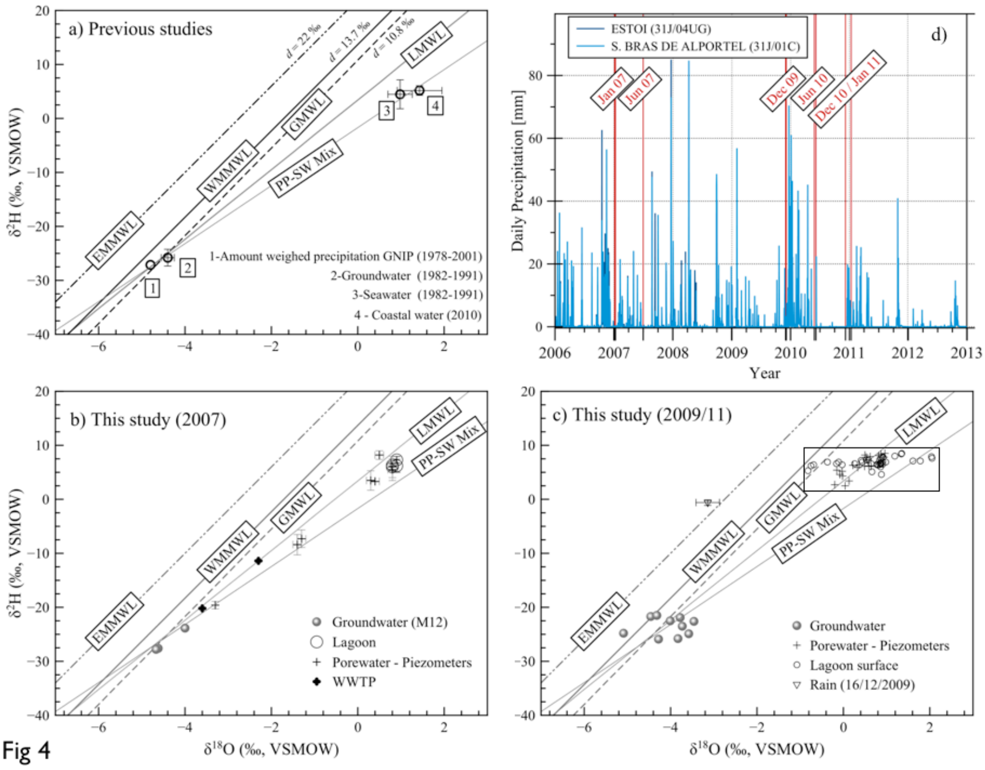

Fig 4

Table 1. Excess ^{222}Rn inventories and relevant fluxes supporting the radon mass balance for the Ria Formosa in winter 2009 and summer 2010 (see Sections 4.1 and 5.1). Notes: [a]Calculated with formulas 4a and 4b, Section 3.1.4.2; [b]Calculated with Formula 3, Section 3.1.4.1; *Referenced to lagoon surface area at MSL, calculated using the residual exchange measured at Faro-Olhão adjusted to the

residual tidal prisms for all the inlets reported in Pacheco et al. (2010) and cross-section area for all the inlets. Minus sign signifies net export (seaward). **Per unit cross-sectional channel area

	Winter 2009	**Summer 2010**
Tidal Amplitude [m]	2.73	2.51
Wind speed [ms^{-1}]	8.4±8.0	6.3±1.2
Inventories	**^{222}Rn inventory ± MAD [Bq m^{-2}]**	
Ebb stage[a]	55.6±30.9	54.2±17.8
Flood stage[a]	73.8±31.5	74.0±17.6
All data[b]	66.1±34.7	65.9±19.6
Fluxes	**^{222}Rn flux ± σ [Bq m^{-2} day^{-1}]**	
Diffusion	5.7±1.9	5.9±1.7
Degassing	1.7±1.8	1.1±0.7
Decay	12±6.3	11.9±1.6
Residual Exchange*	$-5.26(\pm1.03)\times10^{-4}$	$-4.74(\pm0.79)\times10^{-4}$
Tidal Flux**	**^{222}Rn flux ± σ [Bq m^{-2} day^{-1}]**	
Quatro-Águas		
Export	-	85.4±11.1
Import	-	98.6±16.1
Residual	-	13.2±2.8
Barra-Nova		
Export	57.0±6.4	49.8±1.1
Import	65.5±4.2	65.0±4.2
Residual	8.5±1.1	15.2±1.0
Potential Rn sources	**Salinity**	**Activity ± σ [Bq m^{-3}]**
Deserta (Well)	0.95	93.8±59.5
Beach porewater	40.6	304±182
Ramalhete (borehole)	5.06	6625±996

Table 2. Precipitation records over the region during the sampling campaigns described by this study, as measured at the São Brás de Alportel meteorological station (www.snirh.pt, Ref 31J/C). Monthly precipitation is contrasted with rainfall during the sampling campaigns and compared with historical monthly averages in order to evaluate the relative wetness of the periods in the wider temporal context. Accumulated precipitation during the 3 months prior to the month fieldwork took place is also shown and similarly compared to the historical record average. For a more detailed contextual assessment, the chronological record of daily precipitation for the period 2006-2013 is shown in Fig 4, panel d, with the sampling periods overlain for easy reference when evaluating the stable isotope hydrology of the catchment defined by this study and previous research. Under 'Sampling', and 'Type', the type of endmember collected for stable isotope analysis is shown, except when radon survey campaigns were executed in parallel – in this case '*Radon survey*' is added to the column. More details on the individual samples are shown in Table S1.

Date	Sampling		Precipitation [mm]				
			Survey	Month		Previous 3 months	
mm/yy	Period	Type	Total	Survey month	Historical average	Total	Historical average
Jan 07	3rd-6th	Groundwater • M12 aquifer • Beach porewater	0.1	8.8	138	369.7	369
July 07	1st-3rd	Groundwater • Beach drainage Surface water • WWTP • Lagoon West	0.0	0.5	3	83.7	125
Dec 09	1st-8th	*Radon survey* Groundwater • M10 aquifer • M12 aquifer Surface water • Lagoon East • Lagoon West • Seawater Other • Precipitation	10.3	392.2	160	93.6	232
May/June 10	28th-7th	*Radon survey*	0.0	24.1	16	88.6	207
Dec 10	8th-16th	Groundwater • Beach porewater Surface water • River Gilão	0.5	269.6	160	147	232
Jan 11	3rd-12th	Groundwater • Beach porewater	18.7	48.5	138	414.7	369