Author response to reviewer comments

We thank the two anonymous reviewers for their comments on our manuscript, "Using satellite-based estimates of evapotranspiration and groundwater changes to determine anthropogenic water fluxes in land surface models". We detail our responses to the reviewers below in this font (Arial, font size 12 point, blue text). Potential, concrete changes to the manuscript in response to reviewer comments are detailed in the same font, but are additionally underlined to distinguish the manuscript changes from our responses.

Anonymous Reviewer 1

The authors use satellite-estimated ET over the California Central Valley to modify the CLM land surface model in order to better represent diversion and extraction for irrigation. This is an interesting analysis. As it stands, the m/s lacks context, explanation and interpretation in some important aspects, but with moderate revision it should be a worthwhile addition to the literature.

We thank the reviewer for recognizing the contribution of this manuscript, and we will make changes to add to the interpretation as detailed below.

General comments:

- More description of the Central Valley water system is needed : hydrogeology, the surface water system (sources, reservoirs, diversion points) where does the surface water come from, what is the spatial and/or temporal pattern of surface vs groundwater use?

We agree that more details would be useful to the reader, but we feel a strong need to balance this against making the manuscript too long. We will add the following text in a revision, "Relevant aspects of the Central Valley's geology (Planert and Williams, 1995; Faunt et al., 2009), climatology (Zhong et al., 2004), hydrology (Scanlon et al., 2012) and anthropogenic inter-basin water transfers (Chung and Helweg, 1985; Fischhendler and Zilberman, 2005) are extensively reviewed elsewhere." We also will add details about the consumption of blue water in the Central Valley. We feel that it is relevant to point out that the pattern of surface vs. groundwater use varies extensively depending how wet or dry the preceding winter is as many farmers can use both surface and groundwater in their irrigation system. Furthermore, until recently, there were minimal reporting requirements for well owners, so per well water extraction is often publicly unknown.

- in several instances you use "observed" when referring to satellite-based ET estimates. In my view that is stretching the term too far; ET is estimated using a model that requires not only satellite data but also other input data, and the uncertainty in the assumptions and input data is considerable. Using "satellite ET" or "remotely sensed ET" would be appropriate.

While we agree that satellite techniques for determining ET may not be precise as good micrometeorological or lysimetric methods, the precision of some satellite algorithms is sufficiently good to permit their use for water management and water rights regulation by governmental agencies (see, for example, http://idwr.idaho.gov/GeographicInfo/mapping_evapotranspiration.htm). Nevertheless in recognition of the reviewer's concerns, we will rename "observed ET" to "satellite observed ET" throughout the text where there is not already a sufficient modifier present (e.g. remote-sensing). We believe this change should indicate that the data come from satellite algorithms while still conveying our opinion that the satellite data have sufficient quality, precision, and independence to be an "observation" against which modeling results can be tested.

- The relevance of comparing to GLDAS-1 and NLDAS-2 for this study is not clear. Please explain better.
We do not intend to compare GLDAS-1 and NLDAS-2, but we use all of them to increase the number of models and forcings in ensemble average to have more confidence on the model's natural simulations of ET. GLDAS and NLDAS use different forcing and different models (only NOAH is the same), so we just want to increase our confidence in the mean and uncertainty of non-irrigation ET.

Specific comments:
Page 3567, Line 6) "for" rephrase
We agree this could be better phrased. We will change the surrounding text "and conservation of water volume for soil moisture approach" to "and a lack of conservation of water volume for models using a prescribed soil moisture approach"

8) "against" replace with "using"?
We will replace "against" with "compared to" to better indicate that we are using the difference for our irrigation parameterization.

10) Pls better explain what you mean by iterative and partition
We will revise the manuscript to improve clarity here. It will read, "We then incorporate the irrigation flux into the Community Land Model (CLM), and use a systematic trial-and-error procedure to determine the ground- and surface-water withdrawals that are necessary to balance the new irrigation flux. The resulting CLM simulation with irrigation produces ET that matches . . .".

11-12) Is it surprising it matches it well? That is by design, is it not?
It is not surprising that the new ET parameterization matches well, but it is not guaranteed given that a different ensemble of models was used to parameterize non-irrigation ET. The good agreement indicates that irrigation is an essential hydrologic flux in the Central Valley.

P3568, l1) consider including China.
We will add the recent study of Lei et al. (2015) who examine similar issues over the Haihe Basin, China.

4) You can find more analysis on the effect of groundwater extraction on sea water level in this paper: http://www.hydrol-earth-syst-sci.net/18/2955/2014/hess-18-2955-2014.html
We thank the reviewer for bringing this study to our attention. We will cite this study alongside Wada et al., 2010.

10) it may be worth adding that many irrigation areas are in (semi-) arid areas, which increases the contrast.

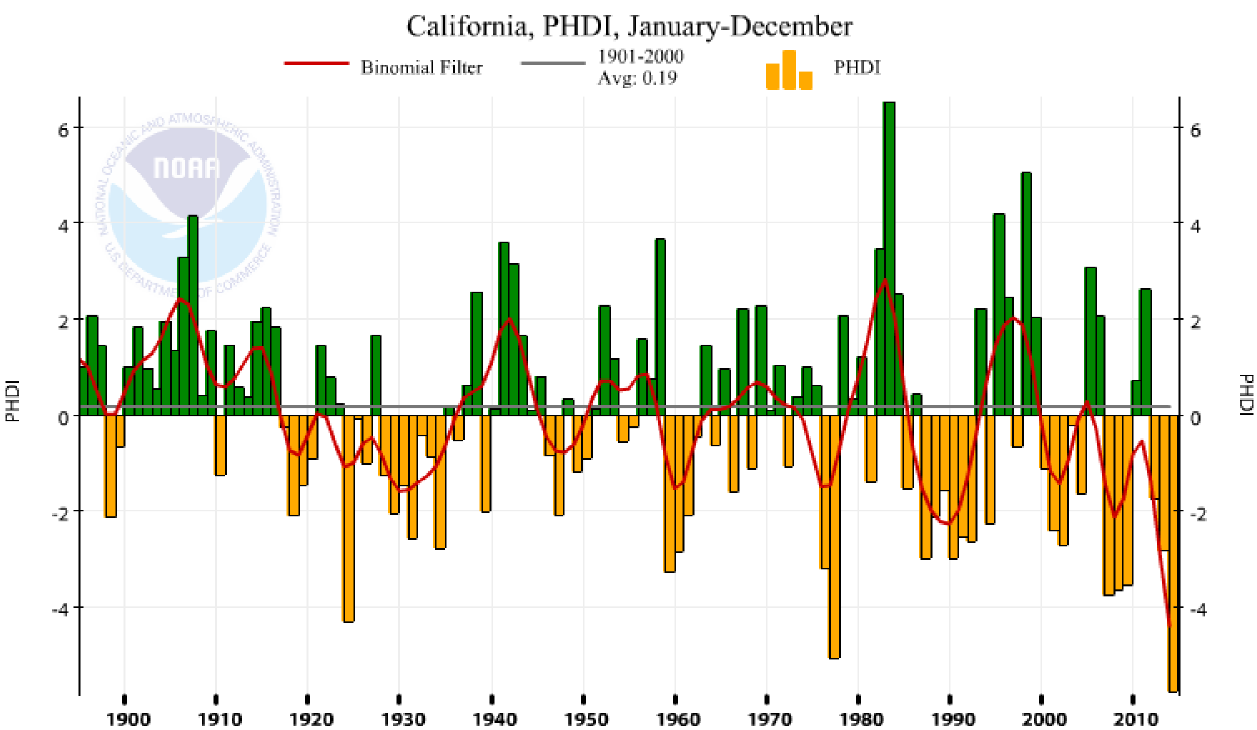
We wholeheartedly agree on this point and further point out the enhanced contrast due to the asynchronous precipitation and growing seasons. We would add the following text to a revised manuscript, "Given that irrigation is predominantly used in semi-arid to arid regions or where precipitation and growing seasons are asynchronous, this lack of parameterization can be highly significant for modeling regional hydrology."

21-25) Pls be explicit which of these limitations apply to which of the numbered items. More in general, please explain in more detail the assumptions and approach in each case, along with the benefits and limitations.

We agree that better connecting the approaches to disadvantages would benefit the reader. We have sought to distill the most essential differences in parameterizations between the four approaches. In our view, adding more details about each study and its assumptions would add greatly to the length of the text without further clarifying our approach in comparison to previous work. In a revised manuscript, we will add text that more explicitly identifies which limitation goes with which approach.

24) "drought and pluvial" change to "dry and wet"; more generally, please do not use the terms dry (below-average rainfall) and drought (extremely low rainfall) indiscriminately.

California tends to have more extreme hydrologic variability. For instance, during the last decade, there have only been three years that have had a Palmer Hydrologic Drought Index within the range of +2 to -2 (see chart from NOAA below). However, we will change "drought" to "dry" and "pluvial" to "wet" in most locations in the manuscript.



29) Why "although" what is the apparent contradiction?
There is no apparent contradiction. We used "although" to draw attention to multiple potential causes of different results in irrigation-climate feedback studies. To enhance clarity on this point, we will change "although" to "while".

3569, 5) change to "more robust"
We will change "robust" to "more robust".

15) Explain what exactly they did that you are building on here.
Lo and Famiglietti used a static surface and ground water irrigation inventory dataset to parameterize their LSM. We will add text in a revised manuscript clarifying this difference.

18) Provide reference for CLM
We will add reference to Lawrence et al. (2011) and Oleson et al. (2008), which covers CLM.

24) suggest "value" instead of "importance"
We will change "importance" to "value" in a revised manuscript.

3570, 22-25) Don't see why this is relevant here?
The potential for restricted groundwater pumping and altered irrigation methods could have a considerable impact on land surface parameterizations due to altered timing and amount of irrigation water. In a revised manuscript, we would add the following text to the end of this paragraph: "and potentially altering the amount and seasonality of irrigation. The potential for rapid hydrologic changes in the Central Valley is one reason why a potentially dynamic, satellite-based irrigation parameterization would be useful for land surface modeling."

3571, 3) Does it have a name?
This implementation of the SEBAL algorithm is unnamed in Anderson et al. (2012) and remains unnamed here.

7) Sounds like the trapezoid method. Is it different?
Tang et al.'s method is effectively the trapezoid method. However, they do not describe it by the theoretical shape (I have also seen it called the triangle method). We would prefer to leave the description as is to keep precision and to avoid adding additional text or potential for confusion.

11-14) How do irrigation areas stand out using this method, not explained. Also, pls explain why the several publicly available ET products (e.g., MODIS, MPI, GLEAM etc) are not used. I imagine it may be because of their coarse resolution, but it is left unexplained.
Although active delineation of irrigation areas is not required for this methodology to work, irrigation areas are quite clear in the Central Valley due to the asynchronicity

between winter precipitation and summer ET. We will modify the manuscript in this section to read, "All three products were clearly able to distinguish peak summertime ET in the Central Valley, which is asynchronous with largely winter precipitation and a characteristic sign of irrigation. Other ET products (Miralles et al., 2011; Mu et al., 2011; Jung et al., 2010) were not used as they were either too coarse in resolution (>0.25° X 0.25° cell size) or were unable to detect irrigation in the Central Valley."

3572, 19) Confused terminology: I assume you mean groundwater discharge into rivers, rather than runoff; furthermore that is not equal to baseflow (baseflow describes a part of the hydrograph, it's not itself an interpretation of hydrological pathway)
To reduce confusion, we changed "groundwater runoff (base flow)" to "groundwater discharge".

3573, eq. 4) Your figure shows an unconfined and a confined aquifer. Please discuss this conceptualisation and explain which of these terms you assume affect which.
Fig. 2 is a very basic conceptualization of how CLM handles confined and unconfined aquifers. We will revise the figure caption as follows "Figure 2: Conceptual schematic of land hydrological processes, modified from Oleson et al. (2008). Blue dash and green lines indicate the irrigation water fluxes applied in the CLM. In the Central Valley, the groundwater is variably confined with some regions having no confinement."

9) that doesnt sound very realistic; presumably farmers would not apply water if it rains. Perhaps summer rain is a rare event? Pls discuss.
Summer rain is very rare in the Central Valley. This fact will be referred to both in the study area section and in the discussion of ET products.

14-15) I am confused about this. Presumably q_recharge is a function of soil water content?
In general, yes q_recharge would be a function of soil moisture content. However, in order to determine GW_WD where pumping data are not available, we force q_recharge in equation 4 to a specified value, which allows us to determine GW_WD as described in this section. We then obtain q_recharge in equation 3. When q_recharge in both equation 3 and 4 match, we then have a GW_WD that we can use to partition the irrigation flux. This process is used only to determine GW_WD.

15) But GRACE total water storage anomalies include contributions from both soil moisture (DELTA SMH) and groundwater (DELTA GW), whereas here you appear to ignore the former. If I interpret this correctly you need to demonstrate that that is a reasonable assumption.
We used GRACE data (Famiglietti et al., 2011) that has already had storage variations from soil moisture, snow, and surface water removed, leaving only delta GW. We recognize that the original phrasing lead to confusion about the level of processing. We will change "GRACE GW observations" to "processed GRACE delta GW".

21) "locate"do you mean "spatially distribute"?
No, we meant "find". We will change "locate" to "find".

3573, 3) What basis do you have for that assumption? Needs discussion and potentially uncertainty analysis.

The US Geologic Survey atlas (Planert and Williams, 1995 - citation in manuscript) reports well depths in the Central Valley from near surface to as deep as >1000 m in the southern part of the San Joaquin Valley. We acknowledge that precise determination of this ratio of confined to unconfined pumping is difficult as well operators are not yet required to publicly post their well depths and as the aquifer is partially confined. We can reasonably presume that a farmer would not pump from a deeper confined layer when water in the shallower unconfined layer is available in order to conserve on expensive well drilling and electrical costs. Confined pumping would be expected to occur in the Southern and Western parts of the Central Valley where surface water is scarcer and the unconfined aquifer is already depleted. In the Northern and Eastern parts, we would expect to find more pumping in the unconfined aquifer as it is shallower there. We would not expect to find confined pumping leading to increased discharge due to a shallow aquifer table rising.

3574, 7) "occurring" rather than "coming"

We have changed "coming" to "occurring".

12) Are there no reservoir dams? Or are they too small to mitigate against year-to-year variations? Pls explain.

There are significant reservoirs in the Central Valley, but they are of insufficient size and operational flexibility to mitigate against multi-year drought (as compared to the Colorado River Basin). Many reservoirs (particularly in the Southern Central Valley) mainly serve to protect against major floods and to hold surface water for release later in the summer.

17-19) That suggests to me that additional constraints are needed. Are there no data on dam releases or the surface water budget that you could use?

We do not believe this section requires additional constraints. Dam releases, outflow through the California delta, and a surface water budget was calculated in Anderson et al. (2012 – cited in manuscript). The purpose of our manuscript is to introduce constraints on the irrigation flux using only remotely-sensed data so that the method can be applied to other, data-poor, regions of the world.

3575, 1) Presumably you mean fig. 5? I don't understand how to interpret fig. 5, pls explain.

We do mean Fig. 5, and we will correct this in a revised manuscript. We will also add additional text to the caption of Fig. 5: "The x-axis represents the trial recharge used in equation 4 to obtain GWWD and the y-axis represents the output recharge from equation 3."

9) why call it an inventory approach? What you describe sounds like a water budget approach.

We called it an inventory approach because that was the description that was used in Anderson et al. (2012). It refers to using the data from dam releases and outflow to construct an inventory. We prefer this over "water budget" as we believe water budget is a more generic term.

11) "Agreement" not "comparison". Also, how did Anderson et al estimate GW_wd?
Anderson et al. (2012) did not estimate GW_wd. They focused on estimating surface water consumption using remote-sensing and intercomparison to the inventory approach. Using the iterative approach in Fig. 5 is one significant advance on Anderson et al. (2012) and is necessary to obtain direction hydrologic fluxes (as opposed to net flux) to better use satellite data in land surface modeling. <u>We will change "comparison" to "agreement".</u>

25-26) This needs an interpretation. I assume this may be a consequence of wetter soil conditions causing greater rainfall-runoff response, but given it is modelled you can (and should) trace why this is predicted.
We agree that more interpretation is needed. <u>We will add the following to the end of the sentence, "which is an expected consequence due to the wet soil from irrigation leading to higher surface runoff".</u>

29) losing streams – this term is missing in Eq. 4. Pls discuss.
Losing streams would be represented by a negative Q_discharge (representing river recharging aquifers), and would not require an additional term. The discussion of this stream property is meant to refer to that groundwater tables no longer intersect stream beds in many parts of the Central Valley (unlike historical conditions). Therefore, changes in irrigation are unlikely to increase Q_discharge.

3577, 2) once again, no need for "may" – you should be able to deduct this from your modelling.
We feel this hedge with "may" is warranted given the heterogeneous cropping patterns that exist at sub-CLM (0.125°X0.125° grid cell) resolution. All of the CLM grid cells in the Central Valley have both annual and perennial crops, with flood and drain crops (rice) present primarily in the Northern Central Valley.

28) "global inventory" - pls explain.
We refer to the irrigation data set of Siebert et al. (2010) here. <u>We will revise this sentence to read, "than a global inventory (Siebert et al., 2010), based approach".</u>

3578, 14-17) sounds like a fairly speculative thought bubble. Argue better or delete.
Right now, observations of soil moisture are either non-existent or too coarse or inaccurate (SMOS, AMSR-E) to enable regional soil moisture mapping. SMAP was specifically designed to produce soil moisture observations at a sufficiently high spatiotemporal resolution for weather and land surface models. <u>We will rephrase this section to ". . . with precise and accurate regional and global soil moisture observations from upcoming missions such as the Soil Moisture Active Passive, whose outputs are specifically designed to improve inputs to numerical weather prediction and land surface models (Entekhabi et al., 2010)."</u>

18) "sufficiently coarse"?? Rephrase.
We will change "coarse" to "low".

26) "dry" not "drought". "missions" - what missions?
We will replace "drought" with "dry" and specify the four missions discussed by the citations at the end of this sentence.

28) "higher spatial scales" – do you mean higher spatial resolution?
Yes, and we will change "scales" to "resolution".

Anonymous reviewer 2:
General comments This paper introduces a new method to account for irrigation water management in land surface models using optical and gravimetry satellite data.
This is an important topic because it will help analyzing the impacts of irrigation water abstraction on the hydrological and the climate system.
We appreciate the reviewer's recognition of these aspects of our manuscript.

The method is developed for the Central Valley in California that is probably a very unique irrigated region with a lot of available data and large irrigated fields that are easily 'seen' by the remote sensing ET products. In many other regions in the world where plot sizes are and perhaps irrigation intensity is much smaller the signal might not be as strong, and the approach might not work at all. Some methods use absolute values as thresholds and it is not clear how they will be determined elsewhere. To be more relevant for the problem, it would be interesting to see at least some discussion on how the approach can be applied in other regions with different irrigation practices and hydro-geolocial conditions. Ideally, the approach should be tested in another region. Some of the methods are not sufficiently well justified and should be clarified for somebody who is not familiar with the CLM modeling system (see detailed comments below).
We note that satellite ET products come in many spatial resolutions. Landsat based products (e.g. METRIC) can come in resolutions as high as 30m, so we do not think that the spatial resolution will be a hindrance in applying this approach elsewhere. We note that the satellite ET algorithms used here do not rely on absolute thresholds, determination of irrigation practices, or knowledge of underlying hydrogeological conditions. Our method and approach was to use as little *in-situ* observations or data to develop the method (hence the use of satellite-based ET and running the land surface model with reanalysis products), but to assess the results using our knowledge of the Central Valley and the *in-situ* observations. We also note that the Central Valley is still an important study region given the conflicting previous studies on the implications of Central Valley irrigation for precipitation elsewhere in the Western United States (Lo and Famiglietti, 2013; Sorooshian et al., 2011), which is especially relevant given current and potential future drying in the Western US. We agree that testing this type of parameterization in other, data-poor, irrigated regions would be a good future research direction.

Specific questions and technical corrections

Section 2.1. A bit more information on the irrigation practices would be useful :Fraction of total area irrigated, main crops, number of crops per year, irrigation infrastructure (canals, sw reservoirs, etc) , water use as fraction of total available water resources? Parts of section 3.2 could be moved here.

Unlike many irrigated regions around the world, the Central Valley is notable for its incredible diversity of crops and lack of a few predominant crops. For example, in one Central Valley county (Fresno) where the one of the authors (Anderson) has previously worked, there are more than 300 varieties of different crops/cultivars (the more than 200 crops number was a conservative number to distinguish between different crops). Some additional information requested by reviewer 2 will already be provided in response to reviewer 1. Additional details, including total blue water consumption and a reference to the Census of Agriculture details on cropping and irrigation area, will be included in a revision.

Section 2.2. Somehow it is unclear why the ensemble ET is superior to any of the individual products. What is the spatial resolution of the ET, Precip, and the CLM grid cell resolution? There are significant uncertainties in the remote sensing estimates that should at least be discussed and perhaps the ET ensemble should not be called 'observed' values (later in the manuscript). What is meant by uncertainty of ET (line 1).

Using an ensemble of satellite hydrology products developed using different methodologies is a well-recognized approach to constraining the value of the parameter one wants to observe. By avoiding a single approach (and its assorted biases) we obtain a range where there is greater confidence in the actual ET value. Following the suggestion of both reviewers, we will revise the "observed" language as detailed in response to reviewer 1. Spatial resolutions are now reported for all products. To reduce confusion, "uncertainty" has been replaced by "standard deviation"

Section 2.3. Some more details for the CLM (spatial and temporal resolution) and a justification for the use of the 9 member ensemble would be interesting.

The reason to use the 9 member ensemble is to include as many as possible of the current the state-of-the-art models' simulations. The 9 member ensembles are based on different models and different atmospheric forcings. Therefore, we have more confidence on their ET simulations in a pre-irrigation, pre-development environment.

Eq. 5 assumes that all water abstracted from ground and surface water becomes ET. In reality, a considerable amount is returned to the soil and gw storage, as loss. It is probably not relevant on monthly time steps if you consider the net abstraction only but at least it should be mentioned. Line 24 on 3572 and Fig.2 seem to suggest that the abstraction (delta ET) will be added to precipitation in the model, in which case it will be redistributed. Will this violate the grid cell water balance? Why is deltaET in Eq. 5 taken as the 6 year mean? There should be considerable differences between wet and dry years that are worth exploring. This can be seen in figure 3a and 3b.

The irrigation water is taken from both surface water and groundwater. The Central Valley aquifer system is a combination of unconfined and confined aquifers; we assume that groundwater withdrawals are equally distributed between both types of aquifers. Because the CLM lacks a confined aquifer component, confined withdrawal is from a hypothetical water store. Unconfined withdrawals were taken from the saturated zone of the soil. The reviewer is correct that Eq. 5 assumes all abstracted water becomes ET. But Eq. 5 is used only to obtain an estimate for SW_WD, which then determines (in part) the P to be input in a simulation. So the CLM cell water balance in a given simulation is not affected. We will mention the limitation with equation 5 in our revision. We will also revise the last part of section 2.4 to read "Since the Central Valley aquifer system is a combination of unconfined and confined aquifers; we assume that groundwater withdrawals are equally distributed between both types of aquifers (Fig 2). Because the CLM lacks a confined aquifer component, confined withdrawal is taken from a hypothetical water store which is constrained together with the unconfined aquifer using equation 4 and GRACE estimated groundwater. Unconfined withdrawals were taken from the saturated zone of the soil."

With respect to the deltaET, we agree that there are some differences between wet and dry years, and that the use of such additional information could use the annual data to make the simulations better. However, high resolution estimates of ET may not be available for other regions or times, so in this study we would rather use the climatological irrigation water demand as determined from the multi-annual mean of the satellite observed ET.

The 'grid search' is unclear. Is there is search distance or is water only taken from the same grid cell? If so, the amount of water available from surface water will highly depend on the resolution of the model. The 'trial and error' approach in figure 5 is not clear and needs a better explanation. What is the justification for using values between 5 and 20 mm? Are these values related to the total Central valley area or only the irrigated areas? Here and elsewhere in the manuscript it would be worthwhile to report number (irrigation depth etc.) related to the irrigated area, and not averaged over the entire area.

Our inclusion of "grid search" as a parenthetical comment was not necessary and needlessly created confusion; the "grid" we were referring to was not the CLM grid, but rather the gridded values of groundwater recharge shown in Fig. 5. We trust that our removal of that comment will help clarify things. As noted on pg. 3573, ln 23, we started the search for a satisfactory value of GW_{WD} at 20 mm because that was the value necessary to match the no-irrigation, baseline simulation. The choice to increase GW_{WD} in increments of 5 mm was arbitrary, but was made because it seemed to provide reasonably fine resolution without requiring an excessive number of model simulations. We will remove "grid search".

Section 3.2. The mean deltaET (376mm) needs to be put into perspective with total water use and available water. Can you add a figure showing the monthly time series of reported (inventory) and simulated abstraction from gw and sw ? What would be interesting is the different partitioning of the two in response to drought conditions.

The inventory of water use for the Central Valley and partitioning of surface and groundwater consumption (not abstraction) is already reported in Anderson et al. (2012) and a figure of inventory water use would duplicate that report (see Table 1 and Fig 2.

from Anderson et al. (2012) below). The simulated abstraction of gw and sw is constant during the summer months (May-October) as we discuss in section 3.2. Following the suggestion of both reviewers, we now report the total blue water consumption to better conceptualize the size of the Central Valley hydrologic system. However, we want to keep the emphasis of the study on the model development aspect instead of focusing on the nuances of the Central Valley.

Table 1. Water Year Statistics[a]

Water Year	P		ET		dGW/dt	dSM/dt	Satellite SW_C
2004–05	479 ± 48		725 ± 76		−24 ± 5	14 ± 3	237 ± 90
2005–06	454 ± 45		700 ± 48		32 ± 7	2 ± 3	282 ± 66
2006–07	186 ± 19		669 ± 71		−113 ± 22	−18 ± 3	351 ± 76
2007–08	253 ± 25		674 ± 49		−95 ± 19	−1 ± 3	324 ± 59
2008–09	254 ± 25		663 ± 59		−145 ± 28	−1 ± 3	262 ± 70
Mean for 2004–2009	325 ± 15		686 ± 27		−69 ± 6	−1 ± 2	291 ± 32
	P		ET		Measured SW_C		
Water Year	SJ	Sac	SJ	Sac	SJ	Sac	SJ + Sac
2004–05	388 ± 39	674 ± 67	709 ± 87	760 ± 52	338 ± 17	452 ± 23	375 ± 19
2005–06	315 ± 31	752 ± 75	690 ± 62	723 ± 18	339 ± 17	91 ± 4	261 ± 13
2006–07	129 ± 13	310 ± 31	649 ± 76	710 ± 59	177 ± 9	708 ± 35	345 ± 17
2007–08	181 ± 18	409 ± 41	663 ± 53	697 ± 41	167 ± 8	493 ± 25	271 ± 14
2008–09	177 ± 18	422 ± 42	637 ± 72	718 ± 30	195 ± 10	491 ± 25	289 ± 14
Mean for 2004–2009	238 ± 11	514 ± 23	670 ± 31	722 ± 18	243 ± 5	447 ± 10	308 ± 7

[a]All fluxes are in mm/year. Mean fluxes are averaged over the study period. SJ and Sac refer to San Joaquin/Tulare Lake and Sacramento basins, respectively. All values rounded to nearest mm. dGW/dt and dSM/dt are not shown for SJ and Sac due to spatial resolution limitations.

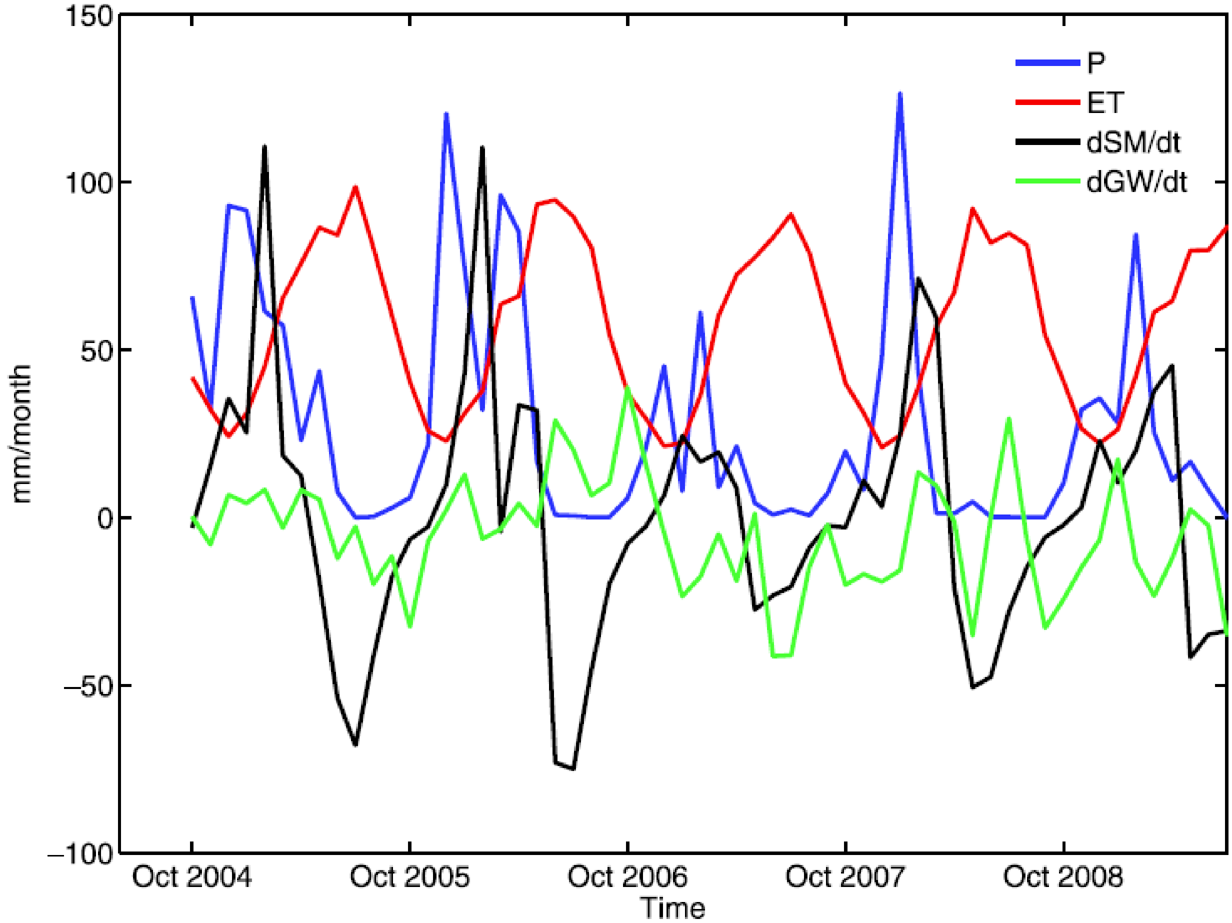


Figure 2. Monthly precipitation (P), evapotranspiration (ET), groundwater change $\left({dGW}/{dt}\right)$, and soil moisture $\left({dSM}/{dt}\right)$ from July 2004–June 2009 (mm/month).

Figure 3.a Explain the range of the shaded regions. The thick lines are mean values ? Figure 3.c. Should “time” be replaced by “month”? Figure 6 needs a better legend. Align the color schemes in figures 6 and 7.

We will add text explaining the range of the shaded region and will alter the figures as the reviewer suggests.