

Responses to: Interactive comment on “Soil-water dynamics and unsaturated storage during snowmelt following

wildfire” by B. A. Ebel et al.

- Reviewer comments in black text, [author responses in blue text](#).

Anonymous Referee #2

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General comments

This paper touches a very interesting subject, the effects of aspect on snowmelt and soil water storage in burned areas. As the authors state, little is known about this subject while it is very relevant for mountainous regions. The topic is relevant to the journal as it touches hydrology and fire – the latter being an underrepresented topic in HESS, despite its effect on earth surface processes. The approach is sound, but the methods deserve additional attention. Although a lot of data is presented, I miss a quantitative statistical analysis of the effects observed. This needs to be included before publication in HESS. Statistical analysis of the results needs to be added to both methods and results sections, and all research questions need to be statistically addressed if possible. Ideally, a statistical analysis would not only cover differences between the separate treatments, but rather the effect of aspect and fire in general on the various parameters assessed. The literature review presented is sound (though note the specific comment below), and the paper is generally well written. Although the reasoning is most often clear, I feel that the clarity of the paper can greatly benefit from a number of small improvements to treatment names, figure layout and other things (hence the long list of specific comments and technical corrections below).

[We thank the reviewer for the many constructive suggestions that improve the clarity of the manuscript. The authors have made many of these changes and feel the suggestions were very helpful in improving the manuscript. The review is thorough, which the authors realize is a substantial investment of time on the part of the reviewer.](#)

[The quantitative statistical analysis suggested by the reviewer can be highly illustrative and quantitatively demonstrate the effects of different experimental factors examined. We believe that some of our results could be appropriately analyzed with parametric tests, such as t-tests, that attempt to discern whether the data collected come from different statistical populations with a chosen significance level. For example, the thermogravimetric soil-water content or SWE data, which have smaller numbers of observations in the datasets \(i.e. tens of points\) are amenable to such analysis.](#)

In the authors' opinion such analysis is not appropriate for datasets with very large sample sizes (i.e. tens of thousands of data points). For example, the automated soil-water content and soil-temperature data presented in this manuscript have a very large number of data points (tens of thousands). In cases of very large data sets, the probability distribution is extremely well defined (i.e. constrained). This means that for such large datasets, the null hypothesis that two datasets came from the same population can easily be rejected with a p value of nearly zero. In the case of the work presented in this manuscript, "significant" differences would be seen at p values near zero for all the experimental factors (i.e. burned vs. unburned and north- vs. south-facing aspect). While this is a quantitative statistical analysis that shows statistically significant differences in terms of probability distributions, it is highly misleading because such analysis says nothing with regards to whether these differences are important relative to hydrologic or ecologic effects. One could apply "effect size" measures, but these are typically more qualitative metrics and would not suffice as quantitative statistical analysis. This issue of large data sets is problematic because such well constrained probability distributions fail tests of normality, such as Kolmogorov-Smirnov tests. Parametric statistical analyses like t-tests and ANOVA, which can be forgiving of non-normality for small datasets (i.e., tens to hundreds of data points) are considerably less forgiving of non-normality (even when data are transformed) for datasets of the size collected here, and can erroneously suggest significant differences between experimental factors.

Specific comments

As stated above, the literature review presented by the authors is sound, although I am surprised by the small number of HESS papers cited (just one). A number of papers were published in HESS about snowmelt (Renner & Bernhofer 2011, Schulz and De Jong 2004, Li and Wang, 2011 to name some) and two about fire (Rosso et al 2007 (runoff, sediment, woody debris) and Stoof et al 2012 (role of canopy interception and soil moisture on streamflow, effects on recharge)). If the HESS papers support part of the work in the present paper, consider including references to them.

We added a citation to Stoof et al. (2012) in the introduction. The Renner and Bernhofer paper focuses more on European climate change, the Schulz and De Jong paper is about snowmelt modeling in Morocco, and the Li and Wang paper reports work from a field site in China. It's not clear that any of these publications is directly relevant to the work presented here. We did not directly focus on sediment transport or woody debris, so it's not clear why we would cite the Rosso et al. paper. We understand the motivation of including HESS citations in this manuscript, but there are few, other than Stoof et al. (2012), which support the work presented here.

Line 35: recharge greater on burned slopes because of earlier soil thaw — and thus reduced overland flow?

We did not observe any substantial overland flow during snowmelt at either north- or south-facing sites. We believe that the earlier thaw means a longer duration of melt input and thus more total recharge. Additionally, because the south-facing snowpack melts over a few days, there is little loss of the snowpack by sublimation or wind scour, in contrast to the north-facing slopes.

Line 66, snow-water equivalent. Can you specify whether this is the amount of water reaching the soil surface? Or total input at the canopy level? I assume the former.

SWE is the total liquid water content of the snowpack. We will define this in a revised manuscript.

Line 71. As many fire scientists may not be aware of snowmelt processes, I recommend defining which part of the variation in location matters most- elevation, aspect, latitude, etc?

In the authors' opinion, so little is known about about post-wildfire snowmelt that it is difficult to generalize about the relative importance of these different factors in determining the system response. In time, it is our hope that sufficient research is conducted to better understand the relative contributions to snowmelt variations.

Line 78 and 79: This Massman and Frank paper indeed studied the effect of fire on thermophysical properties, but their fire was not a wildfire but rather a controlled burn which 'continued to burn or smolder for several hours to days after'. I am not aware of publications reporting the same thing for wildfires (with much shorter residence times), but if you want to make this statement for wildfires, I suggest finding a reference that supports this statement for wildfires.

Yes, the reviewer correctly pointed out the issue with the original citation. We have removed "wildfire" and changed the sentence to "The thermal properties of soils can also be altered by heating during fire (Massman and Frank, 2004)." Alternatively, we could entirely remove the sentence.

Line 90: Note that removal of the vegetation canopy can have similar effects - Stoof et al 2012 (HESS) found significant hydrological changes in a burn that did not affect soil hydraulic properties.

We added a citation to Stoof et al. (2012).

Line 137, 138: Please help the reader remember that the Fourmile Canyon site is the burned site, and Gordon Gulch is the unburned site. It would be very helpful to add the words 'burned' and 'unburned' to these catchment names at multiple locations in the text, such as also in (but not limited to) line 561, 564 and 566.

We agree with the suggestion of the reviewer and have added "burned" and "unburned" in the locations suggested by the reviewer.

Line 152: I did not find anything about recommended naming of soils in HESS, but because it is a European journal I recommend adding the corresponding FAO soil types here.

The FAO equivalent for the soil naming would be a Luvisol, we will include this in the revised manuscript.

Line 168, 39.99 N, -105.48 W. I assume this should be 39.99 N, 105.48 W (without negative sign) or 39.99 N, -105.48 E (with negative sign but E instead of W) – please correct.

Yes, we have removed the negative sign per the reviewer's correction.

Line 178. I understand that it is necessary to abbreviate the treatment types here, but I have a hard time remembering the meaning of the abbreviations throughout the paper. Consider using SRidge (or S_Rid, S-Rid), NRidge, SMid, NMid so that abbreviations are less abstract and easier to understand for the reader. It would also be helpful if the UB sites would have more intuitive names, or if their names are at least explained or consistent (1 2 3 or something with words, there is no system now).

We have changed the unburned sites to use more intuitive names in the revised manuscript. UB2 is now UBSFM (because of its midslope position) and UBControl is now UBSFR (because of its midslope position). These names parallel the abbreviations used for the burned sites, making them clearer for the reader. The authors feel the revised abbreviations are sufficient.

Line 173-183. Is there a reason that you only have one plot of each type (South facing ridge, south facing midslope, etc), i.e. that you didn't replicate the plot types, but only chose to do replicate measurements within each plot? Or do you consider the different plots (mid, ridge) to be replicates for the different aspects? If so, mention this here.

There are always limits on financial and time resources and these limits are reflected in our plot choices.

Line 180. Add reference to Table 1 after 'or southeast facing'.

We agree with the reviewer and will make the suggested change in the revised manuscript.

Line 189 and for other equipment (i.e. Decagon line 208): give manufacturer location

We agree with the reviewer and will make the suggested change in the revised manuscript.

Line 196: did you do something to ensure that previous sampling locations had no effect on current readings?

We moved about 0.5 to 1 m away (laterally) with each ~weekly repetition. Because the snowpack is so shallow, there is very little disturbance associated with the SWE and snow density measurements.

Line 198. I suggest revising the end of this sentence to 'reported as the ratio of: : .' (or 'between')

We agree with the reviewer and will make the suggested change in the revised manuscript.

Line 200. Do you mean precipitation record? ('complete record')

Yes, we have changed the revised manuscript to: "The precipitation record from the nearby Sugarloaf NADP site (CO94 at 39.99 N, -105.48 W; NADP, 2011) was used as a complete record during the snowmelt season to understand trends when data from the ultrasonic sensor and manual SWE measurements were unavailable."

Line 208. The plot S Control has not been defined above. Is it correct that soil water contents were not measured at UBTemp?

This is a typo, it should be UBSCControl. Thermogravimetric and automated (Decagon 5TE) measurements of soil-water content were not made at UBTemp.

Line 214-216. Maybe you can add a sentence here why you didn't do this/ was not considered important to do in your paper.

We changed "further calibration" to "difficult and expensive calibration".

Line 228. I know from field observations in burned areas that soil moisture often has a very steep profile in the top 5 cm of the soil, because of evaporation from the bare soil surface. This can be different in winter/snowmelt conditions, so please include information (an observation or a reference) that provides a ground for this assumption, or mention the limitations of this assumption if this was the best you could do.

The Decagon 5TE sensors cannot be installed shallower than 5cm without compromising the integrity of the measurements. We have added this to the revised manuscript and state that "Minimal evaporation during the winter and spring lessens soil-water content gradients in the top 5 cm of soil."

Line 235. Why were soil water retention curves determined for a subset of sites only?

Funding and time limitations prevented analysis of a large number of soil-water retention curves. Since the submission of this manuscript, additional funding allowed measurement of soil-water retention curves for the UBSF and UBNF sites and we have added this information into the manuscript.

Line 242. I am a bit surprised by the matric head chosen for field capacity (-340 cm), and could not find this in the given reference. Koorevaar and Menelik do state that 'soils with deep groundwater tables at field capacity generally show matric heads of about -3.0 m' (page 85) but the soils in the present study are only 0.5 m deep. It seems hard to create a suction of 3.4 m in a soil that is this shallow. I am not an expert on how to determine field capacity of shallow soils, but do know that the definition of field capacity varies between countries (see <http://www.cabdirect.org/abstracts/20113299698.html>). I recommend explaining why you chose this definition of field capacity, and update the value for shallow soil if feasible.

In the revised manuscript we changed the reference to Richards and Weaver (1944), who found field capacity to be approximately 1/3 of a bar. As noted by Richards and Weaver (1944), "field capacity" can vary quite a bit between soils. But comparing the soil-water content at 1/3 of a bar, which is -340 cm, allows for objective comparisons of water retention for different soils, even if it is not the "true" field capacity.

Richards, L.A. and L.R. Weaver. (1944) Moisture retention by some irrigated soils as related to soil-moisture tension, *Journal of Agricultural Research*, 69, 215-233.

Line 247. Why is it relatively small, compared to elsewhere, or to average snowpack conditions?

The revised manuscript changes "relatively small" to "less than 1 m thick". We meant to state that the snowpack here is relatively thin compared to other sites that are typically studied for snowmelt related to streamflow and water supply.

Line 332: it would be helpful if you could specify (here or in a graph) when this was

In the text we added "(i.e. 5-12 March)".

Figure 6: indicate depth of measurement in caption of figure

We added "between 0-3 cm depth" to the caption.

Line 337: I think UBNF has not been defined before – please include it in the methods section

Yes, in the revised version UBNF plot is mentioned earlier in the manuscript in the methods section.

Line 339: Rephrase to 'soil hydrologic response'

We changed the text to "Unsaturated-zone hydrologic response".

Line 365-366: please include a reference to a table where we can see this. It would be very helpful if the field capacity data were presented in a separate table or figure, to facilitate comparison.

The focus of this work is snowmelt and accompanying soil-water dynamics, not soil-water retention changes. It is the author's opinion that sufficient detail of relative changes in soil-water content at field capacity are visible in Figure 7. No change to the manuscript is necessary.

Line 373. Change to 'did not considerably affect NFM' - a little peak is visible

We added "considerably" to the text, as suggested by the reviewer.

Line 421. It would be very helpful if you could again very shortly define here how unsaturated storage was defined or determined here

The first sentence was changed to "Unsaturated storage (see Fig. 9), estimated by integration of the automated soil-water content measurement profiles, mimicked the soil-water content trends shown previously."

Line 454. Change into 'depletion', if desired by 'depleting (by drainage or evaporation)'. The '/' could confuse readers that you're referring to a ratio between drainage and evaporation, and it is certain that it is depletion, the interpretation is likely but not certain.

The sentence was changed to "... had more substantial water loss by drainage and evaporation in the..."

Line 536 to 555. It is unclear to me whether this paragraph is based on the present study's work, the companion paper that is in review, or on other literature. It would be very helpful if references to Figures, tables and/or references could be added.

In the revised manuscript, we added a few sentences to briefly introduce the Gordon Gulch study and state that it is separate work that is not a companion study to the work presented in this manuscript. Instead, the proximity and similarity of objectives allow the sites to be compared.

Line 551, flow initiation. Do you just mean preferential flow, or really the start of preferential flow? The next sentences suggest that it is mostly the process in general.

We have removed "initiation" to indicate that we are referring to the process in general and its prevalence.

Line 552, thus preventing. I suggest adding the word possibly - although ash can infiltrate into the soil pores, and it has been suggested that this can inhibit infiltration, this has not been proven yet.

Yes, we replaced "thus" with "potentially" in the revised manuscript.

Line 555. Include reference to where we could see that if you measured this, or literature reference where this is different on north and south facing slopes, and say how this reduced structure would affect preferential flow. Part of this may be in the next paragraph but because it is a new paragraph that is not clear in this sentence.

We agree that this portion of the text was not clear. In the revised manuscript, we made this sentence “Another possible explanation for the difference in preferential flow on south-facing slopes is reduced soil structure following wildfire.” the topic sentence of the next paragraph, which focuses on soil structure changes. We believe this makes this section clearer.

Line 559 This was also found by Stoof et al. 2010, who had no soil structure because of repacked columns. Given your next sentence, I have the idea that you want to link loss of water retention in the wet range to soil structure - which is sensible given the literature available on this topic (see for instance Dexter et al 2008 . <http://dx.doi.org/10.1016/j.geoderma.2007.11.010> .). Add a sentence here linking the two things. Effects of fire on aggregate stability soil structure are discussed or referred to by Garcia-Corona et al (2008, http://www.publish.csiro.au/?act=view_file&file_id=WF03068.pdf) and by Bento-Goncalves et al, 2012 (<http://dx.doi.org/10.1016/j.geoderma.2012.01.004>)

Yes, in the revised manuscript we cite the papers suggested by the reviewer as examples of fire impacts on soil-water retention and structure that may impede preferential flow. We also added an additional paper suggested separately by the reviewer by Mataix-Solera et al. (2011).

(Specific comments to tables and figures below)

Technical corrections

Remove the dashes in ‘soil-water content’ and ‘soil temperature’ and ‘soil-water retention’ throughout the paper

If this is consistent with the style mandated by HESS, then we will do this in the revised manuscript.

Is there a double space after ‘South’ in line 25? If so, remove it. Same for line 197 after ‘particularly’

Yes, we have removed these extra spaces.

Line 35, 571 and elsewhere. Undo capitalization of paragraph titles

If this is consistent with the style mandated by HESS, then we will do this in the revised manuscript.

Line 116 – replace ‘2-100’ by ‘2 to 100’ I erroneously thought it was a strange number instead of a range.

Yes, we have replaced “-“ with “to”.

Line 199. Insert space after 'NADP site'

We added a space, as suggested by the reviewer.

Line 207. cm³ cm⁻³ - change to m³ for SI and to be consistent with your tables

OK, we can make the units m³ m⁻³.

Line 208: I think this should be 2-minute intervals (with dash)

We have "2-minute" in the revised manuscript, as suggested by the reviewer.

Line 218: replace 'zero C' by '0 C'

We replaced 'zero C' by '0 C'.

Line 242: Koorevar should be Koorevaar.

We removed this citation and reference in the revised manuscript.

Line 286 and elsewhere (line 295, 625) : Soil temperatures can be lower but not colder, so either say that soils were cooler, or that soil temperatures were lower. Same throughout the paper for 'dry and wet soil water contents' (line 404-405). Soil water contents are low or high, and soils can be dry or wet.

Fine, we can change this in the revised manuscript.

Line 289. Insert space after north-Throughout: insert space after value and before the unit (such as in

line 306: 10cm)

We added a space after "north-" and between "10" and "cm".

Line 321: add dash between sigma and values

We added dash between sigma and values.

Line 332. Add dash between near and surface

We added dash between near and surface.

Line 377: indicate that the 'shaded regions' are in fact the blue horizontal bars

We changed "shaded regions" to "dark blue bars".

Line 464, 465: Define TDR

We defined time-domain reflectometry (TDR) in the revised manuscript.

Line 471, this finding: do you mean the finding that soil water storage was unaffected or that field capacity was slightly different at each plot? Also, remove dash between very and dry, and remove comma after conditions in line 472.

The revised text reads: "Soil-water storage at the end of the snowmelt season was relatively unaffected by aspect or wildfire, essentially converging to field capacity. Values of field-capacity were slightly different at each plot (Figure 9). This finding is important because it suggests that the very dry soil conditions that could limit plant regeneration, established before the wildfire and exacerbated in the top 5 cm during the wildfire, can be erased during snowmelt."

End of line 517: add 'near freezing' after references to avoid confusion.

We chose to remove the Klock (1972) citation from this sentence, which achieves the same clarity.

Line 533. I think you mean aspect-affected differences?

Yes, we have revised the text to state "aspect-affected differences".

Line 542-543. Do you want to say that they 1) were not collected, or that they 2) were collected but not to study preferential flow? In case of 1) change sentence to '... not collected to examine preferential flow at the Fourmile Canyon site'. In case of 2) leave sentence as is

Revised sentence states: "While tracer data were not collected to examine preferential flow at the Fourmile Canyon site..."

Line 558- add reference to figure or (new) table before the comma

We added references to Figures 7 and 8.

Tables and Figures– specific and technical comments

Table 1 remove period after Table caption

The authors looked at several HESS papers and there were periods after the "Table 1" text and after the captions. It is not clear to us that the reviewer's request is pertinent to the HESS format? However, if this is consistent with the style mandated by HESS, then we will do this in the revised manuscript.

Move degree sign to after slope instead of after range

This suggested change was made in the revised manuscript.

Please indicate what value you use for soil depth - is this the average of the different measurements per site, or the highest value? If you can, give standard deviation. Also, it may be more appropriate to give this value in meters (S.I.)

Soil depths are the maximum value of repetitively driving a steel rod to refusal. We have added this information to the Table 1 footnotes. We not feel that the standard deviations are essential information to include in the Table. Units in cm are fine.

Table 2. Can you add a column indicating whether the precipitation for these events was snow or rain? (or otherwise frozen or not frozen?)

All of the events were snow except for the 17 May event, which was a rain/snow mix. We have added this information to the revised Table 2.

Table 3. Add units in top row of table instead of below mean, median, etc.

This will be done in the revised manuscript.

It would be very helpful if the data in this figure were rearranged to facilitate comparison between the treatments (which is what is discussed in the text)- by first giving all data for the 5 cm depth, then all data for 10 cm, etc. The current layout mostly allows assessment of effect of depth.

The Table can be rearranged in the revised manuscript.

Is soil temperature reading really accurate to two digits? I suggest rounding to one digit only.

We have rounded to one digit in the revised manuscript.

Figure captions.

Fig. 7. Add dash to 1-minute intervals. Use number 0 for zero degrees

We have added a dash to 1-min and used 0 for zero degrees.

Figures.

I often found it hard to interpret the graphs because so much information was presented in them. Also, a number of the graphs can be improved by cleaning up tick marks and time axis labels, or by avoiding repetition of figure legends for each figure panel where legends are the same anyhow. Specific comments follow:

Many of the reviewer's comments are stylistic in nature, some we feel clarify the Figures and have implemented these suggestions in the revised manuscript.

Figure 1. The layout of this map is a bit strange as the sites are all cluttered together. Would it be an option to reduce the size of blue area (and not show the creek) so that the pink area can be larger? Also, consider showing a small inset map of the USA too with Rocky Mountains, and make sure that blue and pink are distinct enough when readers print in black and white. The UB north site is not discussed in the methods, and it looks like this site was in an unburned spot in the burned area. Is this true, and were more UB sites chosen within B areas?

The revised Figure 1 shrinks the size of the blue area. The revised methods better explains the expansion of the UB sites.

Figure 2. Indicate in caption what the different dates are that are written out on the x-axis - are these sampling dates? Otherwise use regular monthly intervals starting on the 1st of each month. I also suggest revising final part of caption to "... nearby Sugarloaf climate station (site CO94) operated by..." Or something like that. Now it looks like the NDAP program is CO94. The horizontal bars in the temperature data look a bit odd - I think everybody understands that you drew lines between the daily values, so consider removing these dashes. Do add to the caption that the values are daily values of temperature and precipitation though. Figure 3. Because freezing conditions are important in this graph, it would be very helpful to have a horizontal line at 0C. Consider stopping the air temp axis at -20 because it didn't get -40. Again, indicate what you mean with the dates on X-axis or use monthly time intervals. Finally, consider using a dashed line for either of the two parameters shown here to make differences visible in black and white print too.

There was a horizontal dashed line at freezing in the original Figure version; we have made this line bold and red so that it is more visible. The daily minimum temperatures reached -30 degrees C, which is why the axis labels go to -40 C. The revised Figure caption incorporates the suggestion of the reviewer regarding the placement of the "CO94" text. Per the reviewer's suggestion, we revised the date axis to use regular monthly intervals starting on the 1st of each month.

Figure 4. I have a hard time interpreting this figure and recommend choosing different colours/dash lines, and splitting these data up into two figures. If both subpanels are aligned vertically (one above the other) and use the same x-axis scale, they can be easily compared. Consider putting x axis labels vertical - and give all of them for all sampling moments, or use regular monthly intervals. Finally, it would be helpful if you could define abbreviations and give the sampling interval in the figure caption. What is DAM2?

We have split the Figure into two Figures, as suggested by the reviewer. Horizontal axis labels are easier to read and the revised Figure uses monthly intervals. The sampling interval is already given in the text, but can be added to the Figure caption if that is essential. If the abbreviations are defined in the Figure 4 caption, then the abbreviations would need to be defined in all the other Figure captions as well. If HESS requires definition of abbreviations in the Figure captions, we can do this in the revised manuscript. DAM2 was a typo, we have corrected the label in the revised Figure.

Figure 5. This figure is clear and needs no rearrangement, though do note that the tickmarks are off. I recommend flipping the temperature legend so it runs from negative (left) to positive (right), which is more intuitive, defining abbreviations in the figure caption and mentioning how you acquired these data (automated/continuous measurements using XXX or something like that). Finally, you may want to call the black circles 'black horizontal bars' or something alike, since I initially did not recognize these as circles.

In the revised manuscript we added that the soil-temperatures are from from Decagon 5TE sensors and that the 2-min temporal resolution gives the black circles the appearance of broken lines. We also reversed the temperature scale. Also, the small tickmarks are ten day increments and the large tickmarks are the first days of the month.

Figure 6. I really like the shaded clouds drawn in Fig C to help interpretation of this graph. I think this panel can be further clarified by using one fill type for burned (filled shapes) and another for unburned (open shapes) and then use similar shapes for similar aspects, such as squares for north and triangles for south facing slopes. I do have a hard time interpreting figs A (and B to a lesser degree). Consider splitting these up by adding two more subfigures in which you show B vs UB, or south vs north, whichever you want to stress most. Another or an additional way to clarify this figure is to condense the space between the sampling in November and March by 'breaking' the x-axis here.

The Figure is clear in its current state, no change necessary.

Figure 7. Please add to caption whether field capacity value is for one sample or an average (give n). Can you use the same color as in Fig. 5 to indicate frozen soil?

In the revised text, we added that one sample per site was used. We have added monthly tickmarks to all upper panels. The colors used are clear as-is, no change necessary.

Figure 8. It is hard to quickly see the effect of depth on soil water content distribution in this graph because the bars are all clutched together. I suggest rearranging this graph by splitting each subpanel up into three subpanels, one for each depth. They don't have to be very large but it would improve the quality of this figure. Also, indicate what dashed line is, and for what depth field capacity is valid.

Taking a six panel Figure and splitting the three-depth figures in three and the two-depth figure in two would give a 17 panel Figure, which would be too small to see. No change needed.

Figure 9. Great to see you're using the same y-axis scale for all panels. Note that the tickmarks are off in the bottom panel, and that it would be good to have monthly tickmarks to all upper panels too.

Actually the small tickmarks are ten day increments and the large tickmarks are the first of the month. We have added monthly tickmarks to all upper panels.