We thank Professor Genthon for reviewing the manuscript.

This paper demonstrates that an original algorithm used in the Total Precipitation sensor TPS-3100 also called "hotplate" to derive quantities of precipitation has some errors that can be corrected, and has shortcomings that can be reduced. This is based on both theoretical and experimental approaches. The Hotplate concept uses an original method to quantify precipitation: the differential energy needed to keep 2 overlaid hotplates at constant temperature, one facing up and the other facing down, includes the contribution (latent heat) needed to evaporate any condensed water falling on the upper plate (but obviously not affecting the lower plate facing down). There are many other terms to account for though.Both plates loose sensible energy and the loss rate is related to wind speed and ambient temperature.

However the 2 plates are equally affected so the differential is 0. On the other hand, the radiation balance of the 2 plates is not necessary the same and the differential must be accounted for. The issue of the catch efficiency of the instrument is also raised, particularly for solid particles.

The hotplate concept is very attractive for a number of reasons listed in the paper, including the fact that it is immune to frost deposition and snow/frost clogging, thus particularly appropriate in cold regions. Yet, although the instrument is not new (marketed by YES since the early 2000's), it does not seem to be so widely used (the paper mentions that "70 Yankee Environmental Systems (YES) hotplate precipitation gauges have been purchased by researchers and operational meteorologists" which is not a lot). This may (or may not, price is also an issue of course) sign some dissatisfaction with the results obtained, and an improvement of the algorithm may contribute popularize the instrument.

If the approach and methods appear sound, there is one potential major shortfall with the paper: it is not obvious if and how the revised algorithm can be implemented in an existing units. This is admittedly a technical issue but this is "atmospheric measurement techniques" so one expect technical issues to be addressed.

The paper mentions that the hotplate outputs data in 2 files, one (UHP) which "is provided to all YES customers" and the other (SHP) with no indication how it can be accessed. Of course the later has the data needed to design and use a new algorithm but there is no reference to the second file in the YES Hotplate documentation as of 2011.

We asked Roy Rasmussen at the National Center for Atmospheric Research (NCAR) for access to the SHP data and were granted access after signing a legal agreement that protects the real time processor from piracy. We have presented analyses of SHP data in two theses, and now, in this manuscript in AMTD.

Here is how we will revise section 2.1:

2.1 - Hotplate Data Files

The hotplate outputs data to two files. The previously discussed Q_{top} and Q_{bot} are two of several recorded variables and both of these are essential for the analysis described here. One of the files is known as the UHP or "user" hotplate file. The UHP file is provided to all YES customers. The second file is the SHP or "sensor" file. The SHP file is proprietary but we were granted access to it by the National Center for Atmospheric Research (NCAR; Boulder, CO). Table 1 has the list of all recorded variables and how some of these are symbolized. A complete list of variables (measured and computed), and constants, is provided in the Appendix. With the exception of Unix time, all variables in Table 1 are provided as 60-s running averages, sampled at 1 Hz (YES, 2011).

YES no longer markets the instrument and no longer provides any information on its web site. In such a paper, one should not have to bet on obtaining "private communication" from a manufacturer which has terminated production, to learn how to access the data needed to implement the improved algorithm.

As we understand it, termination of production/marketing is temporary and occurred because of a legal argument between UCAR and YES. Currently, YES is servicing our hotplate.

I believe that the paper has very limited significance and is not acceptable for publication in its current form unless this information, and all information necessary to implement the new algorithm, is clearly provided.

We want to probe your comment in the last sentence. Are you saying that more information about the UW Algorithm is needed? It is our opinion that we provided enough for a user to implement the UW algorithm, provided they have access to the SHP data. Also, please see our previous statement that we will revise section 2.1 to include discussion of how the SHP data was accessed.

On the other hand, it definitively ranks publication status if the information is given so that the reader can implemented the new algorithm, and provided the various issues bellow are addressed, some of which are fairly serious though.

1) The introduction (lines 33 – 37) states that 2 types of instrumentations to measure precipitation have been developed: the capture and optical gauges. This ignores radars which are powerful tools to measure and even profile precipitation. Because of ground clutter and vertical resolution, radars do admittedly not measure precipitation right at the very surface but because they can profile vertically it may be checked whether precipitation rates vary or not as it reaches closer to the surface. Radars do not "obstruct the wind and deflects falling particles in the measurement zone" (line 38). There is no "clogging with snow" with radars (line 47).

In the US National Weather Service (NWS) network, a radar-derived snow amount is dependent on gauge measurements made simultaneous with the radar measurements. A published example of this is the NWS gauge-radar network data analyzed by Martinaitis et al. (2015). In that network, and presumably others, the gauge-calibrated radar-derived estimates of snow can be significantly biased. Factors contributing to this are gauge clogging by snowfall, postevent thaw of snow, and underestimation of snow because wind speed is either not used to correct for gauge undercatch or it is unavailable. Martinaitis et al. (2015) state that "..The accuracy of hourly radar-derived QPE values of winter precipitation is unknown..". Given this caveat, the dependence of radar precipitation estimates on gauge measurements, and the factors you mention (vertical structure, Earth curvature, ray ducting, ground clutter, and etc.), we have opted to not mention that radars can be used to derive snowfall amounts.

Martinaitis, S.M., S.B.Cocks, Y.Qi, B.T.Kaney, J.Zhang, and K.Howard, Understanding winter precipitation impacts on automated gauge observations within a real-time system, J. Hydrometeor., 16, 2345-2363, https://doi.org/10.1175/JHM-D-15-0020.1, 2015

Equation (3) (lines 113 – 120) describes the improvement of the algorithm by the authors by taking into account the solar and thermal radiation contributions the heat balance of the upper plate. Except for the latent heat term, one would expect a similar expression for the lower plate but this is not explicit. Because the hotplate is immune from snow and frost related problems that affect other sensors, it is expected particularly useful in cold snowy regions but then comes in the short wave power input reflected by the surface. If the sensor only measures the downwelling solar radiation, how is the reflected part factored in the lower plate energy balance?

Rasmussen et al. (2011) factored bottom plate power and ambient temperature, both measured at 2 m AGL, into a function that was regressed against wind speeds measured at 10 m AGL. This is evident in section 3a of their paper. Because the bottom plate power is affected by wind, snow-reflected solar, and longwave exchange, they have accounted for (implicitly) the effect you are referring to.

We hope that Professor Rasmussen and Mark Beaubien at YES can comment more on this.

Line 103: the sensible term is a function of U and T. Figure 1 shows that the temperature sensor is very poorly shaded from solar radiation reflected by the surface. Over a snow covered surface this is a likely major problem. The authors should evaluate the impact on precipitation estimation over snow, possibly bring in an empirical correction?

In Table R-1 we estimate this error for nonprecipitating conditions. Here the type–1 accumulations are evaluated using the UW algorithm and the type-2 accumulations are evaluated by substituting into the UW algorithm coincident temperature measurements made within a shielded temperature sensor (Vaisala WXT520). Results are presented for four days. The reported liquid equivalent accumulations are derived by integrating liquid equivalent rates from 16 to 20 UTC (10 to 14 LT). The table shows that positive bias in type-1, relative to type-2, is no larger than 1.4 liquid equivalent mm in four hours. Further, it needs to be acknowledged that solar is attenuated when precipitation is occurring, and thus the effect of sensor heating on precipitation measurements is smaller than demonstrated in Table R-1. In our opinion, more analysis is needed to explain and quantify this effect for both precipitating and nonprecipitating periods. Thus, we feel that the reviewer's recommendation is beyond the scope of the manuscript.

Table R-1 - Accumulations derived for nonprecipitating four-hour daytime cases; 16 to 20 Universal Coordinated Time (10 to 14 LT). Measurements are from the UW hotplate operated at the Medicine Bow Mountain Site described in Zelasko (2017).

UTC Year Month Day	20170128	20170129	20170130	20170131
Type-1 [a] Accumulation, mm [c]	1.7	1.7	1.8	1.4
Type-2 [b] Accumulation, mm [c]	0.5	0.7	0.4	0.1

[a] Type-1 are liquid-equivalent four-hour accumulations derived from UW hotplate measurements using the UW processing algorithm

[b] Type-2 are liquid-equivalent four-hour accumulations derived from UW hotplate measurements using the UW processing algorithm with Vaisala WXT520 temperatures substituted for YES temperatures

[c] The accumulation unit is liquid-equivalent depth over four hours (16 to 20 UTC)

Zelasko, N., Orographic Precipitation in Southeastern Wyoming, MS Thesis, Department of Atmospheric Science, University of Wyoming, 2017

Then, according to line 202, the upwelling IR is also estimated from this temperature supposed to be the ambient air temperature, but probably largely overestimated over snow.

The term is overestimated by sensor heating (Wolfe and Snider, 2012, their section 2c), however, the term contributes is less than 3 % to the power budget. This is demonstrated below (Figure R-1) for the same nonprecipitating daytime cases as in Table R-1. Furthermore, during precipitation the solar is attenuated, and thus the magnitude of the error is less than that for these nonprecipitating cases.

Wolfe, J.P., and J.R.Snider, A Relationship between Reflectivity and Snow Rate for a High-Altitude S-Band Radar, J. Appl. Meteor. Climatol., 51, 1111–1128, 2012

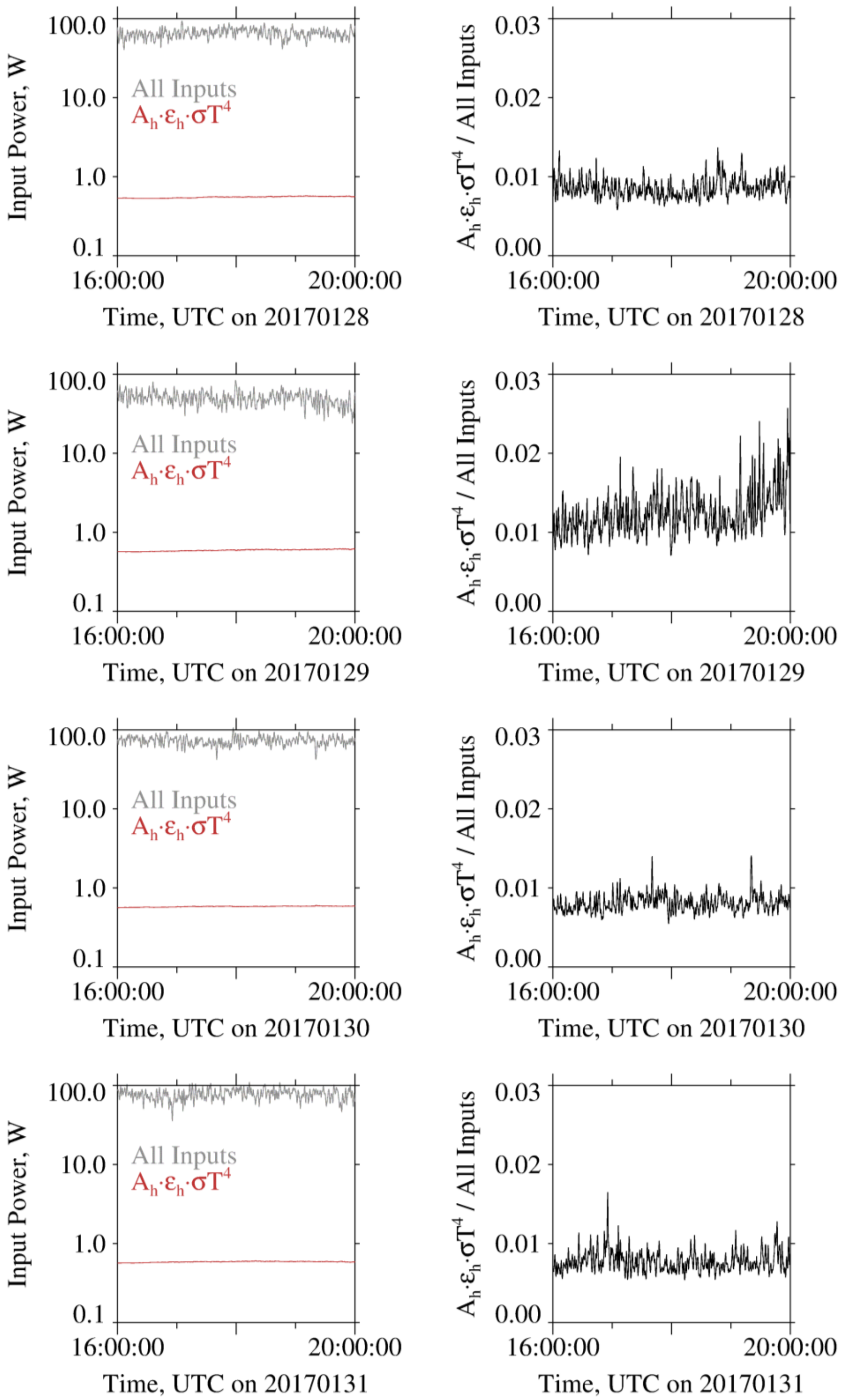

Figure R-1 – Left) Time sequence of $A_h \cdot \varepsilon_h \cdot \sigma \cdot T^4$. Also plotted is the sum of all input terms in the power budget. Right) Ratio $A_h \cdot \varepsilon_h \cdot \sigma \cdot T^4$ divided by all inputs. Results are for the nonprecipitating daytime cases. The field site is described in (Zelasko, 2017).

Line 3176: Define AGL (presumably Above Ground Level)

In the revision, we will define AGL.

Line 376: How would a “violation of the steady state assumption” could explain the delay? Would this have to do with thermal inertia?

We do not see a “delay”, rather a “slowed response”. Two things are contributing to this. 1) Each 1-s sample is a 60 s running average (see L131 and L977 in the manuscript), and 2) thermal inertia. Better understanding of the thermal inertia will require power measurements that are not 60-s averaged. We have asked YES for this.

Lines 422 – 423: This is not clear: the authors use data derived for a flow perpendicular to the plates to determine parameters for a flow parallel to the plates? Does this make sense? Can you clarify?

Hansen and Webb (1992) is the only publication, we are aware of, that reports experimentally-derived values of α and β for a circular plate with concentric rings. And yes, the airflow in Hansen and Webb (1992) was perpendicular (normal to) the plate surface. We did *not* mean to imply that the values of α and β determined by Hansen and Webb (1992) were used in our analysis.

Here are revisions that should clarify the sentence on L420:

This result is based on UW hotplate measurements, acquired at the GLE site, and formulas developed in section 3.6.

Revision for L227 to L229.

In this section, we develop a relationship between *Nu* and *Re* based on measurements recorded in the field when precipitation was not occurring; in a later section we show how that relationship is applied in the new algorithm.

Table 1: “Hotplate data files”. This table is misleading has long as access to SHP data is not explicit.

We apologize for not stating how we acquired the SHP data. As discussed above, we plan to revise section 2.1 so this is clear.

Lines 527-530: A synthesis table describing the various algorithms referenced in the paper could be useful. Only here does one clearly realize that none of the above algo discussion applies to the commercial (YES) one. The average hotplate user, presumably a target reader, probably got his/her instrument from YES and expects he/she will be able to improve his/her instrument with the new algo. It is not necessarily obvious from the beginning that R11 is not YES algo and that the authors describe an algo which objectively improve over R11 but not necessarily over the commercial units. Improvement over the commercial algo is verified only in the end. This should be clearly stated from the very beginning.

On L268-L274, near the beginning of the manuscript, we stated that we **assumed** that the YES algorithm has incorporated R11’s surface area and R11’s distinction between the theoretical and actual conversion factors. At present, we do not know if these effects are actually incorporated into the YES algorithm.

We do not think that a synthesis table is necessary. If the reviewer is insistent, we think it should go toward the end of the manuscript.