Overview and Summary

"Hotplate precipitation gauge calibrations and field measurements" describes theoretical and observational work focused on a novel precipitation gauge. The hotplate precipitation gauge measures precipitation by recording the amount of energy that is consumed by heating, melting, and evaporating precipitation captured on an upward-facing heated plate. The manuscript includes improvements to algorithms used to estimate the wind-induced undercatch of the hotplate, the conversion of energy to evaporation, and the effects of radiation, temperature, and wind on the energy balance and precipitation rate of the sensor. These algorithms are developed and tested using field and laboratory measurements.

Although the hotplate is not widely used, it is a unique, low-maintenance sensor capable of measuring all forms of precipitation in areas where power is available. Based on the hotplate's good performance in SPICE, there may be renewed interest in this technology when the SPICE results become widely available. The refinements and testing of the hotplate described in the present manuscript are therefore both valuable and timely.

Some general comments below indicate areas where there is room for improvement. The manuscript is generally well written, but typos and other more specific suggestions are documented in the specific comments.

General Comments

The description of the hotplates and algorithms discussed in the Introduction and Methods sections should be augmented to clearly state how the R11, YES, Boudala et al. (2010), and UW algorithms differ from each other. As currently written, it is difficult for a reader unfamiliar with this sensor to understand the relationship between the R11 and YES algorithms. The manuscript is focused mainly on improving the R11 algorithm, but the connection between the R11 algorithm and the YES algorithm should be described more clearly. This will help establish the relevance of the manuscript to uninformed hotplate users, who will presumably rely upon the YES algorithm.

The algorithms presented in R11 and the present manuscript were adjusted for wind speed losses. There were also differences in the way the wind speed was estimated, and in how the conversion from power to latent energy (f_2) was estimated. With such possibly competing or self-compensating differences, a good reference precipitation measurement is necessary to properly evaluate the different algorithms. The present manuscript would be strengthened significantly if instead of adjusted single-Alter shielded weighing gauge precipitation measurements, reference precipitation measurements shielded by a double fence were used to validate the improved algorithm and compare it to the R11 and YES-derived precipitation. Particularly at windy sites, wind speed adjustments introduce significant uncertainty in the resultant precipitation measurements (Kochendorfer et al., 2017a;Fortin et al., 2008). The hotplate-derived wind speeds shown in the present manuscript were fairly low, and many of the E 98 values were greater than 0.5 (Table 5). This is good, but the manuscript should still include some discussion or quantification of the uncertainty introduced by the adjustment of the single-Altar shielded weighing gauge measurements. WMO-SPICE included three hotplates, tested for two winter seasons at three separate sites with Double Fence Automated Reference (DFAR) measurements. I will try to help the authors obtain these data if they are interested in expanding the scope of the manuscript. I haven't looked at all of the WMO-SPICE hotplate data, but at the US (Marshall) site at least, the SHP values appear to be available. Feel free to contact me directly to discuss this at john.kochendorfer@noaa.gov.

Why were event totals used instead of higher frequency measurements? 30 or 60 minute rate/accumulation comparisons would provide many more points (or 'events') for evaluation, and would be accompanied with the added benefit of more stationary meteorological conditions and representative averages for wind speed, precipitation type, etc.

I didn't notice any mention of the hotplate power consumption. This should be added to the manuscript, assuming that I did not overlook it. In the Introduction the advantages of the hotplate are carefully documented, but this significant limitation appears to be omitted.

How much testing of this sensor has been performed in rain? I would expect there to be a significant amount of splash out in heavy rain.

Ln 61. Did YES produce more than one type of hotplate during the history of this product? The version of the hotplate firmware used should be included in the manuscript, if this relevant and available.

Specific Comments:

Ln 33 – 37. Heated tipping buckets are also used to measure snowfall (eg. Buisán et al., 2017).

Ln 56. Reference Fig. 1, which includes the radiation sensors. Also specify that only downwelling/incoming radiation was measured (and used to estimate net radiation). Or alternatively specify that the radiation sensors only faced upward, and upwelling/outgoing radiation was not measured.

Ln 71. Here and elsewhere in the manuscript, the term "latent" should be replaced with "latent energy" or "latent heat". Likewise "sensible" (Ln 70) should be replaced with "sensible heat" throughout the manuscript.

Ln 72. State explicitly that the effects of radiation on the energy balance of the bottom plate are assumed to be negligible.

Ln 74. Change "evaluate at Reynolds number" to "estimate a Reynolds number".

Figure 1. I had a hard time identifying the air temperature sensor. I initially assumed that an independent measurement was used. This is in part because I am accustomed to seeing air temperature measurements within larger fan-aspirated or louvered radiation shields, but it would help if the labels on the right side of Fig. 1 were more clearly associated with their appropriate component. Also add RH and pressure to the appropriate labels.

Ln. 97 – 98. Please summarize the formulation of the R11 conversion factors. Explain how they were different.

Ln. 110. Specify "downwelling longwave and shortwave fluxes".

Ln. 116 - 119. Radiative output and input haven't been defined. Also it needs to be made clear that radiative energy budget terms are only for the top plate. The sensible power output term is for both plates? T_h is assumed equal for the top and bottom plate, even when precipitation is occurring? Has this been confirmed with actual measurements of the plate temperatures?

Ln. 133 – 134. "In the infrared… is the relevant property" is awkward as written.

Ln. 136. "The value we picked is…" is awkward as written.

Ln. 137. Specify that this is only for the top plate.

Ln. 141. "the hotplate's reflectance is the relevant property" is awkward as written.

Ln. 142. Specify that the shortwave flux was downwelling.

Ln. 164- 165. Add the word "accurate" and replace "whether the sensed hydrometeors are rain or snow" with, "precipitation type": Rewrite as, "rate is dependent on the *accurate* assessment of *precipitation type*."

Ln. 168. After "pressure" specify that these measurements were all recorded by the hotplate system.

Ln. 182. Please explain/explore why the calibration changed after servicing.

Ln. 188. Delete the word, "vertically" or rewrite. I understand what you are saying (after looking at Fig. 2), but is confusing because the plates are oriented horizontally.

Ln. 194 - 207. I worked it out eventually, but I found most of this section quite confusing. It should be made clear that this entire discussion is focused only on the top plate. It sounds like YES assumed that the source of upwelling longwave radiation (the ground) was the same temperature as the air. This is not a good assumption, as the surface temperature of the earth often differs significantly from the air. It would be worth comparing the resultant downwelling infrared radiation with a measurement recorded using a normal pyrgeometer, from which the incoming longwave flux is typically calculated using the radiation measurement and the body temperature of the sensor. It also begs the question of why YES took this extra step. Was it because they were interested in the net IR flux, or because the use the outgoing longwave flux in their assessment of the bottom plate energy balance? Also the "downward" in "net downward" (Ln. 194) should be deleted. Strictly speaking, "net downward" is an oxymoron. This may have contributed to my confusion. Because an upward facing pyrgeometer is not typically used to estimate net radiation without another downward facing pyrgeometer, I initially assumed that the "net" was incorrect, as opposed to the "downward".

Ln. 197. Add “net” – “Eq. 4 represents the *net* longwave radiant measurement…”.

Ln. 207. Clarify that eq. 6 was only for use in the indoor experiment, where the temperature of the metal plate was estimated using the air temperature.

Ln. 212. Change the word, “settings” to something more appropriate like “variables”.

Ln 214. Change the word, “with” to “using” or “from” – “IR_d *was* calculated *using* Eq. 6”.

Ln. 229. Rewrite as, “that relationship *was* applied…”.

Ln. 230. Rewrite, “dimensionless representation of the sensible power output” - describe the Nusselt number more accurately.

Ln 251 – 252. Treating all precipitation above 0 °C as liquid is a little worrying. There are many examples of solid precipitation occurring above 0 °C (and liquid precipitation occurring below 0 °C) (eg. Kochendorfer et al., 2017b;Wolff et al., 2015). A third conversion factor for mixed (or ambiguous) precipitation would be more defensible. It could be some combination of 9a and 9b, or a transition between the two.

Ln 268 and 271. Is it realistic to assume that the hotplate temperature (T_h) is equal to 0 °C? Is the bottom plate also 0 °C when precipitation is occurring? I thought that the temperature of both plates was nominally 75 °C.

Ln 282. “This is accounting” is awkward as written.

Ln 286. “catch efficiency is accounting” is awkward as written.

Ln. 287. “accounts for the fact” is awkward as written.

Ln. 300. I understand that this is being done for the sake of comparison with R11, but it should be pointed out that there is no good reason to adjust the hotplate derived wind speed to another height. The manuscript should note that it is preferable to apply a catch efficiency function using the wind speed at the sensor’s location.

Ln. 336. Why were only the hangar data used? The temperature range used in the different warm-cold tests varies significantly. In some cases it is quite narrow, and in others quite warm. How important or realistic is this?

Ln. 328 – 338, and Table 3. The derived hotplate temperature is quite variable. There appears to be some cross-correlation between gamma and the hotplate temperature (Table 3), with larger values of gamma associated with smaller hotplate temperatures. These values also appear to be correlated with the warm-cold temperatures of the indoor experiments they were derived from, which suggests that they may not be constant even for the same sensor. A comparison of measured hotplate temperatures (a small thermocouple or an IRT could be used) and derived temperatures would help determine if the actual hotplate temperature varies as much as the derived temperature.

Ln. 361 – 362. This is awkward as written. Remove extraneous text. If there is no reason to question the fact that all of the water made it to the hotplate, there is no reason to bring it up.

Ln 371. UW is used to describe P_{UW}, the UW algorithm, and the UW hotplate. A different designation/abbreviation should be used to differentiate between sensor and algorithm. For example, P_{UW} could easily be mistaken for *P* from the UW hotplate, rather than *P* from the UW algorithm. One solution would be to rename the UW hotplate.

Ln 374 and Fig. 4. R11 should be included in the drip test, and added to Fig. 4 and Fig. 5. Or the omission should be justified in the text.

Ln 382 – 384 and Fig. 4. Augment the figure with cross-hatching or something similar to better illustrate the 1 min averaging periods. Also clarify that these 1-min periods were used for the regressions in Fig. 5, assuming that is what was done.

Fig. 5 and Ln 385 – 390. More detail is needed on how these values were obtained (see above). Also why were 0 mm hr^{-1} precipitation periods excluded? An evaluation of the total accumulation should also be included. It is hard to tell from Fig. 4, but it seems possible that the 'overestimated' YES algorithm might be just as accurate as the UW algorithm after including the 0 precipitation periods and the period after the nondrip-to-drip transition. Also it isn't clear to me what role the minimum threshold plays here. In normal operations, I thought that a 0.2 mm hr^{-1} threshold was used to differentiate between noise and precipitation, but Fig. 4 only seems to include a 0 mm hr^{-1} threshold (to remove negative precipitation), and it is only for the YES sensor. Both the UW and the R11 algorithms include a threshold if I recall correctly. In normal operations how would the zero precipitation periods be handled for both algorithms? If I recall correctly, the YES sensors in SPICE had very few false-positives. The same methods recommended for normal field use should be employed in the evaluation, or at an explanation of why the thresholds weren't used should be added to the manuscript. The evaluation of the total accumulation should be performed with the thresholds applied, although it could certainly also be performed without the thresholds to help demonstrate the potential effects of the threshold in normal field use.

Ln 404. Add an explanation of how events were defined. For example, more than x amount of precipitation, over x amount of time, beginning and ending with x minutes of zero precipitation... Also state whether the NOAH or the hotplate precipitation gauge was used to determine events.

Ln 414. Add "an" as follows: "and *an* upper-limit temperature…".

Ln 427 and 428. "R_e extends smaller" and "there is an order of magnitude narrower R_e range" are awkward as written.

Ln 441-442. Try to find a different term for “upwelling longwave”. For many readers, the term “upwelling longwave radiation” already has a specific use that differs from the longwave radiation leaving the surface of the top hotplate. And throughout the manuscript the correct usage is “longwave radiation”, not “longwave”. The same rule applies to the use of “shortwave”.

Ln 446. Rewrite as, “The first step in the calculation is *the* conversion of the latent *energy* term…”.

Ln 447. Change “latent term” to “latent energy term” or “latent heat term”.

Ln 448. One of the Methods Sections might be a more appropriate location than this Section, but a detailed explanation of this element-by-element vector multiplication should be added to the manuscript, including why it is necessary.

Ln 473 – 475. Explore the effects and uncertainty of the field-based calibration coefficients. How sensitive is precipitation to these? What happens if you swap them from site-to-site? Based only on their variability from site-to-site, there appears to be a significant amount of uncertainty in these terms. Calculate the effects of this uncertainty on precipitation.

Ln 483. “…ratios significantly smaller than unity” is awkward as written.

Ln 484. “…obtained when zeroing the shortwave term…” is awkward as written.

Ln 489. Add “the” to “values of *the* UW algorithm”.

Ln 490. Change, “are detecting” to “detected”.

Ln 498. Add “catch” to “event-averaged *catch* efficiency”.

Ln 506. Change “*Es*” to “values of *E*” – *Es* could be mistaken as a separate term, rather than the plural form of *E*.

Ln 508. Delete “statistically” used at both the beginning and the end of this line.

Ln 521. Specify that the new radiation terms were only for the top plate.

Ln 524. Delete “have” in “we have used”.

Ln. 565. Specify which component “Component of longwave flux” refers to. Based on Ln 118, it looks like it is the entire longwave flux, rather than a component.

Ln 596. Add *hp* (hotplate) to the list of subscripts.

Figure 3. Use consistent terminology. Change “New Algorithm” to “UW algorithm”. Also in the caption explain that the Fig. 3b wind speeds were adjusted to account for the different heights.

References:

Buisán, S. T., Earle, M. E., Collado, J. L., Kochendorfer, J., Alastrué, J., Wolff, M., Smith, C. D., and López-Moreno, J. I.: Assessment of snowfall accumulation underestimation by tipping bucket gauges in the Spanish operational network, Atmos. Meas. Tech., 10, 1079-1091, 10.5194/amt-10-1079-2017, 2017.

Fortin, V., Therrien, C., and Anctil, F.: Correcting wind-induced bias in solid precipitation measurements in case of limited and uncertain data, Hydrological Processes, 22, 3393-3402, 10.1002/hyp.6959, 2008.

Kochendorfer, J., Nitu, R., Wolff, M., Mekis, E., Rasmussen, R., Baker, B., Earle, M. E., Reverdin, A., Wong, K., Smith, C. D., Yang, D., Roulet, Y. A., Meyers, T., Buisan, S., Isaksen, K., Brækkan, R., Landolt, S., and Jachcik, A.: Testing and development of transfer functions for weighing precipitation gauges in WMO-SPICE, Hydrol. Earth Syst. Sci. Discuss., 2017, 1-25, 10.5194/hess-2017-228, 2017a.

Kochendorfer, J., Rasmussen, R., Wolff, M., Baker, B., Hall, M. E., Meyers, T., Landolt, S., Jachcik, A., Isaksen, K., Brækkan, R., and Leeper, R.: The quantification and correction of wind-induced precipitation measurement errors, Hydrol. Earth Syst. Sci., 21, 1973-1989, 10.5194/hess-21-1973-2017, 2017b.

Wolff, M. A., Isaksen, K., Petersen-Overleir, A., Odemark, K., Reitan, T., and Braekkan, R.: Derivation of a new continuous adjustment function for correcting wind-induced loss of solid precipitation: results of a Norwegian field study, Hydrology and Earth System Sciences, 19, 951-967, 10.5194/hess-19-951-2015, 2015.