Review of manuscript *JAS* 3700, "A latent heat retrieval and its effects on the intensity and structure change of Hurricane Guillermo (1997). Part I: The algorithm and observations", by S. R. Guimond, M.A. Bourassa and P.D. Reasor

Recommendation: major revision

Summary: A revised latent heat retrieval scheme originally developed by Roux (1985) and Roux and Ju (1990) is presented. Observations from NASA high altitude aircraft using EDOP and deploying GPS sondes, radar and liquid water content from the NOAA WP-3Ds in Hurricane Katrina, satellites (not detailed), and model runs of Hurricane Bonnie all contribute to the assembly of the scheme. Prior model runs and observations show that most of the upward mass flux occurs in weaker drafts that may not be saturated so a scheme is developed to determine this. The end result is an estimation of the vertical and horizontal distribution for the latent heating of condensation.

The unanticipated mix of items derived from observations in other TCs and the reliance on TC Bonnie simulations coupled with the non-linear organization of the paper makes reading a chore. If there was ever a paper that needed a detailed flow chart revealing each step on the journey to the desired product then this is it. The Guillermo information is scattered about (Figs. 2, 10, 13, 15, 16) which is a symptom of a manuscript that could be better organized. I think the paper would be far more digestible if they reorganized, focused solely on the method, and then applied the scheme to Guillermo in part II. This would also shorten the length to AMS desired limits (25-26 pages).

Major Points

(1) Title and direction of the paper: The title needs to reflect what this paper is really about. The work depends upon EDOP measurements of updrafts in eyewalls from other TCs, deep soundings from several TCs, model runs from Bonnie, vertical velocity data from many TCs, radar and LWC observations from Katrina besides observations from Guillermo. Wait till part II when you apply the new scheme to Guillermo to put the name into the title. Try something akin to: "A new latent heat retrieval scheme for hurricanes. Part I: Methodology".

(2) The abstract does not describe the paper fairly. All the rather unanticipated steps are glossed over. No one would know that they are going to see data from so many different sources. It mentions Doppler estimates of w and a model when there are several other ingredients that are crucial to the story. In fact it stresses the Doppler retrieval suggesting that we will see the w field which drives the latent heat pattern. Actually the w retrieval was done in Reasor et al. (2009) and the fields are simply applied here.

(3) How is the scheme supported by simply placing it in a model? One would think that the scheme might be used in lieu of whatever the model did (it needs latent heat release too) to see if the TC forms and behaves in a realistic fashion, but this was not done.

(4) The manuscript suffers from casually using data that are not well supported. Some of these steps require a leap of faith on the part of the reader. Examples are:

(a) Comparison of the LWC and dBZ. No discussion of LWC errors which are large. Volume that radar sees and what the PMS probes sample are different.

(b) Use of GPS sondes to discern eyewall thermodynamic structure. Here what the sonde fell through matters but is not discussed. Later satellite observations are mentioned but how they are used is not presented.

(5) What updrafts are saturated? I think horizontal scales matter a lot here, not just the magnitude of the updraft. If the updraft exists for 1 km length scale or more I'll wager that you are at saturation. For smaller length scales or corresponding periods the cooled mirrors, depending on how they were tuned by the flight director, may not have time to reach saturation. The overall result seems that the authors have selected an extremely high value of w (5 m/s) where they assume saturation. (Note that the entire updraft in Rotunno and Emanuel's model would be unsaturated if one made saturated versus unsaturated decisions based on w alone!) I'll bet that updrafts greater than 0.5 m/s for more than 5 seconds with at least one second reaching 2 m/s are probably saturated. What would be the impact if you assumed a lower threshold than 5 m/s? Did Eastin et al. (2005) use the FSSP or J-W sensors?

(6) The scheme neglects the latent heat of fusion (end of page 17). Ice processes, however, have been shown to have an impact on TC structure (Lord et al .1984). I hope that this scheme is compared against one with ice to demonstrate the efficacy of the proposed scheme.

(7) The tail radar from the WP-3D is used to estimate the precipitation field. What range from the radar was accepted, what choices are made about what part of the field is attenuated? This discussion might be better in part II. Should at least mention what Reasor et al. (2009) did because it is so important.

(8) Large uncertainty in the heating due to weak updrafts (156%) coupled with the observation that the majority of the upward mass flux is in the weak updrafts suggests that getting the weak updrafts right is very important. This uncertainty gets glossed over in the conclusions. Recommend that you discuss this more near the end of page 31. It seems that you are suggesting that if one gets the 5 m/s updrafts right then the latent heat estimate will be ok.

(9) All the EDOP material – really not relevant to what is going to happen in Guillermo is it? Fig. 1 and 14 seem like extras that are not crucial to your story.

Minor points: page on pdf file and line from top or bottom (-) given for reference

3, 12: of the structure …to… of its' structure

3, 15: to the structural characteristics …to… to its' structural characteristics

4, 9: a higher resolution than what? (other older satellite studies, not the radar derived work)

4, -1: 4.3 km on a side? We knew that that is too coarse to resolve convection based on the in-situ measurements by Jorgensen et al. (1985), Black et al. (1996), and Lucas to name a few.

6, 10: I would say that a comprehensive retrieval would show the vertical velocity fields and how they were estimated but this has been done by Reasor et al. (2009). I would vote for a different goal….how about the application of a new latent heat scheme…later to be applied to the dBZ and w fields of Guillermo.

7, 1: the first surprise, what is the ER-2 doing here? Did the ER-2 fly in Guillermo, no…..

7, 10: The long-track…(this sentence needs a rewrite) 100 m is at 20 km altitude, .300 m at 10 km altitude and 550 m at the surface?

7, -4: seems odd that you need a cardinal heading…why?

7, -2: use tail (TA) for the uninitiated the first time.

8, 12: the authors fail to provide some info here: How far away from the aircraft will they accept data? Attenuation through an eyewall has what impact? Aircraft level in Guillermo is?

9, 1: why the top 38%? (I could guess but I would rather you tell the readers)

10, 2: oddly here are some of the details about the WP-3D radar (about two pages later from where one expected to see it). Are you going to accept data 60 km from the radar…this strikes me as extreme, prior studies used 20 to 30 km. The F/AST technique does not resolve updrafts that are 4-5 km very well. If the updrafts are this scale what % of the w signal can you expect to resolve? Wouldn't this error be your biggest issue?

11, -5: …the dynamically consistent nature of the model budgets (aren't they all consistent?) allows for the assessment…what would really demonstrate if the new scheme was an improvement was if the run with this scheme simulated the TC better, all other subroutines in the model being kept the same.

11, -4: the Gao material has no link to the determination of saturation does it? If not then you can trim this, it is an aside

12, 3: stating a Met 101 class point here – try a reword if you feel you must remind us about phase changes

12, 7: no references are required for such a general statement. People recognized latent heat releases' impact long before Scott and Matt were born. It reads as if these two gentlemen discovered this.

12, -8: the WP-3D has trouble identifying small clouds with the mirror dew point sensors; I would expect it not to be very discerning for times less than 5 seconds depending on who set the response time of the sensor. If a weak updraft exists for more than a few hundred m in the horizontal I'll bet it is shortly to become saturated, smaller turbulent eddies may not be. Did Matt count updrafts of a certain minimum time-scale? If he simply counted drafts of even a few seconds then many of these features could be saturated.

12,-3: are these unsaturated updrafts continuous in z for more than 1 km or so? Hope they aren't, otherwise the model has some strange structures.

13, -4: should at least briefly explain what negative mixing ratios are

13, -2: awfully big convective-scale…more like meso-gamma and bigger than all but the top1-2% of updrafts

14, 6: nice that the terms can be combined, but in the end you can't do it…so why mention as it becomes an aside

15, -4: when could the model be saturated without a positive Q_{net}?

16, 8: what about the latent heat release for regions where there are no precipitation-sized particles? Are you arguing that where precipitation-sized particles are the only place where there is saturation? What about where there is rain but it is subsaturated (below cloud base)?

16, 11: what is the maximum range that you have accepted data from the aircraft? What have you done about obvious attenuation situations?

Fig. 6: the tail radar detects precipitation and the wind field - then you solve for the equation (here don't you have an issue at cloud edge where q_p is zero so all entrainment would make the second term on the right negative?) also how does the radar provide the storage term given that you sample a volume only once? Then Q_{net} is (+) or (-) ….volume is considered saturated and latent heat release determined chiefly by w and rate of change of q_s with height. Well, the top of page 18 really doesn't describe the flow chart. Later on p. 18 you discuss the fact that the storage term period (34 min) is so long as to not affect the precipitation budget. You aren't applying a parameterization scheme for a model time to the observation period are you?

18, -5: now you celebrate the importance of the storage term for shorter periods for a model – what has that got to do with the Guillermo obs?

19, 6: you seemed surprised that Q_{net} would be a large term…why?

19, 9: u is total wind is it not?

19, 11: an explanation of morphing would help here (a sentence or so)

19, -5: got references for the radar studies?

20, 2: what approximations?

20, 6: does the inclusion of the storage term reduce Q_{net} ?

20, 10: a nice reduction for the short time scale of the model…but for Guillermo won't it be far less?

21, 6: when would something be quantitatively significant and not physically significant?

22, 6:…data are averaged….

22, 8: are you using the tail radar? How do you compare the in-situ volume with the tail given that you are not sampling the same volumes at any given time? Aren't side lobes very close to the aircraft an issue?

23, 6: for a 2 km model grid just how much of the volume has 5 m/s or greater?

23, 9: wow – this argument about a dropsonde being representative of the eyewall is a reach. Did it fall in the updraft, downdraft, or some of both? Where did the sonde go with respect to a reflectivity maximum?

24, 6: calling any of passes 2 through 5 as symmetric is a stretch. Latent heating remains insignificant to the NW and W through out almost all the frames of Fig. 2.

24, 10: during the intensification period the eyewall also decreased.

25, 2: but you already did the saturation state error analysis. Might want to rethink how you organize this paper.

25, -3: people have long known, simply by comparing the horizontal resolution for the tail Doppler to in-situ estimates of drafts, that the smaller scale drafts are not well represented by the radar. See the in-situ measurements by Jorgensen, Zipser and LeMone and compare to the eyewall or rainband structures reported in the literature with the Doppler for eyewalls, rainbands, and convective cells.

28, 1: calling the Guillermo dataset sampling uncertainty is a little misleading. We know it is one storm but sampling uncertainty it is not.

30, 4: again, show me a situation where there are strong w's well above the boundary layer (away from mountains) and I'll bet there is buoyancy. You state the obvious.

30, 12: this is Eastin et al. result, not this paper. You have not conducted the analysis so rephrase to give Matt and company the finding….you are going to apply it.

30, 15: I think everyone knows you need saturation to have latent heat release

30, -7: what else would explain the precipitation production?

31, 4: this is for the model or Guillermo?

31, 5: not sure that it is a consequence of the DIV theorem…..

31, 11: prior studies that estimate LE release through precipitation have their errors dominated by the estimate of the precipitation field itself. In your case your main source of error is controlled by the w field and when to assume saturation. You may want to tell the reader the explicit gains you have made – showing heat release as a function of z.

31, -4: how does the EDOP analysis help with Guillermo estimates of w?

32, 2: It seems that you will apply this scheme to a model to see if the new scheme does a better job with Guillermo's RI. You'll initialize with Guillermo's w field. Now will you use the model's regular scheme as well as the new one to see if you get a different and better result?

Fig.1: I suspect that the EDOP estimates have some errors in it. A 5 m/s updraft at about 400 m altitude would demand a convergence approaching 50 m/s over about 4 km width….-1.25 x 10^{-2} s^{-1}; seems quite unrealistic given that there is virtually no buoyancy at this level.

Fig. 2: Cells to the west never achieve the same heights or rain rates as cells to the east and north of the circulation center.

Fig. 5. is this for a cell that is mature? (Surprised by the production of rain below 2 km)

Fig. 13. A pretty fig. that conveys little quantitative information. The reader won't be able to discern the height where the LE release is, but we already know it is largely controlled by dq_s/dz and w, therefore it will be a maximum in the lower troposphere. We already know from prior work (Eastin et al, Reasor et al., Sitkowski and Barnes) that the west side of Guillermo is inactive.

Fig. 14. an aside that is not relevant to Guillermo. You don't need to advertise other projects.