

Best Track Committee Re-Analysis Comments for 1947
Replies by Hagen and Landsea in boldface – October 2013

General comments:

1. Could you please provide binder maps for the suspect cases?

We will provide the binder maps for the systems that have gale force winds, as well as ones that the committee has questions/comments on.

2. Some of the 1947 cyclones are close enough to the land-based upper-air data could be useful in some cases. For example, the upper-air data in the Historical Weather Map series may help with diagnosing the nature of storm #7.

Good suggestion. We will refer to these when there is uncertainty as to the cyclone's status and where there is data to make it feasible.

3. It is not always clear from either the summaries or the aircraft data on the microfilm maps whether the planes were reporting central pressures. Please clarify this as best as possible and make sure it is as clear as possible in the metadata.

At times, it is unclear with the information provided in the sources whether a central pressure or peripheral pressure was being measured. If it is unclear, this will be so stated in the metadata writeup.

1947 Storm #1:

1. The committee concurs with the proposed changes in the genesis timing and the initial intensity. The metadata summary might want to acknowledge the possibility that a closed low pressure area formed before the system reached the Yucatan peninsula.

Agreed and so noted.

2. In the metadata summary, should "NNW as in HURDAT" actually be "NW as in HURDAT"? The re-analyzed best track appears to show a northwestward motion.

Agreed.

3. Given the lack of data near the center, please better highlight the uncertainty about the cyclone in the metadata summary.

It is now noted that there was not a substantial amount of observations near the center at landfall, so if the cyclone actually tracked equatorward of this track, the cyclone may have been substantially stronger.

1947 Storm #2:

1. Is the data for the first three days of the cyclone's life sufficient to justify the proposed downward revision in intensity? The committee concurs with this, but it would like to be more certain that the data is conclusive.

On the 9th and 10th, the strongest observed wind within 200 NM of the analyzed center was 15 kts. There were at least 5 observations within 200 NM of the analyzed center on those two days, with one of them occurring within 100 NM of the analyzed center. At 12Z on 8/9, a ship located approximately 85 NM west of the analyzed position reported 15 kt NE with 1011 mb. At the same time, a land observation from the north coast of South America about 150 NM SE of the analyzed position reported 15 kt SW with 1013 mb. On the 11th by 18Z, the analyzed position is about 50 NM away from Swan Island. Swan Island, which had reported 15 kt NNE with 1012 mb earlier when it was 120 NM away from the island, now reported 10 kt NE with 1012 mb when it was 50 NM away from the island. A couple of sentences have been added to the metadata summary summarizing the above.

2. Please re-examine the central pressures on 14 August, especially concerning the ship report of 972 mb with hurricane force winds, the 977 mb reconnaissance pressure, and the choice of 977 mb to include into HURDAT. Why were the two estimated and measured central pressure values averaged together? Why was the 977 mb chosen for HURDAT instead of the average 971 value? Which of the reports is most likely to be correct, and is there any way to quality control the reports?

We know that a ship on the morning of the 14th measured a pressure of 972 mb with winds of 65-70 kts. We also know that recon measured a central pressure of 977 mb a little bit later, at 1717Z. Due to the uncertainty in what the value of the central pressure was at the time of the ship observation, we did not place a central pressure reading into the 12Z time slot. However, the reanalysis of wind speed was conducted with the assumption that the central pressure at the time was less than 972 mb. Since a central pressure of less than 972 mb yields a wind speed of greater than 88 kt according to the Brown et. al southern pressure-wind relationship, and since the original HURDAT intensity is 90 kt, no change is made to the original HURDAT intensity. The central pressure rose to 977 mb at 1717Z. A central pressure of 977 mb was added to HURDAT at 18Z. We elected to hold 90 kt at the 18Z time slot on the 14th due to Tampico observations on the 15th and our analyzed landfall intensity on the 15th.

3. The committee does **not** concur with the proposed upgrade in peak intensity to 100 kt. This appears to be based on the Monthly Weather Review (MWR) stating that the peak winds in Tampico were 96 kt, which the re-analysis proposes to use as sustained winds. However...

a. A time series of the Tampico data in the binder, while incomplete, shows only 70 kt sustained winds with gusts to 87 kt. This suggests that, at best, the 96 kt was a gust in-between these observations.

The time series of Tampico data shows 74 kt sustained winds with gusts to 87 kts. Agreed to lower intensity back to original HURDAT value of 95 kts at 06Z on the 15th and at landfall.

b. The lowest listed pressure of 1005 mb and generally slow changes in the pressure do not suggest that Tampico was close to the radius of maximum wind (RMW) of a major hurricane.

Agreed. The location of landfall is adjusted 0.1 to 0.2 degrees latitude farther south. Given the sustained winds of 74 kt at Tampico, Tampico was likely just a little bit outside the RMW. We now show the hurricane passing about 40 nmi south of Tampico instead of about 25 nmi south of Tampico.

Please re-examine the intensity at final landfall in Mexico in light of these points. Is it possible given the relatively high pressure in Tampico that the system hit farther south – possibly about midway between Tampico and Tuxpan?

It is possible that the landfall occurred slightly farther south than the proposed landfall position. We have made a very slight additional southward adjustment (0.1-0.2 degrees) and we have lowered the wind speed at landfall from 100 kt to 95 kt.

4. Please contact the Meteorological Service of Mexico for any information they have on this system, including the landfall location and whatever additional surface obs may be available. If there is sufficient evidence to justify making this a major hurricane, it would be desirable to get their approval for such a change.

On the Mexican observations on the EDADS website, there are no observations from Tampico or Tuxpan during August, 1947. On the Mexican observations on the NCDC website (http://docs.lib.noaa.gov/rescue/data_rescue_mexico.html) there is no Mexican data for 1947. We contacted the Mexican Meteorological Service, but they have no further information available.

5. Once the landfall intensities are finalized, please also re-examine the intensities as the cyclone decays over Mexico. The committee has concerns about the current proposal of higher intensities than those of the inland decay model.

Done. The Kaplan and DeMaria Inland Decay Model was re-run based on the new landfall intensity and new positions following landfall. Based on this and the revised analysis, we have lowered the intensities by 5 kts from our previous analysis at 12Z and 18Z on the 15th to 85 kt and 65 kt, respectively. While these revised intensities are still 8 to 10 kt higher than the Kaplan and DeMaria Model at those times, the sustained wind from Tampico of 74 kt at 1635Z suggests an intensity of 75 kt at 1635Z. Thus, we cannot really go lower than 85 kt at 12Z and 65 kt at 18Z.

6. The metadata summary should note that the east-southeast winds in Tuxpan at 0030 UTC 16 August supports a more southerly track after landfall.

A sentence has been added to the metadata summary with this information.

7. The caption on the track map reads “AUG 5 – AUG 16” instead of the correct “AUG 9 – AUG 16”. Also, Tuxpan is spelled “Tuxapan” in the 15 August metadata.

The spelling of Tuxpan and the caption have been fixed.

1947 Storm #3:

1. The committee concurs that this was one tropical cyclone instead of the two closely-spaced cyclones mentioned in the MWR.

Agreed.

2. The committee concurs with the earlier genesis on 18 August.

Agreed.

3. Please re-examine the track near 0000 UTC 19 August. The wind in Havana, Cuba shifts from west at 2130 UTC 18 August to east 20 mph at 0025 UTC 19 August, which is not consistent with the proposed position closer to the Florida Keys. Also, the microfilm map for 0625 UTC 19 August shows a southeast wind in Havana and a low centered near western Cuba. Is it possible this system was farther south than proposed during its early life – near the north coast of Cuba? Please contact Perez for more information on this, including a times series of data from Havana if available.

Agreed. The track has been revised to indicate formation just north of Cuba at 18Z on the 18th, followed by a track with landfall in Cuba as a tropical depression just east of Havana, then back over water after 06Z on the 19th. Perez has been contacted with regards to these changes.

3. What was the basis for the increased intensity from 1200 UTC 20 August to 0000 UTC 21 August?

It is agreed to bring the increase down slightly. Two ships reported 35 kt at 14Z and 18Z on the 20th, which would suggest an intensity somewhat above these observed values. The intensity is now analyzed to be 40 kt at 12Z and 18Z on the 20th.

4. On the microfilm map for 1825 UTC 20 August, there is an observation of south-southeast winds to the west of the proposed center position. Is this an aircraft observation, and what is its significance for the center location?

This observation is from a ship. However, a storm center west of that ship (as implied by the wind direction) is not consistent with the multiple ship and aircraft observations available at 18Z on the 20th. Thus this single observation is discounted.

5. During the time that the center reforms to the north on 21 August, could it help smooth the best track to use a mean center position between the two observed circulation/vorticity centers?

Agreed to use a mean center position (26.8N 88.0W) at 12Z on the 21st to provide a smoother best track on this date.

6. On the microfilm maps, there is an aircraft center report at 1645 UTC 21 August near 28.4N 88.8W with a pressure of 1004 mb and 40 kt winds. This observation is not mentioned in either the daily metadata or the metadata summary. Was it used in the re-analysis?

Yes, this observation was crucial in placing the revised center to 28.3N 89.2W (26.0N 89.8W originally in HURDAT) at 00Z. This is now explicitly mentioned in the daily and metadata summary. (Note that the report is 1645 local – EST – which is 2145Z.)

7. Please re-examine both the track and intensity near the coast of Louisiana. On the track side, the microfilm map for 1830 UTC 22 August shows a northwest wind from a station in southern Louisiana, which suggests the center may have gone farther inland than currently proposed. On the intensity side, the lack of low pressures over Louisiana is not consistent with the 1004 mb recon ob from point 6, or with the equal or lower pressure noted by 1500-1800 UTC 23 August. Is there a possibility that the system decayed before the Louisiana landfall and then regenerated? Is it possible a smaller center passed between the Louisiana stations? Please resolve these issues, including finding the data record from station GPF, which appears to be in the Morgan City/Patterson area.

It is agreed to move the 18Z position to the northwest, slightly more inland than first proposed. Any farther northwestward would cause too much of an abrupt change to the track on the 23rd. It does not appear likely with the surface reports showing a closed circulation at 18Z as well as 37 kt reported at Grand Isle that the system decayed before landfall. Instead, it would appear to be more likely that the system had a rather small inner core which passed between the Louisiana stations. This is now discussed in the metadata writeup. There does not appear to be any documentation (much less any additional data) from the GPF station in the EV2 website. A request for information has been sent to Frank Revitte of the WFO New Orleans office to see if he has more information.

8. The various wind reports from the Galveston area are confusing. The metadata summary says that Galveston had a maximum 1-min wind of 63 kt. However, the binder data table says this was 58 kt after conversion. In addition, the Galveston Original Monthly Record (OMR) states that the station had a 5-minute wind of 66 mph, and a fastest mile wind of 72 mph. Please re-write the appropriate metadata to clarify the Galveston winds, including any conversion factors used.

The winds were indeed a fastest mile wind of 72 mph (63 kt), which is also a 1-minute wind. The anemometer height of 35 m is the reason for the adjusted value of 58 kt. This is now so clarified in the metadata writeup.

9. Is it known how Jarrell et al. arrived at the 992 mb landfall pressure? Have local sources in Texas (like Lew Fincher) been contacted for local accounts of this system?

For Gulf of Mexico landfalling hurricanes during this era, Jarrell et al. nearly always took the Connor (1956) assessment verbatim. This was the case here, as Connor also indicated a minimum central pressure (at landfall) of 992 mb. Lew Fincher has been contacted, but had no additional observations available.

10. Is it known how far the 36 kt ob in Houston was from the RMW? Please check this and if necessary revise the after-landfall intensities based on the information. Is there a text write-up of this storm in the Houston OMR? If so, it is on one of the pages missing from the copy in the binder.

At the time of the peak Houston wind of 36 kt, the cyclone was about 20 nm to the southwest. The system was quite tiny and while an exact value is not known, the RMW was on the order of 10 nm. Thus an intensity of 50 kt at 06Z on the 25th would be consistent with Houston receiving this peak wind outside of the RMW. There was no writeup of the storm in the Houston OMR. If there was, it would have been included (we just didn't want to waste paper printing irrelevant pages).

11. In the metadata discussion, lines 16-18, please reword the sentence to "The Monthly Weather Review assessment was that a second tropical cyclone made landfall in Louisiana, with the original system continuing very slowly over the central Gulf of Mexico."

Done.

1947 Storm #4:

1. Has the submission on this system been revised to match the information in the Hagen addendum?

Now this has been done, but some of it has been changed slightly based on Best Track committee comments below and further investigation.

2. Please tone down the statement in the metadata summary that starts "Given this rather strong evidence...". While the data is pretty conclusive that the system was not a hurricane at the time of the first recon flight, there is little to no evidence about what it was before then. The committee is wondering why this system was given such a high intensity in the eastern Atlantic? Was it mainly due to its intensity later in its life, or is there data on it that hasn't come to light?

That sentence has been toned down. We are not sure why the system was given such a high intensity in the eastern Atlantic.

3. A reconnaissance aircraft on 13 September reported a “unique center” – concentric eyewalls with diameters of 6 and 30 n mi at the time the central pressure was 952 mb. Please review the intensity near this time to take this into account. A historical question: Is this the earliest known case of concentric eyewalls in an Atlantic tropical cyclone?

Based on this aircraft report, the intensity at 18 UTC on 13 September is adjusted down to 110 kt from 115 kt. The intensities are interpolated between 9/12 12Z and 9/13 18Z. Thus, the intensities are decreased by 5 kt from 9/12 18Z – 9/13 06Z. Also, the intensity at 9/14 00Z is decreased by 5 kt due to a slight change in interpolation between data points. A note has been added to the metadata about the historical significance of this perhaps earliest known case of concentric eyewalls.

4. Has the Meteorological Service of the Bahamas been contacted for data on this hurricane? First, it is unclear whether the minimum pressure at Hopetown was a central pressure, or even the extreme low pressure measured by the station (some of the binder sources implied the station was shut down as the core of the hurricane arrived). Second, is there any data available from Freeport or other stations in the Bahamas? Give that the proposed best track significantly reduces the intensity over the Bahamas, they should have some input on this.

Several newspaper articles from the Bahamas from September, 1947 have been obtained from Wayne Neely in the Bahamas. In Hopetown, 16/1930 UTC, “the wind dropped suddenly from 160 mph to 60 mph. Lowest pressure 28.37” [960 mb].” The “station is exposed on a hilltop.”

Hopetown obs: 26.54N/76.96W

16/1430 UTC: 83 kt NW with 983 mb (USWB/NHC Microfilm)

16/1630 UTC: 160 mph (139 kt) NNW (Newspaper and other sources)

16/1830 UTC: 954 mb (min p) (USWB/NHC Microfilm)

16/1930 UTC: 60 mph (52 kt) with 960 mb (min p) (inside RMW) (Newspaper)

In addition, at 2045 UTC, recon had a center fix at 26.5N/77.0W and observed 956 mb min p and 100 kt max winds. At the very least, Hopetown experienced the inside of the RMW and winds decreased significantly during the lull. It is interpreted that they had 960 mb with 60 mph simultaneous, which means the central pressure was likely near 954 mb as the center passed perhaps 5 miles north of the station. Just a couple hours later, the 956 mb measured by this plane likely was from a penetration and occurred in the eye. The 160 mph reading was from an anemometer exposed on a hill top. It is unknown whether or not it was a gust or what type of anemometer it was or if it had any problems. The 954 mb central pressure and 105 kt intensity introduced at 18Z on the 16th are retained.

5. Please re-examine the Florida landfall intensity and location, as there are numerous issues that need to be resolved:

a. The Hagen Addendum implies that detailed observations are available from Hillsboro Light, which the committee has not seen since they aren't in the binder. Please provide these. It should be noted that the Addendum is inclined to disregard the 947 mb pressure measurement at Hillsboro Light without giving a good reason for doing so. What was the rationale for believing this pressure was too low?

The detailed observations from Hillsboro Light have now been provided and have been placed in the binder. It is agreed that the 947 mb pressure measurement there is accurate. The following are the hourly averaged winds (43 meters):

10 am – 11 am: 100 mph

11 am – noon: 75 mph (lull) (min pressure 947.2 mb at 11:25 am – 1625Z)

noon – 1pm: 102 mph

Since the max 5-min wind for the hour between 11 am and noon occurred at 11:52 am (94 mph), then it is possible that the winds dropped below hurricane force for a very short period of time, which includes the time of the minimum pressure. 943 mb analyzed as the landfall central pressure. There were additional numerous observations on the mainland in the Fort Lauderdale area of calm with a pressure in the 950s around the 1800 UTC time. Therefore, the hurricane likely filled between 1630 UTC and 1800 UTC as it moved slowly over eastern Broward County.

b. The committee also needs to see whatever observations are available from Fort Lauderdale. It should be noted that the data table in the September 1947 Florida Climatological Data did not show a pressure for Fort Lauderdale. The 956 mb pressure first appeared in the MWR hurricane season summary. Perhaps the 956 mb pressure is the one that's not correct?

Additional observations from Fort Lauderdale obtained primarily by Daniel Gladstein have now been provided.

c. The eye size/RMW issue is also confusing. The MWR states that Fort Lauderdale was in the eye for an hour, while the Climate Data says it was there for 75 minutes. The Hagen Addendum implies that the RMW was near the Hillsboro Light, which is 10-11 n mi from the current location of the Fort Lauderdale airport. This would be consistent with accounts in the binder of aircraft reporting an eye about 22 n mi across. However, the hurricane was moving 7-8 kt, and the duration of the lull in Fort Lauderdale is not consistent with such an eye crossing directly over the station at that speed. Either way, the proposed RMWs of 20-27 n mi are probably too big even if the eye is about 22 n mi across, unless there is evidence of an outer wind maximum. Are there local newspaper accounts that would help determine where the calm was experienced? It is noted that we have a newspaper front page on this storm hanging in our front hall!

All of this data has now been reanalyzed in detail. It has been concluded that a landfall RMW of 20 nmi is the most appropriate value. The data from Hillsboro Light indicates that Hillsboro Light was inside the RMW.

d. Why is the 135 kt 1-min wind at Hillsboro likely to be in error? The argument about “noisiness of the instrument” has not been raised before, so it appears strange to do so here. However, what’s missing from the analysis is the anemometer height at that station, and how this measurement would reduce to sea level.

There is no good reason to believe that the Hillsboro Light wind data is in error. To convert the 155 mph fastest mile (24-sec averaged) wind to a 1-minute value, one divides by 1.06, which gives a 1-min value of 146 mph. Dividing by 1.15, this is 127 kt (1-min). The height of the anemometer was 43m, so we multiply by 0.895 to convert to 10m. $127 \text{ kt} * 0.895 = 114 \text{ kt}$. Therefore, after converting the 155 mph fastest mile wind at 43m to 1-min and 10m, the max wind at Hillsboro Light was 114 kt. If one converts the max 5-min/43m wind to 1-min/10m, it gives 100 kt, which is a substantially different reading than the 114 kt.

Note: The conversion table that used in the reanalysis of 1944-1953 for converting winds of various averaging times to 1-min values is attached.

Basically, this part of the analysis needs to be re-done from scratch to fit these conflicting pieces of the puzzle together.

Utilizing the above information, a landfall central pressure of 943 mb equals 118 and 112 kt, respectively, according to the southern and north of 25N Brown et al. pressure-wind relationships. The RMW of 20 nmi was close to the climatological value of 16 nmi. 115 kts is chosen for the landfall intensity, the same as the Landsea et al. (2008) first reanalysis analysis for wind speed, but 3 mb higher than their analysis for landfall central pressure. This makes sense, given that a smaller RMW is being analyzed than before.

6. In the metadata summary, please change oceanfall to something better.

Done.

7. Where are the Historical Weather Maps (HWM) and microfilm map analyses for the time the system was over the Gulf of Mexico?

The HWMs for this system while over the Gulf of Mexico have been added. No microfilm for those days exists. This may be because the Miami WBO lost electricity in the hurricane and they may have lost the ability to print the microfilm maps.

8. An advisory quoted in the report on this hurricane in the Mississippi Climatological Data states that the hurricane was located by aircraft over the Gulf of Mexico. Please obtain the data from this and any other flight that occurred after the Florida landfall.

Unfortunately, because of the loss of the microfilm, this aircraft reconnaissance data is not available and additionally it is not archived at NCDC. It is of note that the original HURDAT did not have any central pressures included while the hurricane was over the Gulf, so the flight(s) that did occur may not have measured any central pressures.

9. Please also review the landfall intensity in Louisiana. While the minimum pressure at the New Orleans Moisant Airport (967 mb) was measured outside the eye, the eye passed over the station less than 2 hours later with a clear-cut central pressure of 969 mb. This casts doubt on the proposed landfall pressure in the Hagen Addendum, as well as on the Addendum's proposed central pressure when the eye passed near Baton Rouge. The eye also passed over the Belle Chasse Naval Air Station southeast of New Orleans (974 mb), St. Bernard Village east of New Orleans (967mb), and the New Orleans Weather Bureau Office (969 mb). These pressures should be noted in either the daily metadata or the metadata summary.

These pressure observations have now been noted in the metadata summary. The central pressures that were originally in HURDAT at 12Z and 18Z on the 19th are found to be reasonable, even with the additional data found. Therefore, no additional changes have been made to the central pressures at those times.

Given the wind reports in New Orleans and Baton Rouge, 90 or 95 kt might be the best landfall intensity in Louisiana. Also, please make sure that the wind reports from New Orleans and Baton Rouge are included in the revised metadata summary.

Agreed- chose 95 kt for landfall. The wind reports from New Orleans and Baton Rouge are now included in the metadata. In addition, the additional data and observations from the online publication entitled *The hurricane of September 19, 1947 in Mississippi and Louisiana* published in 1947 by the U.S. Weather Bureau has been included in the daily metadata paragraphs. The source is referenced as (S19M&L) in the September 19 and 20 metadata paragraphs.

10. Typos: In the 17 September daily metadata, change "Heave" to Heavy", and in the metadata for September 19, line 24, replace "apssed" with "passed". In the metadata discussion line 24, change "will" to "while".

Done.

1947 Storm #5:

1. While the committee concurs with the earlier genesis, it is concerned about the high central pressure implied by the 1015 mb ship report, as well as the subsequent lack of data near the center. Please express the uncertainty in the metadata summary.

Done.

2. Please add the 1015 mb 20 mph ship report at 0030 UTC 7 September to the ship log.

Done.

3. Were the track adjustments for this storm over the Gulf of Mexico based on the new (earlier) genesis location and a smoothing out of forward speed discontinuities in the original HURDAT? If so, please state in the metadata discussion.

Yes, both. So now stated.

4. Is there an OMR for Mobile, Alabama available? The committee would like to see how the winds and pressures evolved.

So provided. The data are underwhelming, but do show a distinct wind shift between 16 and 17Z consistent with the revised track.

5. The microfilm maps suggest there was a reconnaissance flight into this system. Several aircraft obs appear on the map at 1225 UTC 8 September, and on the next page in the binder (the same map apparently) there are two additional aircraft obs plotted over the Pacific. One of these shows a position of 30.3N 87.3W, a flight altitude of 1600 ft, a time of 0930E, and southerly winds of 55 mph (kt?). Please search for a more complete record of this flight and include the data in the metadata and the revised intensity.

These data, which were recorded on the microfilm in mph, are now explicitly included in the daily summary writeup. The solid barbs are estimated flight level winds (50 kt) and the dashed barbs are estimated surface winds (25 kt).

1947 Storm #6:

1. Please state the basis for the earlier genesis on 19 September. It appears to be based on a northwest wind at Kingston, Jamaica with little other supporting data. Is it possible this wind was a land breeze given the time of the observation? If a strong case cannot be made, please use the original HURDAT genesis time.

Agreed that the sparse, available observations are not conclusive about an earlier genesis. The original genesis from HURDAT is now retained.

2. Has any detailed data from Perez been incorporated into the revised best track?

Perez had no additional information available for this tropical storm in Cuba.

3. What is the basis for the consistent 0.5 degree right shift in the track from the original HURDAT?

The minor track changes were based upon aircraft reconnaissance, ships, and coastal stations.

4. On the 1830 UTC 23 September microfilm map, there is what looks like an aircraft observation west of the center with 55 mph (kt?) which is not mentioned in either the 23 September metadata or the metadata summary. Please discuss this ob and the significance it may have for the analyzed position and intensity.

The solid barbs are estimated flight level winds (55 mph in this case), though these are not considered as reliable as the somewhat more accurate surface wind estimates (45 mph in this case, which was included in the daily writeup). It has now been clarified that the 40 kt W wind is a surface estimate.

5. On the same map, there is what looks like a 50 mph southerly wind analyzed near Lakeland, Florida. Is this correct? If so, it may be worth referencing in the daily metadata.

This is an estimated flight level wind, which again has such large uncertainties that it is not being weighted heavily.

6. In line 5 of the metadata discussion, change “possibly” to “possibility”. In the last sentence of the metadata discussion, change “addition” to “additional”.

Done.

1947 Storm #7:

1. Please provide maps for the period 0000 UTC 4 October to 0600 UTC 5 October. The committee would like to better examine the evolution of the environment prior to genesis. On a related note, is a microfilm map available for 1800 UTC 5 October, the proposed genesis time?

These are obtained for the 5th, but are unavailable for the 4th. The 18Z October 5th microfilm map and data are consistent with a closed low forming at this time.

2. Please re-examine the nature of this system during its entire life cycle. While the surface temperature gradient near the center is never strong, the HWM does analyze a front as part of the system. In addition, the 500 mb chart in the HWM for 6 October shows a west-southwest wind at Miami and a 500 mb low over the western Florida Peninsula. On 7 October, the surface low is to the north of a large 500 mb cut-off low over the eastern Gulf of Mexico. The analyzed evolution is far closer to that of a baroclinic low than a tropical cyclone. Please provide a strong case for one of the following so the committee may vote on it: a) Keeping the system as a tropical cyclone, b) Calling it a subtropical cyclone instead, or c) Calling it a baroclinic low and removing it from HURDAT.

The 500 mb charts were obtained and do show involvement of an upper level trough in the developing of this cyclone. If satellite imagery were available, explicitly calling this a subtropical cyclone would be the preferred option. Currently in HURDAT2, the first subtropical cyclone is listed in 1968, which corresponds with the advent of routine satellite imagery. It is strongly recommended (as has been mentioned previously) not to use the subtropical designation before the late 1960s, because of this limitation and the need to have this to distinguish subtropical cyclones versus sheared tropical cyclones. It is reanalyzed that this low became a tropical cyclone by 00Z on the 7th with a 50 kt intensity (40 kt at this time originally). Evidence for it to have become a tropical cyclone were the

more symmetric structure of the pressures and winds and having the strongest winds near – 50 to 100 nm – the center. However, there did still exist a moderate temperature gradient across the center at 00Z on the 7th. It is now prominently mentioned that – if satellite imagery were available – that this system may be better described as a subtropical cyclone.

3. The committee notes there are similarities between this system and Tropical Storm Tammy in 2005. However, the source system for Tammy was a tropical wave, whereas the source for this system seems to be a frontal wave. That could be significant for determining if this system was a tropical cyclone.

It is now noted the similarity of this system with Tammy.

4. Please remove the various damage reports from the metadata. A notable offender is the MWR statement in the 6 October daily metadata that "No damage was reported". This is rather blatantly contradicted later in the same paragraph!

Done.

5. In the 6 October daily metadata, please change "mater" to "water".

Done.

1947 Storm #8 (new):

1. The committee does **not** concur with adding this system to HURDAT at this time. The HWM analyzes associated fronts for most of the life cycle, and the submission does a poor job at best of showing that the HWM analyses are incorrect. Please either make a much stronger case that this was not a frontal cyclone or move it to the list of suspect systems not included in HURDAT.

New analyses of this system have been done at a 12 hourly time step (there is not sufficient data for analyses at 06 and 18Z), as well as providing time series of two key ships #327 and #1061. This is now discussed at further length. These analyses and observations are consistent with the occurrence of a high latitude tropical cyclone.

1947 Storm #9 (was Storm #8)

1. Please remove all of the damage description for the metadata. They unnecessarily add to an already lengthy submission.

All of the rainfall and flooding descriptions have been removed. Some of the wind-caused and storm surge caused damage descriptions have been kept, as these have some bearing on the intensity.

2. The committee concurs with the proposed earlier genesis.

Agreed.

3. Please re-examine the track positions during the time that the center was tracked by the Boca Chica radar. In particular, is the position at 1800 UTC 11 October far enough to the west? A historical question: Is this the earliest known tracking of a hurricane from land-based radar?

The positions are moved slightly west and north, but still with the center passing just southeast of Dry Tortugas. This hurricane may very well be the first to be operationally monitored by land-based radar, which is now so noted in the writeup.

4. Please provide the appropriate metadata for the observations at Dry Tortugas. In particular, how elevated was the anemometer? Was the 73 kt wind at the time the instrument broke a sustained wind or a gust? Please re-examine both this observation and the estimated intensity based on it.

The anemometer was at an elevation of 48 m from the ground, which reduces the 73 kt 1 min wind to 65 kt at 10 m.

5. Please resolve the multiple highest winds reports for Key West in the 11 October daily metadata. In one place, it states 57 kt based on the Climatological Data. In another, it states 37 mph based on the Key West OMR.

There were two stations at Key West at the time: one "City Office" (at 24 degrees, 33 minutes N, 81 Degrees 48 minutes W) and one "Airport" (at 24 degrees, 35 minutes N, 81 degrees 42 minutes W). The Airport station (Boca Chica) was the one reporting the 57 kt peak 1 min. wind, while the City Office had the peak 37 mph (32 kt) 5 min. wind and a peak 40 mph (35 kt) 1 min wind. This is now so clarified.

6. As with storm #4 above, if detailed observations are available from Hillsboro Light, please provide them, along with the appropriate metadata. It seems very odd that no pressure is available during the eye passage over the station.

Observations at auxiliary stations such as this Light were done visually, not with a recording barometer. So if the Light tender did not look at the barometer, an observation was not available. It is odd that the tender did not observe the pressure in the eye. He must not have been a weather-weinee.

7. Please re-examine the pressures for the Florida landfall and while crossing the Florida Peninsula. What is known about the 982 mb pressure at Fort Lauderdale? What was the wind at the time of the measurement? Since it was reported by the Miami Herald, how reliable does it look? Even if no changes are eventually made to the proposed pressures, the explanation of why the pressures were chosen needs to be improved.

There is no additional information available about this 982 mb peripheral pressure reading. A run of the Ho et al. inland pressure decay model using 982 mb (and 1009 mb outer closed isobar) suggests at most 976 mb at landfall in southwest Florida seven hours earlier. Thus a rounded value of 975 mb central pressure at landfall is reanalyzed, which is

consistent with some deepening after the 983 mb central pressure observation by aircraft reconnaissance about nine hours before landfall.

8. There is a ~78 kt (mph?) observation shown in the binder log for 0730 UTC 12 October, with the source being the MWR. Where is this ob from? Hillsboro Light? Fort Lauderdale? If it is the latter, this suggests that more detailed obs are available for that station. Please clarify this.

This was a duplicate of the Hillsboro Light observation at the same time and has now been removed.

9. In the 13 October daily metadata, there is a passage from the Florida Climate Data about the center reforming while the storm is over the Atlantic. It appears that the data refutes that scenario. If it does, please explicitly state so in the appropriate part of the metadata summary.

It is agreed that the conclusion in the Florida Climatological Data of the dissipation of the original center and rapid development of a new center is not supported by the available observations. This is now so noted in the metadata summary.

10. In the 15 October daily metadata, is the reference to an “85 kt 1 min wind equivalent” from some pressure wind relations or equation? If so, please state this explicitly.

The estimated maximum 10 min wind at landfall in Schwerdt et al. is 142 km/hr. This converts to an 85 kt 1 min wind equivalent, assuming that the peak 1 min wind relative to a peak 10 min wind is about an 11% increase.

11. The committee concurs that the central pressure near landfall and several hours earlier was near 966 mb. However, are there any actual measurements near the best track times to justify including those pressures in HURDAT?

The 966 mb central pressure in the 00Z 15th slot was from a ship in the eye measuring 966 mb at 0230Z. The 966 mb central pressure at landfall was based upon the simultaneous 974 mb and 74 kt winds at Savannah. This latter value is removed from HURDAT, as it is agreed to be too uncertain for inclusion. A rounded value of 965 mb central pressure estimate is now used at landfall in Georgia.

12. At the Georgia landfall, the proposed revised intensity decreases from 90 to 55 kt in six hours. This appears to be a very fast decrease, and it is certainly a much quicker rate of decay than in the original HURDAT. What actual core observations support this? Are the numbers provided for the Kaplan-DeMaria decay model correct? Please re-examine this.

The Kaplan-DeMaria decay model values are correct, though the reader is now reminded that the landfall occurred at 11Z and the 90 kt values at 11Z are retained at 12Z, given that the location of the maximum winds may have still been at the coast one hour after the center made landfall. However, by 18Z this is a full seven hours after landfall and

substantial exponential decay is expected to occur. As is typical, there were few observations available near the cyclone's center after landfall and the few that were available were below the Kaplan-DeMaria model.

13. In the metadata discussion, second paragraph, line 14, replace “loop” with “turn” (since it wasn't really a loop) and on line 15, replace “further” with “farther”.

Done.

1947 Storm #10 (was Storm #9):

1. Please re-examine the adjustments made to the track when the center was near Bermuda. The proposed new track has the center about 60 n mi from the island as compared to the 40 miles mentioned by Tucker, and there is a question whether the observed winds and pressures are consistent with a center that far away. It is noted that a fix report for 20 October says “eye diameter estimated 15 miles”, which would suggest a need to keep the track closer to Bermuda to produce the observed impacts.

The track had been moved westward slightly late on the 20th to account for the aircraft reconnaissance fix position. It is agreed to retain the original HURDAT position at 12Z (near the time of closest approach to Bermuda).

2. Please provide the appropriate metadata for the 104 kt wind observation at Bermuda. It is noted that in the 20 October daily metadata this observation is given with a question mark after it. Is there some uncertainty regarding its source or validity? If so, please state it in the appropriate parts of the metadata.

The observation was obtained from the microfilm and, upon further inspection, it is likely that this was a gust (120 mph) with a sustained wind of 55 kt WSW. This is now clarified. Additionally, a report provided by the Bermuda Weather Service indicated that the strongest sustained (15 min) wind was 78 kt, equivalent to about 87 kt 1 min wind. This report also corroborated the minimum pressures observed at Bermuda at 990 mb.

3. Was the large size of this hurricane taken into account in estimating the intensities? The sentence in the metadata summary about the wind–pressure relationships for 20 October suggests it was not.

While the outer closed isobar was quite large, the hurricane maintained a rather tight inner core. The 15 nm diameter eye suggests an RMW of about 10-15 nm. Climatological RMW from a central pressure of 961 mb at 32N indicates an RMW of 24 nm. The hurricane was moving at about 15 kt. However, the outer closed isobar was a somewhat low 1008 mb. The combination of the above suggests going a bit higher than the pressure–wind relationships, which is what had been indicated.

4. Please re-examine the proposed revised intensities, the proposed positions, and the ship reports near 2200 UTC 21 October. First, do the multiple observations of 85 kt look correct – can the ship reports be quality controlled? Second, since the proposed intensity is 85 kt and the ships are 125-175 n mi east of the center, this suggests that either a) The RMW has become very large, b) The center is closer to those ships than currently proposed, or c) There are stronger winds closer to the center. Which is the most likely of these possibilities?

The reports – from the same ship an hour and 10 minutes apart – are the only observations from that ship, the J. Friske. It is likely that the RMW had gotten, but it also appears reasonable to move the hurricane closer to the ship and the positions at 18Z on the 21st through 06Z on the 22nd are adjusted accordingly.

1947 Additional Notes:

1. Suspect #2: Suspect 2: in the discussion, replace “abd” with “and”. Pressures of 1003 and 1005 mb, if these are 2 independent measurements, could warrant inclusion of a new storm into the HURDAT. However, these observations are not in the supplied documentation. Indeed, the binder table for suspect #2 seems to include a lot of observations from areas away from the system. Please fix this so the committee can see if this was actually a tropical storm.

Typo is fixed. The HWM maps for the 12th through the 14th are now provided. A possibly closed low (either tropical depression or trough) moved westward into Belize from the Caribbean Sea late on 12 June. On 13 June, the system was located along the southeast Mexico coast near the Bay of Campeche. There were no observed gales with this system, but of note are two land station based observations of 1003 mb and 1005 mb respectively at 12Z on the 13th. However, no strong winds were observed nor was there confirmation that the system had a closed circulation on either the 12th or 13th. By the 14th, the system either dissipated or perhaps was pulled quickly northward ahead of an advancing cold front as a weak low near 29N 87W. Without evidence of a closed circulation and tropical storm force winds on the 12th and 13th, this system is not considered a tropical storm and thus not added to HURDAT.

2. Suspect #8: The committee concurs with leaving this system out of HURDAT, but notes that the given positions don't match the HWM very well. Please check them.

The position on the 14th has been corrected.

3. Suspect #12: The committee concurs on leaving this system out of HURDAT and much appreciates the detailed resolution of this mystery system. Is there any evidence that convection was occurring near the center? If so, it might be worth noting that this could have been a hybrid system of a frontal nature.

Given the 2.13” 24-hour rainfall that occurred on the 27th-28th of November, the system had substantial convection. It is now noted that this system may have been a hybrid of a frontal nature.

4. The committee concurs with leaving the remainder of the suspect systems out of HURDAT.

Thank you.