

Best Track Committee Re-Analysis Comments for 1944

Responses given in boldface – April 2013 – Hagen/Landsea

General comments:

1. There are several metadata sections containing write-ups of damage and rainfall caused by storm landfalls. These are mostly unnecessary, and they should be removed unless they are explicitly being used to help evaluate landfall intensities.

Done.

2. The times are not depicted consistently in the metadata. For example, 0000 UTC is sometimes given as 00Z and sometimes give and 0 UTC. Please fix this.

Done. Time has been given with 2 digits for times at the top of the hour. For times not at the top of the hour, 4 digits are given.

3. Connor has references to a data set called the “614 Hurricane Files” in his ‘Hurricanes and Tropical Storms in the Gulf of Mexico 1875-1956’. What are the “614 Hurricane Files” and have they been more directly tapped as part of the re-analysis?

These “614 Hurricane Files” apparently referred to some paper files archived at the old New Orleans Weather Bureau office in the 1950s. These are no longer available.

1944 Storm #1:

1. The 12 July metadata mentions a position of 15N 65.5W from the microfilm maps? Is this correct? The microfilm map in the binder appears to show a position closer to 14N 62.5W.

The 12 July 1230 UTC microfilm map had not been scanned and was not previously in the binder. It has now been scanned and placed in the binder. Yes, it is correct. The word “low” is stamped on the map near 15N, 65.5W. However, the 00 UTC microfilm map shows a TS symbol with a question mark near 14N, 62.5W.

2. The microfilm map for 1230 UTC 13 July shows an observation of east winds 35 kt/mph near 21.5N 70.5W. This datum does not appear in the obs print out. Is this a ship or an aircraft? Is there a pressure associated with the ob? If this ob is correct, it and the southwest wind on the north coast of the Dominican Republic might suggest a farther west initial position.

Agreed. The observation on the 1230 UTC 13 July microfilm map at 21.5N, 70.5W is an aircraft observation taken at 5,000 ft. It is uncertain whether it is a flight-level wind or an estimated surface wind. However, if it is a flight-level wind, then it is 35 kts at flight-level of 5,000 ft. If it is an estimated surface wind, then it is Beaufort force 7 (30 kts at the surface). Surface wind estimates from that high up are not very accurate or reliable. The flight-level wind measuring capabilities in the mid-1940s were not yet very good either. Also, it does

not appear that there is a pressure listed with that observation. 1945 appears to have been the first year when Atlantic aircraft recon had both a pressure altimeter and a height altimeter. So there were not any aircraft pressures in 1944.

The aircraft ob was taken at 10E (14 UTC). There is also an observation from Grand Turk of NE 10 kt with 1015 mb. The data does allow for an additional westward adjustment of about 0.3 degrees longitude, but not more than that. Also, the light southwest wind on the north coast of the Dominican Republic may or may not be affected by topography. Very slight westward adjustments have been made to the positions are 13/06Z – 14/00Z.

3. The metadata summary needs more detail on two points: a) The decrease in peak intensity from 80 to 65 kt, where it should be stated there is insufficient evidence to justify a downgrade, and b) the revised time of extratropical transition.

After re-inspecting the data, it appears that it is uncertain how close the plane got to the center, although it appears that there were two times that day when an aircraft attempted to get near the center (around 17/1800 UTC and around 18/0000 UTC). Both times, the plane reported a Beaufort force peaking at force 11. But it is uncertain whether the RMW was reached. There appears to be enough evidence to decrease the HURDAT peak intensity by 10 kts to 70 kts. The position given at 18Z compared with our analyzed position is 1.5 degrees away, but the position at 18/00Z is less than 0.8 degrees away. We have increased the peak intensity to 70 kts from the 65 kts originally analyzed. However, we start the decrease by 65 kts again at 18/00 UTC due to the plane being much closer to the center than at 17/18Z. Also, on the flight on the 15th at 12Z, it appears that the aircraft very likely flew very near the center. We had 48 kts in the Excel sheet (but it appears that the wind barb is a 55 kt FL wind). We reduced the intensity from 60 to 50 kts on this day, but it is possible it may have been a little stronger due to the rudimentary aircraft data and flying techniques used during the first year of aircraft reconnaissance. We will increase the intensity back to 55 kts at 12Z on the 15th. On the 16th, there was some data within a reasonable distance of the center than showed weaker winds in the 35-45 kt range. Based on this, we have kept our 65 kt analysis on the 16th, showing a peak of 70 kts on the 17th.

The followed intensities (kts) have been changed (revised → re-revised):

7/14 18 UTC: 35 → 40

7/15 00 UTC: 40 → 45

7/15 06 UTC: 45 → 50

7/15 12 UTC: 50 → 55

7/17 00 UTC: 65 → 70

7/17 06 UTC: 65 → 70

7/17 12 UTC: 65 → 70

7/17 18 UTC: 65 → 70

All of the intensities that were re-revised are now closer to the original HURDAT value than we were recommending before.

Regarding the time of extratropical transition, text has been added to the metadata summary explaining the justification for keep it tropical 12 hours longer.

4. The committee concurs with the rest of the proposed changes.

Thank you.

Additional comments from Pasch: TC #1: It is stated that “The intensity is reduced every day from the 13th to the 17th as observational data indicates that the cyclone was weaker than originally shown in HURDAT.”. Or is it merely because no synoptic observations of the intensities originally indicated in HURDAT could be found? A lack of inner-core observations to support the original intensities is not sufficient justification for reducing the winds. For example, on July 13 and 14, the storm moved on a track that kept the core and the strongest side of the circulation sufficiently to the east and northeast of the Bahamas so that those islands did not experience, or report, the strongest winds.

It is agreed to retain the original intensities on the 13th and 14th and to boost the revised intensities upward to be closer to the original values on the 15th-17th.

1944 Storm #2:

1. The original summary in the Monthly Weather Review (MWR) stated that the system dissipated near Haiti on 26-27 July, as does the Weather Bureau report in the binder. Looking at the maps, the evidence for the system existing after passing Haiti and Jamaica is a little thin. Is it possible that scenario is correct and the system should be shown as dissipating? Please re-examine the track on 26-27 July. As an alternative, is it possible that the original farther south HURDAT track is correct?

Agreed to have the system dissipate late on the 27th. The data indicates that it is fairly clear that the cyclone weakened faster on the 27th than shown in both the original HURDAT and in our first reanalysis. The 28th is now removed from HURDAT.

1a. This issue is best seen on the 0030 UTC 27 July microfilm map. The proposed 0000 UTC position is 17.5N 76.4W. The map shows a wind of east 20 kt/mph near 17.2N 75.8W, which is not compatible with the proposed position. Either the system has degenerated to a tropical wave, or the original HURDAT position is better than the proposed position. The latter is supported by the aircraft obs of westerly winds along 73-74W.

Agreed to indicate the latitude farther south, closer to the original HURDAT latitude, which fits much better in accordance with the data at 0030 UTC on the 27th. The position at 00Z on the 27th has been adjusted south from 17.5N to 16.5N. The latitudes have been adjusted accordingly. We also eliminated the 28th from HURDAT, indicating a weakening to a tropical depression by 12Z on the 27th with dissipation after 18Z on the 27th.

The following latitudes have been changed (revised → re-revised):

7/26 18Z: 17.3 → 16.6
7/27 00Z: 17.5 → 16.6
7/27 06Z: 16.5 → 16.8
7/27 12Z: 17.5 → 17.0
7/27 18Z: 17.4 → 17.2

The following intensities (kt) have been changed (revised → re-revised):

7/27 06Z: 40 → 35
7/27 12Z: 35 → 30
7/27 18Z: 30 → 25

2. Have efforts been made to find extra data from the Lesser Antilles, Dominican Republic, Haiti, Jamaica, Honduras, and Belize for this system?

All available sources for Caribbean data have been investigated and no additional information is available.

3. In the 26 July metadata, can “concrete” be replaced with “definitive”?

Done.

4. Metadata summary: Given the uncertainty about how far the center was from Hispaniola, the speculation that interaction with the island caused the storm to weaken should be removed.

Done.

Additional comments from Pasch: Since this storm apparently became very disorganized after July 26, and the Monthly Weather Review states “Indications are that the small center struck the high mountains of the Haitian Peninsula...”, shouldn’t the reanalysis shift the track even farther north to show it moving over the south coast of Haiti on July 26? The observations from Port Au Prince seem to support this storm moving closer to that city. Neither the original nor the currently reanalyzed best track should have resulted in much weakening.

These comments are contradictory to the main ones from the committee. Overall, the evidence is best for shifting the track south farther away from Hispaniola, though there is significant uncertainty in the actual track of the system. This is now better reflected in the metadata writeup.

1944 Storm #3:

1. Please better explain the earlier genesis. What is the specific data that sets the genesis time at 0000 UTC 30 July? It is noted that while the 1200 UTC 30 July Historical Weather Map (HWM) shows a westerly wind at Santo Domingo, it shows several other easterly winds south of the proposed center position. Is data available from San Juan that could help explain this?

Although it is certainly possible that a closed circulation existed by 00Z on the 30th, it appears that there is not definitive evidence of this. However, there is enough evidence by 12Z on the 30th of westerly winds on the south side of what appears to be the developing circulation for it to be considered a closed circulation by 12Z on the 30th. Therefore, the proposed 00Z and 06Z points on the 30th have been eliminated. Unfortunately, no additional San Juan data is available on the EV2 site or elsewhere.

2. What is the basis for the delayed increase to hurricane strength?

An abundance of surface and aircraft observations indicate that the cyclone had not yet reached hurricane intensity on 31 July. Observations indicate a tropical storm with 45-55 kt winds with weak winds on the southwest side. This had been added to the metadata summary. The WB report also states that aircraft made a center fix on the morning of the 31st that showed max winds of 50 mph.

3. There is a note on written on the 0030 UTC 2 August microfilm map about the Morehead City Naval Base reporting 80 mph winds in Wilmington. Please explain the significance of this and add mention of it to the metadata.

The observation in question was an estimated observation at 2250 UTC from the Coast Guard Station at Oak Island. The WB report states that the wind reached 59 mph there at 4:30 pm before the anemometer failed. Max wind was estimated at 70-80 mph (60-70 kts) at 2250Z. Min pressure of 989 (or 990 – different sources vary by 1 mb) occurred at 2330Z. Wind shift from E to S to SW indicates center passed west of Oak Island. 985 mb – 4-5 mb deeper than the observation - is a better estimate of the landfall central pressure.

4. The Weather Bureau report suggests strongly, although not conclusively, that the 990 mb pressure at Oak Island was not in the eye or inside the radius of maximum wind (RMW). Please find more information on this, and please develop an alternative landfall intensity assuming that this pressure was not inside the RMW.

The WB report suggests that Oak Island was not in the eye. But there is not information either way to inform us whether it may have been at or just inside the RMW. It is agreed that 990 mb is not a central pressure reading. It is likely that 985 mb is a better estimate of the central pressure at landfall based on this information. There are no extra observations available from Oak Island that can be found. 985 mb = 66 kt north of 25N. Given the small RMW, 70 kt is chosen for the landfall intensity. 985 mb is also analyzed. This is a change from 65 kt/990 mb in the first reanalysis.

5. The committee concurs with the proposed earlier dissipation. Is there any evidence to suggest the system could have dissipated even earlier?

A 00Z 4 August analysis has now been conducted with station and ship data. The observations are ambiguous as to whether the system had dissipated. The current

HURDAT position provides a reasonable location, if the cyclone still retained a closed low. Therefore no changes are made to the position early on the 4th.

Additional comments from Pasch: First line of the July 30 observations discussion, replace “firsts” with “first”. On the synoptic chart for 0030 UTC August 2, it is noted “Moorehead Cy Naval Base reports 80 mph at Wilmington at 1850 EWT”. I assume that it is not known whether this report, if accurate, is that of a sustained wind or a gust. It, along with the reports of significant damage in the Wilmington/Cape Fear area (in particular Carolina Beach) suggests an intensity a little higher than 65 kt. I recommend that the winds not be reduced quite so much, i.e. down from 80 kt to 70 kt rather than to 65 kt.

Done. Agreed to make the upward boost to 70 kt at landfall. See also responses in #3 above.

1944 Storm #4:

1. Is any detailed data from Barbados, Grenada, or Mexico available for this storm?

There were no additional detailed observations of wind/pressure available for Barbados or Grenada. But we do have a helpful newspaper article from Mike Chenoweth. Based on this article, it is concluded that the cyclone was likely at hurricane intensity when it made its closest approach to Grenada around 06Z on the 17th. A 65 kt intensity is assigned at that time and the track was adjusted 0.4 degrees south of the original analysis to 12.3N, 61.5W at 06Z. From the 65 kt analyzed intensity at 17/06Z to the 90 kt analyzed intensity at 18/12Z (973 mb ship central pressure), we increase the intensity by 5 kt per 6 hrs in to interpolate the intensity during that 30 hour period. Going back in time from 17/06Z, we decrease the intensity by 1 T# per day, which is approximately a 15 kt decrease going back 12 hours to 18Z on the 16th, when we now have a 50 kt intensity. After the WB in Miami received the reports out of Grenada and the islands, they stated that the cyclone has winds of 60-70 mph and passed over Grenada last night. The Mexican synoptic maps and some station data were obtained from the EV2 website. Aside from a maximum wind report at Cozumel (47 kt ENE on the 22nd), no new information was obtained.

2. What are the analyses that show the system was a 40-kt tropical storm by 1200 UTC 16 August and thus justifies the earlier track time? The daily metadata says no gales or low pressures were noted that day.

There are no available analyses that provide evidence of the existence of a tropical storm or closed circulation by 12Z. The 12Z point on the 16th has therefore been eliminated. Therefore, no changes have been made to the original HURDAT time of genesis.

3. Despite the impressive damage reports from Jamaica, there are no data explicitly showing the system was a major hurricane at landfall. Is there sufficient evidence to justify changing the original HURDAT intensity of 105kt?

It is agreed that there is not sufficient evidence to change the original HURDAT intensity of 105 kt at landfall in Jamaica. It is noted that the wind caused damage is consistent with a Category 3 at landfall.

Additional comments from Pasch: The upward revisions in intensity on August 17 and 18 look reasonable based on the ship report on August 18. In the last paragraph of the metadata discussion, please change “(Note that the 978 mb central pressure estimated based on a 987 mb peripheral pressure measurement discussed in the report is likely substantially underestimated in intensity...” to “(Note that the 978 mb central pressure estimate based on a 987 mb peripheral pressure measurement discussed in the report is likely substantially too high...”.

Done.

1944 Storm #5:

1. 17 August metadata – please re-phrase the part about “the low is three-quarters closed”. The HWM for that day does not show a low.

Done.

2. What is the basis for revising the genesis time to 1200 UTC 18 August? Is the closed circulation based on the winds at Belize City and on the northern coast of Honduras? If so, please state this explicitly in the metadata summary. Given the lack of data near the system center, the argument for the earlier genesis needs to be strengthened if the committee is to approve it.

The system did attain a closed circulation around 12Z on the 18th as it was passing between Cuba and the Yucatan Peninsula, based in part by the westerly winds observed in Belize City and along the northern coast of Honduras. This is now mentioned in the metadata writeup.

2a. On the 1200 UTC 18 August microfilm map, are the data points east of the low aircraft obs? If so, these need to be noted in either the metadata summary or the daily metadata.

Yes, the data points on that microfilm map east of the low are aircraft observations from around 16Z. The aircraft was flying at an altitude of 6000 ft and reported 25 kts SSE around 15.7N/83W and 25 kt S around 14.5N, 83.5W. These are now added into the daily metadata.

3. The microfilm maps for 15-16 August show a low pressure area near Puerto Rico and Hispaniola with evidence of a sharp wind trough and possibly a closed circulation. The HWM on 17 August shows a low just east of Jamaica, which is not analyzed on 18 August. Please investigate to see if this was a short-lived tropical depression separate from the system that later became the tropical storm.

We did not find a closed circulation associated with this that could definitely be an earlier genesis time of Storm 5 from what we already analyzed. However, given the somewhat sparse data available on the 15th through the 17th, the possibility exists that either this cyclone began earlier or there was a separate, short-lived tropical cyclone on those dates. This is now so indicated in the metadata writeup.

4. Is there any Mexican data available for this storm?

Mexican synoptic maps and station data were obtained from the EV2 site. No additional relevant observations were obtained.

Additional comments from Pasch: Based on the 39-kt 1-minute wind from Brownsville at about 1730 UTC August 22 and the fact the center of the storm was still offshore at 1200 UTC August 22, I think that the intensity should not be reduced from 50 to 45 kt at the latter time (as was in the original HURDAT).

Agreed to maintain the 50 kt intensity up until landfall around 18Z on the 22nd.

1944 Storm #6:

1. The Weather Bureau report on this storm is very emphatic that the system did not form from an extratropical low. While the various map analyses show a baroclinic or quasi- baroclinic low over the northwestern Gulf of Mexico on 8-9 September, there is a lack of data near several of the original HURDAT positions. In addition, the microfilm map for 1830 UTC 10 September, suggests there were two lows – the tropical storm near the Mouth of the Mississippi River and a second low near the southwestern coast of Louisiana. Please note the aircraft observation south of Lake Charles, Louisiana that show a wind shift, which could be a cold front extending southward from this low.

Please re-analyze the origins of this system to determine – as best as the data will allow – if there were two cyclones present. Given the original HURDAT and the Weather Bureau report, the committee does not concur with the proposed changes to the origin and early track positions.

Agreed to retain the system as a tropical cyclone at genesis. After re-evaluation of the data late on the 10th, there is no strong evidence that there were two separate cyclones. It is now noted that the wind shift south of Lake Charles, Louisiana at this time could be either a cold front or a trough.

2. The reported 992 mb pressure at Pilottown, Louisiana, and the apparent small-scale center indicated in the Weather Bureau report in the binder would justify the revised proposed intensity of 55 kt. However, what other evidence is available to support this increase? For example, the Louisiana Climate Data is quoted regarding rainfall from the storm. Is there anything in the publication about the wind effects? Has the New Orleans Original Monthly Record (OMR) been checked for a write-up?

The Louisiana Climatological Data and the New Orleans OMR of September 1944 had no discussion of this cyclone. However, the Alabama Climatological Data and the Mobile OMR

3. If the report of the small center is correct, that could also affect the landfall location on the Alabama coast. The OMR data from Mobile shows the wind shifting from east to south to west, suggesting the center went near or west of that station. In addition, the 1003 mb pressure in Mobile might indicate that the small center near Pilottown broke up before the final landfall. However, the pressure in Pensacola was the same as that in Mobile, which is not consistent with a center closer to Mobile. An alternative scenario could be that the center was much closer to Pensacola and that the Mobile wind shift was a saddle point. Please investigate this, including obtaining detailed data for the Pensacola Naval Air Station (especially pressure data).

Detailed pressure data from Pensacola Municipal Airport (Hagler Field) was obtained. Pressure data with at least 1 ob every hour (sometimes 1 ob every 40 minutes) was available. The minimum value was 1004 mb at 10/2330Z with 33 kt SSE. The wind direction barely changed within several hours before and after the time of the minimum pressure, suggesting that the center did not pass closer to Pensacola. Instead, it is more likely that the center passed west of Mobile and broke up after passing Pilottown. Based on the Mobile observations, an additional track change of 0.2 degrees is implemented at 11/00Z to show the center passed just to the left of Mobile instead of just to the right.

4. What is the 1-minute wind for the 47-kt 5-minute observation at the Pensacola Naval Air Station? Please include this in the metadata summary.

A 47 kt 5-min wind would convert to a 1-min wind of 50 kt using a conversion of 1.06.

Additional comments from Pasch: I question whether this system was ever an extratropical cyclone over the Gulf of Mexico. The surface synoptic data, while they do indicate a frontal zone near the northern Gulf coast, do not really support a significant temperature gradient across the center of the cyclone. Moreover, climatologically this would be a very rare event for the time of year.

Agreed. Also see responses in #1 above.

1944 Storm #7:

1. The HWM show a tropical storm symbol east of the Lesser Antilles on 8 September and a tropical wave for several days before that. However, there is no data to support these analyses. Has COADS been checked for data during the pre-genesis period? Please include some discussion of the period before 9 September in the daily metadata.

The COADS observations from September 2 to September 8 had been obtained. These in combination with the Historical Weather Map data do not support genesis before the 9th of September. No change is made to the time of genesis, though the cyclone certainly could

have developed farther east a few or even several days earlier. Discussion for several more days before genesis is now included.

2. This storm features mysterious references to central pressures. The most interesting is that of Tannehill, which says the pressure was probably about 909 mb. Is there any information on where this came from? Navy ships? Extrapolation of the measured pressures outside the center to the center? Aircraft? Another such is the 943 mb pressure originally in HURDAT at 1200 UTC 12 September. When did this figure get introduced into HURDAT?

Given the importance of the storm, please do as much as possible to pin down where these figures came from. It is suggested that people who worked on the earlier versions of HURDAT be contacted to see if they can provide insight on the 943 mb pressure. The bottom line is that it is unlikely these figures were made up – they had a source somewhere.

There are no known observations with either the 909 mb central pressure suggested by Tannehill, nor the 943 mb in HURDAT at 12Z on the 12th. Inquiries to Charlie Neumann (the HURDAT developer in the 1970s) did not provide any additional insights. Given that 943 mb is already included into HURDAT and that it may be reasonable, this value is retained. Without corroboration of Tannehill's suggested 909 mb, this value is too suspect to incorporate into HURDAT.

2a. The handling of the 933 mb ship pressure at 1800 UTC 13 September is confusing. The proposed revised HURDAT and the metadata summary suggest this was a central pressure, or at least a possible central pressure. However, Andy Hagen's addendum states that this value should be removed as a central pressure. Please clarify this. Also, please state as explicitly as possible whether this report and the 952 mb report five hours earlier are central pressures. The proposed intensity for 1800 UTC 13 September should be revised upward if the 933 mb value is not a central pressure.

Both the 952 and the 933 observations are COADS values without any corresponding wind information. Therefore, there is no way of knowing what the central pressure was at those times. The central pressure was equal to or less than 933 mb at 9/13 1700 UTC. The 933 mb central pressure has been removed from HURDAT. A central pressure of less than or equal to 933 mb suggests a wind speed of at least 121 kts according to the north of 25N Brown et al. pressure-wind relationship. The intensity at 18Z on the 13th has been revised upward to 125 kts.

3. The metadata summary states that Cape Hatteras, North Carolina reported a 74-kt 5-minute wind, which converts to about an 80-kt 1-minute wind. Two questions: 1) When (or where) was this wind measured? The Weather Bureau report states that the station anemometer blew away when the wind was a 57-kt 5-minute wind and that the winds increased thereafter. In addition, the daily metadata for 14 September does not have this ob. 2) This was apparently on the southwest side of a hurricane that was accelerating northward. Has the acceleration been taken into account in the estimated intensity of 110 kt at this time?

The 74 kt Cape Hatteras wind was estimated after the instrument blew away and therefore should be considered unofficial.

3a. Cape Henry, Virginia measured a 117-kt fastest-mile wind. How does this convert to a 1-minute wind, and what implication might this have for the intensity at the time?

The 117 kt fastest mile observation is problematic. This is about a 30 second average, which would convert to a peak 111 kt 1 min wind. The anemometer height is 16 m above the ground, which slightly reduces to 109 kt 1 min wind at 10 m above the ground. On the other hand, the peak 5 min wind of 75 kt converts to a peak 1 min wind of 80 kt and then to 78 kt 1 min wind at 10 m above the ground. Thus the two ways to arrive at a peak 1 min wind from other measurements do not agree. It is possible that the 117 kt fastest mile observation was atypical of the circulation of the hurricane and thus the 78 kt value of the peak 1 min wind at Cape Henry is a better estimate. It is analyzed that a portion of the Virginia coast received 1 min Category 2 conditions.

4. Please re-examine the landfall intensity in New England. On one side, the observed winds at New York City and Block Island, along with the observed forward motion, make it likely that the original 75-kt intensity is too low. On the other side, the climatological behavior of storms in that area and the possibility that the hurricane was starting extratropical transition suggests that 95 kt could be too high. Since the RMW appears to be known, can it be determined which (if any) of the coastal stations the RWM passed over?

Agreed to adjust the landfall intensity in New York down to 90 kt and the second landfall in Rhode Island down to 85 kt. Only Block Island, RI may have experienced the RMW (or close to it) with their 71 kt 5 min peak winds (adjusts to 74 kt 1 min peak winds)

5. The old and proposed best tracks both show a 30-kt intensity at 1200 UTC 16 September. However, the daily metadata mentions a 35-kt ob at 1400 UTC that day. Please either revise the best track intensity or explain why the ob was disregarded. On a related note, the daily metadata has a reference to “30 kt extratropical storm” which one way or another needs modifying.

Agreed to revise the intensity upward to be consistent with the available observations.

6. Where are the OMRs for this storm? They don't seem to be in the binder.

The OMRs were obtained for this reanalysis, but were misplaced several years ago. Fortunately, all of the relevant data have been transcribed and included in the database/metadata sections. Additionally, the OMRs are all available on-line again via the EV2 website.

Additional comments from Pasch: At the end of the September 13 observations discussion, there is a very definitive statement from Tannehill that the central pressure of this hurricane “was certainly below 27.00 inches...”. It seems that Tannehill, normally a very reliable source, had some observation(s) to support an extremely high intensity, i.e. at least

borderline Category 5. Can this be investigated further? In the first paragraph of the metadata discussion, third to last line, replace “intensity” with “intense”. Given that the center of the hurricane passed about 25-30 n mi east of Hatteras, which reported a pressure of 947 mb, and that station was outside of the RMW, shouldn’t the central pressure at 1200 UTC September 14 be lower than 942? At most 940 mb, or even a little lower, seems more reasonable.

See responses to #2 above. The sentence in question has been completely replaced. Agreed to go with 940 mb at the bypass of Hatteras. This leads to a slight boost in intensity to 110 kt from the 105 kt suggested in the first version.

1944 Storm #8:

1. While the committee concurs with not moving the genesis from 19 to 18 September, the metadata summary is confusing in saying that the genesis time is not changed despite the HWM and microfilm maps showing a closed circulation. Please re-write this.

Done.

2. The daily metadata for 21 September states that Campeche, Mexico had 65 kt winds at 0500 UTC. However, the metadata summary states that “there were no specific observations of hurricane force winds”. Please correct either the ob or the metadata summary. It is noted that this ob is found in the Weather Bureau report on this storm along with the notation “not used for reanalysis”. Does this mean that the report was found after the original re-analysis was submitted, or that it was rejected for some reason?

Yes, it means that the report was found after the original reanalysis was submitted. The observation should be valid.

3. The metadata summary mentions the original HURDAT showing the cyclone dissipating over the Pacific. Was the translation speed taken into account in the proposed revisions?

Yes. The original HURDAT had the cyclone accelerating to 15 kt toward the south-southeast after the second Mexican landfall, reaching the Pacific at 12Z on the 22nd as a 20 kt tropical depression with dissipation thereafter. The reanalysis shows a significantly slower movement – 8 kt, which is consistent with the cyclone dissipating over southern Mexico before reaching the Pacific.

4. There is a typo in the metadata summary: “pressure0wind”.

Done.

Additional comments from Pasch: Although the initial intensity is reduced, the reanalysis still shows the system beginning as a tropical storm. Why not start the cyclone as a 25- or 30-kt tropical depression at 1200 UTC September 18 near the location indicated by the

HWM? On line 17 of the metadata discussion correct the typo “int he” to “in the” and on line 19 correct “pressure0wind” to “pressure-wind”.

This comment is contradictory to the committee. Therefore genesis remains unchanged from that shown in HURDAT. Typos are corrected.

1944 Storm #9:

1. While the committee concurs with an earlier extratropical transition, is there enough data near the core on 26 September to justify the proposed 24 hour change? Please strengthen the argument for this change in the metadata summary.

Based upon the increased temperature gradient, asymmetric structure of the pressure field, and winds in the southwestern quadrant responding more to forcing of a cold front, the cyclone is reanalyzed to have become extratropical around 12Z on the 26th – 24 hours earlier than originally shown in HURDAT. However, the exact timing of extratropical transition is more uncertain than usual due to the lack of observations near the center of the cyclone on the 26th and early on the 27th.

Additional comments from Pasch: In the first line of the metadata discussion, replace “perhaps associated with a tropical wave that came off of Africa” with “likely associated with a tropical wave that emerged from Africa”.

Agreed.

1944 Storm #10 (new):

1. Please include maps from 27-28 September in the binder.

Done.

2. The committee does not yet concur with adding this system to HURDAT. Please strengthen the argument of why this system was a tropical cyclone instead of a baroclinic low. It needs more than “temperatures were warm around the cyclone”.

On 28 and 29 September, a broad area of low pressure developed in a uniformly warm airmass, southwest of an analyzed frontal boundary. The circulation was too broad on these days to be considered a tropical cyclone. By 30 September, the low had consolidated, strong winds were located near the center, and the system remained within a uniformly warm airmass. It is analyzed that the system became a tropical cyclone around 00Z on the 30th. On that date, a ship with 35 kt SW wind and 1003 mb pressure was observed at 23Z about 100 nm from the center.

3. Since the only observed gale was 35 kt, what is the basis for the 45 kt peak intensity? Would 40 kt be better?

The 1003 mb peripheral pressure suggests winds of greater than 38 kt from the north of 35N pressure-wind relationship. The cyclone is analyzed to have reached a peak intensity of 45 kt on the 1st based upon the pressure-wind relationship.

4. There are several observations on the 1 October HWM in the binder that are not included in the text metadata. Most notably, there is an ob of S 35 kt/mph east of the center near 38N 41W that needs to be mentioned regardless of whether it is kt or mph. This report is also missing from the obs table in the binder. Please re-write the metadata for 1 October to include these data, and please add them to the obs table.

The HWM values are plotted in mph, so this value is 30 kt. Typically, observations are only included into the daily summary and the database if the highest values are gale force or stronger and/or the pressure is 1005 mb or lower. In deference to your interest, this 30 kt observation has been added into both the daily summary and the database.

Additional comments from Pasch: In the metadata discussion, line 4, replace “a ship with 35 kt SW wind and 1003 mb pressure was observed at 23Z” with “a ship observed a 35 kt SW wind and a 1003 mb pressure at 23Z”. In line 5, replace “pressure suggest” with “pressure suggests”. Also, which observations support setting the winds of this storm any higher than 40 kt?

Typos corrected. See response to #3 above.

1944 Storm #11 (old #10):

1. Please include maps for 28-29 September in the binder.

Done.

2. The 1830 UTC 1 October microfilm map shows three aircraft obs near 15.5N 60.5W with southeast and south winds, apparently at altitudes of 2500-3000 ft. Based on these, does the track need to be moved farther westward?

It is agreed that the system needs to be moved significantly toward the west on the 1st and 2nd.

3. Otherwise, the committee concurs with the proposed changes.

Thank you.

Additional comments from Pasch: Agree with showing genesis about a day earlier than in the original HURDAT, but why start the system as a tropical storm? Couldn't the intensities be 30 kt at 0600 UTC, 35 kt at 1200 UTC, and 40 kt at 1800 UTC September 30?

Agreed to begin the system as a tropical depression.

1944 Storm #12 (new):

1. Please include maps for 7-8 October in the binder.

Done.

2. Please provide a stronger argument for the system acquiring tropical characteristics at 0000 UTC 11 October. What data is available at that time to justify this? You might want to include a detailed temperature analysis to support this proposed genesis time.

The amount of data is minimal around 00Z on the 11th (only two ships within 200 nm of the center). The reason for beginning the system at 00Z on the 11th is the very well developed cyclone apparent at 12Z. Given the structural changes observed between 12Z on the 10th and 12Z on the 11th, the best estimate is that genesis occurred around 00Z on the 11th – about midway between the two better sampled times.

3. Please re-examine the position and intensity at 1200 UTC 12 October. While the two ship reports of hurricane-force winds appear to justify the proposed 70 kt intensity, both reports are 150 n mi from the proposed center position. This would not be consistent with a tropical cyclone. Also, the ship report of 996 mb and 10 kt near 36.5N 38.5W at 1100 UTC supports a farther east position.

Agreed to adjust the position of this new hurricane toward the east by a degree (to 39.0W) at 12Z. The peak intensity is retained at 70 kt.

4. The committee currently does not concur with the idea of the cyclone making landfall in Portugal as a tropical storm. At the time, the system was moving quickly eastward in strong westerly flow, and there is no evidence that a closed circulation still existed on 15-16 October. In addition, the 15 October HWM suggests a temperature gradient developing across the system even though no fronts were analyzed. Please contact the Meteorological Services of Portugal and Spain to obtain more detailed data on the nature of this system during and after landfall. If that data is not available, the track should be changed to show extratropical or dissipated (trough/open wave) before landfall. The bottom line is that we need confirmation from the people on the spot before saying such a rare event occurred.

It is agreed to indicate the cyclone dissipating by 00Z on the 16th. With the system moving around 25 kt around 00Z on the 16th, a closed (earth-relative) circulation was no longer likely to have occurred and no observations could confirm a closed circulation (though measurements were sparse on the north side of the system). Moreover, an extratropical transition is now indicated to have occurred around 12Z on the 15th, consistent with a significant temperature gradient and a more asymmetric circulation occurring.

Additional comments from Pasch: The surface synoptic data seem ambiguous as to whether this system was tropical or extratropical. For example, the observations on the 1200 UTC October 13 map suggest a temperature and dewpoint gradient across the cyclone. Since this is a “once in lifetime” event, I think the data should be unequivocal. Has there been any communication with the meteorological services of Spain and/or Portugal regarding this system? I believe that this is a system that will need to be voted on by the Best Track Change Committee (BTCC).

See replies to similar comments #2 and 4 above. Revised assessment has the system becoming extratropical on the 15th and dissipating before making landfall in Portugal.

1944 Storm #13 (old #11):

1. The committee concurs with the proposed changes up to 13 October.

Thank you.

2. What averaging period is the 122-kt wind in Havana, Cuba? The 18 October daily metadata states that it is a 1-minute wind. However, the metadata summary and Andy Hagen’s addendum suggest that it was a fastest mile wind. Please clarify this.

It is a fastest-mile wind. So it is about a 30 s averaged wind. The 18 October daily metadata was incorrect in stating that it was a 1-min wind. A 122 kt fastest-mile wind converts to a 1-minute wind of 115 kts.

3. The press clipping in the binder indicates a 937 mb pressure was measured near Central Merceditas, Cuba, although no time was given. Hagen’s addendum states this was between 1100-1200 UTC. Has this time been confirmed by Perez?

The time is unknown, but is estimated based on the location of where the 937 mb was measured and the approximate time that the center of the hurricane was nearest that location given the best track of the hurricane.

4. The MWR discusses a ship caught in the eye between Havana and the Dry Tortugas. Is there any pressure data available from that ship in COADS?

Unfortunately, no.

5. Please make sure that the adjusted wind value of the Dry Tortugas ob is included in a revised metadata summary. It is noted that the Weather Bureau report states that the 104-kt winds were measured at two consecutive hours. Does this make a difference in determining the adjustment?

The 104 kts was measured at anemometer height 48 meters. This converts to a 92 kt 10 meter wind which is the case regardless of it being measured at one or multiple consecutive hours.

6. Was the Sombrero Key weather station also mounted on top of a lighthouse? If so, please indicate this where appropriate in the metadata.

Yes, done.

7. Please provide more information on the Sanibel Island 87-kt ob in the metadata. Was it a 5-minute wind? Was the station at a high elevation? This could have implications for the landfall intensity in Florida.

According to Table 1 in the MWR article (page 222), this was a 5 min wind. The height of the Lighthouse where the measurement was taken is 30 m. Adjusting this to 10 m and a peak 1 min, the wind is 85 kt.

8. Is the 962 mb observation at Sarasota known to be a central pressure? The center passed near Sarasota, but the accounts are not explicit as to whether it was calm at the time of the minimum pressure.

It is not completely certain that the 962 mb is a central pressure, though this is consistent with other measurements. The value is added into HURDAT as a central pressure, but additional caveats are included in the writeup to convey the uncertainty.

9. Are the capitals necessary for “SHIP” in the metadata summary paragraph on the Florida landfall?

Agreed to uncapitalize.

10. Is the dissipating warm front mentioned in the 20 October daily metadata extending southwest from the center or southeast? The HWM suggests the latter.

Corrected.

11. The Jacksonville OMR shows that the wind shifted from SE to N to NW as the center passed, while the Savannah OMR shows that the wind shifted from E to N to W. Please check to make sure the track passes just east of these locations, as the current track looks a little too far west.

Agreed.

12. The Savannah, Georgia OMR mentions a report on the hurricane attached to the OMR. Is a copy of this report available?

While the report was not available on the OMR file from the EV2 website, we did obtain a report of the cyclone's impact in Georgia from the Georgia Climatological Data October 1944 report. This, while interesting reading, did not provide any substantial information to make any changes.

13. The committee concurs with the earlier time of extratropical transition.

Thank you.

Additional comments received from Pasch: First paragraph of the metadata discussion, line 15, replace “(70 kt at 18Z on the 13th). This indicated a major downward...” with “(70 kt) at 18Z on the 13th. This required a major downward...”. Also in the first paragraph of the metadata discussion, third to last line, replace “loving” with “moving”. Note that the 937 mb (central) pressure was reported at Central Merceditas, Cuba (a sugar mill) located 25-30 n mi west of Havana. At Havana (Batista Field), a minimum pressure of 960 mb was reported which is 23 mb higher than the central pressure. This is pertinent to my comments above concerning the central pressure for TC #7 when it was near Hatteras (i.e. that it should be lower).

Typos corrected.

1944 Storm #14 (new):

1. The committee concurs with adding this system, with the caveat that there is only one ship report that supports it. This should be stressed in the metadata summary.

The criteria for inclusion of a new tropical storm into HURDAT have been the following: “at least two separate observations of sustained tropical storm force winds (at least 34 kt) or the equivalent in sea level pressure (roughly 1005 mb or lower). The two separate observations could come from the same ship/station or two different platforms” (Landsea et al. 2008). Thus this policy is consistent with the observations available here.

2. It is noted that the 3 November HWM shows a ship report with a 1004.7 mb pressure north of Panama. This ob is apparently not in COADS. The COADS data shows two ship reports of 1005 mb on 3 November, which do not seem to be the report plotted on the HWM. These reports lend support to a low pressure area north of Panama on this day.

It is likely that this 1004.7 mb/5 kt S/81F report in HWM plotted at 10.3N 79.9W is the same as 1005 mb/5 kt S/81F report by ship 46070 in COADS at 13Z at 10.5N 80.5W. (The manual plotting by the analysts for the HWM were not always accurate their plotting positions.) There is a second ship in COADS at 01Z (#12220) which did report 1005 mb and SW wind (no speed) at 9.5N 80.5W, which has now been added into the daily summary.

3, Have the Meteorological Services of Nicaragua, Costa Rica, Panama, and Colombia been contacted for additional data from this time?

These Meteorological Services have been contacted, but they have provided no additional information on this system.

Additional comments received from Pasch: Although the real impetus for designating this new storm is a lone ship report, the peripheral surface observations do indicate a cyclonic circulation was present over the southwestern Caribbean Sea. This supports the existence of a tropical cyclone, although the fact that, on the day after the ship report, aircraft “failed to locate any disturbance”, it is possible that the ship encountered a transient convective system. This is probably a case where the BTCC will need to vote on inclusion.

Commentary is included regarding the peripheral surface observations supporting the existence of a cyclone circulation over the southwestern Caribbean Sea.

1944 Additional Notes:

1. There does not seem to be any binder material for the suspect systems. Please add this.

There were no materials to include beyond what is written up in the Additional Notes section.

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

Thank you.

Additional comments from Pasch: None of the additional systems presented appear to be worthy of inclusion as new storms.

Thank you.