***Best Track Committee Re-Analysis Comments for 1951***

**(Responses by Andrew Hagen and Chris Landsea in bold face – July 2014)**

General comments:

1. While the microfilm archive has a significant amount of aircraft data, there is a concern about how much of the collected aircraft data is not in the archive. For example, there is an image of a dropsonde message for Hurricane Able which seems to be the only dropsonde in the archive for the entire season. It appears highly unlikely that this would have been the only dropsonde ob transmitted over the teletype system during the season. Please contact NCDC to see what other recon data may be stored there courtesy of the teletype reports. It is noted there several of these messages have a product header of WRCA MIA. That may help in tracking them down.

**We have contacted NCDC about the availability of 1950s and 1960s dropsondes from Navy/Air Force. Unfortunately, NCDC does not have any of these records of these early efforts to observe the central pressure of tropical cyclones. They indicated that they might be available in hard-copy form either in Atlanta or Tampa. However, the option of traveling to either location to possibly obtain some additional dropsondes is beyond the scope of this current project.**

1951 Storm #1 (new):

 1. The committee concurs with the addition of this system.

**Thank you.**

 2. This storm is well-documented, including multiple references in the Monthly Weather Review (MWR). Is any information available as to why it was originally left out of HURDAT?

**The reason for the system’s non-inclusion remains a mystery. It was not included in the Navy, Air Weather Service, or U.S. Weather Bureau post-season summaries, nor in Cry (1959).**

 3. Is any time history available for the ship that reported 50-55 kt wind on 6 January? Is this report in COADS?

**Unfortunately, the ship Tilly Ross only appears on one microfilm map – the one from 18Z on 6 January. It appears that this ship is not in COADS. There is no time series available other than that one observation.**

 4. There is a question of how valid the Brown et al. wind-pressure relationships might be for a January cyclone that formed from a baroclinic system. Perhaps a 50-kt intensity would be better at the time for the 997 mb ship report on 9 January?

**Agreed. 1/8 12Z through 1/9 06Z have been changed from 55 to 50 kt.**

1951 Storm #2, Able:

 1. The committee concurs with the proposed later genesis time. However, please provide a better justification of making the cyclone extratropical during its first 18 hours. Do the observed temperatures and dewpoints support this?

**Agreed that the system exhibits minimal temperature/dewpoint gradients across the center on the 16th and the TC stage is retained.**

2. In the metadata summary, it is assumed that the winds at Walker’s Cay in the Bahamas had a high bias. While it is certainly possible – the original HURDAT did not accept these winds at face value – it looks rather strange to make such an assumption given the lack of available information. Please find out more about these data, including whatever information is available from the Meteorological Service of the Bahamas, where these winds might have been in relation to the radius of maximum wind (RMW), and whether they were sustained winds or gusts.

**The Meteorological Service of the Bahamas has been contacted, but they have no further information of these observations. It is likely that they were visual estimates and thus quite possible that they were somewhat high biased.**

 3. In the metadata summary, it is stated that the RMW on 19 May was near climatology. Please state what the basis is for this, as there is no mention of the RMW size either here or in the 19 May daily metadata.

**According to NHC microfilm and the ATSR, there were two aircraft reports of eye radius on the 19th – 12.5 miles at 1319Z (micro) and 15 miles at 2233Z (micro, ATSR). These observations had been included in the Excel spreadsheet – the eye radius is in the temperature column. The sizes from recon have now been added to the May 19 metadata paragraph.**

 4. Was the aircraft fix on the afternoon of 21 May near 1900 UTC as stated in the metadata summary or 2015 UTC as stated in the 21 May daily metadata? Please clarify this.

**The 2015 UTC fix was a low level fix at 36.6N, 74.0W and only reported that 90 kt max winds were estimated. Sometime shortly before that time, an aircraft measured a 700 mb height in the eye of 9,320 ft between 18Z-20Z. The timing is estimated from data, commentary and coding on the microfilm maps. Since the 700 mb height could have been measured anytime between 18-20Z, the metadata summary states “around 19Z.”**

4a. On a related note, in the microfilm records, there is a dropsonde that appears related to the Air Force fix at 2015 UTC 21 May, as it has the same latitude and longitude as the fix. Is this the drop made on that fix and has it been decoded? While the observed surface pressure is unclear, the last significant pressure level in the drop was 955 mb. Please decode this to see what the surface pressure was. It is noted that this drop apparently has a 700-mb temperature of +10 C, which may help with a central pressure extrapolation.

**Yes, this drop is from the same flight around the same time at 2015Z on 21 May. While we can decipher some of the drop coding, we are unable to determine the surface pressure directly, if it is even on the drop message. However, the 700 mb temperature of +10.0C along with a height of 9,330 ft (2,844 m) suggests a surface pressure of 974 mb. Also the 850 mb temperature of +18.1C along with a height of 3,930 ft (1,198 m) suggests a surface pressure of 976 mb. The 976 mb value appears to be a valid central pressure, which is now added in at the 18Z May 21st HURDAT time slot. 976 mb central pressure suggests 77 kt from the Landsea et al. north of 35N pressure-wind relationship. 75 kt is analyzed as the intensity at 18Z on the 21st, down significantly from 100 kt originally.**

5. While the committee concurs with a reduction in the peak intensity, there is concern that the proposed reduction to 80 kt may be too large. Please re-examine this after resolving point 4a.

**The obtained central pressure value for late on the 21st provides further corroboration of the system peaking at 80 kt, at and just before the 973 mb central pressure at 1105Z.**

6. “…consistent with the lack of observed tropical-storm-force winds…”. Wouldn’t the lack of strong winds in coastal North Carolina be more related to the size of the system rather than the intensity? Please clarify this or remove it.

**Yes, this has been removed.**

7. Please move the discussion of the 16-20 May system to the appropriate part of the Suspects section.

**Done.**

1951 Storm #3, Baker:

1. In the metadata summary “on the 5th at 1630 UTC, aircraft reported…”. What is the position of the aircraft fix? Is it closer to the position of the baroclinic low analyzed on the microfilm maps? Or is it closer to the potential location of Baker? It is noted that if the aircraft-reported 996 mb pressure is correct, it was more likely associated with what was left of Baker, as the various maps strongly suggest that the central pressure of the baroclinic low was well above 1000 mb at the time. Please re-examine the proposed earlier dissipation in view of this.

**There were no aircraft flights on the 5th. The center fix at 1630 UTC on the 5th was from a ship passing through the eye, which is now so clarified. The position of the fix was 40.2N, 56.3W. It is agreed that it is more likely that this position is closer to what was left of Baker than the position of the baroclinic low. The earlier dissipation has been re-examined and has been removed. Revised positions have been determined by interpolating the fixes from 04/1225Z with 05/1630Z, but also adjusting for any available observations close to Baker’s center and for smoothness. The intensities have also been reanalyzed, and a 996 mb central pressure is added to HURDAT at 18Z on the 5th. The metadata summary has been completely revamped.**

1a. The above-mentioned aircraft fix in the metadata summary is not in the daily metadata for 5 August. Please fix this.

**The above-mentioned fix has now been added to the 5 August metadata paragraph. A couple of minor changes were also made to the metadata summary to change the word “aircraft” to “ship.”**

2. The committee concurs with the remainder of the proposed changes.

**Thank you.**

1951 Storm #4, Charlie:

 1. The introduction states that Charlie made four landfalls at major hurricane intensity. Is this correct? The track chart suggests only three landfalls at major hurricane intensity.

**If Cozumel is counted as a landfall, then it made four landfalls (three in Mexico and one in Jamaica). The reanalysis indicates that the center crossed the southern tip of the island of Cozumel.**

2. Please provide a stronger case for the proposed new genesis time on 9 August. While the surface maps show there was a low with significant winds in the eastern Atlantic, it is unclear from the current data whether this was a tropical cyclone or a more monsoonish low.

**Agree to delay the genesis until 00Z on the 12th, just six hours earlier than HURDAT. Discussion is included as to the observations on the 9th and their interpretation as either a low or a strong monsoon trough.**

 3. The daily metadata for 14 August has an excerpt from the MWR mentioning squalls of 90 kt. Is there any more information on this available?

**It can be inferred from the available information that the MWR text is a typo or error, and that they meant to say that the squalls of 90 kt occurred on the 15th. The 90 kt squalls were observed on the aircraft flight on the 15th as Charlie was approaching Martinique. This was the same flight that measured a 999 mb central pressure at 1450Z on the 15th near 14.5N, 59.7W. The flight on the 14th did not measure any strong winds as stated in the metadata paragraph. On the flight on the 14th, a message on microfilm stated that max winds encountered were 25 kt. The ATSR summary mentioned a very weak vortex that just barely had a closed circulation on the 14th. One sentence has been added to the August 14th metadata paragraph to explain this.**

 4. Please contact MeteoFrance on Martinique and the Meteorological Service of Dominica to see if they have data for this storm other than the normal six-hourly observations.

**MeteoFrance has no additional information for this tropical storm.**

 5. The landfall intensity in Jamaica need both re-examination and clarification. First, while it is clear that Charlie was a small and intense hurricane that weakened significantly after passing Jamaica, the proposed change of intensity of 30 kt in six hours on 18 August looks extreme – especially since subsequent hurricanes have hit Jamaica without such weakening. Second, the section of the metadata summary dealing with the central pressure estimates of from the Schloemer equation has a lot of hand waving, and at the very least it needs re-writing. Third, for other past hurricanes the Meteorological Service of Jamaica has published data from sites other than Kingston and Montego Bay, suggesting the possibility they might have more information. Please contact them to see if this is the case, especially regarding any additional pressure measurements.

**The intensity at 06Z on 18 August has been now been changed from 80 to 85 kt: To review the observations, an aircraft penetration measured a central pressure of 971 mb at 17/1837Z. 95 kt was selected for the intensity at 17/1800Z. Landfall occurred at 18/0300Z and 110 kt was analyzed (although it is possible that it could have been a Cat 4). The hurricane was over land until 18/0700Z when it emerged back over water. At 18/2112Z, an aircraft fix measured a central pressure of 982 mb. The analyzed intensity at 18/18Z is 75 kt. It is agreed to raise the intensity slightly to 85 kt at 06Z on the 18th, and this has been implemented in the metadata.**

**The Meteorological Magazine (1952) is a publication that discusses impacts on Jamaica, and the Meteorological Service of Jamaica has no additional information available.**

 The committee concurs with making Charlie a major hurricane at landfall in Jamaica, and it could be stronger than the proposed 110 kt. However, it is not yet ready to sign off on any specific value due to the uncertainty in the central pressure.

**Hopefully, the information provided in the response (above) and the associated changes made to the metadata will be sufficient.**

 6. Please contact the Meteorological Service of Mexico for any additional information they may have on Charlie’s two landfalls.

**We have obtained the available Mexican data from microfilm and from the EDADS website – microfilm says 965 mb (min p) at Cozumel, and the EDADS site says 65 kt E with 971 mb simultaneous pressure at Cozumel. The Meteorological Service of Mexico has no additional information regarding this hurricane.**

7. The discussion on whether Cozumel experienced the eye or not is confusing. Please re-write it.

**This section of the metadata has now been re-written. We know that the minimum pressure at the station in Cozumel was 965 mb. Since there were 65 kt east winds with a 971 mb pressure, it is likely that the eye of Charlie passed just south of the station. Since the station was likely over the northern part of Cozumel Island where the city is, the reanalyzed track is one or two tenths of a degree to the right of the previous track, and shows the center moving over the southern tip of the island, rather than south of the island. One aspect that remains uncertain due to a lack of enough observational evidence is whether the station remained outside the RMW or experienced the inside of the RMW.**

 8. Are the reported wind shifts at Tampico consistent with the station being near the southern edge of the eye as reported in the MWR? Does the landfall point need to be shifted to better fit the observations?

**The available observations – see plotted time series – indicate that Tampico experienced a direct hit.**

 9. Please re-write the section of the metadata summary dealing with the central pressure and intensity at the Tampico landfall. First, there is again a lot of hand waving. Second, the proposed best track should provide an idea of how fast the hurricane was moving at landfall. Third, Tampico does not seem to be “located somewhat inland”, which may change how its data was interpreted.

**The section about the central pressure and intensity at the Tampico landfall was not only re-written, but a 968 mb central pressure has now been added to HURDAT for the 19Z landfall and at 18Z. This is because Tampico is a coastal city and is not located “somewhat inland.” For coastal cities, the methodology is to typically disregard land exposure if a station is right on the coast and the winds are coming from off of the ocean. This yields a wind speed of 93 kts. Although 95 kt would normally chosen in this situation, there was an observed 95 kt wind. So 100 kt is kept using the methodology of selecting a wind speed slightly higher than the highest observation.**

1951 Storm #5, Dog:

 1. The metadata summary mentions use of a ship on 1 September that may have been misplotted. Is there a history of data from the ship that can be used to resolve this issue?

**This ship - #5461 – is from COADS, which unfortunately has no additional observations reported from the ship.**

2. Please contact MeteoFrance on Martinique to get the details of the 100 kt wind on 2 September, as well as any other information they have on this hurricane. Also, please do not state in the metadata summary “It is likely this is an estimated wind” unless you know for sure that it is.

**MeteoFrance in Martinque has been contacted and they were able to clarify that the measurement was a gust of 50 m/s (97 kt) in Fort de France at 1238Z. They also provided a lowest sea level pressure of 997 mb at 1130Z in Caravelle.**

 3. At 1800 UTC 3 September, the proposed intensity is reduced to 60 kt. While the reported pressure of 993 mb supports a lower intensity than the current 85 kt, the 90 kt winds estimated by the aircraft and the large area of estimated hurricane force winds suggests the proposed reduction is too large. Please re-examine the intensity at this time and at 0000 UTC 4 September.

**Agreed to boost the winds up to 70 kt, based upon a combination of the pressure-wind relationship, the quick forward speed, and the large area of estimated hurricane force winds.**

1951 Storm #6, Easy:

 1. While the committee concurs with the proposed earlier genesis, the proposed intensities need some re-examination and possibly some reduction. The 40 kt intensity at 1800 UTC 1 September seems to be based on a suspect ship a large distance from the center, while a second ship closer to the center had lighter winds.

**The “second” ship was actually the same ship but at any earlier time - 06Z. It is the case, though, that the analyzed 18Z position of the cyclone is farther from the 35 kt ship than the 06Z position and the 25 kt ship report. It is also noted the ship would have to been traveling at over 25 kt to have been in the locations reported at 06Z and 18Z. So it is quite possible that one (or both) positions of the ship given were incorrect. Given these uncertainties, we agree to bring the winds down some.**

 2. In regards to the reconnaissance flight of 4 September, what exactly did this flight encounter? The description in the metadata certainly sounds like the plane encountered a tropical cyclone, but the position given is a long way from even the original HURDAT track. Was the navigation on this aircraft off by over 100 n mi? Was there a second tropical storm rotating around the north side of Easy? Please obtain whatever data is available on this flight. It is also recommended that, unless it can be shown that the plane actually encountered Easy, this report not be used in any intensity assessment.

**No other additional information is available for this aircraft reconnaissance mission. Given the quite rudimentary navigation equipment available at the time, it is quite possible for the reported positions to be off by over 100 nm. There is no indication that a second tropical storm was rotating around the north side of Easy. Because of the uncertainties with this aircraft reconnaissance mission and what it was reporting, no changes are made to HURDAT on the 4th and early on the 5th.**

3. Is there a time available for the “off-time” ship observation on 5 September?

**Unfortunately, no.**

 4. The microfilm data show that the 120 kt flight-level winds on 5 September were measured using the single drift method. Can it be determined what level they were measured at, and does the associated intensity need to be revised?

**Flight-level winds in hurricanes conditions were extremely unreliable, due to the cumbersome, subjective, and uncertain way these data were estimated (not instrumentally measured) in this era. Please see page 4444 from Hagen et al. JClimate (2012) for more details on flight-level data. Thus the flight-level winds are not considered in the best track intensity determination for this era.**

 5. Please better explain why 95 kt was chosen for the intensity at 1800 UTC 6 September, and compared to 105 kt (closest to the appropriate wind-pressure relationship) or 100 kt?

**More discussion is included on the reasons of larger than climatological RMW and slow translational speed for going below the pressure-wind relationship. However, re-examining the case, it does appear that going 100 kt is more justifiable and this is now what is used.**

 6. The Navy aircraft in the hurricane early on 7 September had to turn back before it reached the eye. Given this, is there sufficient data to justify downgrading the intensity from 140 kt at 1200 UTC that day?

**While the Navy aircraft was unable to provide a penetration fix on the 7th, an Air Force reconnaissance did reach the center and provided a 937 mb central pressure at 1925Z (and is added into HURDAT at the 18Z slot). Given that reading and a central pressure from the 6th at 2006Z of 957 mb, it is simply analyzed that the hurricane intensified steadily during the 24 hour period. The 937 mb reading suggests around 125 kt from the pressure-wind relationship (with near average speed and climatological size). The intensities are ramped up from 100 kt at 18Z on the 6th to 125 kt at 18Z on the 7th. There is no indication that the hurricane peaked in intensity at 12Z on the 7th in between these central pressure measurements.**

 7. The microfilm archive for 8 September has a group of coded messages, including one that states “in eye now”. Have these messages been decoded to get the surface pressures? It is noted that there are no aircraft pressure in the metadata for 8 September.

**We were unable to decode the messages, but no central pressures were recorded on the 8th of September (actually no pressure at all reported) based upon the 12 and 18Z microfilm entries as well as the descriptions in the ATS.**

 8. Please resolve whether the 967 mb aircraft pressure reported near 1815 UTC 9 September was a central pressure or not. The daily metadata and the metadata summary are in poor agreement on this point.

**It is unknown whether this is a central pressure and thus it is not added into HURDAT. This is now clarified in the daily and summary sections.**

1951 Storm #7, Fox:

 1. Is the ship name in the 5 September daily metadata the Janecke Naess or the Janeckenaess? It is spelled both ways in the paragraph.

**The name appeared differently in the reports from the Monthly Weather Review and the Annual Tropical Storm, so these are exact quotes. It appears that “Janecke Naess” is the correct name, so this has been so noted.**

 2. In the 7 September daily metadata, an excerpt from the Annual Tropical Storm (ATS) report mentions an “average” wind of 82 kt. Is there indication what is meant by “average”? were these winds flight-level or surface? If they were flight-level, please re-examine the peak intensity to see if 100 kt is justifiable.

**See reply above for Easy regarding flight level winds.**

 3. Is there any record of the ship that reported 978 mb on 8 September in COADS?

**Yes, there were a few entries in COADS for this ship (the “Ruanine”). However, the strongest/deepest observations in COADS was 70 kt NE with 992 mb at 00Z 8 September.**

4. Please re-examine the interaction between Fox and Easy on 8-9 September. Given how close the two hurricanes were to each other, the tracks look remarkably smooth with no obvious sign of interaction. Please also better explain the basis for the changes in Fox’s track during this time.

**The tracks are consistent with some indirect Fujiwara interaction, as the slow speed of Easy from the 7th to the 9th to the west of the storms’ centroid along with the fast speed of Fox on the same dates to the east of the storms’ centroid is what one would expect. One consideration of the tracks is that Fox only had two center fixes total from aircraft reconnaissance, while Easy had ten total but only three on the 8th and 9th. Without inner core data at least a few times a day, one typically has quite smooth tracks. The significant eastward changes to the track of Fox at 00Z and 06Z on the 8th are due to the ship “Ruanine”, while the eastward changes at 18Z on the 8th and 00Z on the 9th are due to the aircraft reconnaissance and a few nearby ship observations. This is now discussed in the writeup.**

 5. In regards to the extratropical transition and dissipation on 10-11 September: Is it possible that Fox became the strong baroclinic low? Or is the data sufficient to show the northern low became the predominant system?

**With the data available, it is not possible to say with certainty whether Fox became the strong baroclinic low or whether the northern low became the predominant system. But a merger of the two lows does better describe what occurred, despite which one was predominant. The wording is now so changed in the writeup.**

 6. If the assessment of the extratropical transition does not change, then in the metadata summary please change “Fox had clearly been absorbed into a very large and powerful extratropical...” to “Fox had clearly been absorbed by a very large and powerful extratropical cyclone...”.

**The wording is now changed to: “observations show that Fox had merged with another baroclinic system and had become a very large and powerful extratropical cyclone before 12Z.”**

1951 Storm #8, George:

 1. The committee concurs with an earlier genesis time. Is it possible that genesis occurred as early as 0600 UTC 19 September? The microfilm map for that time shows an ob with 1008 mb and 30 kt likely north of the center, although this wind is much higher than the aircraft winds later in the day.

**It is possible, but there is enough evidence of a closed circulation before 18Z on the 19th. However, the possibility of an earlier genesis time is now discussed in the metadata writeup.**

 2. Is any additional data from Tampico available for this system?

**No additional observations are available from Tampico, Mexico for this system.**

 3. In the metadata summary “The cyclone is analyzed to have reached…”. Please re-write this for clarity.

**Done.**

1951 Storm #9, How:

 1. The committee concurs with the proposed later genesis.

**Thank you.**

 2. Please include a discussion of the frontal system that the Historical Weather Maps (HWM) suggested was involved with the storm. Was any cool air associated with the front entraining into the circulation of How?

**On 29 September, the frontal analysis in the northern Gulf of Mexico appears accurate, as there is a significant temperature and dew point gradient across it from north to south. On the September 30th HWM map, with temperatures in the 70s over Florida north of the warm front and with 12Z temperatures in the 60s along the north Gulf Coast, the front is significantly weaker, if there is still any front there at all. The front dissipates shortly thereafter, because by 1 October 12Z, there is no remaining evidence of a front. It is possible that some weakly cool and dry air may have entrained into the circulation of How around September 30th. It looks that any cool/dry air modified moderately as the front dissipated. Discussion regarding this front has been added to the metadata summary.**

 3. What data is available from the Fort Myers area to refine the landfall time and intensity? It would be interesting to see if the pressure there was lower than that well inland in Clewiston.

**The EV2 site did have the hourly observations from the Fort Myers airport station, which helped to fill in the gaps from the 6 hourly microfilm observations. 998 mb was the lowest hourly pressure observed at 1230Z and 1329Z (with SW 19kt and WSW 16 kt, respectively). Highest hourly winds were SSW 22 kt at 0830Z. These are consistent with the high end tropical storm making landfall to the north of Fort Myers.**

 4. In the 2 October daily metadata, please change “on it’s treck over Florida” to “on its trek over Florida”.

**Done.**

5. While the data on 4 October suggests the peak intensity was probably less than the original 95 kt, the 80 kt ship report west of the center that day suggests the possibility that the system was stronger than 80 kt. Please re-examine the intensity on 4 October.

**It is possible it may have been 85 kt on the 4th, due to the 80 kt ship (So. Ashville) at 00Z on the 4th. It is also possible that the ship’s winds may be biased slightly too high, as these observations were taken by Beaufort Scale. A reexamination of the reports from this ship reveals that the ship reported an observation 5 times from 10/3 00Z to 10/4 00Z, the first of which was 55 kt NE with 1003 mb at 30.4N, 79.5W at 00Z on the 3rd. While there are some other ships in the general vicinity of this ship at the time that reported lower, there might not be quite enough evidence from this map along that the ship’s winds are biased too high. At the 12Z and 18Z observations on the 3rd, So. Ashville reported 50 kt. At the 4th ob (the 21Z ob), we can compare the So. Ashville ob (32.4N, 77.6W) with the ob from the ship Boliva at 21Z (32.4N, 77.7W) since it was 0.1 degrees away. So. Ashville reported 60 kt NNW with 996 mb and Boliva reported 50 kt NNW with 996 mb. Since these wind magnitudes are similar with So. Ashville being located slightly closer to the center, So. Ashville’s winds appear to be reasonable. At 10/4 00Z, So. Ashville’s winds are 80 kt, but there is no ship report near it to compare. However, at 10/4 00Z, the ship Petersburg recorded 70 kt. After this thorough review of obs, it has been concluded that there is no evidence that So. Ashville’s winds are biased too high. Therefore, an intensity of 85 kt is analyzed at 00Z on the 4th, utilizing the methodology of assigning a wind speed 5 kt above the highest available wind observation. The 85 kt is only held through 12Z on the 4th, because an 80 kt intensity is still analyzed from 18Z on the 4th through 00Z on the 5th due to the 975 mb aircraft central pressure at 19Z on the 4th. The only additional changes made to the intensity are to increase the intensity from 80 to 85 kt on the 4th from 00Z-12Z.**

 6. The hand-plotted data for 0000 UTC 9 October show what looks like a low pressure center near 58N 20W, which is to the northeast of the proposed last position for how. Is it possible that the storm retained its identity into 9 October? Please re-examine the dissipation time.

**The system should indeed be extended substantially further in time as a high latitude extratropial cyclone and now is shown through 18Z 11 October.**

1951 Storm #10, Item:

 1. The committee concurs with reducing this system from a hurricane to a tropical storm. However, please state the justification for doing so more strongly.

**Thank you. The justification for downgrading it to a tropical storm has been made more strongly.**

 2. Is there anything known about the 997.3 mb minimum pressure mentioned in the MWR? The metadata does nothing except quote the report.

**No, but it was likely from the aircraft measurements. 997.3 mb vs. 998 mb is within the noise. In that same quote one sentence earlier, MWR mentioned that the peak conditions were measured by aircraft.**

1951 Storm #11, Jig:

 1. Please better analyze the thermal structure of the storm on 15-16 October (including any available upper air analyses) to see if the cyclone was extratropical or subtropical during this time. It should be noted that there are multiple aircraft reports in the 15 and 16 October daily metadata mentioning an apparent lack of convection near the center. This calls into question how tropical the system was.

**Further examination of the thermal structure reveals little to no surface baroclinicity and no frontal features near the center of the cyclone on the 15th through the 17th. Upper-level data, as plotted in the Historical Weather Maps, is not available within 250 nm of the center of the system on the 15th and 16th, so little about the inner core structure can be determined from these. The apparent lack of convection near the center is not inconsistent with either a sheared tropical cyclone or a subtropical cyclone. Certainly, the system could have been a subtropical storm, but without satellite imagery, this category cannot be used. These points are now included in the writeup.**

 2. The size of the RMW was mentioned for 18 October. Can they be calculated for the remainder of the storm’s life and mentioned in the metadata summary?

**The RMW calculation comes from converting the eye diameter to a rough RMW value, following the relationships identified in Kimball and Mulekar (2004). Only one eye diameter was provided by the aircraft reconnaissance for this cyclone.**

1951 Storm #12 (new):

 1. Many of the significant winds and pressure reported from this system come from one ship which traversed the cyclone, identified in COADS as #11054. If possible, please check this ship against other nearby observations to see how good the data is, especially in areas outside the storm. One example where the data looks a little suspect is the microfilm map for 1800 UTC 6 December. This ship reports 30 kt and 987 mb, suggesting a central pressure near 984 mb. None of the other nearby observations support such a tight pressure core.

**Multiple comparisons of this ship’s winds and pressures versus other numerous ships were available for a few days. These data suggest that the observations from this ship had little to no bias in either parameter.**

 2. Please obtain data (including upper air data) from the Azores during the passage of this system on 9-11 December. It is noted that the 12Z rawinsondes plotted on the HWM show a significant warming at 500 mb in the Azores from 10-11 December. Are other rawinsondes available from the island station that would provide more details?

**Yes, all available rawindsondes have been obtained and can be viewed within NAWIPS via “narchive 1952 feb2-3”. These do suggest mid-tropospheric warming took place in connection with this cyclone being in the vicinity of the Azores.**

 3. Are any SST data or analyses available for this system for days other than 6 December?

**Yes, SSTs are routinely available in COADS and plotted in the HWM. From the 7th through the 10th, the SSTs in and around the cyclone are in the mid-60s F.**

 4. The committee tentatively concurs with the addition of this system pending the quality control check on the ship in point 1.

**Thank you.**

 5. [Richard Pasch] I am not so comfortable with the inclusion of the last storm, 12, from

December 1951. Whereas it does appear to be non-frontal and the strongest winds appeared to be fairly close to the center, Roth cites an upper-air report from a weather ship near the center of a 500 mb temperature of -14C. This seems awfully cold for even a subtropical cyclone to me. However, I will give a shaky thumbs up for this system.

**It may be that the time of year and location would not preclude a tropical cyclone (or subtropical cyclone) from having SSTs around 18-19C with 500 mb temperatures of -14C. Indeed, one would have to have cooler mid and upper level tropospheric temperatures to still obtain conditionally unstable conditions with SSTs of relatively cold 18-19C.**

1951 Additional Notes:

 1. Suspect of 18-23 February: Please re-examine this system on 18-19 February. The microfilm map for 0000 UTC 19 February shows a 50 kt ship report close to the center (with a second 50 kt ob well north of the center), and comparison to the map for 1800 UTC 18 February shows that significant deepening occurred during that six hour period. On the other hand, the HWM suggest that a secondary surge of cold air entered the system surface and aloft and that the deepening could have been baroclinic. Please examine the thermal and wind structures to see if this system was possibly a subtropical storm (or perhaps a frontal hybrid) on 19 February.

**Given the secondary surge of cold air entering the system around 00Z on the 19th, it is likely that the significant deepening observed (two 50 kt ships and 996 mb) was at least in part baroclinic in origin. However, it is acknowledged that the system could have a frontal hybrid, which is now mentioned in the writeup.**

 2. Suspect of 8-10 March: The committee concurs with leaving this system out. There is a 30 kt ship east of the center on the 9 March HWM. Is this report in COADS, and if so, are there any other reports from it while it was near the low?

**Yes, this ship is in COADS and it was reporting every 12 hours. The peak wind it had was 25 kt SE at 12Z on the 9th and lowest pressure was 1011 mb (with 20 kt ENE) at 00Z on the 10th. (Note that the Historical Weather Maps were plotted in mph, not kt. Thus the observation in HWM is also 25 kt, not 30 kt.)**

3. Suspect of 15-20 May: Please re-examine this system. It is noted that the observations of 1008 mb on 18 May are accompanied by 20-35 kt winds, suggesting a central pressure of 1004-1006 mb. In addition, the pressure gradient was apparently quite strong northeast of the center in an area where there were no ship reports. If possible, please calculate the wind based on the observed pressure gradient to see if it exceeded 35 kt. Finally, please provide a spreadsheet data file for this system.

**The peripheral pressures do indicate a central pressure of 1004-1006 mb. However, given the system’s location and time of year, the standard pressure-wind relationships may not be as applicable and thus not provide a second observational piece of evidence that the system reached tropical storm intensity. The strongest pressure gradient with no ships available appeared to be on the northeast quadrant on the 18th at 00Z – 9 mb over ~150 nm. This gives a gradient wind calculation of 18 m/s (35 kt). Assuming a ~20% reduction to the surface, this would support about 28 kt. A spreadsheet for the suspect is now provided.**

 4. Suspects of 20-22 June and 24-25 June: The HWM show a 35-kt ship near 17.5N 83W on 21 June, and suggest a low pressure area was centered near or north of the coast of Honduras on 22 June. They also show a ship report of 1006 mb with a southwest wind over the Bay of Campeche on 23 June. Please re-examine these systems to see if 1) they were one and the same system, and 2) the system became a tropical storm at any point. Also, please provide scanned maps of the cases.

**After further review, it is likely that these two suspects were the same. The writuep now indicates one disturbance from the 20th through the 25th of June. Note that HWM of this era indicated winds in mph, not in kt. Thus the “35” report is 35 mph or 30 kt. There were no 35 kt or stronger winds reported and the lowest pressure indicated was 1006 mb. Thus it may have been a tropical storm, but the available observations do not confirm this. Scanned maps of the system have now been provided.**

 5. Suspect of late August-early September: Two questions on this system – What was the wind in Charleston at the time of the 1004 mb pressure, and are there any more details available about the obs from Frying Pan Shoals? The committee concurs with leaving this system out pending the requested data check.

**The original records were obtained via EV2 for Charleston, both the airport and the city office stations, but the time of the 1004 mb pressure was not recorded. It may have occurred near the time of the peak winds, but this is not certain. No additional observations were available for Frying Pan Shoals.**

 6. Suspect of 16-25 October: Please re-examine this system. First, the observation from one of the Cape Verde Islands has 20-30 kt winds at the time of the 1005 mb pressure, suggesting a lower central pressure and the possibility this was a tropical storm. Second, what are the details of the other 1005 mb observation on 17 October? Third, the Meteorological Service of the Cape Verde Islands or NCDC should be contacted to obtain more detailed obs from the islands. Finally, please provide scanned maps for this case.

**It is agreed that the Cape Verde Island observation would suggest a central pressure of 1002-1003 mb and that the system might be a tropical storm. This is now added in the discussion. The other 1005 mb observation was from a ship at 14.0N 21.7Z with 15 kt SE wind at 06Z on the 17th. Unfortunately, no additional information is available from the Cape Verde Islands or NCDC. The scanned maps for the 16th to the 25th have now been provided as requested.**

 7. The committee concurs with leaving the remainder of the suspect systems out of HURDAT. Please provide scanned maps of the 31 October - 4 November case.

**These maps have now been scanned and included.**