

Best Track Committee Re-Analysis Comments for 1950
Replies to comments given in boldface – Hagen/Landsea – February 2014

General comments:

1. There is a note in the Air Weather Service (AWS) season report about how, during Hurricane Able, central pressures measured by dropsonde were often 10 mb higher than those reported during low-level penetrations. Does this apply to the other storms of 1950, or any of the other storms of the early aircraft era? When the data is available to do so, please check the reported central pressures of the 1950 storms against those that would be extrapolated from 700 or 850 mb. This should help locate suspect central pressure reports.

1950 was the first year that dropsondes were being mentioned. There were several times in the reanalysis of 1950-1953 (more often in 1950) where the dropsonde pressures were higher than the low-level penetration pressures. It is believed that most of the time when dropsondes were dropped, they were dropped from a height of 700 mb. Since the art of dropsonde dropping was in its infancy, it is likely that they were not been able to always hit the middle of the eye. On low-level penetrations, extrapolated central pressures from flight-levels of 1000 ft or less were likely accurate to within 2 or 3 mb. During the reanalysis, the methodology was to take the lowest reliable central pressure reading, the most common of which were low-level penetration central pressures without dropsondes. As it was observed that many dropsonde pressures were 5 to 10 mb higher than low-level penetration central pressures, it was decided to treat all dropsonde pressures as an upper-bound for what the central pressure could have been, and these dropsonde pressures were not placed into HURDAT.

2. As in 1949, there are several instances of “possible central pressure” in relation to aircraft data throughout the submission. These are confusing. Please determine whether these are central pressures or not, and state this unequivocally in the text.

Where possible, it is now stated whether or not a pressure is a central pressure measurement. However, for some of them, it is impossible to know with certainty. In cases like that, a central pressure value is not added to HURDAT.

3. There are several aircraft winds in the submission which appear rather precise (not rounded to the nearest 5 kt), and there are multiple references to winds “measured” by aircraft. Were the aircraft in 1950 carrying instrumentation what would allow measured flight-level winds? If so, what was the instrumentation and what is known about the error characteristics? Was there a mix of measured and estimated flight-level winds during this season? The committee notes that neither the AWS report nor the Navy’s 1950 Annual Tropical Storm Report (ATSR) mentions anything about such instruments. On a related note, if flight-level winds are mentioned in the various metadata summaries, please include a note on what the flight level was.

The “estimated” aircraft winds were visually estimated by the aerologist. Whenever the word “estimated” it used, it is almost always associated with visually estimated surface winds based on viewing the sea state.

The “measured” winds were not actually measured by an instrument. As described in Hagen et al. (2012), these were calculated by taking drift measurements. The navigator calculated the position of the aircraft and the flight-level winds. If the navigation was accurate, then the flight-level winds would likely be reasonable representative. If the navigation was off, it would throw off the flight-level winds calculations. When the plane was in strong winds (roughly hurricane force), the navigator would often fall behind on these calculations since they were conducting manual calculations to determine the flight-level winds and current position of the aircraft). The uncertainty and errors in both the flight-level winds and current position of the aircraft would increase substantially as flight-level winds increased. This is why flight-level winds in high wind conditions are not reliable for the early reconnaissance period.

4. On a related note, there are multiple references to flight-level winds being reduced to the surface in the write-up for Hurricane King. Is this based on these winds being measured? Second, are the reduction factors mentioned (0.77-0.79) appropriate based on current practices? Please clarify this and modify/remove the comments if necessary.

For Hurricane King and a few other storms, the reanalysis methodology attempted to determine what the flight-level winds would yield if they were adjusted to the surface, using the appropriate factor from Franklin et al. (2003). These values were not weighted highly, if at all, to make changes to the HURDAT intensity. But after further consideration of the issue, reducing flight-level winds to the surface in this early era of aircraft reconnaissance has such large uncertainties and unknown biases that this has been removed from the reanalysis.

5. Please make sure that the binders (and their online equivalent) have scanned copies of the Historical Weather Maps (HWM) and microfilm maps for all of the suspect systems.

The 1951 binders now have scanned copies of the Historical Weather Maps for all of the suspect systems.

6. Please remove all references to rainfall totals or property damage unless they are needed to show the location/intensity of a cyclone.

So removed.

7. There are many parts of the submission that give the impression that the authors were thinking out loud or speculating. Some of them will be highlighted in the individual storm sections below. Please remove or clarify all of these statements.

So removed.

8. In situations where there was conflict between aircraft and ship winds, which received the most weight in the proposed intensities? Please clarify this where necessary.

Ship winds (which were usually Beaufort Scale) received more weight than aircraft winds. However, ship pressures were weighted equally with aircraft extrapolated pressures on low-level penetrations. This is now clarified in the metadata write-ups.

9. The HWM suggest there were weather ship taking upper air soundings during 1950, as plotted on the 500 mb charts. Please examine these data when necessary. They may prove very useful in determining the nature of the proposed new storms #15 and #16.

Yes, there were a couple of weather ships in the Atlantic that were taking surface observations and some upper air soundings. These are now examined where appropriate, including in helping to determine the nature of new storms #15 and 16.

1950 Storm #1, Able:

1. In the 8 August daily metadata, there are references to “AINMP EIN”? What are these and what do they tell us about the cyclone/potential cyclone?

These refer to routine aircraft reports that were radioed back to the Air Weather Service. However, the microfilm charts only reach to about 60W longitude, and thus do not show these observations. Plots of available ship observations on the 8th are very sparse and do not allow for beginning Able this early.

2. In the 12 August daily metadata, there is a reference to a recon flight wind shift indicating a vortex north of the aircraft. Is there any plot of this data available?

Yes, these are plotted on the microfilm pages at 12 and 18Z on the 12th and 00Z on the 13th. The notation is with square observations with the “D-1”, “D-2”, “D-3”, etc labels.

3. The justification for the 995 mb central pressure at 1800 UTC 13 August appears shaky, especially since an aircraft central pressure three hours later was higher. Please provide a better justification or remove this central pressure.

Agreed to replace this with the observed 997 mb.

4. Can the Cape Hatteras and Nantucket references in the 20 August daily metadata be removed? They seem to contribute little to the knowledge of the intensity.

Some of the observations are removed, but most retained as it is important to note whether North Carolina and Massachusetts received tropical storm force wind impacts.

5. Has Environment Canada been contacted for additional information on the storm in Nova Scotia?

Unfortunately, Environment Canada has no additional information available for this storm.

6. What is the scientific basis for the method of determining the radius of maximum wind (RMW) stated in the metadata summary – multiplying the aircraft-reported eye radius by 1.5? If this is not determined to be a good methodology, please re-calculate all of the RMW's that follow.

As described in Hagen et al. (2012): “An eye diameter was often reported by the aircraft, which can be converted to an RMW using the Kimball and Mulekar (2004) relationships.” This 1.5 relationship in Kimball and Mulekar was obtained from the extended best track database comparing the RMWs versus the eye diameters.

7. Please better explain the basis for the 95 kt intensity at 1800 UTC 20 August. Is it possible this is too high given the latitude and the observed size of the RMW?

It is agreed to reduce further the winds to 90 kt (originally 105 kt) based upon the high latitude and very large RMW.

1950 Storm #2, Baker:

1. The committee notes a west wind on the 18 August HWM near 11N 52W. Could the genesis be extended back to that time, especially in light of the proposed 50 kt intensity on the first HURDAT point on 20 August?

Agreed to begin genesis 42 hours earlier farther to the east.

2. The reductions in intensity on 21 August appear justified. However, they leave a discontinuous jump to a 90 kt intensity at 0000 UTC 22 August. Please re-examine this intensity to see if it should be lowered, explaining how the Antigua data was taken into account.

This is brought down slightly to 85 kt at 00Z 22nd, smoothing through the intensity changes. Given the lack of additional quantitative information from Antigua (see below), a low end Category 2 hurricane (85 kt) would be consistent with the wind impacts described on the island and also provide a more realistic intensity change.

3. Please contact the Meteorological Service of Antigua for additional information on the passage of this system over Antigua, Barbuda, and Montserrat, especially on whether the winds at Antigua were measured or estimated. Since Baker seems to be rapidly weakening as it passed Antigua, it may take detailed data to help calibrate the actual intensity.

The Meteorological Service of Antigua has been contacted about this hurricane. However, they have no additional information about it.

4. Please include the land station highlights for 23 August. It is noted that the microfilm map for 1200 UTC that day suggests a pressure below 1005 mb. What is the basis for that?

Typically, no highlights are included when there are no low (1005 mb or deeper) pressures or high (35 kt or stronger) winds. However, a 30 kt observation from Isabella, PR is now included. The 1005 mb isobar was based upon a 1003 mb aircraft measurement, which had already been included in the daily summary.

5. Please obtain whatever additional observations are available from Haiti, the Dominican Republic, the Turks and Caicos, and the Bahamas for 24 August. This may provide additional information on the location and intensity that day. It is noted that the microfilm maps suggest the possibility that the center jump re-formed from south to north across Hispaniola that day.

No additional observations are available from these countries. The possibility of the center jumping/reforming from south to north across Hispaniola on the 24th has been added into the summary.

6. Near the bottom of the 28 August daily metadata, there is reference to the ship **Rincon Hills**. Was this ship included in the ship highlights for that day?

Yes, but the ship name had not been included originally.

7. In the 30 August daily metadata, there is a remark stating “not sure if central pressure...”. Please clarify whether this aircraft pressure was a central pressure and remove the remark.

It would be wonderful to clarify these cases with certainty. Unfortunately, this is not to be, as it has been left ambiguous with the information provided to know whether this was a central pressure or a peripheral pressure measured by the aircraft reconnaissance. Given that it is unknown, the pressure value is not added into HURDAT.

8. In the metadata summary, please clarify why the intensity on 30 August was set at 90 kt.

In the absence of data to show otherwise, it is agreed to retain the 95 kt in HURDAT on the 30th, while over the Gulf of Mexico.

9. In the 31 August daily metadata, there is a remark about the Santa Rosa Island winds stating “gust or sustained? Measured or estimated?” Please clarify which of these possibilities is the case and remove the remark.

No further information could be obtained about this observation, as it is not available in the EV2 website. The measurement remains ambiguous.

10. What is the source of the Fort Morgan, Alabama observation mentioned by Connor? Was it a sustained wind or a gust? What is known about the sampling interval and elevation of

the anemometer? Are more details available? It is noted that Connor does not state that the 979 mb ob was a central pressure as asserted in the 31 August daily metadata. What is the basis for saying it was?

No additional details about the Fort Morgan observation were obtained, after a search of the EV2 website. Connor's "Estimated Lowest" column was for the cyclone's lifetime lowest central pressures either observed or estimated. Thus he considered the 979 mb from Fort Morgan to be a central pressure, as did Ho et al.

11. On a related note, are any observations available from the Pensacola Naval Air Station, located between Fort Morgan and the Pensacola airport station?

Yes, the Pensacola Naval Air Station hourly observations are available and do indicate a minimum observed sea level pressure of 988 mb with a simultaneous observed peak wind gust of 65 kt S at 0247Z. These measurements are consistent with a Category 1 hurricane making landfall about 35 nm to its west.

12. In the metadata summary, what is the basis for analyzing that Baker weakened over western Cuba?

It is agreed to retain the 50 kt instead of showing a slight weakening when clipping the westernmost tip of Cuba.

1950 Storm #3, Charlie:

1. Do the proposed track adjustments on 21-23 August allow for a consistent speed of motion during this time? Is there a possibility that the west wind referenced at 1200 UTC 21 August as part of these changes is bad data?

It is agreed to indicate a more realistic motion than the southwestward track shown both in HURDAT and the draft reanalysis. It is certainly possible that the 12Z 21st west wind is bad data, which is now so mentioned.

2. On the microfilm map for 0600 UTC 28 August, it looks like there is an observation marked D-10 with a pressure of 998.5 mb. Is this the center fix mentioned in the daily metadata summary ("circling in eye of hurricane")? If so, is this a central pressure? It is noted that the metadata summary mentions a 985 mb aircraft central pressure that is not referenced in the daily metadata. Is this referring to the D-10 observation, and is the 985 mb correct or incorrect? Please clarify this and change the proposed intensities if necessary.

This is a 998.5 mb pressure (incorrectly interpreted to be 985 mb), though it is uncertain if this is a central pressure reading. It is treated as a peripheral pressure value, especially given the 981 mb pressure observation from ship at 21Z.

3. The metadata summary and the 28 August daily metadata both reference a low pressure reading based on pressure changes on a ship. Please state which microfilm map this data is on.

This is from the 00Z 29th micro map “Pressure rose 27 mbs in last 3 hrs” for the ship Washington.

4. While the plotted aircraft fixes on the small-scale track chart appear conclusive that the loop on 30-31 August did not occur (barring major navigation errors), it is interesting that both HURDAT and the AWS report chart showed a loop occurring. Can it be determined why the looping tracks were originally chosen?

No, as no justification for the loop was described in either the AWS report or the MWR writeup.

5. In regards to the Air Force recon fix on 1 September, is any pressure available?

Unfortunately, no.

6. Please re-write or delete the discussion of the central pressure on 30 August employing the Schloemer equation. It is confusing.

So removed.

1950 Storm #4, Dog:

1. Please clarify the time of the report from the **Sibrodin** mentioned in the 29-30 August daily metadata. It is unclear from the microfilm maps and the AWS report whether the first gale was observed at 1800 UTC 30 August or at the time hand-written on the map, 2000 UTC 29 August.

The 20Z 29th report from the Sibrodin mentioned “disturbance” but not quantitative information. The first definitive gale is at 18Z 30th.

2. What was the basis for moving the track closer to Antigua? Was it the surface observation or other fix data? Please state this in the metadata summary.

The revised track shows Dog to have moved two-tenths of a degree closer to the island based upon both surface observations and aircraft reconnaissance.

3. Please contact the Meteorological Service of Antigua for more details on the extreme conditions, particularly when the worst conditions were experienced and the associated metadata for the station. It is noted that the 125 kt wind report from Antigua is not consistent with the intensities suggested by the aircraft fix pressures.

We have contacted the Meteorological Service of Antigua, but they have no additional information regarding this hurricane. Given that it is unknown whether the 125 kt observations is a gust or sustained wind and that the 97-113 kt values obtained from pressure-wind relationship several hours later values are not consistent with the 125 kt observation from Antigua being a sustained wind, an intensity of 115 kt is analyzed for late on the 31st and early on the 1st.

4. Since the eye appears to have passed close to St. Martin/St. Maarten, please contact the appropriate meteorological services to obtain the data recorded from the storm.

The French and Dutch Meteorological Departments have been contacted regarding this hurricane, but they have no additional information.

5. The committee concurs with the proposed reductions in intensity on 3-7 September given the available pressure data. Since the eye and the RMW apparently increased in size during this time, please provide more information on how this change was accounted for in the proposed intensities.

It is acknowledged that the rather large downward revisions from the 3rd through the 6th are done with only semi-quantitative information, but there was enough confidence in these to make the adjustments. However, these reduced intensities on the 6th are consistent with a substantially weaker hurricane late on the 7th, as measured by aircraft central pressure at that time. Some additional discussion has been added.

6. There is a typo in the 12 September daily metadata: "Micorfilm".

Corrected.

7. The section in the metadata summary regarding the passage near Nantucket should be re-written for clarity.

This has been rewritten as requested.

8. If necessary, please contact the United Kingdom Meteorological Office for the impacts of the extratropical portion of Dog on the British Isles.

We have contacted the United Kingdom Meteorological Office and they were able to provide some additional information. Specifically, peak gusts of 70 and 78 kt were recorded in England, consistent with the 65 kt sustained wind analysis provided earlier for 00 and 06Z on the 17th.

1950 Storm #5, Easy:

1. Please re-examine the intensities from 1800 UTC 2 September to 1200 UTC 3 September. First, both of the quoted low pressures from the Isle of Pines were apparently

measured outside of the RMW, which suggests a central pressure below 993 mb at the time of the measurements. Second, the 0000 UTC 3 September microfilm map appears to show an observation of 992.6 mb with 35 kt winds. Third, there is a ship report of 75 kt and 987 mb at 0300 UTC 3 September, which suggests a central pressure in the 980-985 mb range. However, the subsequent 989 mb pressure in Havana (apparently inside the RMW) casts doubt on how good the ship report is.

The central pressure around the time of landfall in Cuba at 07Z on the 3rd may have been somewhere in the neighborhood of 980 mb based on the data during the 12 hours leading up to that landfall. If the 10 kt per mb rule is used for an upper-bound of what the central pressure could have been at times of specific observations, and assuming all of the obs can be trusted, then the central pressure was at most 995 mb at 02/1800Z, 992 mb at 02/1900Z, 989 mb at 02/2000Z, and 980 mb at 03/0300Z. As the aircraft central pressure is in the same ballpark based upon these measurements, this 995 mb aircraft central pressure at 1830Z is retained.

1a. Please contact Perez for additional information on this system, including more detailed observations from the Cuban stations. Also, please examine how good the 987 mb ship report is.

Ramon Perez has been contacted, but has no additional information for this Category 1 hurricane impact in Cuba. On the 03Z map, the ship "N Gorona" has a 75 kt wind barb with "870" which mean 987.0 mb. Another faint wind barb near 21.4N to the south of the N Gorona appears to say "74 K" to the right of it, which means 74 kts. The position of one of these observations may be plotted slightly incorrect. Unfortunately, no additional observations can be found from the ship "N Gorona."

2. Please re-examine the aircraft-reported 980 mb central pressure near 1310Z 3 September. The data in the ATSR shows tropical-storm-force winds at the time of the pressure, suggesting it was not a central pressure.

The data in ATSR at 20Z on the 3rd shows 5 kts with 980 mb central pressure inside the eye. So the 980 mb for 20Z appears to be more certain. It seems that there may be some conflicting info as to when the 980 mb was measured. This is because on 3rd, 980 mb was recorded at 13Z and again at 20Z, and on the 4th, 973 mb central pressure was recorded at 1454Z and again at 1915Z. Back on the 2nd, 996 and 995 mb central pressure were recorded by aircraft several hours apart as well. It is possible that since these values are nearly identical perhaps they only pertain to one time each of these days, and the other flight was just using the central pressure obtained from the other flight in their report. Regarding the 980 mb central pressure at 1310Z on the 3rd reported in the Air Weather Service report, it seems unlikely that the hurricane would have been stronger at 1310Z and then weakened by 20Z. While this is of course possible, the confusing data does not allow for any additional increases to the wind speed in HURDAT at 12Z on the 3rd due to uncertainty as to what the central pressure was at 1310Z.

4. Please review the 973 mb aircraft pressure at 1454 UTC 4 September. While the text of the ATSR says 973 mb, the associated code and map say 983 mb, while the report on the microfilm map says 975 mb.

The ATSR says 973 mb at 1454Z. Microfilm says 975 mb at 1452Z. Air Weather Service Report (AWS) says 973 mb at 1915Z. It seems likely that a 973 mb central pressure was measured by an aircraft at least one time on the 4th. The data within the same source and among different sources is not consistent, so it is unlikely that we will know with certainty.

5. In the metadata summary, please re-write the discussion on whether Easy made landfall at Cedar Key or not. If it made landfall, please clearly state when and where. If it cannot be determined that the circulation center crossed the coast, then the event should be removed as a landfall and counted as a strike.

The metadata summary has been re-written to better clarify Easy's landfall. The center of Easy's eye made landfall at 17Z on the 5th at 29.1N, 82.8W.

6. Please try to obtain more information on the type, averaging period, and elevation of the Cedar Key wind data. Also, please find out if a pressure was measured at 1800 UTC while the station was inside the eye. This would help refine the central pressure at the time.

The anemometer height was 25 m and the averaging time was 5 min with 89 kt N. (This converts to 88 kt going to 1 min and 10 m.) There appears to be no available pressure data from Cedar Key after the 958 was measured at 1530Z. However, additional data from Cross City was obtained, which shows that the RMW never reached Cross City. When Cross City recorded its minimum pressure of 992 mb, winds were 48 kt from the NNE. The highest wind of 61 kt at Cross City was recorded shortly after that as the pressure was rising. This was found on the EDADS website – Surface Weather Observations (SWO), then typing in 09/05/1950 and clicking on the Cross City data. Ocala's min pressure was 992 mb around 22 UTC on the 5th. Winds were not as strong at Ocala as they reached at Cross City. The Florida Climatological Data has max wind and min pressure data from other stations.

7. It is noted that the lowest pressure at Cedar Key occurred outside the eye. Please add a comment to the metadata summary on the possibility that this was due to an eyewall mesovortex, as apparently seen in the 1947 hurricane in South Florida and New Orleans.

Done. A sentence has been added to the metadata summary.

8. What is the basis for the proposed 90 kt intensity for Easy's final landfall in Florida? In addition, please change the description of the landfall location to something other than "several dozen miles north of Tampa".

The 90 kt intensity for Easy's final landfall in Florida has substantial uncertainty, as there were no measurements of wind or pressure inside the radius of maximum wind. However, Anclote Light was still recording 70 kt (at 33 m) with 986 mb at 07Z, 3 hours after landfall.

This converts to 64 kt adjusting to a 10 m wind. Running the Kaplan-DeMaria inland decay model at 07Z, 3 hours after landfall suggests maximum winds at that time of about 68 kt, assuming a 90 kt landfall intensity. Thus, 90 kt at landfall is not inconsistent with the Anclote Light observations three hours later. Additionally, it did appear with the preceding information at Cedar Key that the cyclone was gradually weakening before final landfall. Thus the initial landfall at 17Z was 105 kt, the 18Z intensity 100 kt, the 00Z 6th intensity 95 kt, and the 04Z final landfall was 90 kt. This gradual weakening is also generally consistent with the original HURDAT analyses.

“Several dozen miles north of Tampa” has been changed to “about 15 nmi north of New Port Richey, FL.”

9. What is the basis for the proposed 75 kt intensity at 0600 UTC 6 September, given that the Inland Decay model says 73 kt, the original HURDAT says 65 kt, and the highest observation is 64 kt? Would 70 kt be better?

75 kt was chosen utilizing the methodology to round the inland decay model value to the nearest 5 kts. Additionally, an analysis of a 70 kt intensity at 06Z would mean a 20 kt decrease in the first two hours after landfall from 90 kt at 04Z to 70 kt at 06Z. Instead the analysis is 90 kt at 04Z, 75 kt at 06Z, and 60 kt at 12Z. Additionally, there were no observations inside or at the RMW at this second Florida landfall, so an analysis 10 kt higher than the highest wind observation is appropriate. Therefore, 75 kt is kept.

10. It is noted that radar fixes were available from Key West for Hurricane King a month later, but there is no mention of them in the write-up for Easy. Were radar fixes available from Key West/Boca Chica during this storm?

There are no radar fixes available from Key West that could be found in the sources utilized, which includes the same sources/references as King.

1950 Storm #6, Fox:

1. The committee notes a comment in the AWS report stating that the aircraft fix positions are questionable, as is the path of the storm center. Given that, please re-examine the track changes based on the aircraft data, especially on 10-11 September, to see if they are justifiable.

It is agreed that the variability (wobbles) in the track on the 9th through the 11th are not justifiable based upon the available observations and the uncertainty in the aircraft reconnaissance fix locations. However, there is some justification for the major eastward adjustments on the 10th due to the combination of the aircraft fix and a fortuitous ship measurement. The track has been smoothed, but still adjusted eastward some on the 10th and 11th.

2. In metadata discussion, there is a typo in “Major track changes are made at 06 to on the 10th”.

This is now corrected to say: “Major track changes are made at 06Z through 18Z on the 10th.”

3. The 10 September daily metadata mentions a ship report from the Luciano Manara at 0300 UTC that day. This observation is not mentioned in either the ship highlights for that day or in the Excel spreadsheet. Please find this observation if possible, since it alerted forecasters that Fox had formed.

Unfortunately, this observation is not accessible. However, a 12Z measurement from the same ship is available and is useful for determining the cyclone’s center on that date.

4. Please review the central pressures and intensities on 14-15 September. First, the discussion on applying Schloemer’s Equation for the 14 September flight data needs to be rewritten for clarity. Second, the microfilm maps for 15 September report a dropsonde measured pressure of 975 mb. Is this a central pressure? If so, it needs to be entered as such in HURDAT, and it casts doubt on the 946 mb estimate 6 h earlier.

The discussion about the Schloemer’s equation has been rewritten for clarity. The aircraft-deployed dropsonde measured a sea level pressure of 975 mb around 1830Z – about three and a half hours after the earlier fix – which was likely launched in the periphery and not in the eye. This aircraft apparently did not perform a penetration fix.

5. Regardless of what intensities are chosen, it should be noted that the lack of central pressure measurements make the intensities of Fox more uncertain.

This uncertainty has been accentuated.

6. The committee concurs with the reduced intensities during the final weakening and extratropical transition.

Agreed.

1950 Storm #7, George:

1. There is a typo in the 27 September daily metadata: “wind continuous sheet, lightning”.

Corrected. This is actually: “vivid continuous sheet lightning”.

2. On 3 October, an Air Force plane reported a 969 mb pressure at 1230 UTC. If this is a central pressure, please add it to HURDAT.

It appears, though this is not certain, that this was a central pressure. It has now been added in, but with that caveat.

3. A structural quirk of George is that it had a large pressure envelope, but a relatively small wind field as shown by the lack of tropical-storm-force winds on Bermuda. Has this been taken into account in the intensity assessment, especially for the proposed 90 kt intensity at 1200 UTC 3 October?

The structural quirk of George having a large outer circulation, but relatively small inner core is mentioned. Based upon the eye diameter, it is estimated that the RMW was about 25-30 nm with climatology for 969 mb and 32N being 25 nm. Thus with a near average RMW – as best that can be determined – a substantial alteration for the wind suggested by the pressure-wind relationship is not called for.

4. Please state the basis for speculating the George might have been a Category 3 hurricane or remove the speculation.

This speculation has been removed.

1950 Storm #8, How:

1. Do the proposed changes to the position on 1 October match the observations as well as smooth out the initial acceleration?

HURDAT started this system at 06Z on 1 October as a 30 kt tropical depression at 25.0N, 87.3W. However, HURDAT previously showed a dramatic deceleration during the first 12 hours of the life of this cyclone. The revised 06Z position on the 1st corrects this unrealistic speed change by shifting the initial location by more than 1 degree west-northwest of the previous HURDAT position, consistent with the sparse observations available.

2. Is the speculation that this system might not have had a closed circulation necessary? While the wind circulation was not well defined, the reported central pressures appeared to be sufficiently lower than the environment to support a circulation. Please re-write this section of the metadata summary or remove it.

Much of this discussion has been removed and the remaining portion has been revised.

3. Please contact the Meteorological Service of Mexico for more data on this system.

The Meteorological Service of Mexico has no additional information on this system. However, the EV2 website did provide new observations from Soto La Marina (23.8N 98.2W). Their observations were only at synoptic times and only for sea level pressure. Their lowest was 1010 mb at 12Z 4 October. This allowed for a more accurate assessment of landfall around 12Z as a tropical depression.

4. The microfilm maps include a report from the ship **Banpico** at 0700-0800 UTC 4 September that included southerly 25-30 kt winds, 12 to 15 ft swells, and a pressure of 29.58 inches. The position is given as 35 miles south of KSWA, which is an unknown identifier. This report is not included in the daily ship highlights or the Excel spreadsheet. Please examine this report to see 1) if it is related to How, and 2) is there a need to change the best track based on it?

KSWA is Swan Island. This ship, while the winds may be realistic, appears to have a barometer that is substantially too low. This is now discussed in more detail in the Additional Notes section for the disturbances from 3-7 October in the Caribbean/Gulf.

5. What is the basis for changing the landfall time in Mexico by three hours?

With the additional information available from Soto La Marina, the original landfall time and place from HURDAT have now been retained.

1950 Storm #9, Item:

1. Please reconcile the proposed origin of this system with that of the AWS report, as well as the relationship of this system to a low pressure area listed under suspect systems #5. The AWS report presents significant detail on how Item originated from a disturbance that formed over the southwestern Caribbean Sea and subsequently drifted northwestward across the Caribbean and the Yucatan Peninsula into the Gulf of Mexico. This description appears to include the low pressure area off of the Yucatan Peninsula on 5-6 October that is listed as a tropical depression in suspect systems #5. If the AWS report and the re-analysis designation of this system as a tropical depression are both correct, then the track for Item needs to be extended into the Caribbean.

The origin of Item was complex. A broad low/trough was present in the western Caribbean on the 4th and 5th. A ship measured 25-30 kt S winds and 1002 mb early on the 4th, but there is no confirmation of a closed low on that date and the ship's pressure may have been biased low. It is possible that the system was a tropical cyclone on the 4th and/or the 5th. On the 6th through early on the 7th, the disturbance was over the Yucatan of Mexico and the circulation became disrupted both by land and interaction with a cold front. The system was not a tropical cyclone on the 6th and 7th. Late on the 7th, the system moved over the Gulf of Mexico and began to develop. As the system is not analyzed to be a tropical cyclone before early on the 8th, the genesis timing for Item at 06Z on the 8th is not changed and no additional points are added to Item's best track.

The committee requests a detailed re-analysis of the 2-8 October period over the western Caribbean Sea. Maps of the whole period should be included in the binder, and the metadata summaries of both Item and suspect systems #5 should be re-written to include the details of the AWS report. Regardless of what the final decision is on the origin of Item, it is recommended that the origin issues – and why they are important – be given a separate paragraph at the start of Item's metadata summary.

Agreed.

2. The committee concurs with the revised positions on 8 October.

Agreed.

3. Please contact the Meteorological Service of Mexico for more detailed data from Veracruz and any other information that might be available on this system.

The Meteorological Service of Mexico did not have any additional information regarding this hurricane. However, the EV2 website did have some observations for Veracruz, which did help provide a more complete time series of measurements from that location.

4. Please re-examine the intensity on 10-11 October, including the proposed reduction in the peak intensity. First, were the 105 kt winds reported by the aircraft measure as implied in the metadata summary, or estimated? If they were measured, they could be useable in determining the intensity. Second, the winds in Veracruz were 65-70 kt outside of the RMW on what should have been the weaker side of the hurricane. While it cannot be ruled out that the winds in Veracruz were enhanced by terrain funneling, the data suggests the possibility that the intensity was stronger than the proposed 80 kt.

After further examination of the information provide from the microfilm reconnaissance reports and the Air Weather Service writeup, the winds reported by aircraft were visual estimates of surface winds. Because of these, as well as the 65-70 kt winds in Veracruz outside of the RMW on the weaker side of the hurricane, the peak intensity on the 10th through to landfall on the 11th is analyzed at 90 kt.

5. Can a central pressure be estimated from the Veracruz data using the Schloemer equations?

Yes. A run of the Schloemer equation is possible at 03Z just before landfall is possible to estimate the central pressure. Using an RMW of 15 nmi from the last recon about 12 hours previous (along with pressure of 1000 mb, radius of this pressure of 25 nmi, and environmental pressure of 1011 mb), this suggests a central pressure of 987 mb. If, instead, the RMW had contracted to 10 nm, then the Schloemer equation would suggest a central pressure of 976 mb. These central pressure values suggest maximum winds 68 and 83 kt, respectively, from the Brown et al. south of 25N pressure-wind relationship. The actual central pressure is too uncertain to include into HURDAT.

6. Does the data support adding a short tropical depression stage as the cyclone dissipates over Mexico?

Agreed to add a 6 hr tropical depression stage before dissipation.

1950 Storm #10, Jig:

1. For the apparent penetration fix on 14 October, are there any 700 mb heights and/or temperatures that can be used to estimate a central pressure?

Unfortunately, no.

2. While there appears to be insufficient evidence to downgrade Jig below major hurricane status, it is noted that the aircraft wind reports for this storm are lower than seen in other storms in 1950. How much of this is due to the boxing-type flight pattern is unclear.

Agreed, this discussion is added into the summary writeup.

3. What observations are being referred to in reducing the intensity on 16 October? Ships or aircraft?

Ships, as there were no aircraft missions on the 16th. This is now so clarified.

4. While the committee concurs with the earlier extratropical transition, the proposed 85 kt intensity might be too high. Please re-examine this.

Agreed to reduce the intensity downward to 80 kt at 06Z on the 16th at the time of extratropical transition. It is noted, though, that three ship observations indicated 70, 60, and 60 kt on the 17th, so any additional reductions may not be warranted.

5. Typo in the last sentence of the metadata summary: “absorbed bya very”

Corrected.

1950 Storm #11, King:

1. Please contact Perez for more information on this system from Cuba. While the proposed reductions in intensity as the hurricane crossed Cuba appear reasonable based on the available data, the original HURDAT intensities are so high that there is a need to make sure nothing important was missed.

Perez assessed King as a Category 1 for Cuba. Perez has been contacted, but has no addition information on this system from Cuba. Charlie Neumann told us that the cards for 10/17 and 10/18 were unintentionally reversed when some students dropped the HURDAT cards in the 1960s. This is why the 955 mb central pressure is listed on the wrong day.

2. If possible, please find more information about where the 955 mb pressure reported by the National Weather Service employee was actually measured. While it appears likely it was in the eye given the other available pressure data, it would be good to get confirmation of that.

There was a source that said: “20-minute lull at SW 10th Street and 31st Avenue in Miami” from (0452Z-0512Z) with a 961 mb min pressure. There was another source that said a 955 mb central pressure was measured at the home of an off-duty National Weather Bureau Meteorologist. Tough to make a connection between these two, but it must have been somewhere west of the Weather Bureau Office.

3. Is it known what the anemometer height was at the old Miami Weather Bureau office? If so, please provide it.

Yes. It was 68 meters. The max 1-min wind of 106 kts was measured at an anemometer height of 68 meters.

4. Please re-examine the proposed landfall intensity in Miami, since it is calling for an increase from Category 3 to Category 4 strength. On one hand, the proposed 115 kt intensity is 5-10 kt above that supported by the wind-pressure relationships and the surface observations. On the other hand, King very much resembled Hurricane Charley of 2004 at landfall, and that storm had an aircraft-based best track intensity of 954 mb and 125 kt winds at 1700 UTC 13 August. This makes the proposed 115 kt for King seem reasonable.

After reducing the peak observed winds from the Miami Weather Bureau Office to the surface, the peak measured winds were near the border of Category 1/Category 2 strength. However, given the tiny size, it is not surprising that the peak winds would not be observed. The overall combination of 955 mb central pressure for an average moving cyclone with a tiny RMW and a moderate synoptic-scale pressure gradient on the north side are supportive of 115 kt – Category 4 – for landfall of this system.

5. The proposed intensities from 1200Z on 17 October to 0600 UTC 18 October are 70, 90, 115, and 115 kt? What is the basis for showing no increase in strength after 0000 UTC 18 October? Could this be smoothed out?

This has been smoothed out to now indicate 70, 90, 105, and 115 kts, respectively, from 1200Z on 17 October to 0600Z on 18 October, including 115 kts at the 05Z landfall.

6. The eye apparently passed closer to Clewiston, Florida than to Belle Glade as it approached Lake Okechobee, with Clewiston reporting a lull while Belle Glade did not. Please adjust the track a little to the west in this area unless the radar fixes strongly suggest otherwise.

Agreed. The 06Z position was adjusted from 25.8, 80.2 to 25.9, 80.3 and the 12Z position was adjusted from 27.0, 80.8 to 27.1, 81.0. The new 06Z position also fits better with the Opa Locka ob as well as the timing of the various Miami obs.

1950 Storm #12, Unnamed:

1. The committee wonders when this system was added to the hurricane tracks? Was it for Cry's 1963 revision or some later time?

Apparently, this tropical storm was added in Cry et al.'s 1959 publication: Cry, G. W, W. H. Haggard, and H. S. White, 1959: North Atlantic Tropical Cyclones. Technical Paper No. 36, U.S. Weather Bureau, Washington, D.C., 1959, 214 pp. No details were provided for its addition nine years after the fact.

2. Looking at the data on the 1200 UTC 21 October binder map, is it possible that even the revised position is too far to the southwest? The 50 kt wind observation at 1400 UTC is about 200 n mi from the center as things currently stand.

Agreed, the position has now been adjusted a bit closer to the east – about 150 nm – to the ship for the 12Z 21st location. However, the ship at 00Z 22nd (note below) would suggest that the cyclone's location was too far east. The final positions are a compromise due to the various – and contradictory - ship observations.

3. It is noted that at 0000 UTC 22 October there is a ship with south winds 25 kt west of the proposed center location with a pressure 1001 mb. Does this require an adjustment of the track and intensity around this time?

See above for locations. This peripheral pressure suggests maximum winds of at least 47 kt from the Landsea et al. north of 35N pressure-wind relationship. The intensity of 60 kt at that time is retained.

4. Please contact the Meteorological Service of Portugal for any information they can provide on this system as it passed over the Azores.

The Meteorological Service of Portugal has no additional information for this system.

1950 Storm #13, Love:

1. Please re-write the first paragraph of the metadata summary to focus on the features that spawned Love. Hurricane-force winds over the Atlantic do not appear relevant to this.

Agreed.

2. Please provide binder maps for at least a couple of days prior to 17 October.

Agreed.

3. Please provide a stronger argument for dropping the first 12 h of the track on 18 October. The observations seem to suggest the possibility that a circulation existed during that time. If the early points are maintained, would it be possible to smooth through the major intensity change during the first 6 h?

HURDAT starts this system at 00Z on the 18th and this is unchanged, though it is not certain that a closed low had developed until about 12Z on that date. A major track change is introduced at 00Z on the 18th based upon ship observations with minor track revisions indicated for the remainder of this system. HURDAT initially indicated a 35 kt intensity at 00Z on the 18th, jumping to 65 kt immediately at 06Z. While there does exist a 60 kt ship measurement at 00Z, this observation appears substantially high biased compared to the nearby ships. Initial intensity is set at 40 kt at 00Z on the 18th, a major downward revision. This is more realistically ramped up to 60 kt by 12Z, supported by aircraft reconnaissance observations late on the 18th.

4. There are two ships with 993 mb pressure on the binder map for 0600 UTC 21 October, and they are both marked as being biased low. These should be acknowledged somewhere in the metadata.

There were two observations from the same ship around 06Z (05Z and 08Z) that listed 993 mb pressures. However, comparisons against other nearby ships suggest that these were significantly low biased.)

4. Please re-examine the landfall timing and location in Florida, as well as the speed and direction of motion at landfall. The microfilm map for 1000 UTC 22 October suggests that the center was near or south of Cedar Key based on the winds and pressures at Cedar Key and Cross City. The next map, one hour later, shows missing data at Cedar Key and a wind shift at Cross City that implies a position at least 30 n mi farther north than that from the data an hour earlier. The position implied from the 1000 UTC map is also not consistent with the reports from the aircraft near that time. Please obtain detailed observations from Cedar Key to help resolve this issue.

The original Cedar Key Surface Weather Observations record was obtained from the EV2 website. This simply confirmed the observations plotted on the microfilm. One issue that is apparent upon inspection of the 12Z 22 October map is that the Cedar Key pressures are too low by 3-5 mb. After reconsidering the observations, it is agreed that the landfall occurred farther north – 29.7N 83.4W – at 11Z. Given the Cedar Key pressures apparently being low biased, the highest observed wind at landfall was 35 kt, and the description of a weakening system at landfall, intensity at landfall is analyzed at 50 kt.

1950 Storm #14, Mike (new):

1. The big mystery of this system is why the Weather Bureau and Navy hurricane forecasters did not include it in their post-season summaries. It is also a mystery why it was not added later in a fashion similar to storm #12. Please search for any correspondence on this system that may help solve the mystery.

A search has been conducted, but there is no discussion that we can find that would let us know why this system was not included in the post-season summaries or why it was not added in after the fact like unnamed storm #12.

2. Is there enough data in COADS to perform a quality control on the pressures from the ship **Alvelos**? It is noted that the pressures surrounding this system are quite high on the HWM, and it could still be a tropical storm even if the ship pressures are biased somewhat low.

There is only one observation from the Alvelos, making it problematic to perform a quality control on the measurement. Agreed that the high environmental pressures (~1016 mb) along with the 1006 mb peripheral pressure observation are a second piece of evidence that the system was a tropical storm.

3. What was the basis for the 40 kt peak intensity? It is noted that the AWS report used 45 kt, while the **Alvelos** reported 35 kt.

Given that it is quite unlikely that the strongest part of the cyclone was sampled by the Alvelos, the intensity is estimated slightly higher than this measurement.

4. The committee concurs with adding this system to HURDAT, unless the search for documentation on why it was originally left out finds convincing evidence to the contrary. It should be noted in the metadata summary that this addition is based only on one piece of data.

Actually, there are two pieces of information that support this systems inclusion as a tropical storm: the 35 kt observed winds and the 1006 mb peripheral pressures embedded within quite high (~1016 mb) environmental pressures.

1950 Storm #15 (new):

1. Please provide binder maps for at least a couple of days prior to 27 October.

The binder maps for October 25th and 26th have been added in.

2. Please better state the basis for showing a closed circulation at 1200 UTC 27 October and for the 45 kt intensity 6 h later.

The first definitive evidence of a closed low occurs at 12Z October 28th. Given the sparse number of observations on the south side of the system on the 27th and early on the 28th, a closed low could not be confirmed but is quite likely to have occurred at those times. Agreed to indicate a more gradual intensification and now show 40 kt at 18Z on October 27th.

3. The committee concurs with adding this system to HURDAT.

Agreed.

1950 Storm #16 (new):

1. Please include the HWMs for this system in the binder, including for at least three or four days before genesis. A first look at them suggests a low was present as early as 6 November. There may be a need to revise the discussion of the system's origin based on the big picture in the HWMs.

These have been added in and the writeup on the system's origins have been revised.

2. The committee concurs on adding this system to HURDAT.

Agreed.

1950 Additional Notes:

1. Suspect #1: There is a typo "28 October". Also, is the 50 kt ship in COADS so it can be quality controlled?

Typo fixed. The ship is not in COADS.

2. Suspect #5: See the comments in the Hurricane Jig section. In addition, please look at the northern system to see if it could be a subtropical or tropical depression.

The writeup for this has been significantly revised. It does not appear that the northern system was a subtropical or tropical depression.

3. The 4-5 October HWM show an unusual feature between the Bahamas and Bermuda – a squall line. On 5 October this system is analyzed as extending southward from a small area of low pressure. Due to high pressures and lack of gales on the HWM, it is unlikely to have been a tropical storm. However, please search for any additional ship reports on it and for any microfilm maps that may cover this area.

This system was checked, but did not have any observed gales, nor did it appear to have a closed circulation. Thus this system is not added into HURDAT, though a brief discussion is included within suspect #5.

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

Agreed.