Best Track Committee Re-Analysis Comments for 1964

**(Responses by Landsea in boldface – February 2019)**

General comments:

1. Please ensure that all of the available aircraft data has been collected, quality controlled, and employed as part of the re-analysis. There are places in the re-analysis, such as Cleo’s crossing from Cuba to south Florida, where the storm wallets on the network drive have aircraft data that apparently was not seen in developing the submission. Also, there are flights on the microfilm maps that need to be referenced when necessary. Finally, please check that the center fix designations of radar and penetration are correct, as there are some that are mis-labelled.

**These have all been addressed and corrected as best we can.**

2. Please change “oceanfall” to “emerged over the {body of water}” wherever it occurs.

**Done.**

1964 Unnamed (AL011964):

1. The Committee concurs with a later genesis time for this system. However, it would prefer 1200 UTC 3 June instead of 0000 UTC 4 June based on the available data.

**Agreed and changed.**

1a. It is noted that the microfilm (MF) maps for 1200 and 1800 UTC 2 June are identical, suggesting one of the maps is missing. Please add the missing map to the collection.

**The 18Z map on June 2nd is not available.**

2. Please re-examine the proposed landfall time on the Yucatan Peninsula. The data plotted on the 1800 UTC 4 June MF map suggests the cyclone is still offshore of Yucatan, with the data at 0000 UTC 5 June suggesting the center is near or over the coast.

**Agreed and changed.**

3. Please try to locate the aircraft data shown on the MF maps on 5-6 June, and please include a discussion of the aircraft data on the 1200 UTC 6 June MF map in the daily summary for that day.

**As was common for the era, aircraft reconnaissance for systems that were not named tropical cyclones were not archived as thoroughly as unnamed cyclones (like this one). No additional aircraft data were located. Some discussion has been added about the aircraft data on the 6th.**

4. Please move the 1800 UTC 5 June position farther east to near 85.3W to better match the recon and surface observations.

**Agreed and changed.**

5. Please re-examine the dissipation-reformation scenario proposed for 6-7 June. The MF maps suggest that a circulation existed through the period, especially when considering the recon observations on 6 June. So, it may be that declaring the system a disturbance is not correct. However, if the re-examination does indicate that the circulation dissipated on 6 June, the Committee prefers the reformation time to be 0000 UTC 7 June instead of 1800 UTC 6 June.

**Agreed to retain system as a tropical depression on the 6th.**

6. Please re-examine the structure of this system during 7-9 June to see if it had frontal or hybrid characteristics during this time. The 1200 UTC 8 June MF map shows a dewpoint gradient across the cyclone from 75F southeast of the center to 60F over the Carolinas, although the gradient is not strong near the storm center. More importantly, the 500 mb maps show a shortwave trough with relatively cold temperatures near the system. Is it possible that this system evolved more as a subtropical cyclone than a tropical cyclone?

**It is possible that this system evolved more as a subtropical cyclone than a tropical cyclone, though without satellite imagery available such a designation is not available for use. Such discussion is now included into the writeup on the 8th.**

1964 Unnamed – AL021964:

1. Please better explain why 45 kt was chose for the intensity at the initial HURDAT point. This seems high compared to the ship reports of 35-40 kt.

**Agreed to show initial intensity at 06Z on the 28th as 40 kt.**

1a. The Committee has concerns about whether the system actually had a closed circulation on 29-30 July due to the rapid motion. However, since the aircraft reported central pressures of 1006-1012 mb are much lower than the surrounding environmental pressures, the system should be kept as a tropical cyclone through this period.

**Agreed. Additional discussion added regarding this uncertainty.**

2. Please correct a typo “URDAT” in the bullet points for the “Maps and Old HURDAT” section on 31 July.

**Corrected.**

3. Can a QC check be made on the data from ship GKSY? It is the only data that justifies calling the cyclone a hurricane before it became extratropical, and the 997 mb is much lower than any other pressure in the area of 1-2 August.

3a. Pending the QC check on the ship, the Committee concurs with upgrading the system to a hurricane.

**Unfortunately, this observation from ship GKSY was the only one available. However, the other ship that reported hurricane force - #6373 in COADS – has a detailed time series for a few days and the wind reports appear to be unbiased and consistent. Agreed to indicate hurricane status late on the 1st and early on the 2nd.**

4. The Committee concurs with the proposed earlier time of extratropical transition.

**Agreed.**

4a. The NA map series apparently shows a later dissipation time for the extratropical low than the MF maps do. Please coordinate with Dave Roth on this.

**Agreed. System extended through 12Z on the 9th.**

1964 Abby (AL031964):

1. Please re-examine the genesis of this system. It is noted that the MF maps suggests two low pressure areas were present on 5-6 August – an eastern one that became Abby and a second near and south of the Louisiana coast. Indeed, the two center are apparent in the recon data on the 1200 UTC 6 August MF map. Is it possible that the two centers merged, or that the second center influenced the development of Abby?

**The trough near the Louisiana coast appears to have moved northwestward and dissipated over southeastern Texas early on the 6th, without developing into a tropical cyclone. This discussion is now added into the Daily Summary.**

2. Is there any data available from the oil rigs near the cyclone?

**There are no additional data available from the oil rigs in the vicinity of the tropical storm.**

3. Please discuss the recon flight data on the 1200 UTC 6 August MF map in the daily write-up.

**Agreed and added in.**

4. The landfall intensity needs better justification for departing so far from the wind-pressure relationship. On one side, the storm was small with an eye diameter of 12-20 mi and a correspondingly small radius of maximum wind. However, it also moved relatively slowly at about 7 kt. Please better explain how this justifies a peak intensity of 15 kt above that of the wind pressure relationship, which is an increase above what is already in HURDAT.

**Agreed to bring down the intensity at 18Z to 55 kt, same as originally shown in HURDAT.**

4b. In relation to the landfall intensity, it should be noted that 1) landfall occurred three hours after the aircraft report of the 1000 mb pressure, and 2) the lowest pressure and maximum wind in Matagorda (which support a central pressure near or perhaps below 1000 mb) occurred 2.5 hours after landfall. It is possible that the storm deepened some between the aircraft fix and landfall.

**It is agreed that based on the Matagorda observations at 23Z that Abby continued intensifying after the recon fix (18Z) until landfall around 2030Z. Thus the intensity is estimated to be 60 kt at landfall (with no central pressure value, but it may have been near 995 mb) with the possibility that Abby came ashore as a hurricane.**

1964 Brenda (AL041964):

1. How does the genesis of Brenda related to the frontal low seen on the MF maps on 6-7 August? Unless this system was not related to Brenda, the proposed later genesis time may not be right.

**On the 6th and 7th of August, an extratropical cyclone east of the Carolinas moved east-northeast into the central Atlantic. Tropical Storm Brenda had its origins within a dissipating frontal boundary associated with this extratropical cyclone. No revisions are made to the new genesis time of 06Z on the 8th (which is 12 hours later than shown originally).**

2. Data in the Storm Wallet indicate that the maximum reported winds on Bermuda were 38 kt at 1210 UTC 8 August, and that the lowest pressure of 1008 mb at 1300 UTC had simultaneous surface winds of E 20 kt. Please include these data in the write-up.

**Done. This did allow for the 1008 mb value at 12Z August 8th for central pressure to be changed to 1006 mb and the 45 kt intensity was retained.**

1964 Storm #6, Cleo:

1. Please include any appropriate maps prior to 18 August.

**The maps before the 18th show no features of interest. Moreover, it appears unlikely that Cleo originated from a tropical wave that emerged off of the coast of West Africa on the 15th or was encountered by the German vessel on the 18th as suggested in the Monthly Weather Review. The backwards extrapolated positions from the first definitive location and translational speed late on the 20th suggest positions substantially farther east than what is suggested on the 15th and 18th. It is possible instead that Cleo originated from the next tropical wave that emerged from the coast.**

2. Could the genesis be moved up to 0600 UTC 20 August based on the ship with SW 20 kt and 1010 mb? This was apparently enough for the MWR to say a circulation was present, although it was not used in the original HURDAT.

**Unfortunately, the ship’s latitude (and possibly longitude) is not reasonable. The aircraft reconnaissance’s latitude near 18Z was 13.0N and a day later (21st 18Z) was near 14.7N. The ship early on the 20th suggests a center of 14.5-15.0N, which would be nearly two degrees too far north. Thus because of this big discrepancy on the location of the ship, it is preferable to start Cleo’s best track at the time of arrival of the aircraft reconnaissance.**

3. Please check the designation of the recon fixes on 22 August. The Annual Tropical Storm Report (ATSR) suggests that the radar fix at 0059 UTC is actually a penetration fix (extrapolated pressure from 700 mb of 997 mb using todays; formulas), and that the penetration fix at 0645 UTC is actually a radar fix.

**These are corrected.**

4. Please re-examine the proposed decrease in the landfall intensity for Marie Gallant. On one side, it is noted that the original 115 kt intensity in HURDAT is significantly above the intensity suggested by the wind-pressure relationship. On the other hand, the storm was small, rapidly intensifying, and moving quickly, all of which support an intensity above that of the wind pressure relationship. What is the possible intensity using the intensifying subset of the wind-pressure relationship? Also, since decreasing the landfall intensity from 115 kt to 110 kt would cross the Category 3-4 threshold on the Saffir-Simpson Hurricane Wind Scale, it would be a good idea to talk to the French to see which side of that threshold the impact was more consistent with. Overall, the Committee does not oppose the change of the landfall intensity, but it would like to see this double checked before concurring.

**The intensifying subset for this central pressure south of 25N is 100 kt, not significantly different from the standard relationship. Certainly, the wind-based impacts of Cleo on Guadeloupe were tremendous and consistent with that of a major hurricane. However, structural damage is not going to be able to help distinguish between 110 versus 115 kt. Agreed to indicate 110 kt for the landfall in Guadeloupe.**

5. For the 2050 UTC 23 August fix, the 139 kt winds reported are flight-level winds and not estimated surface winds. In addition, this was from a research flight and not a Navy mission. Please correct these in the write-up. Also, please re-examine **all** of the pressures measured this day using the current extrapolation formulas when applicable. One example is a fix at 1735 UTC where the fix form reported an extrapolated pressure of 950 mb, but the 700 mb height and temperature would suggest 945 mb using today’s formulas.

**The winds are corrected to be flight-level winds. The 1735Z fix is added into the reanalysis and suggests that Cleo peak earlier in the day as 130 kt/938 mb hurricane.**

6. The Committee concurs with the proposed changes in the landfall in Haiti.

**Agreed.**

7. The storm wallet on the local network drive has much of the recon data from the flights on 26-27 August as Cleo crossed from Cuba to South Florida, including the research flight that was the last mission before Cleo’s Florida landfall. Please incorporate this data into the submission.

**These have all now been added into the database and writeup.**

8. The Committee does **not** occur with the proposed upgrade to a Category 3 hurricane at landfall in South Florida. The landfall pressure of 968 mb is marginal for a major hurricane (see point 8a), and the reported surface winds and damage are not commensurate with a major hurricane.

**Agreed to indicate a South Florida landfall of 95 kt, Category 2 hurricane.**

8a. The storm wallet has a large number of unofficial pressure observations for Cleo in south Florida. Some of the barometers appear to have been tested for accuracy, and these tests appear to be the basis for the 968 mb pressure in Ho and Jarrell. Others, however, have lower pressures than 968 mb. While it is unlikely that these lower pressures are correct given that the original post-analysis did not use them, they need to be examined and their accuracy assessed as best as possible.

**While there was an unverified observation of 963 mb near Sunrise, this measurement was not considered accurate compared with other observations that were verified closer to the coast line. It is possible that some slight deepening occurred after landfall, but such an effect is likely to be only a millibar or two.**

9. Please re-examine, and if necessary increase, the intensities from 1800 UTC 27 August to 1200 UTC 28 August, as the Decay SHIPS winds may be too low for a system that stayed as close to water as Cleo did. At the very least, the Committee would prefer an intensity of 45 kt at 0600 UTC 28 August, based on the 40 kt observation at Patrick Air Force Base at 0400 UTC and that stronger winds likely existed over the water.

**The Kaplan-DeMaria inland decay model does roughly take into account the distance of the center of the tropical cyclone to the coast. Agreed to indicate 45 kt at 06Z on the 28th.**

10. Please re-examine the aircraft fixes at 1300 and 1900 UTC 3 September. The write-up for the 1300 UTC fix says the pressure is 988 mb. However, the fix form says it is 9880 mb (typo?) and the extrapolated pressure from 700 mb is near 980 mb. On the 1900 UTC fix, the reported pressure is 982 mb. However, the 700 mb data suggests a pressure near 975 mb using current extrapolation formulas.

**These are added into the analysis.**

11. Please re-examine the 90 kt intensity on 4 September. Give that Cleo was undergoing extratropical transition, would an 85 kt intensity be better than the 90 kt suggested by the estimated central pressure?

**Agreed and so changed.**

12. Please coordinate with Dave Roth to reconcile the differences between the proposed track after extratropical transition and the Northern Hemispheric map series.

**Agreed and updated.**

1964 Storm #7, Dora:

1. Please re-examine the central pressure (and the associated intensity) for the aircraft fix at 2120 UTC 2 September. The minimum reported pressure on the fix form is 989 mb, which was apparently measured at low level. However, the minimum 700 mb height for the fix is 2873 m, which on the fix form in the storm wallet suggests a pressure of 975 mb. In addition, the ATSR data table for that Navy mission suggests the temperature at the time of the minimum 700 mb height was 17C, which would yield a minimum pressure of 972 mb using modern formulas. Since the 700-mb height on the next fix was 2973 m, it is possible there was a typographical error. However, that would not explain the values that appears in the ATSR.

**This appears to be a typo common to both the fix form and the ATSR. This is now mentioned in the writeup.**

2. Please also re-examine the remainder of the proposed intensities on 2-4 September, as there is a question of how the pressure could go from 989 mb to 976 mb with only a 5-kt increase in the wind.

**Agreed to revise the intensities downward late on the 2nd and early on the 3rd. Now the 989 to 976 mb drop in central pressure corresponds to a 10 kt increase in intensity.**

3. Please re-examine the proposed changes to the intensity on 7 September, particularly the short-lived re-intensification based on the pressure fall from 964 to 956 mb. The 700 mb data for the 964 mb fix near 1600 UTC extrapolates a pressure of 959 mb, and other fixes on this day have extrapolated pressures near 959 mb. Could the pressure rise and fall that is the basis for the proposed intensity changes be smaller than current thought?

**Agreed that the pressure is 959 mb instead of 964 mb near 12Z. No short-lived reintensification phase is shown. Instead the intensity is flatlined at 95 kt on the 7th.**

4. Please examine and explain why Ho had a 961 mb press for the Florida landfall of Dora, while Jarrell had 966 mb – with the latter being in better agreement with the observations. If Ho is correct, the 961 mb pressure and the estimated 110 kt winds in St. Augustine after the eye passage would suggest the possibility that Dora was a major hurricane at landfall. If Jarrell is correct, then the proposed (and current HURDAT) 95 kt looks good.

**It is noted that Ho et al. used the 966 mb in St. Augustine and “computed from pressure profile along or near coast” along with an RMW of 34 nmi from reconn to arrive at 961 mb central pressure. However, this value appears to be too low because of the following: 1) the land-based radar and the recon indicate that the center of Dora went directly over St. Augustine and the minimum pressure was recorded in the eye; and 2) an RMW of 34 nmi is not consistent with the much smaller values suggested by radar. Thus 961 mb is not likely to be the central pressure of Dora at landfall.**

5. Please delete the discussion of the hurricane-force winds in Jacksonville, which, while interesting, does not contribute to the analysis of the best track or intensity.

**Agreed and removed.**

6. On 11-12 September, the highest winds for the land stations near the center were 10-15 kt. Were higher winds occurring at stations near the coast? If so, please mention them in the write-up.

**The stronger winds were indeed occurring over the water near the coast. This is now so mentioned in the writeup.**

7. The Committee concurs with the earlier time of extratropical transition.

**Agreed.**

8. Please leave in the final point for the current HURDAT at 0000 UTC 16 September, as there is still some indication in the MF map that the Dora center still existed.

**Agreed and retained.**

1964 Storm #8, Ethel:

1. Please re-examine all of the aircraft fix data and the interpreted intensities on 6-7 September, as the pressures mentioned in the write-up are somewhat mis-matched with the raw recon data. For example, the write-up states the fix at 2113 UTC 6 September had a pressure of 1002 mb. The fix form in the storm wallet, however, shows a 1006.4 mb minimum pressure at the time. Also, in the write-up the fix at 1357 UTC 7 September is shown as having a pressure of 1000 mb. However, the fix form says MSLP unknown, dropsonde inoperative, and 700 mb height not available. Finally, there is a reference to a 3048 m 700 mb height on the storm wallet fix log for the 1948 UTC 7 September fix, but the raw data shows this was measured outside the center. The raw data suggests there were no reliable central pressure measurements on 7 September after 0100 UTC.

**Agreed to indicate 1006 mb for the 00Z September 7th slot. Additionally, agreed to show no further central pressure values on this date.**

2. After the re-assessment in point 1, please also re-examine the intensities on 8 September. The proposed weakening to 50 kt at 0600 UTC looks a bit strange given the lack of a supporting rise in the central pressure. Also, the central pressure fell from 999 mb to 986 mb despite the apparent sheared center in satellite imagery. This suggests the possibility that the system reached hurricane strength near 1800 UTC, although the 75 kt currently in HURDAT may be too high.

**Agreed. An intensity of 50 kt is now indicated from 18Z on the 7th through 06Z on the 8th, then intensification to 65 kt by 18Z on the 8th.**

3. The Committee concurs with the proposed earlier time of extratropical transition and the earlier time of final dissipation.

**Agreed.**

1964 Storm #9, Florence:

1. Please correct a typo in the discussion for 5 September – “surface within” should be “surface circulation within”.

**Done.**

2. Please re-examine the proposed time of making Florence a tropical storm. The proposal to move it forward to 6 September is based partly on the observed pressures near 1002 mb in the Cabo Verde Islands, which given the apparent monsoonal nature of the system suggests the winds could be lower than the wind-pressure relationships. It is also based partly on an unreadable wind barb on a map. Based on the uncertainty, the Committee suggests keeping the tine of the upgrade at its original time in HURDAT. As an alternative, perhaps 0000 UTC 7 September could be justified based on the 30 kt ship with 1005 mb.

**Agree to delay intensification to a tropical storm until 00Z on the 7th.**

2a. Please contact the Meteorological Service of the Cabo Verde Islands for data and information they have on the impacts of this system, including what the report with the unreadable barb actually was.

**The Meteorological Service of the Cabo Verden Islands have no additional information on this system including the observation in question.**

3. Since two ship reports played a role in changing the track on 9 September, please include both of them in the Ship highlights section.

**Done.**

1964 Storm #10, Gladys:

1. The Committee prefers to keep the time that Gladys first became a hurricane on 14 September at the original HURDAT time of 1200 UTC. It concurs with the decreased intensity of 65 kt at 1800 UTC based on the aircraft data.

**Agreed and changed.**

2. On 15 September, showing at 10 kt decrease in intensity for a 2 mb pressure rise seems to be a little too much. It is recommended that this be smoothed out by making the 1200 UTC intensity 60 kt.

**Agreed and changed.**

3. In regards to the peak intensity of 17 September: Since Gladys was at 25N at peak intensity, and the intensifying wind pressure relationship suggests 118 kt, is there convincing evidence to lower the 120 kt peak intensity currently in HURDAT to 115 kt? It is noted that the other pressures near peak intensity were near or above 950 mb, which could support a slightly lower peak intensity.

**The original peak intensity (at 12Z on the 17th) was 125 kt. The intensifying wind-pressure relationships suggest 116.5 kt for 945 mb at 18Z based upon the average of the south of 25N and north of 25N relationships. (Gladys was right at 25N at the time.) Thus 115 kt is analyzed at 18Z on the 17th, which is also the time of lowest pressure and peak (re-analyzed) intensity.**

4. For the aircraft fix at 0100 UTC 19 September, the pressure extrapolated from the 700 mb data is 945 mb, supporting the premise that the reported 962 mb pressure is wrong.

**This revised pressure is added. Intensity analyzed at 110 kt from 00Z 18th through 06Z 19th.**

5. Please re-examine the intensities for 1200 and 1800 UTC 20 September, as well as those on 21 September. In addition to the storm filling during this time, aircraft data suggests that the RMW became quite large late (80 n mi or more) on 20 September, and that the convective structure likely decayed. Please see the research aircraft fixes in the storm wallet for more specifics.

**Agreed to bring down the intensities on the 20th and 21st more, to substantially below the pressure-wind relationship.**

6. In the discussion section for 25 September, is “September 26” supposed to be “September 25”?

**Yes, corrected.**

1964 Storm # 11, Hilda:

1. Is any land station data available from Cuba for Hilda?

**There are no additional observations of high winds or low pressure from Cuba for Hilda.**

2. Given the 941 mb pressure at the end of a rapid deepening phase, the Committee would prefer a peak intensity of 120 kt at 1800 UTC 1 October and 0000 UTC 2 October.

**Agreed and values adjusted.**

3. While the Committee concurs with a Category 2 intensity at the Louisiana landfall, it would prefer a 95 kt landfall intensity, as it has concerns about the lack of weight given to observations in the landfall area. The data from Franklin and Thibodeaux are estimated and thus must be used with caution. However, how likely is it that the eyeball estimate of the winds in Franklin (115 kt) would be 30+ kt off from the actual winds? There is also the case of the oil rig *Ocean Driller* on 3 October. A newspaper article in the storm wallet states that the top deck of the platform was set at 50 ft above sea level during Hilda’s passage (and was hit by waves), although it does not state the anemometer height above the deck. This suggests that this platform was less elevated than its current day counterparts. Also, the impression given by the MWR excerpt was that the winds remained above the 120 mph maximum value of the anemometer for a period on the order of minutes to hours, which suggests sustained winds instead of a peak gust. Thus, some weight should be given to this observation where appropriate.

**Agreed to increase the intensity some on the 3rd based upon the oilrig measurements back up to 100 kt at 00 and 06Z.**

**Also agree to boost the intensity slightly at landfall up to 90 kt with the acknowledgement that visual estimates of extreme wind are poorly calibrated and unreliable. How would someone be able to quantify extreme winds over land based of 85 kt versus 115 kt with any reliability? Given that almost no one ever experiences such winds or even moves so fast (except in a closed airplane), the ability to distinguish such values by visual assessment is not a skill anyone would have except perhaps a wind engineer (or storm chaser). Moreover, the metric used for intensity is a one-minute wind. Any human visual observer would perhaps focus on the gusts, but would be unable to provide the averaging need to determine what the winds were over a 1 minute period. So was the eyeball estimate of the winds a peak gust or 1 minute averaged value? Given the lack of objectivity in the estimate, the answer really isn’t meaningful. No weight is given for visual estimates of extreme winds.**

4. Thus, please re-analyze the intensities on 1-4 October using the details specified on points 2 and 3.

**Intensities substantially adjusted as requested: peak intensity now indicates 120 kt for 18Z October 1st and 00Z October 2nd, Category 3 intensity maintained through 06Z on the 3rd, and Category 2 90 kt landfall intensity shown.**

5. The Committee concurs with the revised time of extratropical transition and final dissipation of Hilda.

**Agreed.**

1964 Storm #12, Isbell:

1. The Committee concurs with the proposed later time of genesis.

**Agreed.**

2. There are two typos in the 11 October metadata discussion: 1) There is a reference to 2300 UTC October 23, which should probably be October 10, and 2) “48 hours than originally” should be “48 hours earlier than originally”.

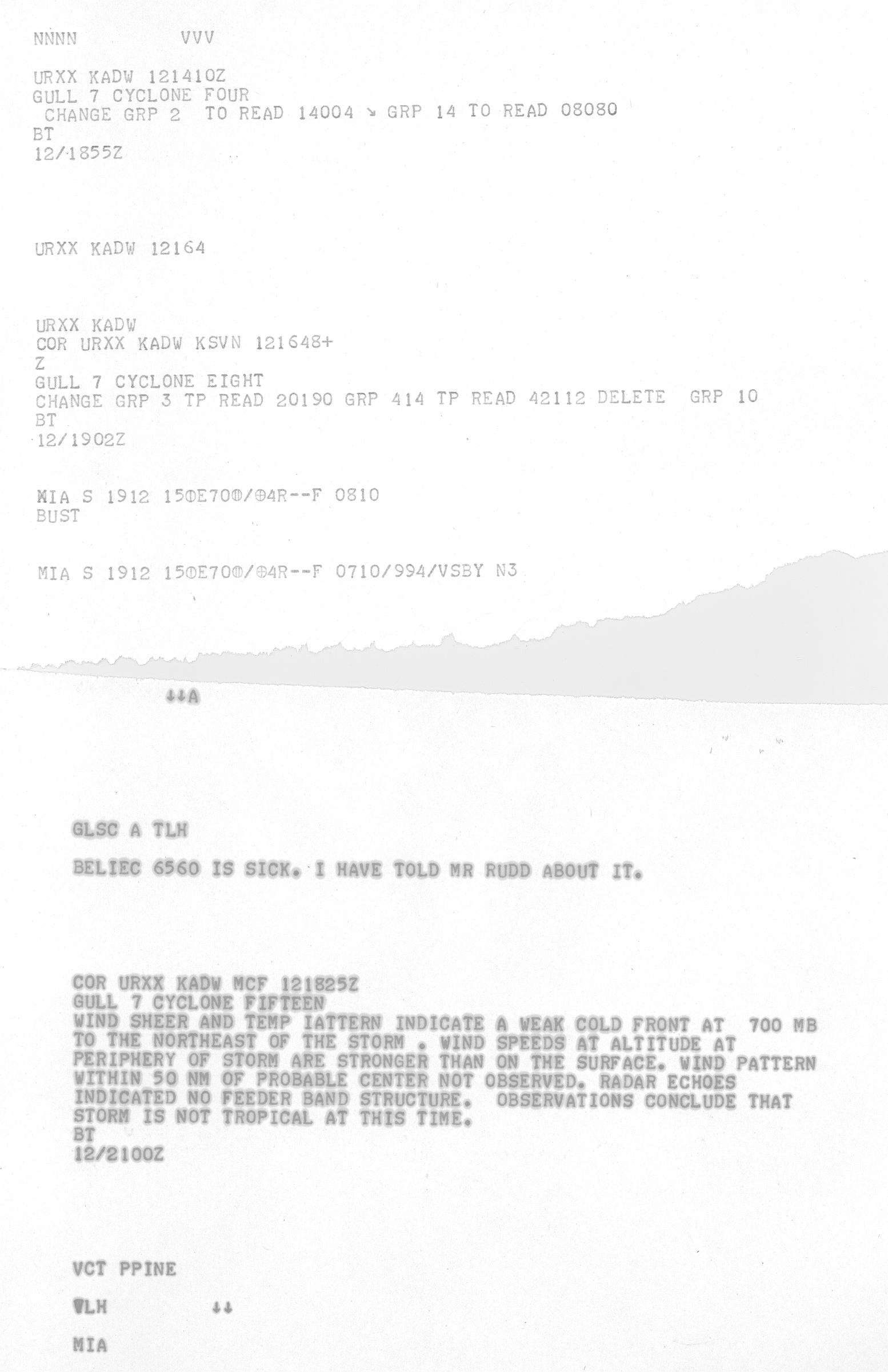
**Corrected.**

3. The committee concurs, although with reservations, about the short-lived upgrade to a tropical storm on 11 October. Given how far the 35-kt winds were from the proposed center position at 0000 UTC 11 October, there is some question about how representative they are, and this concern should be more strongly noted in the write-up.

**Agreed and added to the writeup.**

4. The Committee does not concur with the earlier upgraded to a tropical storm on 12 October. First, when are the “earlier observations of higher winds”? If they are on 12 October, they need to be documented here. If they are the ones from 10 October, then they are probably not relevant. Second, the weather officer on the Air Force plane that investigated the system near 1800 UTC 12 October commented that the storm is not tropical at this time due to the structure and the presence of a cold front nearby (see the image below). Although it is hard to reconcile this with the clear-cut evidence that the system was a tropical storm at 0000 UTC 13 October, the Committee recommends staying with the original HURDAT unless there was a tropical-storm-force wind observation that was not originally included in the write-up. (A large data set from this flight is available in the storm wallet, but a decoding key for the old aircraft data code is needed to interpret it.)

**Agreed to retain the system as a tropical depression through 12Z on the 12th.**



5. Please do a better job of justifying why the proposed intensity of 70 kt at 0600 UTC 16 October is below that of the 75-kt ship report. Since this was a north wind on the west side of the northward moving hurricane, the winds on the east side could have been stronger (with the caveat of baroclinic sting jets.)

**Agreed to indicate 75 kt at 06Z based upon this ship observation.**

6. Please also better explain the landfall intensity in North Carolina, including how and why the selected intensity agrees or disagrees with the wind-pressure relationships.

**Central pressure at landfall is assessed at 992 mb, based on an observation of 15 kt N and 994 mb at Cherry Point. 992 mb central pressure suggests 56 kt from the north of 25N and 60 kt from the north of 35N pressure-wind relationships. As Isbell was in the process of merging with the oncoming extratropical cyclone, an intensity of 55 kt is chosen for landfall and at 12Z. This is a substantial boost in the intensity from the 40 kt shown originally.**

7. The Committee concurs with the proposed earlier dissipation.

**Agreed.**

1964 Storm #13, Twelve:

1. According to the storm wallet, the recon fix on 7 November was at 1730 UTC. There should be a note in the write-up that because it was over land the pressure may be more uncertain than usual.

**The time of the recon fix is added in and the note about uncertainty included.**

2. The Committee is split on whether 60 kt is a high enough intensity. There is no direct evidence to support calling the system a hurricane. However, the reports of very high winds and tides strongly suggest it was a hurricane, as do the press reports (and later climatological summaries) from Nicaragua. It should be noted that the Brus Laguna area of Honduras where the 90 mph wind were reported is well away from the landfall location (of what was supposed to be a small system) and the closest approach to that area would have been several hours after landfall. The 997 mb pressure several hours after landfall is also noteworthy, if uncertain. Given all of the uncertainty, the Committee has the following requests and questions before making a final decision on the landfall intensity:

2a. Please re-examine the track to see if it is possible to bring it closer to the Brus Laguna area without violating the other data. It is noted that while it is unclear if the storm passed directly over Puerto Cabezas, Nicaragua, the news reports in the storm wallet are clear that it came close enough to cause strong winds.

**The track has been shifted a half degree farther north to bring it closer to Brus Laguna, which still is consistent with other synoptic-scale observations.**

2b. Please contact the Meteorological Services of Honduras and Nicaragua for any other information they may have on this system. The letter in the storm wallet from the Honduras suggests they may not have anything more to offer. However, the Nicaragua Meteorological Service might have something, particularly data from Puerto Cabezas.

**Unfortunately, neither the Meteorological Service of Honduras or Nicaragua have anything additional available on this system.**

2c. The Honduran letter references Caukira, where the tides were allegedly “18 feet”. This location is unknown. However, there is a Krukira, Nicaragua which is on the coast just north of Puerto Cabezas. If this is the place where the “18 feet” tides occurred, what strength of storm would be required to cause such tides? (Caveat: “18 feet” may refer to a combination of tides and waves.)

**It is likely that “Caukira” is now referred to as “Cauquira”, which is located at 15.3N 83.6W. SPLASH output indicates 18 feet for storm surge at the boundary between a Category 4 and a Category 5 hurricane. It does not seem plausible that the system reached major hurricane status, much less Category 4 or 5. On the other hand, it is possible that this inundation was referring to rainfall-produced flooding along Rio Patuca in the general region of Cauquira. Such an interpretation would be consistent with evacuations AFTER the system came through of nearby Caratasca Lagoon.**

2d. Please try to locate any available satellite images of the cyclone.

**There are no satellite pictures archived of this system that can be located.**

**Given the uncertainties, the reanalysis does show a major increase in intensity at landfall (from 35 to 60 kt), but not enough evidence to indicate that it reached hurricane force.**

1964 Tropical Storm (new – AL131964):

1. The Committee concurs with adding this system to HURDAT. Please add the appropriate upper-air maps to the map directory.

**Agreed and done. These upper-air maps indicate that the system had some subtropical characteristics, which is now mentioned in the writeup.**

2. Please coordinate the pre-TC write-up with Dave Roth of WPC. The WPC North American (NA) map series suggests this system is not traceable to a trough over Arkansas, although it agrees on the presence of a low over North Carolina on 24 July.

**Agreed to begin the system earlier - at 00Z on July 23rd.**

3. Please re-examine the data for 1800 UTC 24 July. The COADS data mentioned in the write-up has pressures of 1011-1012 mb, while the MF maps shows 1014-1018 mb. The only observation that shows 1012 mb on the MF map is well east of the apparent center and looks very low compared to the other nearby obs. If the MF map data is right, how can there be a 1009 mb central pressure?

**Agreed to remove the 1009 mb central pressure at 18Z on the 24th.**

4. Please delay the extratropical transition of this system to 1200 UTC 26 July. The surface observations suggest that the cold air did not reach the center until after 0600 UTC, and there is a 35 kt ship close to the center at 0600 UTC that suggests an inner wind core was still present.

**Agreed to delay the extratropical transition until 12Z on the 26th.**

1964 Additional Notes:

1. None of the proposed additional systems should be included in HURDAT. However, if available, please provide the MF maps for the January suspect.

**Agreed to not include any additional systems into HURDAT. The microfilm maps are available in the library.**