**Best Track Committee Re-Analysis Comments for 1938**

1938 Storm #1 (new):

 1. The committee concurs with the addition of this system. There is a concern that the timing of the tropical transition might be better pushed back 6 hours or so. (Separate comments from Richard Pasch: The synoptic data strongly support the existence of this new hurricane. Agree that the exact timing of extratropical to tropical transition is uncertain, but the choice of 3 January 1200 UTC is not unreasonable.)

**Agreed. Timing of tropical transition delayed by six hours.**

 2. Please include analyses for 7 January in the binder.

**Done.**

1938 Storm #2 (originally #1):

 1. Please add binder maps for 5-7 August. The committee notes that the Historical Weather Map (HWM) for 5 August shows a ship with wind W 10 kt and 1010 mb near 12N 45W, which would be somewhat consistent with a cyclone near the Leeward islands three days later. Please mention this in the discussion of the genesis.

**Done.**

 2. Are there any detailed observations from San Juan that show where the center passed in respect to the station? It would be a good idea to get the San Juan OMR (which may have been used in the original analysis) to confirm the proposed change in the track.

**The San Juan OMR is not available. The Climatological Data for the West Indies and Caribbean Service was obtained, however, for August 1938. This did not provide any quantitative observations of use, but suggest a rather weak cyclone. Text from this is now included into the metadata.**

 3. Please better explain why the 35 kt wind observation on 9 August and the 1002 mb pressure observation [on 10 August] were discounted. In particular, why is the latter considered bad?

**It is agreed that the 35 kt observation late on the 9th indicates that the cyclone maintained tropical storm intensity up until dissipation after 18Z on the 9th.**

**The latter observation is likely bogus, as one hour and 35 nm apart from the 1002 mb ship, another ship (HO008166) reported 1016 mb, which fits in better with other observations.**

 4. Perez considers this to be a tropical storm in Cuba. What data is Perez using to make this determination? Also, please provide binder maps and plotted analyses for 10-11 August. Richard Pasch comments: Does Perez have any data that support his statement that this system made landfall in Cuba on 10 August as a tropical storm?

**The maps and analyses for the 10th and 11th have been included. Ramon Perez was contacted and was unable to provide any specific observations that supported that the system had a closed low on the 10th or 11th, nor any measurements of 35 kt on those dates. Thus the cyclone is still is shown as dissipating late on the 9th over Hispaniola.**

5. Richard Pasch comments: In the metadata discussion, Tortola is misspelled as “Tortolla”.

**Corrected.**

1938 Storm #3 (originally #2):

 1. The committee notes that this system was moving very fast from 10-12 August – on the order of 25-30 kt. Is it possible that during part of this time that it was an open wave with tropical-storm-force winds north of the vorticity center? The metadata summary states that no west winds were observed. Is that because there were no ships in the areas where west winds might have been? Please review the genesis time in light of this.

**There were few to no ships in the area where west winds could have been. On the 11th at 00Z, with our analyzed position at 13.9N, 67.3W, there were ships at 11.8N, 67.5W and 12.0N, 68.0W. The former reported a light or missing SE wind with 1013 mb, and the latter reported 15 kt E with 1011 mb. On the 11th at 12Z, with our analyzed position at 15.0N, 72.7W, ships at 14.5N, 70.5W and at 14.3N, 69.6W recorded 15 kt SE. On the 11th at 21Z, with the interpolated analyzed position at 16.6N, 76.8W, a ship at 15.4N, 76.2W recorded 15 kt SE with 1016 mb.**

**On the 12th at 00Z we had those obs from Kempshot, Jamaica – the 40 kts and 1006 mb.**

**On the 12th before 12Z, we had the 83 kts at Grand Cayman followed by the 996 mb with 70 kts. Note that 996 mb was not a minimum pressure value; it was Grand Cayman’s 12Z pressure at synoptic time.**

**It is possible that this was an open wave on the 10th and part of the 11th, but we cannot know. This point is now mentioned explicitly in the metadata: the system was moving very fast and it could have been an open wave on the 10th and early on the 11th. But there is not enough evidence to say that it was not closed due to a lack of available observations south of the center. Thus the original HURDAT of genesis at 00Z on the 10th is retained.**

 2. On a related note, what additional observations can be obtained from the Windward Islands, the Netherland Antilles, or northern Venezuela? These could help resolve the issues in point 1.

**After consulting the NOAA Data Rescue website -** [**http://docs.lib.noaa.gov/rescue/data\_rescue\_home.html**](http://docs.lib.noaa.gov/rescue/data_rescue_home.html) **- no additional observations were readily available for these countries.**

 3. What data is available from the northeastern Yucatan Peninsula? The committee notes that observations from the Mexican Meteorological Service were used, yet no such obs are quoted in any of the summaries. Indeed, only a few are seen in the binder data summary, and only one is available from Cozumel, which was the closest observation to the center. Please explain this.

**Mexican surface maps and daily summary have become available on-line and indicate that the cyclone went through the Yucatan Channel. The hurricane-force winds likely stayed offshore both Mexico and Cuba, which is consistent with analyses from both countries.**

 4. The committee does not concur with the proposed downgraded intensity at the Louisiana landfall. While the hurricane-force winds reported along the Louisiana coast in the Monthly Weather Review (MWR) are estimates, it is likely stretching the Lake Charles data too much to justify a 5-kt change in the landfall intensity – especially since Lake Charles is 25-30 miles inland and it is unknown whether the radius of maximum winds passed over the station. Local media accounts from Lake Charles might help resolve this. Richard Pasch comments: Since there was an estimate of hurricane force winds at Grand Cheniere, and the data are not definitive enough to distinguish between an intensity of 60 kt or 65 kt, my recommendation is to retain this system as a minimal hurricane at landfall in Louisiana.

**Agreed, original intensity of a 65 kt Category 1 hurricane is retained.**

 5. On a related note, since the proposed 995 mb landfall pressure is based on assumptions and large extrapolation from the Lake Charles data, it should not be included as a central pressure in HURDAT. The section of the metadata summary starting with “Assuming that Lake Charles…” should be re-written.

**Agreed.**

1938 Storm #4 (originally #3):

 1. Please provide binder maps for 21-22 August.

**Done.**

 2. What observations are available from the Netherland Antilles, given how close the system was to them near genesis?

**The only observations available from the Netherland Antilles are from Curacao at 12Z daily. These on the 21st and 22nd did not provide any low pressures, strong winds, or westerly winds.**

 3. One Mexican observation from Cozumel was available for the previous storm, yet none are available for this storm. Please obtain Cozumel obs from Mexico if at all possible given how close the proposed track is to the island, or at least obtain information on the impacts of this system at Cozumel.

**The Mexican synoptic map, summary and newly available daily observations (via NCDC’s new EV2 system) were obtained for this hurricane. Crucial observations from Cozumel, Progresso, and Merida were obtained, which indicate that the system was a major hurricane at landfall in Yucatan.**

4. The committee has concerns about the proposed weakening over the Yucatan Peninsula. Storms such as Gilbert (1988), Emily (2005) and Dean (2007) have all weakened far more than the proposed 20 kt used here. Admittedly, those were all much stronger storms than this seems to be. Please re-examine this situation to 1) find storms more analogous to this one, 2) examine whether the use of the Florida decay rate is in the Ho et al model appropriate, and 3) examine the possibility this was a major hurricane before the Yucatan landfall. Richard Pasch comments: The weakening from 80 kt to 75 kt after the hurricane moved into the Bay of Campeche does not seem reasonable, especially since it is acknowledged that the system could have been stronger at landfall in Mexico on 28 August. It is suggested that an intensity of 80 kt should be maintained on 27 August, and up to landfall on 28 August.

**The significantly boosted intensity at landfall (105 kt at 02Z) is now subsequently followed by 80 kt at 06Z and 75 kt at 12Z. The hurricane made oceanfall in the Gulf of Mexico around 10Z with the previously documented ship recording 979 mb central pressure just before 12Z. The decay rate is similar to (but just slightly less than) Kaplan-DeMaria, which is now described in the writeup.**

 5. In the metadata summary “hurricane force winds were recorded 28 hours before landfall…”: There is an inference here that the observation of hurricane-force winds more than a day before landfall played a role in changing the landfall intensity in the original HURDAT. The committee concurs with the proposed change. However, the metadata summary should be re-written to play down the importance of the older data and to play up the possibility of interpolation in the original HURDAT.

**Agreed. Note: No additional observations became available for Tampico with the new EV2 website.**

1938 Storm #5 (new):

 1. The committee notes that the 7 September HWM shows an observation of NNW 25 kt near the Cape Verde Islands, and the 8 September HSM shows an observation of W 10 kt near 13N 35W. While these suggest the possibility of a tropical cyclone, these positions don’t extrapolate very well to the proposed genesis location for this storm. Please examine these and other available data to see if they could be an earlier part of this cyclone.

**It is noted that Sao Tiago in the Cape Verde Islands recorded a NNW 25 kt with 1013 mb at 12Z on the 7th and that HWM indicated a low east of there. However, this appears to be a separate system from the new tropical cyclone being added to HURDAT here. No further development of this second system apparently occurred.**

 2. The committee concurs with adding this system, but also realizes that the supporting evidence for it is a little thin. Please reflect the uncertainty in the metadata summary. Richard Pasch comments: Observations, albeit limited, do seem to support adding this system as a new tropical storm. However, the life cycle depicted in the best track seems a little odd as the system is not shown as strengthening to a tropical storm until just before it weakens.

**Agreed and added. The life cycle does seem somewhat unusual with a TD stage lasting four days followed by a one day TS stage, but this is all that can be concluded from the available data.**

 3. Please include the binder maps for 15-16 September. This will also aid the analysis of another nearby system (see Additional Systems below).

**Done.**

 4. Richard Pasch comments: Also, in the metadata discussion, 10 lines before the end, replace “further” with “farther”.

**Done.**

1938 Storm #6 (originally #4):

 1. Please include a binder map for 9 September.

**Done. After obtaining this and adding on the rather numerous COADS observations, a closed circulation had developed by 12Z on the 9th just offshore from the West African coast. Thus genesis is begun 18 hours earlier than indicated in HURDAT.**

 2. On the plotted map for 0000 UTC 11 September, there is a S 50 kt/mph observation plotted next to a S 25 kt/mph. Is the former observation the basis for raising the intensity at this time to 45 kt? If so, this looks a little strange given the second, much lower, wind observation nearby. Please clarify this and include the explanation in the metadata summary if need be.

**Note that the initial work on this system conducted by Donna Strahan back in 2005 was plotted in units of mph. Thus the observations in question are actually a S 40 kt and a S 20 kt reports, as correctly indicated in the excel database and daily summary. The 40 kt observation is the reason for boosting the assessed intensity from 35 to 45 kt at 00Z on the 11th. Given the rather poor navigational tools available in 1938, it is quite likely that the relative positions of the two ships with regards to the storm’s center may be off by 30 or even 60 nm.**

 3. The HWM show a ship with W 20 kt/mph and 1008 mb near 17N 37W at 1200 UTC 13 September. If this is correct, is it possible that proposed position for that time (already nudged northward) needs to be nudged farther northward?

**There was a 15 kt WNW at 16.6N, 37.4W in COADS at 12Z on the 13th. Agreed to adjust the position northward by 0.2N to 17.2N at that time.**

 4. The committee notes that the original HURDAT assessment of a 140 kt peak intensity is not directly supported by any data. The current data is not sufficient to justify changing this, but the uncertainty should be stated in the metadata summary.

**Agreed and done.**

 5. The 938 mb central pressure at 1200 UTC 21 September may be at least partially based on the work of Myers and Jordan in the July 1956 MWR. They extrapolated the ship pressures and the observed pressure gradient to derive an estimated pressure of 27.75 in (939.8 mb) at 1200 EST (17Z) 21 September and this combined with the observed changes may have been used for the HURDAT value. It is noted that they derive a similar central pressure for the afternoon of 20 September, which was not used in HURDAT. Unless some other source for the 938 mb reading can be found, the committee suggests changing this value to 939 or 940 mb. The metadata summary should be re-written to reflect this.

**Agreed to change the 938 mb central pressure in HURDAT at 12Z on the 21st to 940 mb passed upon the work of Myers and Jordan.**

 6. As with some of the storms of 1937, there is an issue with the use of the Landsea et al wind pressure-relationship for a system undergoing extratropical transition. The metadata summary should reflect this uncertainty.

**Agreed and added.**

 7. Two comments on the 21 September daily metadata: First, “W-E cold front” should probably be “N-S cold front”. Second, this is a very long write-up. Please shorten it by removing the parts about the damages and observations that are not necessary to determine the track and intensity.

**Done.**

 8. The committee agrees with the proposed landfall intensities of 105 kt at Long Island and 100t in Connecticut. The committee’s consensus is that the system was a tropical cyclone undergoing extratropical transition during the two landfalls, then became extratropical over New England after landfall. Therefore, it concurs with changing the time of extratropical transition in HURDAT. Richard Pasch’s comments: The surface data do not indicate that cold air penetrated the core of the cyclone at landfall in New England. Therefore, I agree that the system was still a tropical cyclone at 1800 UTC 21 September. Also, based on the wind measurement at Fishers Island and the observed storm surges, the intensity at landfall should be that of a major hurricane. A value of 105 kt, as shown in the revised best track, seems quite reasonable.

**Agreed.**

1938 Storm #7 (originally #5):

 1. There is a need for a zoomed-out binder map for 10 October.

**Done.**

 2. Since the pressure at Tela, Honduras was 1005 mb at 00Z 11 October, is it possible that genesis occurred late on 10 October.

**Agreed. Genesis is now indicated to be 18Z on the 10th of October.**

 3. There is a need for a summary for 18 October to show the dissipation of the cyclone.

**Done.**

4. Richard Pasch’s comments: No significant changes were made to the best track positions or intensities for this storm, and the minor modifications appear to be justified.

**Thank you.**

1938 Storm #8 (originally #6):

 1. The HWM show a low near 23N 65W on 15 October. Does the COADS data show this to be correct, and could this be related to the cyclone?

**The HWM and COADS were obtained for the 15th. The available observations suggest a sharp trough. Thus genesis is not begun on this date.**

 2. The HWM for 16 October show Bermuda near the apex of the trough with winds N 15 kt/mph and a 1013.2 mb pressure. This implies a low pressure area of about 1012 mb, which would at least be closed isobars given the surrounding pressures. Please re-examine the genesis time in light of this.

**Agreed to show genesis at 12Z on the 16th, just southeast of Bermuda.**

 3. Is there any data available from Bermuda for this system?

**No other additional observations (beyond the 12Z measurements) are available.**

 4. The committee concurs with the reduction in intensity. It also concurs with there being insufficient data to remove the system from HURDAT and the statement to that effect in the metadata summary.

**Thank you.**

 5. Richard Pasch’s comment: Consider the possibility that the 35-kt ship report on October 17 may have been closer to the center (i.e. adjust the best track somewhat to the north and west, or the ship may have actually been farther south or east), in which case the downgrade from a tropical storm on that date is not warranted.

**The position of the cyclone on the 17th cannot be moved farther north due to two other ships the both indicate easterly winds between 33.5-35N. The 35 kt NE ship over 300 nm north of the cyclone was likely more associated with the strong pressure gradient existing poleward due primarily to a high pressure and rather indirectly due to the cyclone itself. Thus the reduction in intensity on the 17th is retained.**

 6. Richard Pasch’s comment: Also, in the metadata discussion, line 27, replace “further” with “farther”.

**Done.**

1938 Storm #9 (originally #8):

 1. The committee is not in favor of the genesis of this system south of Hispaniola. While there is a ship with southwesterly winds south of the Mona Passage on the 6 November HWM, at that time Port-au-Prince, Haiti has a lower pressure than Santo Domingo, Dominican Republic. This casts doubt on the center location on that day. The committee also has concerns that a weak cyclone would not survive passage over the mountains of the Dominican Republic. Please re-examine the genesis in light of these points, and see if genesis north of Hispaniola on 7 November is more appropriate. Richard Pasch’s comments: The location of origin of this storm, practically along the south coast of Hispaniola, looks very unrealistic.

**Agreed. Genesis is now indicated to occur north of Hispaniola at 06Z on the 7th.**

 2. On a related note, can additional data be obtained for Santo Domingo that might shed some light on the genesis?

**Only 12Z observations via HWM are available for Santo Domingo.**

 3. There is a typo in the 7 November metadata – “20.63” should be “29.63”.

**Corrected.**

 4. Does the data allow the track over the Bahamas to be shifted somewhat to the north, closer to the stronger winds?

**Yes, the center can move moved some to the north on the 8th, though the system does appear to have substantial troughing to the south of the center.**

 5. Who are Millas and Ortiz mentioned by Perez? They need to be added to the list of references on the project web site (http://www.aoml.noaa.gov/hrd/hurdat/References.html).

**Given that we do not have access to Millas and Ortiz, these references are removed from the writeup.**

 6. The HWM show a circulation over the northwestern Caribbean Sea near 18N 82W on 11-12 November. Is this associated with the remains of the cyclone?

**The troughing apparent in the northwestern Caribbean Sea on the 11th and 12th may be a combination of the remnants of the cyclone along with a former frontal boundary.**

1938 Additional Notes:

 1. The April MWR (pages 106-107) mentions a long-lived relatively low-latitude storm of “limited area” that moved eastward across the Atlantic from 12-23 April. There is at least one report of storm-force winds and a (non-central) pressure of 988 mb. The HWM suggest the system became non-frontal for at least part of its life. Please investigate this system to see if it were a subtropical or tropical storm.

**(Now, System #1): This has now been investigated, but not added into HURDAT. The following is the writeup: “HWM indicates a long-lived extratropical low moved across the Atlantic during mid-April. The system exhibited gale to storm force winds and central pressure below 987 mb. While the cyclone may have gotten some of its energetics from convective processes, the system remained too baroclinic to have transitioned to a tropical (or subtropical) cyclone. Thus the cyclone will not be added to HURDAT.”**

 2. System #5: Is the system actually dissipated on 11 September? It looks a little strange to have dissipated and then a position. Also, it might be a good idea to check the Texas OMRs/Climate data to determine the pressures as the system moved ashore.

**(Now, System #6): The status is changed to “Tropical Depression” on the 11th and “Dissipated” on the 12th. Additional OMRs were obtained, with 1008 mb at Corpus Christi late on the 10th being the lowest observed. Thus there is no indication that the system became a tropical storm.**

 3. System #6: Are the listed positions correct? The HWM shows NW winds 30 kt/mph and 1007 mb in Veracruz, Mexico on 10 September, with a wind shift to south winds the next day. In addition, a station east of Veracruz also reported 1007 mb on 10 September, suggesting that a low of some sort was over the Bay of Campeche south of the proposed positions. Please re-examine this.

**It is agreed that the positions are too far north and they have been shifted southward. This system may have either been a small tropical depression or some funneling of the flow along the Mexican coast to enhance the Veracruz wind observations.**

 4. System #9 (former storm #7): The committee does not concur with the removal of this system at this time. There is a concern that this storm was analogous to Josephine (1996), which formed near a frontal boundary but stayed in the warm air long enough to become a tropical cyclone. While this system clearly became extratropical by 1200 UTC 24 October, the hand-plotted map at the time of the first gale (0000 UTC 24 October) shows temperatures in the 70’s surrounding the system. If the land station data is available, please re-plot the maps for the time period 1800 UTC 23 October-0600 UTC 24 October so the committee can better assess the structure. AN OMR for Apalachicola might also be useful in assessing the frontal nature. Richard Pasch’s comments: I do not agree with removing the original TC #7 from HURDAT. It seems unlikely that the system was extratropical while located so far south and west in the Gulf of Mexico.

**All of the OMR’s along the U.S. Gulf coast were obtained for this system. This allowed for more in-depth thermal and structural analysis of this system at 00Z on the 24th. This confirms that the system was frontal – the frontal boundary extended through the cyclone making this an extratropical cyclone, rather than the system being located in the warm sector equatorward of a frontal boundary. Note also that the first indication of tropical storm force conditions – at 00Z on the 24th – the system had reached 27N latitude. Thus with this additional analyses, it is still the recommendation to remove this cyclone from HURDAT.**

 5. The low pressure area north of proposed new storm #5 looks very interesting in the HWM on 13 September, with a central pressure below 1005 mb and much higher pressures to the north and northeast. Please investigate this system to see if it were a tropical or subtropical cyclone.

**HWM, COADS and the MWR tracks of lows indicate that a tropical depression existed from the 12th through the 14th of September. On the 11th of September, a stationary frontal boundary was located just south of Bermuda. From the 12th through the 14th, a small closed low occurred after the frontal boundary had dissipated. On the 15th, the system was absorbed into a frontal boundary. Peak winds observed within the cyclone were 25 kt and lowest pressure of 1006 mb. As there is no indications of gale force winds, this system is not added into HURDAT.**

 6. System #10: Has COADS been checked for this system?

**Yes, COADS was part of the data sets investigated for this system. This is now so indicated.**

 7. The committee concurs with leaving the other possible systems out of HURDAT.

**Thank you.**

8. Richard Pasch’s comments: A couple of systems, #3 and #5 could very well have been tropical storms, but agree that there is insufficient evidence to include them.

**(Now #4 and #6): Agreed.**