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September 13, 2010

Ms. Barbara Schroeder
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National Marine Fisheries Service
1315 East West Highway
Silver Spring, MD 20910

Ms. Sandy MacPherson
Fish and Wildlife Service
7915 Baymeadows Way, Suite 7915
Jacksonville, FL 32256

RE: Loggerhead Proposed Listing Rule
75 Fed. Reg. 12,598 (March 16, 2010)

Dear Ms. Schroeder and Ms. MacPherson:

On behalf of the Southern Shrimp Alliance, I am pleased to submit the attached comments regarding the Proposed Rule Listing Nine Distinct Population Segments of Loggerhead Sea Turtles as Endangered or Threatened.

Sincerely,

John Williams
Executive Director

COMMENTS OF THE

SOUTHERN SHRIMP ALLIANCE

REGARDING THE

PROPOSED RULE LISTING NINE DISTINCT POPULATION SEGMENTS OF LOGGERHEAD SEA TURTLES AS ENDANGERED OR THREATENED

Submitted: September 13, 2010

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The Southern Shrimp Alliance ("SSA") submits the following comments regarding the Proposed Rule Listing Nine District Population Segments of Loggerhead Sea Turtles as Endangered or Threatened, 75 Fed. Reg. 12,598 (March 16, 2010)("Proposed Rule"). SSA is a non-profit alliance of members of the shrimp industry in eight states committed to ensuring the viability of America's domestic shrimp industry. SSA serves as the national voice for the shrimp fishermen and processors in Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, and Texas. In preparing these comments, SSA has undertaken new and substantially more detailed analyses of the factual, scientific, and legal basis for the Proposed Rule. Our comments reflect the results of this new and more detailed research and analysis.

I. <u>Introduction</u>

In August 2007, the National Marine Fisheries Service ("NMFS") and the Fish and Wildlife Service ("FWS")(collectively the "Services") evaluated whether loggerhead turtles should be divided into Distinct Population Segments ("DPSs"). The Services concluded: "[B]ased on the best scientific information available, the ... current listing is valid...." Loggerhead Sea Turtle 5-Year Review: Summary and Evaluation, NMFS and FWS, August 2007 ("2007 Status Review") at 3. Regarding whether loggerhead turtles should be reclassified as endangered, the Services concluded: "Based on the best scientific information available, we do not believe the loggerhead turtle should be delisted or reclassified." *Id.* at 40. Three years later, the Services promulgated the Proposed Rule reversing their previous positions that were based on "the best scientific information available."

What changed? The answer is that the Services assembled a new Biological Review Team ("BRT") that developed and applied new population assessment and extinction prediction models. The results were described in a report titled Loggerhead Sea Turtle 2009 Status Review Under The U.S. Endangered Species Act, August 2009 ("BRT Report"). That Report is incorporated by reference into the Proposed Rule and provides the foundation for the Proposed Rule. However, the BRT Report and the Services' reversal of position are based on an admittedly slender reed. The BRT Report itself notes the flaws and unreliability of the models.

- "Estimates of quasi-extinction risks are known to have high degrees of uncertainty...." BRT Report at 36.
- The models' values "are not indicative of a true probability of quasi-extinction...." *Id.*
- "[The] analysis indicated potential overestimations ... of anthropogenic mortalities...." *Id.* at 67.
- "[O]ur matrices may provide a false sense of urgency." *Id.*

One year after the BRT Report, the National Research Council of the National Academies of Science ("NAS") assessed the scientific merits of the analysis and models in the BRT Report, the foundation for the Proposed Rule. Assessment of Sea-Turtle Status and Trends: Integrating Demography and Abundance, National Research Council of the National Academies, July 15, 2010 ("NAS Report").

- Regarding the scientific rigor with which the BRT analyses were done, NAS stated: "Reviews of federal population assessments and research plans are not sufficiently rigorous and transparent." NAS Report at 7. In other words, a thorough analysis of data was not done.
- As to the models used in the BRT Report, the NAS Report states there was "no attempt to fit models to data." *Id.* at 90.
- Regarding whether the best available scientific information was used, the NAS concluded: "Inadequate information is available for population assessments because the data have not been collected, or if they have been collected, they have <u>not been analyzed</u> or made accessible in a manner that allows them to be useful." *Id.* at 104. (Emphasis added.) In other words, in a glaring failure of scientific process, the Services had data they failed to use. Moreover, the data the Services did use were inadequate to support their conclusions.
- Further commenting on the BRT Report, NAS states the models were "a heuristic exercise with little or no real power for prediction...." *Id.* at 90. NAS' characterization of the BRT models as a "heuristic exercise" is particularly damning given that Webster's New Collegiate Dictionary defines "heuristic" as "unjustified or incapable of justification." In simple English, NAS concludes the models and analysis in the BRT Report cannot justify the Proposed Rule.

That the NAS' scientific review of the methodology and process used to justify the Proposed Rule revealed the methodology and process, and the resulting analysis, to be fundamentally flawed should end the debate about whether the Proposed Rule is arbitrary and capricious and not in accordance with law.

Furthermore, the Proposed Rule is legally defective because it is contrary to the Services' own policy and to Congressional intent. The intent of Congress is reflected in the policy adopted by the Services in 1996 regarding DPS designations, 61 Fed. Reg. 4722 (Feb. 7, 1996)("DPS Policy"). That Policy states Congress intended that DPS designations be used "sparingly." S. Rept. 151, 96th Cong., 1st Sess., at 6, cited at 61 Fed. Reg. 4725. The sentence in the Senate Report cited with approval in the DPS Policy also states a DPS designation should occur "<u>only</u> when the biological evidence indicates such action is warranted." *Id.* (Emphasis added.) Courts considering whether the DPS Policy requires the Services to follow this Congressional intent have held: "The DPS Policy expressed an intent to follow that instruction." *Northwest Ecosystem Alliance v. United States Fish and Wildlife Service,* 475 F.3d 1136, 1144 (9th Cir. 2007).

Congress elaborated further on the appropriate evidentiary standard for DPS designations stating that listing a DPS "may be necessary when the "preponderance of evidence indicates that a species faces a widespread threat but conclusive data is available with regard to only certain populations." S. Rept. 151, 96th Cong., 1st Sess., at 6. (Emphasis added.) In a recent decision, the United States Court of Appeals for the Ninth Circuit noted with approval the fact that in applying the Evolutionary Significant Unit ("ESU") Policy, the admitted twin of the DPS Policy, NMFS used the Congressionally mandated standard that there must be "conclusive evidence" to justify a DPS listing. *Modesto Irrigation District v. Gutierrez,* No. 09-15214, 2010 WL 3274499 (9th Cir., Aug. 20, 2010) at *3.

Congress intended that the Services be held to a high evidentiary standard in making a DPS designation. The Services have incorporated that intent into the DPS Policy. The courts have approved this evidentiary standard. The Proposed Rule fails to meet the required legal standard.

In light of these evidentiary and legal failings, all of which are discussed in detail below, SSA urges that this Proposed Rule be issued in final form with no DPS designations and no change in listing status. To proceed with DPS and endangered designations will have unmitigable impacts on people whose lives and livelihoods will be affected if the Proposed Rule is made final. The data discussed below are so tenuous that to proceed with this rulemaking is completely inappropriate. This is particularly true when NMFS is now undertaking a major survey of the in-water abundance of loggerheads. As the Administrator of the National Oceanic and Atmospheric Administration stated in an August 24, 2010 letter to Congressman Walter Jones, this study will develop new models for estimating population levels and will provide new and more complete abundance and population trend data to be used in those models, consistent with the recommendations in the NAS Report. NAS Report at 103-105. To act now, in the absence of data that may fundamentally alter the bases for the Proposed Rule, and without analyzing data the Services already have, is unconscionable and would fly in the face of the recommendations in the NAS Report.

II. The Models

A. The Data Inputs

Everyone who has taken a computer or software design course knows that when models are constructed and applied, the results are only as good as the data inputted and the assumptions made. As one wag indelicately put it, "garbage in, garbage out." Thus, it is important to examine the data used, and the assumptions made, in the models that provide the foundation for the Proposed Rule. That examination must also be done in the context of the

ESA's standard that the Services must use the best available scientific and commercial data. 16 U.S.C. §1533(b)(1)(A). The models fail this standard.

1. <u>The Most Current Nesting Data Were Not Used</u>

Given that the Proposed Rule depends on the models to assess the current status and future trend of the loggerhead population in each proposed DPS, it is intuitively obvious and important that the model use the correct current population numbers, *i.e.,* we have the right starting point. That did not happen.

For the single most important component of the proposed Northwest Atlantic DPS, the Peninsula Florida Recovery Unit, the model used the nesting data only through 2007. That data showed a decreasing nesting trend. BRT Report at 44, Figure 4. 2008 data showing an increase in the nesting population was not used. Given the ESA's requirement to use the best available scientific data, this is a curious omission – an omission made even more curious by the fact that the 2008 data had already been adopted and used by the Services' in their Loggerhead Recovery Plan. NMFS and FWS 2008, at 1-9, Figure 5. That Recovery Plan preceded the BRT Report by approximately eight months and the Proposed Rule by approximately fourteen months. By excluding the most recent population data, data already used by the Services in the Recovery Plan, the Services effectively changed the results that should have been obtained from their models.

Similarly, the models did not use the 2008 nesting data for the proposed North Pacific DPS. BRT Report at 40, Figure 1. As is the case for the proposed Northwest Atlantic DPS, adding the current data would have changed the result because the new data showed a significant increase in the current nesting population. Curiously, according to the data files NMFS provided to SSA, the BRT Report notes the existence of the 2008 data but then fails to use it. BRT Report at 40.

For the Indian Ocean, 2000 was the last year of nesting data used in the model. *Id.* at 42. The recognized expert on Indian Ocean loggerheads, Ronel Nel, reviewed the draft BRT Report and the models, saw that the nesting population data used stopped at 2000, and provided the Services with Indian Ocean loggerhead nesting data through the 2008-2009 nesting season. Available at <u>www.nmfs.noaa.gov/pr/species/statusreviews.htm</u>. The several years of additional data were not used. The unused data showed a pronounced increase in the current nesting population. Using the correct current data would have changed the model's results.

Omitting data that show an increase in nesting populations will inevitably produce a downward nesting trend. Data showing an increase in the nesting population necessarily changes the results. For whatever reason, in the preceding examples, the Proposed Rule relies only on data showing a decline in the nesting population. The Proposed Rule is not based on the best scientific information available as required by the ESA.

2. In-Water Survey Data Were Ignored

The failure to use the most current nesting population data is exacerbated by the BRT's failure to consider existing data from in-water (versus on the beach) studies. These data also showed an increase in loggerhead populations. The 2009 Turtle Expert Working Group Report ("TEWG Report") prepared for the Services by experts appointed by the Services, examined the results of four in-water turtle abundance surveys covering a large portion of the loggerhead habitat in the southeastern U.S. These studies reported increased abundance for neritic loggerheads in the southeastern U.S. which is home to the dominant nesting population in the proposed Northwest Atlantic DPS. TEWG 2009 at ix. This very large cohort of male and female loggerheads will be added to the breeding population in the immediate future. In other words, the conclusions in the Proposed Rule about continuing declines in nesting females are suspect. The results of the in-water population counts need to be considered. They were not – notwithstanding the fact that the data were reported by the Services' own Turtle Expert Working Group.

The finding in the TEWG Report about the number of females that will be added to the nesting population are confirmed by a 2010 study conducted for NMFS by the South Carolina Department of Natural Resources. This NMFS sponsored study confirmed the increased abundance of maturing loggerheads next in line to join the breeding population. The study showed that today's loggerhead abundance, over all sizes and sexes, is 43 times greater than in the 1950-1976 period, 14-17 times greater than in the late 1970s, and 23 times greater than in the late 1980s and early 1990s. Contrary to the conclusion in the models and the Proposed Rule, loggerhead extinction is not imminent.

The South Carolina study also showed that the abundance of the smallest sized turtles appears low. If this pattern holds, another decrease in nesting will follow the next pulse of adults through the system. These patterns of alternating high and low adult abundance are consistent with a density dependent species like loggerheads. Just as the models fail to account for the coming increase in adult loggerhead numbers, the models fail to account for the next decrease. Instead, the models make straight line projections based on old data showing only population decreases. This failing further demonstrates that the models are an inappropriate tool to measure fluctuating population trends and to predict extinction. The inappropriateness of ignoring the in-water data sources was also made clear in the NAS Report that concluded that turtle population assessments are based “too heavily” on estimates of adult females on nesting beaches and fail to account for, or consider, other data sources. NAS Report at 103.

3. Incorrect Population Parameters Were Used

One of the population parameters that is vital to the accuracy of the model is the number of nests per female per season. This parameter was modeled by the BRT as being constant for the next 100 years. This is incorrect because older female nesters produce more nests per season than new nesters. TEWG 2009 at x. The models assume a constant rate for the number of nests. Clearly, this cannot be accurate given the large number of females about to be added to the breeding population and the possibility of a naturally fluctuating decrease that may follow some years later. The point, as the NAS concluded, is that the models, which assume constant, straight line data, cannot be relied on to accurately predict population trends or to predict extinction.

4. Incorrect Anthropogenic Mortality Numbers Were Used

In evaluating the future status of the loggerhead population in the proposed Northwest Atlantic DPS, the past is prologue and it is important to account for past history that affects the future. The Services have long contended that loggerhead bycatch in the shrimp fishery is the most significant source of loggerhead mortality and is largely responsible for declines in the proposed Northwest Atlantic DPS. In 2002, NMFS estimated the lethal take of loggerheads in southeastern U.S. shrimp trawls at 62,294. Epperly *et al.* 2002, NMFS Biological Opinion 2002. With improvements in turtle saving net designs called Turtle Excluder Devices (“TEDs”), the estimated lethal take of loggerheads was expected to decline to 3,948 annually. *Id.* In 2009, NMFS estimated that the annual loggerhead mortality in shrimp trawls had actually dropped to 647 by 2007 due to further TED refinements and use and a decline in fishing effort. NMFS 2009 Biological Opinion at 65. This represents a 99% reduction in mortality.

If fisheries bycatch is the culprit in the past decline of loggerhead populations, the cause would clearly be due to historical practices that have now been successfully modified. If the historical shrimp fishery was implicated in the decline of loggerhead nesting populations that began about 1998, one would be looking for causative agents corresponding to increased takes of loggerheads that were ten-plus years of age given the facts that (1) nesting begins at age 30, and (2) loggerheads are not large enough to be caught in shrimp nets until they are about ten years old. Thus, one would be looking for something that began about 20 years prior to 1998, the late 1970s. The late 1970s featured a steep increase in shrimping effort.

Using the same rationale, the causative agent for the increase in nesting populations that began in 2008 would be something occurring in the late 1980s and early 1990s. That is when TEDs came into use. The population assessment and prediction models relied on in the Proposed Rule never discuss, or account for, the reduction in loggerhead mortality associated with changes in shrimping. Rather, the models assume the past has not changed and assume incorrect annual mortality numbers into the future.

The facts are that the Services continually and repeatedly failed to input and use the best available scientific information. Significantly, this is not a case of competing data sets or different interpretations by experts. All the data discussed in this and the preceding subsections was developed, accepted, and used in other contexts by the Services, by experts such as the Turtle Expert Working Group appointed by the Services, or by governmental agencies commissioned by NMFS. The BRT, however, did not use the data, contrary to the ESA's mandate to use the best available data.

5. Opinion Was Substituted For Fact

The BRT Report compounds the problems discussed above through the methodology used to develop other "data" which was then inputted as the anthropogenic mortality affecting loggerhead population levels and trends. For each category of anthropogenic mortality (habitat destruction, overuse by fishing and similar activities, disease and predation, and other), a group of unidentified "experts" was asked to give their opinion as to whether the threat is high, medium, low, or very low. Thus, this model was built, not on data and analysis, but on opinion.

This problem was highlighted in the NAS Report and echoed in the comments of Peer Reviewer #7 who stated:

> I am concerned that using opinion provides an illusion of knowledge about vital rates for which there really is no good knowledge. The opinion-based threat "estimates" resulted in population [change] estimates for different DPSs that are so uncertain ... as to be arguably of little value.

Review found at http://www.nmfs.noaa.gov/pr/species/turtles/loggerhead_status_peer_reviews.htm. Reviewer #7 went on to call the model's methodology and results "so unrealistically pessimistic as to be only marginally helpful." *Id.*

Exacerbating the defect of relying on opinion instead of facts and analysis, the model assigns to each threat ranking an actual mortality rate, *i.e.,* the number of dead turtles supposedly represented by a high, medium, low, or very low ranking. In other words, the BRT started with the opinions of unknown "experts" and then assigned each threat ranking (high, low, etc.) an arbitrary anthropogenic mortality rate that is then converted into a finite anthropogenic mortality number which is then inputted into the models as if it were scientific data to estimate loggerhead population trends. This anthropogenic mortality rate is added to natural mortality to develop a picture of total mortality that is then used in the models to predict population trends and extinction. However, the picture is wrong, and so are the models. As Reviewer #7 noted, this opinion and arbitrarily assigned mortality level "analysis" resulted in the computation that the loggerhead mortality caused by fishing in the open ocean (*i.e.,* not coastal shrimping) was so high as to not be believable. Reviewer #7 stated:

> There is no way that oceanic turtles (especially juveniles) incur an added 10-30% mortality due to bycatch. The only significant USA source of mortality to oceanic juveniles ... is pelagic longlining, which kills a trivial proportion of oceanic juveniles (*e.g.,* at most a few hundred mortalities per year in U.S. longline fleet vs. probably hundreds of thousands if not millions of animals in the oceanic juvenile stage. Even if you assume all reported mortality from the Mediterranean affects [only the Northwest Atlantic population], it's still probably in the low thousands which is probably <<0.01 of the number of oceanic juveniles out there. This is one prime example of the problem ... with the opinion-based assignment of mortality rates.

Id.

Another example of the problem associated with this opinion based system of divining anthropogenic mortality with respect to the proposed Northwest Atlantic DPS comes from reviewing the mortality estimates in Section 5.2.6.5 of the BRT Report. A tabulation of the estimated number of loggerheads taken as fisheries bycatch discussed in that section totals 3,743. BRT Report at 134-138. The tabulated mortality estimate of 3,743 is only 0.016 of the proposed Northwest Atlantic DPS loggerhead population estimated in the 1998 TEWG Report. However, based on the opinions of the unknown experts and the arbitrarily assigned mortality rate for the high, low, etc. threat rankings, the models relied on in the Proposed Rule assumed a fisheries bycatch mortality of between 0.13 and 0.50 of the population. BRT Report at 73. These <u>assumed</u> mortality rates used in the models are eight and thirty-one times higher than the <u>actual</u> take estimates listed in section 5.2.6.5 of the BRT Report. Once again, the Services relied on opinion and arbitrary numbers rather than actual data. Again, the Proposed Rule has not used the best available scientific data.

B. <u>The Diffusion Approximation Model</u>

The first of the two assessment models used by the BRT was a diffusion approximation model ("Diffusion Model"). This model was used to assess the probability of the loggerhead population going extinct in 100 years. A detailed scientific analysis of the fatal flaws in the application of the Diffusion Model is attached as an Appendix to these Comments. Suffice it to say that the Services' application of the Diffusion Model was so flawed as to make the results unusable.

First, as discussed in the immediately preceding section, the data used in the model was not the best available data. Second, by selectively using only certain data, the BRT skewed the results.

Third, the intuitively obvious thing required to determine the probability of extinction is to know the population level below which the species cannot fall without being doomed to extinction. Since the whole point of the analysis is to determine if and when the population might face extinction, one would expect the Services to specify a population threshold, or range, that is the critical viability point. That point would be used to judge when the population is approaching, or at, the level at which it cannot survive. The Services never provide that number. Thus, the Diffusion Model somehow concludes loggerheads face imminent extinction without ever telling us the extinction level – without ever telling us how many loggerheads must cease to exist before we reach the critical threshold below which the species cannot survive. How the Services can predict when extinction occurs without knowing the extinction level is a mystery.

Fourth, the Diffusion Model as applied by the Services does not actually provide direct probability estimates of extinction. Instead, the Services used a newly minted term called the Susceptibility to Quasi-Extinction ("SQE"). Of course, SQE has nothing to do with actual extinction since the Services neglected to develop that number. Rather, SQE is a probability of the probability that a specified percent of the current population will exist 100 years in the future. As the BRT Report admits, the values produced by the Diffusion Model "are not indicative of a true probability of quasi-extinction...." BRT Report at 36.

Fifth, scientists who support the concept of diffusion modeling concede that projections should be limited to 2.5 times the number of years for which nesting survey data are available. Here, many of the data series used by the Diffusion Model are for 20 years or less. Thus, the maximum reliable projection might be 50 years, assuming the data used were complete and accurate, which it was not in this case. However, the Diffusion Model makes predictions and projections out to 100 years, and it is that 100 year projection on which the Proposed Rule improperly relies.

Finally, scientists who support the concept of diffusion modeling also say that the assumptions employed must be evaluated to determine if the model is producing results within appropriate boundaries. In a major failure of scientific protocol and procedure, the Services did not do so. As discussed in the attached Appendix, the Diffusion Model as applied by the BRT produced results that were so clearly outside appropriate and acceptable boundaries as to make them meaningless.

In sum, because of these fundamental flaws, and those further discussed in the Appendix, the Diffusion Model cannot be used to justify the Proposed Rule.

C. The Threat Matrix Model

The second model relied on by the Services in the Proposed Rule was the Threat Matrix Model. Like the Diffusion Model, the Threat Matrix Model is fundamentally and fatally flawed.

First, it is worth recalling that the National Academy of Sciences called the models a "heuristic exercise with little or no real power for prediction...." NAS Report at 90.

Second, the Threat Matrix Model, as discussed above, is improperly based on the opinions of unidentified experts, with threat rankings given arbitrarily assigned actual mortality values, values inconsistent with the actual data.

Third, the BRT Report itself admits the Threat Matrix Report is fatally flawed. The mortalities used in the model "may not represent actual mortalities...." BRT Report at 58. The mortalities in the model "may overestimate the anthropogenic mortalities...." *Id.* Further, there were "potential overestimations by the experts of anthropogenic mortalities...." *Id.* at 67. The threat matrix "may provide a false sense of urgency." *Id.* And finally, the "analysis does not provide estimates for the likelihood or probability of extinction." *Id.* at 53.

The Threat Matrix Model is so poorly done, as evidenced not only by its methodology but also by the words of the NAS and the BRT itself, that its results, like the Diffusion Model, can be given no credence.

III. The Proposed DPS Designation – The Discreteness Standard

According to the DPS Policy, the first threshold a population segment must cross to qualify as a DPS is that it must be discrete. 61 Fed. Reg. at 4725. To be discrete, a population segment must meet one of two conditions. One condition, that it be delimited by international governmental boundaries, is nowhere mentioned in the Proposed Rule. Therefore, this condition cannot be a basis for any discreteness finding. The second condition is that the population segment is "markedly separated" from other populations of the same taxon because of physical, physiological, ecological, or behavioral factors. *Id.* Genetic or morphological discontinuity may provide evidence of this separation. *Id.* It is this second condition upon which the Proposed Rule relies.

At the outset, it is important to understand the required framework for analysis. First, the words "marked separation" contain two different standards. There must first be a separation and then that separation must be marked. Second, the DPS Policy states the word "marked" is to be given its "commonly understood" sense. *Id.* at 4723. Courts have construed the commonly understood meaning of "markedly" to be "appreciably." *Nat'l Ass'n of Homebuilders v. Norton,* 340 F.3d 835, 851 (9th Cir. 2003), *citing* Webster's New World Dictionary. Third, as noted above, Congress established an evidentiary standard, incorporated by reference into the DPS Policy, that DPS designations may be made only when the preponderance of biological evidence shows conclusively that it is warranted. Finally, the evidence used for this determination must be the best scientific and commercial data available. 16 U.S.C. 1533(b)(1)(A). As the Supreme Court has held: "The obvious purpose of the requirement that each agency 'use the best scientific and commercial data available' is to <u>ensure</u> that the ESA not be implemented haphazardly, on the basis of speculation or surmise." *Bennett v. Spear,* 520 U.S. 154, 176 (1997). (Emphasis added.) The discreteness finding in the Proposed Rule meets none of these standards.

Pursuant to the DPS Policy, these legal standards are to be applied to determine if the proposed DPS is markedly separated from other populations because of (1) physical, (2) physiological, (3) ecological, and (4) behavioral factors. Genetic and morphological discontinuity may provide evidence of these four factors. 61 Fed. Reg. at 4725. Unfortunately, the Proposed Rule does not analyze the four discreteness factors in any organized fashion. Instead, and perhaps to camouflage the weakness of its case, the Proposed Rule presents a free flowing and undifferentiated discussion. More importantly, the Proposed Rule reaches conclusions without analysis or explanation, as if making the statement is sufficient proof by itself. Despite the failure of the Proposed Rule to present an organized analysis of each of the four discreteness factors, this Comment will be organized to specifically address each factor.

A. <u>Physical Factors</u>

The Proposed Rule begins by asserting there are nine DPSs that are markedly separated "as a consequence of physical, ecological, behavioral, and oceanographic factors...." 75 Fed. Reg. at 12,602. "Oceanographic" factors are not one of the four bases for designating a DPS according to the DPS Policy. 61 Fed. Reg. at 4725. This Comment will assume this newly minted standard is properly considered a physical factor and will treat it as such.

The BRT Report begins with the assertion that loggerheads within the three ocean basins, Atlantic, Pacific, and Indian, are isolated because of physical barriers created by the continents. BRT Report at 17. This claim is belied by Figure 1.

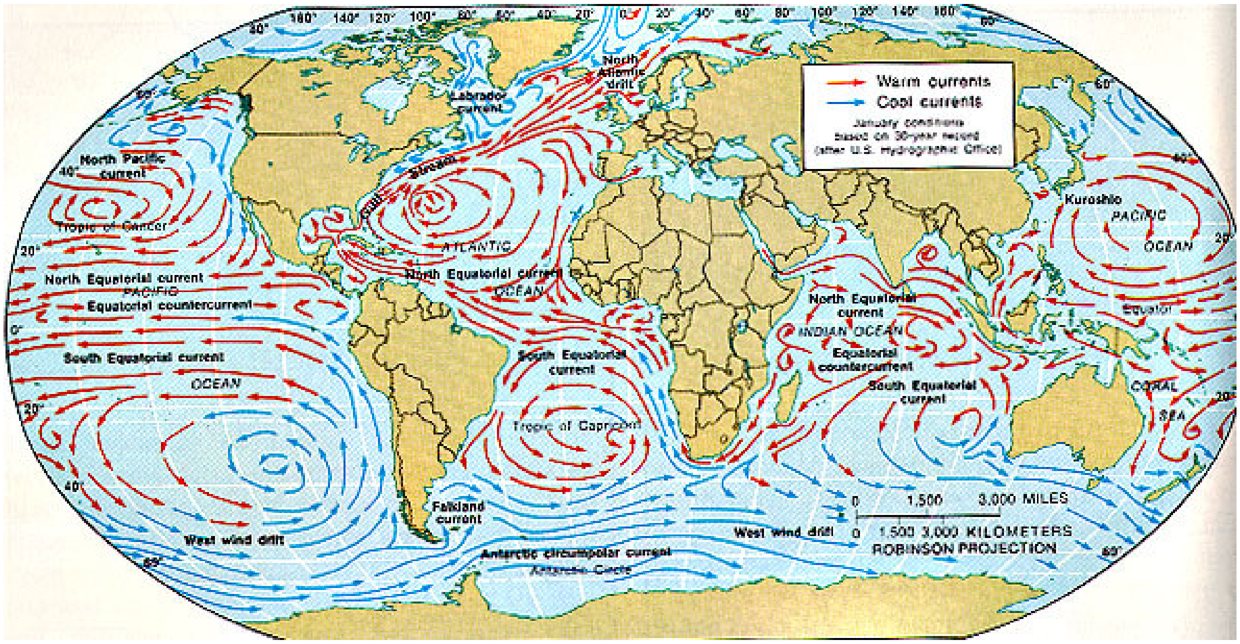

Figure 1. Source www.indiana.edu/~geol105/images/gaia_chapter_4/oceancirculation.jpg.

Figure 1 shows the extent to which ocean currents carrying loggerheads move among and between ocean basins, eliminating physical separations. Moreover, the statement in the BRT Report that loggerheads within the three ocean basins are physically isolated is followed immediately by the admission that dispersal from the Indian Ocean into the South Atlantic Ocean "is possible via the Agulhas current ..." which moves between the Indian and Atlantic Oceans. BRT Report at 17. In fact, it is more than just "possible" because the BRT Report admits that females nesting in the southwest Indian Ocean have been found in the Atlantic, carried there by the Agulhas current. *Id.* at 9. Indian Ocean hatched turtles are almost certain to be found in the Atlantic Ocean given the prevailing currents shown in Figure 1. Indeed, one of the world's foremost experts on Indian Ocean loggerheads and a Peer Reviewer of the BRT observed that loggerheads found on the Atlantic side of South Africa are likely to be loggerheads from the Indian Ocean side of South Africa, carried into the Atlantic by the prevailing currents depicted in Figure 1. Available at www.nmfs.noaa.gov/pr/species/statusreviews.htm. Further confirming this transfer between ocean basins, Bowen and Karl 2007:4900 note at least two transfers of female transmitted DNA between the Atlantic and Indian Oceans, one of which was very recent.

A simple review of Figure 1 also shows there is no separation of the Indian and Pacific Oceans. There is no land mass between the basins. Equally important, the prevailing currents can, and do, move turtles freely between these two ocean basins. Even the BRT Report admits that nesting females tagged in the South Pacific have been found in Indonesia in the Indian Ocean. BRT Report at 8-9.

The claim in the Proposed Rule that loggerheads in all ocean basins are physically separated is not supported by the facts. The significance of these facts is that the migration among population groups of between one and ten animals per generation is sufficient gene flow to prevent genetic differentiation between populations. Bowen *et al.* 2005:2390. In that regard, Bowen and Karl 2007 noted regarding loggerheads, "there appears to be sufficient leakage [of genes] between ocean basins to prevent long-term isolation...."

The evidence, including the admissions in the BRT Report, refutes the argument in the Proposed Rule that a DPS designation can be based on physical factors regarding ocean basin separation. Within individual basins, the story is the same.

Regarding the Atlantic basin, the Proposed Rule asserts that, with one exception, there are no observations of tagged loggerheads moving across the equator in the Atlantic, this statement being the justification for dividing the Atlantic basin into northern and southern components. 75 Fed. Reg. at 12,607. However, immediately preceding this statement is the admission that a DNA haplotype supposedly "unique" to the Brazilian nesting group has been found in foraging populations of juvenile loggerheads in the North Atlantic. *Id.* This cannot happen unless loggerheads are crossing the equator, contrary to the conclusion of the Proposed Rule that loggerheads in the northern and southern Atlantic are isolated from each other.[1]

Moving to the Pacific basin, the BRT Report states "extensive mtDNA studies" show that northern and southern Pacific Ocean "populations are isolated" and that juveniles do not disperse across the equator. BRT Report at 17. The only published study cited in the BRT Report to support this statement is Hatase *et al.* 2002a.[2] However, a review of the Hatase paper reveals that an Australian haplotype (southern Pacific) was indeed represented in the Japanese nesting population (northern Pacific). This does not happen without animals comingling across the equator in ecological timescales. Similarly, turtles caught off Baja California included 95% of the haplotypes common at Japanese nesting areas and 5% of an

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[1] Further, the claim that the DNA data suggest a separation of north and south Atlantic populations 250,000-500,000 years ago, BRT Report at 26, is unwarranted given the presence of the DNA haplotype common in Brazil in foraging populations in the north given only a moderate difference in microsatellite DNA allele frequencies and the lack of monophyletic mtDNA haplotypes in either the north or south Atlantic.

[2] Here, and elsewhere, the BRT Report and the Proposed Rule also rely on unpublished data, personal communications, etc. Reliance on such "data" is improper because it has not been subject to public comment, let alone peer review. *Western Watersheds Project v. Hall,* 2007 U.S. Dist. LEXIS 70710 (D. Idaho, Sept. 24, 2007) at *15-18, *aff'd.,* 338 Fed. Appx. 606 (9th Cir. 2009).

Australian haplotype. Bowen and Karl 2007:4892. This indicates the northern and southern Pacific populations mix on their trans-Pacific migrations with resulting gene flow across the equator.

In a vain effort to rescue its case, the BRT Report and the Proposed Rule assert that the temperature of the water separating the ocean basins is such that turtles cannot move with the currents because these low temperatures are a barrier. BRT Report at 17, 75 Fed. Reg. at 12,609. There are three key points in response. First, the BRT Report admits dispersal from the Indian Ocean into the Atlantic Ocean and between the Indian and Pacific Oceans occurs via existing currents, thus contradicting its thermal barrier argument. Second, as noted above, tagging and other data support this dispersal. Third, the temperature of the waters separating ocean basins is well within the temperature range of loggerheads. This is true for the northern and southern Atlantic and the northern and southern Pacific that are separated by warm equatorial waters and for waters flowing between the Indian and Pacific Oceans. It is also true for the waters separating the Atlantic and Indian Ocean basins where the water temperature, 16°-18° centigrade, is clearly within the temperature range of loggerheads.

In sum, the physical factors standard for designating a DPS is not met. The Proposed Rule fails to meet the evidentiary standard established by Congress and the DPS Policy.

B. <u>Physiological Factors</u>

The Proposed Rule asserts there are nine DPSs that are markedly separated "as a consequence of physical, ecological, behavioral, and oceanographic factors...." 75 Fed. Reg. at 12,602. Conspicuously absent is any mention of physiological factors. The Proposed Rule apparently concedes there is no physiological basis for a discreteness finding.

Nevertheless, the BRT Report makes the unsupported statement that unique DNA haplotypes could represent adaptive differences. BRT Report at 31. Presumably, this would be physiological adaptation. However, the statement in the BRT Report is pure speculation with no supporting evidence proffered.

Any geographically separate population can have adaptive differences. But simple DNA differences do not by themselves support the existence of adaptive differences. This is particularly true for the mitochondrial DNA control region that has been the focus of loggerhead genetic analysis. The control region is extraordinarily variable because it is not directly included in adaptation. Claiming adaptation and selection, without evidence and, therefore, without acknowledgement that it is pure speculation, is completely inappropriate in population genetics and evolutionary biology. It should be rejected in the Proposed Rule as well.

The Proposed Rule also contains an unorganized, random, and difficult to follow discussion of size differences in loggerhead turtles. For convenience, this Comment will address the size discussion in the Proposed Rule here. However, the Proposed Rule discusses size issues only with respect to the Atlantic basin, including the Mediterranean. There is no discussion of turtle size with respect to any other ocean basin.

With respect to the Atlantic Basin, the Proposed Rule states the mean size of reproductive female loggerheads in Brazil is 92.9 cm straight carapace length and then immediately admits this "is comparable to the size of nesting females in the Northwest Atlantic...." 75 Fed. Reg. at 12,607. (Emphasis added.) This evidence does not support a DPS designation between the South Atlantic and the Northwest Atlantic.

The Proposed Rule then compares this 92.9 cm mean straight carapace length for South and Northwest Atlantic reproductive female turtles with an average straight carapace length of 72-80 cm for Mediterranean and Northeast Atlantic nesting females, except that 91% of these turtles are less than 86.5 cm in curved carapace length. *Id.* at 12,608. Leaving aside the fact that the comparisons of mean to average and of curved to straight carapace length are apples and oranges, simply reporting size differentials without correcting for age, sex, nutrition, and water temperature tells nothing about any inherent size differences. Age, sex, nutrition, and water temperature (especially for cold blooded animals like turtles) will greatly affect growth rates and corresponding size. The fact, for example, that deer living in urban settings might be smaller because they have a more restricted diet than deer living in rural or protected park settings does not mean there are two different species. Similarly, males are generally larger than females. A comparison of sizes without identifying what is being compared and without considering the differential between males and females is not meaningful and does not mean there are two different species or that there are genetically based size differences between populations.

The size differences discussed above, as well as alleged differences in the size of reproductive females, including egg size and mass, are likely the result of nutrition caused by where the animals forage. For example, Hawkes *et al.* 2006 found that at Cape Verde in the proposed Northeast Atlantic DPS, post-nesting females foraging in pelagic waters were smaller than other females foraging in coastal waters. Similarly, Hatase *et al.* 2004 found that some loggerheads in the north Pacific utilize oceanic waters for virtually their entire life while others used coastal waters. The oceanic turtles were smaller. What the Proposed Rule asserts is a basis for DPS designation is likely a reflection of nutritional differences, not genetic differentiation.

Again, the Proposed Rule fails to meet the required evidentiary burden for a DPS designation even if the Proposed Rule was claiming physiological factors as a basis for the proposed designation, which it is not.

C. **Ecological Factors**

With respect to the entire Atlantic basin, including the Mediterranean, the Proposed Rule focuses on only one ecological factor, loggerhead foraging patterns. The Proposed Rule begins by admitting that juvenile loggerheads hatched on Northwest Atlantic beaches share common foraging grounds with oceanic juveniles from the Mediterranean and the Northeast Atlantic. 75 Fed Reg. at 12,697. The Proposed Rule then states that neritic juveniles also comingle on common foraging grounds, although they show an increased tendency to return to areas closer to their natal origins. *Id.* Subsequently, the Proposed Rule confirms this mixing and the existence of common foraging grounds stating that juvenile loggerheads hatched on Northeast Atlantic beaches are found on foraging grounds throughout the North Atlantic as well as in the western and central Mediterranean. *Id.* at 12,608. Similarly, the BRT Report notes that loggerheads hatching on Mediterranean beaches are found foraging in the Atlantic. BRT Report at 12-13. Bowen *et al.* 2005 and Bowen and Karl 2007 also explicitly note the mixing of juvenile loggerheads throughout the Northwest Atlantic, Northeast Atlantic, and Mediterranean. There is a complete absence of physical separation. Through two life stages generally comprising 29 years of an approximately 55 year lifespan, loggerheads born on Northwest Atlantic, Northeast Atlantic, and Mediterranean beaches share common foraging grounds.

Loggerheads from different populations also mix on foraging grounds in other ocean basins. For example, mixing occurs between eastern Australia (South Pacific) and western Australia (southeast Indian Ocean) at foraging grounds near the Torres Strait and in the Gulf of Carpentaria (*i.e.,* the north central coast of Australia). BRT Report at 22.

All of this is evidence of ecological similarity, not difference. As Bowen and Karl 2007 stated: "How can we define stocks when reproductive populations are thoroughly mixed at one life stage...?" Curiously, the BRT Report echoes this view. The BRT Report states that globally, loggerheads have different nesting grounds but loggerheads from different populations often mix in common foraging grounds [citations omitted], thus creating unique challenges when attempting to delineate distinct population segments...." BRT Report at 17. In other words, one must really stretch to create DPSs.

Nevertheless, the Proposed Rule soldiers on, claiming a difference in foraging grounds for adult females in the Atlantic basin. 75 Fed. Reg. at 12,607-12,608. Absent from the Proposed Rule is any discussion of the foraging behavior of the other 50% of the adult population – males. As generally admitted in the scientific literature, adult males range widely, breeding with females from various nesting assemblages. The necessary consequence of this is a geographically broad foraging range complemented by considerable male mediated gene flow. Thus, in an effort to find some difference in loggerhead populations, the Proposed Rule relies on the foraging behavior of one life stage (adults) and of only half the animals in that life stage (females).

As discussed below in detail, relying on alleged differences in a small segment of the population to make a DPS designation violates the ESA because the designation is based not on the entire DPS but on a subset of the population. Indeed, adult nesting females comprise only one percent of the total turtle population. Consultation on the Atlantic Highly Migratory Species Fishery Management Plan and Its Associated Fisheries, National Marine Fisheries Service, June 14, 2001, at 36. Similarly, the NAS Report concludes that adult sea turtles (male and female) comprise less than five percent of the non-hatchling population. NAS Report at 89. In apparent concurrence, the BRT Report notes that adult females comprise 0.7% of the total loggerhead population in the North Pacific. *See* www.nmfs.noaa.gov/pr/species/turtles/loggerhead_lifehistory.xls.

As is the case with the preceding DPS factors, the Proposed Rule fails to meet the required evidentiary burden with respect to ecological factors.

However, an important ecological factor not discussed by the BRT is the importance of ocean circulation on hatchling survival and dispersal as described by Putman et al. (2010). Hatchlings that emerge on beaches close to current systems that transport them away from predators and to feeding grounds have higher survival than hatchlings that occur on beaches further removed from such current systems. Because of natal homing, beaches that produce the most surviving hatchlings should also have the highest numbers of adults returning to nest. Most if not all the major nesting areas in all the ocean basins occur along continental coastlines in close proximity to ocean currents. Loggerhead reproductive success is a function of beach location such that they are susceptible to density dependent processes even at current population levels.

D. <u>Behavioral Factors</u>

For the behavioral factors, the Proposed Rule simply does not make the case for a DPS designation. With respect to the Atlantic Basin, including the Mediterranean, the Proposed Rule cites only one behavioral factor to justify a discreteness finding, nesting season. The Proposed Rule begins its analysis by stating that the nesting season in Brazil peaks during the summer months and that nesting in the southeastern U.S. also peaks during the summer months. 75

Fed. Reg. 12,607. This is evidence of behavioral similarity not difference. The fact that the warm season occurs in different months in the two hemispheres is a function of the earth's rotation, not turtle behavior. The behavioral pattern of the turtles is the same in both regions. If nesting season is a valid basis for classifying groups into DPSs, then it argues against a DPS designation separating the Northwest Atlantic, Northeast Atlantic, and Mediterranean into different groups. The Proposed Rule admits nesting in the Northeast Atlantic and Mediterranean occurs primarily in the summer months. *Id.*

E. <u>The Genetic Evidence</u>

In considering the genetic "evidence" presented in the Proposed Rule, it is important to establish the correct analytical framework.

1. <u>The Evidentiary Standard</u>

As discussed above, the standard is that there must be a "marked separation." This is, in fact, two standards. There must be a separation and, if that exists, it must be marked. Moreover, there must be a "preponderance" of evidence that is "conclusive" in order to justify the separate and markedly separate findings.

2. <u>The Proposed Rule Does Not Measure Marked Differences</u>

An accepted scientific basis for finding there is a marked, *i.e.,* appreciable, difference is to conduct a statistical analysis of the extent of the difference. There are accepted and well understood norms for this analysis. Another test could be the gene flow resulting from migration given that a migration of between one and ten animals per generation[3] is generally considered sufficient to prevent genetic differentiation between populations. Bowen *et al.* 2005:2390. Another consideration is the extent of DNA allele and haplotype differences. However, the sharing of alleles and haplotypes even at different frequencies indicates common ancestry and gene flow. Finally, an analysis of the degree of DNA sequence divergence for mitochondrial DNA or nuclear DNA can provide insights into genetic differentiation.

Nowhere in the Proposed Rule – for none of the nine proposed DPSs – is any of this done. The Proposed Rule fails to conduct the analyses necessary to determine if the data support a conclusion of marked separation. On this basis alone, the conclusory statements in the Proposed Rule regarding the alleged marked genetic separation of the proposed DPSs are unsupportable and fail to meet the required evidentiary threshold. In fact, even the Proposed Rule admits that the genetic differences it relies on are characterized by "[allele] frequency differences rather than fixed genetic differences." 75 Fed. Reg. at 12,602.

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[3] For loggerheads, a long lived species, a generation is approximately 30 years.

3. <u>The Marked Separation Finding Is For A Subset Of The Proposed DPSs, In Violation Of The ESA</u>

The courts have been clear that the ESA "preclud[es] any listings below the ESU/DPS level." *Modesto Irrigation District v. Gutierrez,* 2010 WL 3274499 at *3. Similarly, In *Alsea Valley Alliance v. Evans,* 161 F.Supp.2d 1154 (D. Ore. 2001), the court conducted a lengthy review of the legislative history of the amendment adding the existing DPS language to the ESA. The court's words and its citation to that legislative history are instructive.

> The term "distinct population segment" was amended in the ESA in 1978 so that it "would exclude taxonomic [biological] categories below subspecies [smaller taxa] from the definition." H.R. Conf. Rep. No. 95-1084, at 17 (1978).... Congress expressly limited the Secretary's ability to make listing distinctions among species below that of subspecies or distinct population segment of a species.

Id. at 1163.

These judicial precedents are fully consistent with court decisions regarding other ESA sections. Section 7(a)(2) requires that federal agencies not undertake, authorize, or permit actions that are likely to jeopardize the continued existence of a listed species, which is defined to include DPSs. 16 U.S.C. §1536(a)(2), 16 U.S.C. §1532(16). In *Rock Creek Alliance v. United States Fish and Wildlife Service*, 390 F.Supp.2d 993 (D. Mont. 2005), the issue was an agency determination that a proposed action would not jeopardize the continued existence of a DPS. Plaintiffs challenged that finding arguing that some subpopulations of the DPS would be jeopardized by the agency action. The court rejected this argument, finding FWS must examine the status of the listed species "across its entire range" before making a jeopardy determination. *Id.* at 1010.

To designate a DPS, the Services must examine whether the entire proposed DPS is markedly separate. They cannot limit their examination to a subset of the DPS. To do so would be listing below the DPS level. Applying this well established ESA legal principle to the Proposed Rule, the Services cannot examine the genetic structure of only adult females and then conclude the entire DPS, male and female, juvenile and adult, should be designated as a DPS. The reality is that the Proposed Rule improperly alleges genetic separation based on an examination of only a subset of the entire DPS.

Mitochondrial DNA ("mtDNA") is maternally inherited and the Proposed Rule generally limits its genetic analysis to mtDNA. Therefore, the analysis in the Proposed Rule generally reflects only female gene flow. Nuclear DNA, on the other hand, is inherited from both parents and reflects total gene flow, *i.e.,* from males and females. Moreover, mtDNA represents only a fraction of the entire genome. Consider that mtDNA is composed of approximately 16,500 nucleotides (DNA building blocks) while the nuclear DNA is composed of billions of nucleotides. Cronin 1993.

Thus, limiting genetic analysis to only mtDNA can yield misleading results. Patterns of mtDNA differentiation and a corresponding lack of nuclear DNA differentiation are very common in vertebrate species, particularly marine species. Bowen and Karl 2007:4897. For example, brown bears living on islands in Southeast Alaska that are geographically separated from mainland Alaska have different mtDNA haplotypes from mainland bears. However, and here is the key point, they do not have differentiated nuclear DNA frequencies. Paetkau *et al.* 1998. In the instant case, the BRT Report admits that an examination of nuclear DNA in loggerheads shows a "significantly" smaller degree of genetic differences than does an examination limited to mtDNA. BRT Report at 30.

The analytical point is that mtDNA analysis examines only part of the genetic structure of a species, and a small part at that. The legal point is that basing a DPS designation on alleged genetic differences in only one part of the proposed DPS is, in effect, basing the DPS listing on a subset of the population.

4. The Proposed Rule Does Not Use The Best Scientific Information

Listing decisions must be based on the best available scientific and commercial information. 16 U.S.C. §1633(b)(1)(A). In the instant case, the Services state they have nuclear DNA data for males and females, *i.e.,* for an entire proposed DPS, but never provide any quantitative analysis. Instead, the Proposed Rule simply asserts the data support its conclusion. This assuredly violates the standard in the DPS Policy that the Services will explain their reasoning in detail. 61 Fed. Reg. at 4723.

5. A Review Of Nuclear DNA Shows No Marked Genetic Separation

For the proposed Northwest Atlantic DPS, the principal focus of these Comments, the Proposed Rule begins its analysis by stating that mtDNA data indicate oceanic juveniles show no structure, neritic juveniles show moderate structure, and nesting females show strong structure. 75 Fed. Reg. at 12,607. Yet, in the very next sentence, the Proposed Rule admits that an examination of nuclear DNA reveals "no significant population structure among nesting populations." *Id.* Similarly, within the Mediterranean, the BRT Report admits: "Gene flow among the Mediterranean rookeries estimated from nuclear DNA was significantly higher than that calculated from mtDNA, consistent with the scenario ... [of] male-mediated gene flow." BRT Report at 30. (Emphasis added.) In other words, looking at the entire genome gives you a different picture than selective mtDNA analysis. Looking at the entire genome shows no marked separation.

The issue of male mediated gene flow (*i.e.,* males breeding freely and regularly with females from different nesting areas) is an important one. A fundamental and fatal flaw of the Proposed Rule's conclusion regarding genetic separation is that it ignores the fact that male mediated gene flow is common in loggerheads and other sea turtles (Bowen and Karl 2007) and well documented in the northwest Atlantic (Bowen *et al.* 2005), Mediterranean (Carreras *et al.* 2007), and the west Pacific and Indian Oceans (FitzSimmons *et al.* 1997). The documented mixing of male and female loggerheads from different nesting areas and different proposed DPSs on common foraging grounds facilitates male mediated gene flow between different nesting assemblages and between different ocean basins. The proposed DPSs simply cannot be considered genetically distinct if the overwhelmingly predominant nuclear genome (*i.e.,* nuclear DNA) is being mixed by male mediated gene flow. The Proposed Rule also ignores the fact that the migration among population groups of between one and ten animals per generation is sufficient gene flow to prevent genetic differentiation.

In considering the marked separation issue in the context of genetics, it may be helpful to consider the ruling in *Northwest Ecosystem Alliance v. Fish and Wildlife Service*, 475 F.3d 1136 (9th Cir. 2007). There, the issue was the listing as a DPS of that portion of the gray squirrel population found in Washington State. The court upheld an FWS finding of no marked genetic separation of the Washington gray squirrels. *Id.* at 1149-1150. The basis for that finding cited by the court supports the conclusion that Northwest Atlantic loggerheads are not a legitimate DPS. Like the gray squirrels at issue, loggerheads in the Northwest Atlantic do not have private microsatellite alleles, share microsatellite alleles with other loggerheads, and do not have monophyletic DNA haplotypes within regions. The very same genetic factors that led FWS to deny DPS status to the Washington State gray squirrel apply with equal force here.

A closer analysis of the genetic data, something not done in the BRT Report, illustrates the absence of genetic separation, let alone a marked separation. For example, regarding the Atlantic basin, the BRT Report declares that four proposed DPSs – Northwest Atlantic, Northeast Atlantic, Southern Atlantic, and Mediterranean – are "genetically distinct" and "…although they may commingle on oceanic foraging grounds as juveniles, adults apparently are isolated from each other." BRT Report at 30. However, as shown below, it is not adequate to simply declare populations genetically distinct without proper qualification and without considering all available data.

The conclusion of genetic distinctiveness is based primarily on mtDNA haplotype frequencies. However, the frequency analysis done in the BRT Report does not consider the overall genetic relatedness of the proposed DPSs. The data actually suggest the proposed DPS are not genetically distinct because they share mtDNA haplotypes and microsatellite DNA alleles.

If we compare genetic data for loggerhead turtles in the northwest Atlantic (eight nesting populations) and southern Atlantic (Brazil one population), Bowen *et al.* 2005) we see the following:

mtDNA control region	F_{st} = 0.5915
5 loci microsatellite (*i.e.,* nuclear) DNA	F_{st} = 0.0523.

F_{st} is a measure of microsatellite allele and mtDNA haplotype frequency differences.[4] A higher F_{st} value indicates larger differences in haplotype or allele frequencies, and more genetic differences between the populations.

The values above show higher F_{st} values for mtDNA[5] than microsatellite DNA, which clearly indicate male-mediated gene flow between the proposed DPSs. Using standard accepted methodology, we can estimate the number of reproducing migrants between the populations (Nm) from the F_{st} value for microsatellite DNA. Here, that value is Nm = 4.53. This suggests there are more than four migrants per generation between the northwest Atlantic and southern Atlantic which is enough to prevent genetic differentiation of populations.

Thus, the actual and potential male and female-mediated gene flow between the north and south Atlantic indicates that declaring these groups genetically distinct or markedly separated is a selective interpretation that ignores the genetic data. Furthermore, the small sample size for the Brazil population (11 turtles) from one area indicates the mtDNA haplotype frequencies are probably not representative of the southern Atlantic as a whole and shared haplotypes may occur that have not been sampled.

Another comparison of genetic data between proposed DPSs further documents the selective interpretation of data in the BRT Report. We compare mtDNA haplotype frequencies

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[4] Note mtDNA haplotype frequency differences are assessed by Bowen *et al.* 2005 with a quantity technically different from F_{st}, (*i.e.*, Φ_{st} or γ_{st}), that consider mtDNA sequence differences between haplotypes in addition to haplotype frequency. F_{st} and Φ_{st} or γ_{st} are not directly comparable but their relative magnitude indicates the degree of differentiation of haplotypes and alleles. We use F_{st} for mtDNA here for simplifying the technical terminology.

[5] When populations are fixed for different haplotypes or alleles (*i.e.*, they share no haplotypes or alleles), F_{st} will equal 1.0, indicating no gene flow. Brazil is actually fixed for one mtDNA haplotype not seen in the northwest Atlantic but it is similar in DNA sequence to haplotypes in the northwest Atlantic. Because Φ_{st} considers mtDNA sequence differences in addition to haplotype frequency, this results in Φ_{st} values less than 1.

directly (*i.e.*, not transforming them to F_{st}) for the proposed DPSs in the northwest Atlantic and Mediterranean. Two haplotypes of the 14 haplotypes observed are considered, A2 and A3. To understand and interpret the Table, start with the Florida Peninsular North GOM (Gulf of Mexico) group. Table 1 shows that haplotype A2 was present in 14.3% of the samples from there.

Table 1. Frequencies of two common mtDNA haplotypes, A2 and A3, of 14 haplotypes observed in nesting female loggerhead turtle populations from two regions, the northwest Atlantic and Mediterranean, from several studies (Laurent *et al.* 1998, Pearce 2001, Encalada *et al.* 1998) synthesized by Bowen *et al.* (2004). N denotes sample size.

Population		mtDNA haplotype frequency	
	N	A2	A3
Northwest Atlantic			
FL Pen N GOM	49	0.143	0.041
Fl Pen S GOM	45	**0.378**	0.089
Fl Pen S Atl coast	64	**0.438**	0.000
Fl Pen N Atl coast	14	0.000	0.000
Georgia	43	0.023	0.000
South Carolina	20	0.000	0.000
North Carolina	28	0.000	0.000
Dry Tortugas	58	**0.862**	0.000
Quintana Roo Mexico	20	**0.550**	0.100
9 population total	341	0.334	0.023
Mediterranean[6]			
Greece	81	**0.963**	0.000
Turkey	32	**0.594**	0.406
2 population total	113	0.858	0.115

Note that the frequency of haplotype A2 is similar in some populations from *both* regions (*e.g.*, the frequencies in bold type). For example, the northwestern Atlantic population at Dry Tortugas and the Mediterranean population at Greece have similar frequencies of haplotype A2, 86%-96% (with comparable sample sizes). Likewise, a northwest Atlantic population at Mexico and a Mediterranean population at Turkey have similar frequencies of haplotype A2 of 55%-59%. In other words, the same haplotype is found in DPSs that the Proposed Rule finds are genetically separate. This is clearly not the case. There is no marked separation. Haplotype A3 illustrates the same phenomenon as it is present in both the Mediterranean and Northwest Atlantic groups. These examples show that not only are there shared mtDNA haplotypes between proposed DPSs, but there are similar mtDNA haplotype frequencies in the northwest

[6] Carreras *et al.* (2007) present mtDNA data for additional Mediterranean populations. The frequencies of haplotype A2 in ten populations are similar to those shown above (0.59 to 1.00). GOM indicates Gulf of Mexico.

Atlantic and Mediterranean, depending on which nesting areas are considered. This type of thorough analysis shows that the conclusions in the Proposed Rule declaring the proposed DPSs "genetically distinct" is simply incorrect.

The key point in Table 1 is that for two examined regions (northwest Atlantic and Mediterranean) mtDNA haplotypes are shared at similar frequencies in some nesting areas. This fact, combined with the mixing of juveniles from both regions in foraging habitats and with common male-mediated gene flow in loggerheads, suggests the northwest Atlantic and Mediterranean populations are not genetically distinct or markedly separated. Indeed, one could emphasize the similarities of the mtDNA haplotype frequencies in the two regions and infer that female-mediated gene flow occurs between the northwest Atlantic and Mediterranean. Male-mediated gene flow is also well documented in loggerheads (Bowen *et al.* 2005, Bowen and Karl 2007).

Before leaving the Atlantic basin, it is worth noting that for the proposed Northeast Atlantic DPS, nesting females at the Boa Vista rookery in the Cape Verde Archipelago share mtDNA haplotypes in similar frequencies to nesting areas in the northwest Atlantic (Monzon-Arguello *et al.* 2009). Even the BRT Report finds that the Boa Vista rookery is most closely related to the rookeries of the northwest Atlantic. As with the Northwest Atlantic and Mediterranean analysis detailed in Table 1, the facts are that there is no marked genetic separation between the proposed Northwest and Northeast Atlantic DPSs.

As the preceding analysis shows, an assessment of the actual genetic data show that the northwest Atlantic, northeast Atlantic, and Mediterranean populations are genetically similar, with shared mtDNA haplotypes with similar frequencies in some nesting populations. These observations of genetic patterns within and between regions indicate that declaring the regions (and the proposed DPSs) "genetically distinct" or "markedly separated" does not adequately reflect the data.

6. <u>The Data Sets Used Were Too Limited To Justify Any Conclusion</u>

Not only must the Services use the best scientific evidence available, but the evidence must be "conclusive" under the applicable legal standard. Here, it is impossible to meet the conclusive evidence standard because the data relied on in the Proposed Rule come from samples that were too small to provide an accurate picture. An illustration will help. If one thinks of sampling two different areas to detect the presence of apples and oranges, and if the data contain but two samples, one showing two apples in the first area and one showing two oranges in the second area, the only conclusion is that there are two separate and distinct populations. But an appropriately sized sample might show that apples and oranges are present in both areas. This is the problem with the data used in the Proposed Rule and why it cannot meet the evidentiary standard discussed above.

For example, the southern Atlantic population had an mtDNA haplotype not seen in northwest Atlantic nesting populations but clearly seen in northwest Atlantic juveniles. The sample size for the southern Atlantic loggerheads was confined to only eleven animals from one location in Brazil. This is too small a sample and too limited a geographic coverage to properly assess the presence of haplotypes in northern and southern Atlantic populations.

7. <u>There Is No Reproductive Isolation</u>

Failing to make the case for a marked separation based on a complete genetic analysis, the Proposed Rule retreats to the assertion that female loggerheads choose to nest on specific beaches and, therefore, loggerheads in the proposed DPSs must be "reproductively isolated." The BRT Report asserts that, globally, loggerheads "comprise a mosaic of populations, each with unique nesting sites...." BRT Report at 17. Again, this statement focuses exclusively on females, totally ignoring the entire DPS and totally ignoring the widespread admission in scientific papers that male mediated gene flow is common among loggerheads. *See* Bowen *et al.* 2005, Carreras *et al.* 2007, and FitzSimmons *et al.* 1970. The documented and admitted mixing of male and female loggerheads from different nesting areas and different proposed DPSs on common foraging grounds demonstrates male mediated gene flow between these different nesting assemblages and ocean basins. Because males mate with females from multiple and different nesting areas, these areas are not "reproductively isolated." Further, supposedly "isolated" nesting assemblages produce both male and female turtles, and it is an undisputed fact that males from various nesting areas mate with females from different nesting areas. All of this means there is no "reproductive isolation."

Although the documented existence of male mediated gene flow is fatal to the Proposed Rule's "reproductive isolation" argument, there are numerous other factors that individually and collectively undermine that argument. For example, as discussed above, there is concurrence in the scientific literature that if between one and ten animals from a population migrate to a part of the population in another geographic area once in a generation, it is sufficient to keep the overall population from genetically differentiating and from being isolated. As discussed above, that is the case here.

Furthermore, the claim of "reproductive isolation" fails to account for the fact that turtles from the northwest Atlantic colonized the northeast Atlantic and Mediterranean in recent ecological times. 75 Fed. Reg. at 12,606. In addition, the Proposed Rule fails to account for the fact that nesting site fidelity within regions is not what the Services would have us believe. For example, Hatase *et al.* (2002) observed that while only five of 2,219 tagged juveniles nesting on Japanese beaches changed locations, a significant fact was that the relocation differences were large, ranging from 74 to 630 km (44.4 to 378 miles). These relocation distances were equal to or greater than the distances between nesting colonies. Similarly, Hatase *et al.* (2004) found that of 443 tagged females, 74% did not return to the study area beaches. Although mortality will account for some of that, selection of another nesting beach is also a central factor to be considered. In another study, Tucker (2009) tagged 52 females along a 6 km (3.6 mile) beach in southwest Florida. Forty-six percent returned to lay eggs on beaches outside the study area. The recently issued NAS Report also documents long distance beach locations stating "LeBuff (1974) demonstrated a loggerhead female relocating from southwest Florida to southeast Florida, and at least two tagged females have switched from Tortuguero to other locations in the Caribbean (citations in Bowen *et al.* 1992)." NAS Report at 69. Finally, even the Proposed Rule calls natal homing within regions "imprecise." 75 Fed. Reg. at 12,602. At every level of analysis, the Proposed Rule's "reproductive isolation" argument is not supported by the facts.

Stepping back from the evidence for a moment, it is also important to recognize that the Proposed Rule is mixing two different concepts. Reproductive isolation is not the same thing as genetic isolation. Reproductive isolation is used in science to denote the isolation that accompanies species formation where two species cannot interbreed and produce fertile and viable offspring. Restricted gene flow (*i.e.,* interbreeding among areas) is common across geographical groups of the same species. Such normal events should not be termed "reproductive isolation" as that term is used by scientists in population genetics. As Bowen and Karl 2007 noted regarding loggerheads, "there appears to be sufficient leakage [of genes] between ocean basins to prevent long-term isolation and allopatric specification (Roberts *et al.* 2004, Bourjea *et al.* 2007)."[7]

The claim of "reproductive isolation" also has no legal meaning. Each nesting site for every species in the world is unique in that it exists in a different geographic locale. Using such a geographic standard, every site or area to which members of any species return to breed or nest would become a "unique" site sufficient to "justify" a DPS designation. Such a legal "standard" is, in fact, no standard and it assuredly conflicts with the DPS Policy that DPS designations should be used only "sparingly." Even if the words "reproductive isolation" had legal meaning in some context, they do not under the ESA in the instant case. The net effect of arguing that female loggerheads are reproductively isolated because of "unique" nesting areas

[7] Allopatric specification is when geographically separated populations acquire genetic differences over time so that they cannot reproduce and have in fact become separate species.

is to classify an entire species based on the characteristics of only part of the proposed DPS, nesting adult females. To do so, violates the ESA.

The reality is that the recent common ancestry of loggerheads in different ocean basins (less than 12,000 years ago), the acknowledged movement of animals between and among ocean basins, and the acknowledged male mediated gene flow among loggerheads indicate that, worldwide, loggerheads are not “reproductively isolated.” While there may be limited female mediated gene flow between ocean basins, loggerheads interbreed in evolutionary and ecological timescales and are not “reproductively isolated.”

8. <u>The “Evidence” Cited In The Proposed Rule Is Contradictory</u>

In addition to all of the preceding flaws, the information in the Proposed Rule is typically contradictory and conflicting. For example, the Proposed Rule begins its limited evidentiary analysis by admitting its error. The Proposed Rule asserts in one sentence that loggerheads at Brazilian rookeries have a “unique mtDNA haplotype....” 75 Fed. Reg. at 12,607. Two sentences later, the Proposed Rule admits the haplotype is not “unique” because it has been found “in foraging populations of juvenile loggerheads of the North Atlantic....” *Id.* That the haplotype is found throughout the Atlantic means it is not “unique.” Instead, it indicates common recent ancestry and male mediated gene flow throughout the Atlantic basin.

After claiming genetic separation between the proposed Northwest and Northeast Atlantic DPSs, the Services admit that nesting females of the Boa Vista rookery in the Northeast Atlantic, despite their proximity to other Northeast Atlantic rookeries and to the Mediterranean, are “most closely related to the rookeries of the Northwest Atlantic.” 75 Fed. Reg. at 12,608. Thus, there is admittedly no marked genetic separation between these two proposed DPSs. Further, recall that the Proposed Rule admits loggerheads from the northwest Atlantic colonized the northeast Atlantic and Mediterranean.

The same problem infects the genetic analysis with respect to other ocean basins. An Australian haplotype (Southern Pacific Ocean) is found in Japanese nesting populations (Northern Pacific Ocean) indicating comingling of these groups. Similarly, the proposed Southern Pacific DPS (eastern Australia) does not appear to be markedly different from nesting assemblages in western Australia in the proposed Southeast Indian Ocean DPS because the two groups share two mtDNA haplotypes. BRT Report at 21-22, Bowen and Karl 2007:4892. Furthermore, turtles caught off Baja California included 95% of the haplotypes that are common to Japanese nesting areas and 5% of Australian haplotypes. The BRT Report even admits gene flow between these populations. BRT Report at 22.

As noted above, Bowen and Karl 2007 state "there appears to be sufficient leakage [of genes] between ocean basins to prevent long-term isolation and allopatric specification (Roberts *et al.* 2004, Bourjea *et al.* 2007.)"

F. <u>Similarities</u>

Previous sections detail how and why the Proposed Rule fails to meet the Services' evidentiary burden that the proposed DPSs are discrete. In that regard, it might be helpful to provide an overview of how these loggerhead populations are similar, something the Proposed Rule pointedly refuses to do.

Table 1 of the BRT Report sets forth the "Life history parameters used for the nine DPSs." BRT Report at 39. This table helps to illustrate the similarities among the proposed DPSs. For example, while the Proposed Rule designates three DPSs in the Indian Ocean based on alleged differences, Table 1 admits that "All [life history] parameters are identical for the three DPSs in the Indian Ocean." *Id.*

A similar tale is told as to other proposed DPSs. For example, there is great similarity in the fecundity parameters. The sex ratios for all nine proposed DPSs are identical. The remigration interval (*i.e.,* frequency of female nesting) is approximately three years in every instance. The age at first breeding (listed under habitat use in Table 1) is 30 years for loggerheads in each proposed DPS and the standard deviation from that is also identical (*i.e.,* 5). Clutch frequencies per female range from two to five among the proposed DPSs but these numbers likely reflect measurement errors rather than real differences. Tucker 2009 showed that clutch frequency estimates obtained using telemetry yielded higher values than those obtained by nocturnal patrols.[8] Even so, clutch frequency is identical for six of the nine proposed DPSs. Regarding the habitat use life history parameter in Table 1, the BRT Report admits that for eight of the nine proposed DPSs "habitat use was similar." *Id.* at 59.

G. <u>Conclusion</u>

The Services' legal position as to genetic differences appears to be that differences exist and are "marked" when the Services say so. That is not a legally defensible standard. The Proposed Rule fails to meet the evidentiary standard incorporated into the DPS Policy that a DPS designation can be made "only" when a "preponderance" of evidence shows "conclusive" data. In attempting to present a case for DPSs based on genetics, the Proposed Rule is riddled with inconsistencies, incomplete analyses, and the selective presentation of data. The Proposed Rule ignores key data such as male mediated gene flow. When that data were

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[8] Clutch frequency is also influenced by the age of nesters because young nesters produce fewer nests, a factor not accounted for in Table 1.

considered and reflected in an analysis of the complete genome, *i.e.,* nuclear DNA, it showed no significant genetic differences. The Proposed Rule attempts to rescue its case by arguing that one percent of the population, nesting females, is reproductively isolated. But given the fact that males mate with females from different nesting areas, that males and females mix on common foraging grounds, and that turtles move among and between ocean basins, there is no "reproductive isolation."

IV. The Proposed DPS Designation – The Significance Standard

Pursuant to the DPS Policy, after an affirmative discreteness finding is made, a population segment must then be determined to be significant to the species to which it belongs. 61 Fed. Reg. at 4725. The question of significance does not arise unless and until a discreteness finding is made. In the instant case, no valid discreteness finding has been made. Therefore, it is unnecessary to consider the significance issues. However, and only for the sake of argument, this Comment will review the significance criteria.

Pursuant to the DPS Policy, the consideration of significance may include, but is not limited to, the following four factors.

- Persistence of the discrete population segment in an ecological setting unusual or unique for the taxon.
- Evidence that loss of the discrete population segment would result in a significant gap in the range of a taxon.
- Evidence that the discrete population segment represents the only surviving natural occurrence of a taxon that may be more abundant elsewhere as an introduced population outside its historic range.
- Evidence that the discrete population segment differs markedly from other populations of the species in genetic characteristics.

Id.

As with the determination of discreteness, the terms "markedly" and "significant" are to be given their "commonly understood" sense. *Id.* at 4723. "Markedly," as discussed above, means "appreciably." Webster's New World Dictionary defines "significant" as important or momentous. *See also, Northwest Ecosystem Alliance v. Fish and Wildlife Service,* 475 F.3d 1136, 1146 (9^{th} Cir. 2007) ("[T]he term 'significant' has 'its commonly understood meaning,' which is 'important.'"). Further, as with a discreteness finding, the evidentiary standard is that a significance determination is to be made only when the preponderance of biological evidence allows a conclusive finding. Finally, the requirement that the best scientific and commercial data available be used in making a significance finding applies with equal force. None of these standards are met in the Proposed Rule.

The Proposed Rule finds significance based only on the first, second, and fourth significance factors. 75 Fed. Reg. 12,609.[9] Regarding the first factor, persistence in an ecological setting unusual or unique for the taxon, the Proposed Rule makes the obvious, but legally irrelevant, statement that each of the proposed DPSs reside in unique geographic settings. It is true that each of the major ocean basins differ to some degree with each other. That said, this is not a legally sufficient distinction. That loggerheads are located in different geographic areas does not make each area "unique." If the reasoning of the Proposed Rule were applied generally, then every lake, every river, every sea, and every ocean would be considered a unique ecosystem justifying a DPS significance finding. And this does not even begin to touch terrestrial ecosystems. In short, under the standard applied in the Proposed Rule, virtually everything is unique. A standard in which everything fits is, in fact, no standard at all and is contrary to Congressional intent codified in the DPS Policy that DPS designations be made "sparingly."

The thrust of the "unique" ecosystem standard in the first significance factor is that each ecosystem has different habitat characteristics, perhaps such that turtles from one ecosystem would not survive in another "unique" ecosystem. That is not the case here. A few examples help to demonstrate. The Proposed Rule admits that turtles from the proposed Northwest Atlantic DPS colonized the proposed Northeast Atlantic and Mediterranean DPSs. 75 Fed. Reg. at 12,606. This alone shows these are not unique ecosystems with respect to turtle habitation. The Proposed Rule further admits that loggerheads in the proposed Northwest Atlantic, Northeast Atlantic, and Mediterranean DPSs share common foraging grounds. *Id*. at 12,609. That mixing of foraging grounds also occurs between the Indian and Pacific Oceans. BRT Report at 22. Again, this shows the absence of any unique ecosystem with respect to turtle habitation. Further, as the Proposed Rule admits, and as discussed above, mtDNA haplotypes from animals in one proposed DPS are found in animals within other ocean basins. This would not, and could not, occur if the ecosystems were unique such that loggerheads from one area did not exist, and live, within other supposedly unique ecosystems. Finally, as shown in Figure 1, the ocean currents are such that there is a mixing of waters between and among these ocean basins. In fact, as noted above and admitted in the Proposed Rule, these currents carry turtles

[9] Although the Proposed Rule states it is relying on three significance standards, the Proposed Rule in fact relies exclusively on only two of the significance standards with respect to the proposed DPSs in the Atlantic basin, including the Mediterranean. For the four Atlantic and Mediterranean proposed DPSs, the Proposed Rule states they are significant only because their loss would result in a significant gap in the range of the taxon and because the proposed DPS differs markedly from the others in its genetic characteristics. 75 Fed. Reg. at 12,611-12,612.

from one supposedly separate and unique ecosystem to the next. These and other factors discussed above show the absence of unique ecosystems.

Regarding the significant gap factor, the Proposed Rule is internally and legally inconsistent. The basis for the DPS listing is that there is no overlap in breeding populations, *i.e.,* there is "reproductive isolation," and that there is no chance the animals in one proposed DPS could ever recolonize another DPS if the first DPS disappeared. *See* 75 Fed. Reg. at 12,611-12,612. If that is the case, then each population segment is completely separate and it would not matter to the rest of the taxon if one segment disappeared because, according to the Proposed Rule, there is no breeding relationship between the segments. Since the Proposed Rule says there is no breeding or other relationship, there can be no significant gap.

The genetics factor is easily disposed of. To argue significance based on genetic differences, the Proposed Rule relies entirely on the same information contained in the discreteness analysis. For the reasons articulated in that section of this comment, the marked genetic argument fails.

V. <u>The Proposed DPS Designation – The Listing Factors</u>

If a population segment meets the separate tests of being discrete and significant, it must then satisfy the ESA standards for listing as a threatened or endangered species. 61 Fed Reg. at 4725. However, pursuant to the DPS Policy, the listing factors are considered only if the proposed DPS is found to be both discrete and significant. *Id.* Here, neither requirement is met and, therefore, there is no need to proceed with a consideration of the listing factors. Nevertheless, and only for the sake of argument, this Comment will evaluate the listing factors.

The ESA sets forth five factors to be considered in making a threatened or endangered finding. 16 U.S.C. 1533(a)(1). Those five factors are:

1. the presence or threatened destruction, modification, or curtailment of a species habitat or range;
2. overutilization of the species for commercial, recreational scientific, or educational purposes;
3. disease or predation;
4. the inadequacy of existing regulatory mechanisms; or
5. other factors affecting the species continued existence.

Whilst the Proposed Rule attempts to justify upgrading the status of certain proposed DPSs to endangered, the effort falls far short of the evidentiary standards discussed above that are applicable to the designation of DPSs. Typically, the Proposed Rule makes a statement about some alleged problem but then fails to indicate how that problem relates to loggerheads or fails to measure the impact on loggerheads. This Comment will focus on the proposed

Northwest Atlantic DPS although the analysis below is equally applicable to other DPSs proposed for endangered status. Before proceeding to an analysis of the five listing criteria, three threshold matters bear comment.

First, the BRT Report contains an estimate of the severity of the threats faced by loggerheads because of each of the five ESA listing factors. For the proposed Northwest Atlantic DPS, the BRT Report states the factor of inadequacy of regulatory mechanisms was "not considered to be reducing survival rates directly." BRT Report at 73. Thus, this factor is not at issue and cannot be used to justify an endangered designation for the proposed Northwest Atlantic DPS. As to three other factors, habitat, overuse, and disease/predation, the BRT Report lists the threats to loggerheads in the proposed Northwest Atlantic DPS as low and very low. *Id.* Although these Comments have criticized the methodology used to derive the threat estimates in the BRT Report, if the Services believe the methodology is sound, then the Services cannot challenge the conclusion that for these three listing criteria there is a low or very low threat to loggerheads in the proposed Northwest Atlantic DPS. A low or very low threat does not provide a legally sound basis to designate the proposed DPS as endangered and perhaps not as threatened.

The second threshold matter comes from a closer look at the analysis of loggerhead survival rates in the BRT Report. The natural survival rate for neritic adults (*i.e.,* large pre-breeding and breeding males and females) is stated to be 95% in all DPSs. BRT Report at 39. Table 1. The BRT Report also states that anthropogenic mortalities for neritic juveniles and adults in the proposed Northwest Atlantic DPS are between 13% and 50% of the 95% of loggerheads left after natural mortality is subtracted. *Id.* at 73. In other words, using the high end of the BRT's anthropogenic mortality estimate, approximately 52.5% of the proposed Northwest Atlantic DPS neritic juvenile and adult population dies annually.[10] The Turtle Expert Working Group appointed by NMFS estimated the neritic juvenile and adult population of the proposed Northwest Atlantic DPS to be 230,000. TEWG 1998. Given that, the BRT is asserting that 120,750 neritic juveniles and adults from this population die annually, almost entirely because of anthropogenic mortality. Yet, even the BRT Report admits that the largest source of turtle mortality in the proposed Northwest Atlantic DPS, fishery bycatch, totals only 3,743 turtles annually. BRT Report at 134-138. Not only is this inconsistency an important threshold for understanding just how the Proposed Rule is proceeding with an unsupported basis for listing

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[10] Survival rates are converted into their corresponding mortality rates using standard methodology where the log of the survival rate equals the mortality rate. Assuming natural survival is 0.95 and survival after man caused mortality is 0.50, overall survival is 0.4750.

loggerheads as endangered, but it also casts an even greater cloud over the models used by the BRT to come up with these incredibly inaccurate numbers.

This leads to the third threshold issue, the size of the loggerhead population against which impacts are measured. NMFS has determined that nesting females comprise less than one percent of the total population. Consultation on the Highly Migratory Species Fishery Management Plan and Its Associated Fisheries, National Marine Fisheries Service, June 14, 2001, at 36. The BRT Report confirms this noting that adult females in the proposed North Pacific DPS comprise only 0.7% of the total loggerhead population in that proposed DPS. *See* www.nmfs.noaa.gov/pr/species/turtles/loggerhead_lifehistory.xls. Bearing the one percent number in mind, the BRT Report provides an assessment of the number of nesting females at each of the major nesting areas. BRT Report at 9. Nesting areas with more than 10,000 females are in one category. Areas with 1,000-9,999 nesting females are in another grouping and areas with 100-999 nesting females are in a third. *Id.* If all nesting areas are combined, the nesting female population is a minimum of at least 27,300 and a maximum of at least 93,000. These are minimum numbers since the two largest nesting areas are stated to have at least 10,000 nesting females, *id.*, and the above computation uses this minimum number. Translating the number of females into a total loggerhead population provides a range of at least 2,730,000 to at least 9,300,000 loggerheads. The midpoint is at least 6,015,000 loggerheads. It is these 6 million loggerheads that the Services believe are in danger of extinction. Using the same data sources and methodology, the proposed Northwest Atlantic DPS has a loggerhead population of between at least 1,230,000 and at least 3,300,00 animals. The midpoint is at least 2,265,000 loggerheads.

A. <u>Impacts To Habitat Or Range</u>

As to the habitat modification or destruction factor, the Proposed Rule is woefully inadequate. Rather than dwell extensively on each point, this Comment will briefly explain why the Services have not met their evidentiary burden to justify an endangerment listing as to the proposed Northwest Atlantic DPS.

The Proposed Rule says numerous beaches in the southeastern United States are eroding. 75 Fed. Reg. at 12,625. That is different than offering proof that beaches used by loggerheads are eroding, and, if they are, if it is significant.

At 12,625, the Proposed Rule identifies erosion control devices as a problem noting, for example, that 18% of Florida beaches and 3% of North Carolina beaches are armored. *Id.* Again, that is different than saying loggerhead nesting beaches are being armored. Nor does the Proposed Rule even attempt to quantify or otherwise assess the impact of beach armoring.

Nor does the Proposed Rule explain how any impacts from armoring are offset by the admission at 12,626 that beach nourishment programs increase loggerhead nesting.

At 12,625, the Proposed Rule asserts a correlation between nesting density and jetty construction. Correlation is not the same thing as causation. Indeed, the Proposed Rule only concludes jetty construction "may" discourage nesting. *Id.*

At 12,625-12,626, the Proposed Rule identifies stormwater and other runoff as creating localized erosion channels that wash out nests and cause pollution. The Proposed Rule offers no proof regarding the extent beach washout is occurring, let alone how many nests may actually be affected. As to pollution, the Proposed Rule says pollution only has the "potential" to affect nesting. That is different than saying pollution is affecting or is likely to affect nesting. In fact, the Proposed Rule admits the effects of these contaminants "are not yet understood." *Id.*

At 12,627, the Proposed Rule condemns light pollution. However, the Proposed Rule offers no quantification of impact for adults. Are 2 or 20,000 turtles affected? As to hatchlings, the Proposed Rule says thousands are disoriented. Again, saying they are disoriented is different than saying they fail to get to the ocean. 75 Fed. Reg. 12,627.

At 12,626, the Proposed Rule states vehicular traffic is allowed on certain beaches. Are all the listed beaches actually used by loggerheads? If so, what percentage of total habitat is actually affected? The Proposed Rule never tells us. 75 Fed. Reg. 12,626.

At 12,626-12,627, the Proposed Rule asserts fishing affects loggerhead diet. The example given is a reduction in the amount of horseshoe crabs in the loggerhead diet. Stating that the diet has changed is different from saying the change is adversely affecting loggerheads or that the turtles do not have enough to eat. *Id.* Similarly, at 12,626, the Proposed Rule states shrimping is probably having a major effect on loggerhead diet. *Id.* Again, the nature and extent of that alleged effect is never articulated. These are significant omissions given that even the BRT Report acknowledges that loggerheads "are able to exist on a wide variety of food items with ontogenetic and regional differences in diet." BRT Report at 15.

At 12,627, the Proposed Rule states sediment dredging for port navigation channels and beach nourishment has the potential to, or can, destroy or degrade benthic habitat. Again, this is different than saying such activities are occurring in habitat used by turtles and, if they are, whether it is .001% or 100% of that habitat that might be affected. 75 Fed. Reg. 12,627.

At 12,627, the Proposed Rule states climate change may result in trophic changes affecting turtle prey abundance and/or distribution. Again, it is mere speculation that there may be impacts and the Proposed Rule offers no concrete evidence supporting the assertion of an impact on loggerheads. *Id.*

At no point do the Services meet their burden to articulate and support with evidence the claim that habitat modification is occurring in a way that will affect loggerhead turtles sufficiently to justify an endangerment designation. The Services also do not make any effort to reconcile this list of alleged justifications for endangerment with the conclusion in the BRT Report that the cumulative impact of all of the items listed above is low or very low. BRT Report at 73, Table 8.

B. Overutilization

The Proposed Rule next addresses the overutilization factor. The Proposed Rule asserts that the loggerhead harvest for human consumption is a "significant" threat, noting that 12 of 29 Caribbean nations allow a loggerhead harvest. 75 Fed. Reg. at 12,627. The same evidentiary problems that plague the analysis of the habitat factor are present here. Saying 12 nations allow a harvest is different than saying that a harvest actually occurs and, if it does, the amount.

However, the Turtle Expert Working Group appointed by the Services does assess the extent of loggerhead harvest in the Caribbean. Contrary to the impression the Proposed Rule attempts to create by making unsupported assertions of possible impact, the TEWG concludes:

> We reviewed the recent history of the directed harvest of loggerheads in the Western North Atlantic and found that the commercial fishery in Cuba would have been the only one to harvest substantial numbers of loggerheads during the past several decades.

TEWG Report, 2009 at xi (emphasis added). The TEWG goes on to conclude the take from the Cuban fishery was reduced to a low level in the early 1990s and has been "close to zero" since 1996. *Id.*

How a harvest of "close to zero" threatens loggerheads with extinction is not clear. To state that the Proposed Rule is reaching to justify an endangered designation for the proposed Northwest Atlantic DPS is something of an understatement.

C. Disease or Predation

The Proposed Rule also addresses the disease and predation factor. 75 Fed. Reg. at 12,627-12,628. There, the Proposed Rule identifies various diseases and infections. However, the Proposed Rule admits the effects and significance are unknown. The Proposed Rule then states beach predators "may" take significant numbers of eggs, non-native vegetation "may" form root mats that can invade and desiccate eggs, and trees "can" provide too much shade over nests. It is unclear how the Proposed Rule goes from a "may" effect statement, to a definitive conclusion that predation and disease constitute an existing or actual "significant" threat to loggerheads in the proposed Northwest Atlantic DPS.

The assertion of a "significant" threat caused by disease and predation does not withstand scrutiny on three levels. First, the Proposed Rule does not say the threat actually exists, only that it "may" be an issue. Second, even accepting the possibility a threat "may" exist, the Proposed Rule fails to indicate the nature or extent of the threat or how many loggerheads might be affected. Third, the statement that disease and predation constitute a "significant" threat is completely contrary to the conclusion of the BRT Report that the cumulative threat from disease and predation is low or very low. BRT Report at 73, Table 8.

D. <u>Existing Regulatory Mechanisms</u>

The Proposed Rule next addresses the adequacy of existing regulatory mechanisms factor. 75 Fed. Reg. at 12,628. The argument in the Proposed Rule is that because (1) there are habitat changes occurring and (2) turtles are being taken as a direct harvest or as fishery bycatch, then national and international regulatory mechanisms must be inadequate.

The evidentiary and analytical failures in this section are glaring. First, the Proposed Rule never discusses what mechanisms are believed to be inadequate. Second, the alleged failures are founded on the premise that the sections discussed above have so clearly proven harms to loggerheads that the inadequacy of existing regulatory mechanisms can be assumed. As reviewed above, the Proposed Rule fails to establish that the harms actually exist or if they exist that they are significant or quantifiable.

Third, the BRT Report, the Report that the Services assert is the foundation for the Proposed Rule, states that the inadequacy of regulatory mechanisms factor was "not considered to be reducing survival rates directly." BRT Report at 73. Even acknowledging that these words could be interpreted as leaving the door open to indirect impacts, the Proposed Rule never identifies any indirect impacts. Indeed, the Proposed Rule points to the BRT Report as identifying the "regulatory mechanisms that apply to loggerhead sea turtles globally and within the Northwest Atlantic Ocean." 75 Fed. Reg. at 12,628. Yet, the Proposed Rule ignores the BRT Report's conclusions.

E. <u>Other Natural or Manmade Factors</u>

Finally, the Proposed Rule addresses other natural or manmade factors which allegedly justify listing the proposed Northwest Atlantic DPS as endangered. 75 Fed. Reg. at 12,628-12,630. Again, the evidence is missing.

At 12,630, the Proposed Rule identifies propeller and boat collision injuries as a matter of concern. The Proposed Rule states 14.9% of stranded loggerheads had vessel related injuries. Again, the Proposed Rule does not indicate the number of animals (14.9% of 10 is different than 14.9% of 10,000), does not compare that number to the total population, and does not assert that the vessel related injury actually caused the stranding, let alone that the stranding caused a mortality. *Id.*

At 12,630, the Proposed Rule says the weather justifies listing loggerheads as endangered, citing hurricanes and cold stunning caused by changing weather. Having cited cold stunning, the Proposed Rule then admits it is "not a major source of mortality." No one disputes weather changes and that hurricanes happen. But the earth has experienced varying weather conditions since creation. The Proposed Rule, in relying on the weather to justify listing loggerheads as endangered, fails to explain what has changed. Moreover, if changing weather is a basis for listing a species as endangered, then every species throughout the world could be listed. *Id.*

At 12,630, the Proposed Rule returns to its discussion of habitat change and states dredging and climate change can impact turtles. As noted in the discussion of the habitat change factor, the Proposed Rule only says these things "may" affect turtles without evidence or quantification of the alleged impact. *Id.*

The Proposed Rule cites entrapment in saltwater cooling as justifying an endangered listing. 75 Fed. Reg. at 12,630. However, the Proposed Rule concludes that between 1991-2005, the actual average annual mortality was only nine turtles.

At 12,628-12,630, the Proposed Rule discusses the incidental bycatch of turtles in various fisheries. This Comment will leave it to the representatives of the named fisheries to focus on their individual fisheries. As to shrimping, this Comment has previously discussed the extent of the incidental taking. However, it is important to place the entire fisheries incidental bycatch into context. The BRT Report lists all the bycatches which cumulate to an annual level of 3,743. BRT Report at 134-138. This compares to a Northwest Atlantic loggerhead population of between at least 1,230,000 and at least 3,300,000.[11] The midpoint would be 2,265,000. It is hard to understand how a take of 0.17% of the population, using the midpoint number, would be likely to jeopardize the loggerhead population causing it to be endangered.

F. <u>Conclusion</u>

The Proposed Rule classifies the proposed Northwest Atlantic DPS as endangered. The Proposed Rule fails to make the case. Rather, the Proposed Rule is built on speculation and

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[11] A 2001 NMFS Biological Opinion states that nesting females comprise one percent of the total population. Consultation on the Atlantic Highly Migratory Species Fishery Management Plan and Its Associated Fisheries, June 14, 2001, National Marine Fisheries Service, at 36. The BRT Report lists the number of females nesting on beaches in the proposed Northwest Atlantic DPS as between at least 12,300 and at least 33,000. BRT Report at 9. These are minimum numbers because the Peninsula Florida nesting assemblage is said to have more than 10,000 nesting females, *id.,* and this Comment uses the minimum 10,000 number.

surmise, the precise basis rejected by the Supreme Court in *Bennett v. Spear,* 520 U.S. 154, 176 (1992). There, the Court stated:

> The obvious purpose of the requirement that each agency "use the best scientific and commercial information available" is to ensure that the ESA not implemented haphazardly, on the basis of speculation and surmise.

The Proposed Rule also states it is relying on its models to justify the endangered listing. 75 Fed. Reg. at 12,651. The problems with those models are discussed above and need not be repeated except to recall that the National Academy of Sciences called the models a "heuristic" exercise, *i.e.,* unjustified and incapable of justification, NAS Report at 90, and the BRT Report itself admits the modeling overstates the alleged harms, BRT Report at 36 and 67.

Notwithstanding all of these criticisms, if the Services intend to rely on the BRT Report and its models, the Services cannot escape the conclusion in the BRT Report that the actual threat to loggerheads posed by four of the five ESA listing criteria is non-existent, low, or very low. A threat that is non-existent, low, or very low does not justify classifying a species as endangered. In fact, it cannot even justify a threatened designation. As to other factors principally fisheries bycatch, it is hard to see how a bycatch impacting 0.17% of the population can lead to its extinction.

Not only does the Proposed Rule fail to support the recommended endangered listing with evidence or analysis, but it also ignores critical studies discussed in this paragraph placing all of the listing factors into perspective. Along the U.S. coast, young loggerheads emerge from nests on beaches and migrate offshore to the Gulf Stream System ("GSS") where they become entrained, and live, in the North Atlantic Gyre for years. Putman *et al.* 2010 showed that loggerhead nest density declines as the distance between the coast and the GSS increases. In fact, distance to the GSS accounted for over 90% of the variation in nest density, regardless of beach latitude. The reason, as Putman *et al.* 2010 points out, is that hatchlings rely on yolk reserves for sustenance and the longest distance a hatchling can swim using residual energy from its yolk is about 40 km (24 miles). Consequently, hatchlings emerging on beaches within about 40 km of the GSS have an increased likelihood of reaching their offshore destination where they begin feeding and where they can find shelter in sargassum weed from predators. Putman *et al.* 2010 also notes that their findings may be applicable to other significant loggerhead nesting areas near major ocean currents, including nesting areas in Japan, east Australia, Masirah Island of Oman, Tongaland of South Africa, Brazil, and the Yucatan Peninsula in Mexico. Nowhere does the Proposed Rule even consider this significant new analysis demonstrating that the distance from the GSS is a much greater influence on survival than are the issues raised in the Proposed Rule regarding the listing factors.

VI. Conclusion

The Proposed Rule is a conclusion in search of a justification. How the BRT perceived its mission is captured in its own words. The BRT stated "... loggerheads from different populations often mix in common foraging grounds ... thus creating unique challenges when attempting to delineate distinct population segments...." BRT Report at 17. (Emphasis added.) The BRT's job was not to attempt to delineate DPSs as a preconceived result. Their job was to assess the best available scientific data, which they failed to do. It appears that the BRT set out to identify DPSs instead of fairly considering whether DPSs are justified at all.

The Services' 2007 stock assessment of loggerheads concluded that based on the best scientific data available, DPSs could not be justified and endangered designations were not warranted. Three years later, the Services reverse course, based in large part on new population assessment and extinction prediction models. Yet, even the BRT noted the models had a "high degree of uncertainty," likely "overestimate[d]" anthropogenic mortality, did not reflect a "true probability" of extinction, and may convey a "false sense of urgency." Echoing this, the National Academy of Sciences found that neither the population assessments nor the models were subject to "sufficiently rigorous" review to ensure accuracy and scientific validity, that available data have not been analyzed, and that the conclusions from the modeling were unjustified and incapable of justification.

The Proposed Rule is riddled with inconsistencies regarding both the DPS designations and the endangered listing. For example, the Proposed Rule argues that the direct harvest of loggerheads in the proposed Northwest Atlantic DPS justifies an endangered listing, while ignoring a report by experts appointed by the Services that the directed take approximates zero. In another example, the Proposed Rule claims physical separation of loggerheads in the three ocean basins justifies DPS designations while simultaneously admitting that within ocean basins loggerheads are mixed in foraging areas and that loggerheads move between ocean basins, carried by the prevailing currents. A third example is the claim that DPS designations are justified because loggerheads are reproductively isolated since females generally nest on the same beaches. Not only does this ignore the admission in other parts of the Proposed Rule that nesting beach fidelity is far from absolute and that loggerheads from the northwest Atlantic colonized areas thousands of miles away in the northwest Atlantic and Mediterranean, but it also importantly ignores the other half of the breeding population – males. Nowhere does the Proposed Rule claim that females from one nesting area only mate with males from that area.

What the acknowledged data actually show is that males and females from different nesting areas and different ocean basins mix on common foraging grounds and mate, belying the argument of reproductive isolation. There is no need to belabor this point and to repeat the discussion in all of the preceding sections. Suffice it to say that these examples are but three of many examples of the inconsistencies and inaccuracies in the Proposed Rule.

In addition to its fundamental scientific flaws, the Proposed Rule has fundamental legal flaws. Two bear mentioning. First, the Proposed Rule fails to meet Congressional intent codified in the Services' DPS Policy that DPS designations should be made "only" when a "preponderance" of evidence indicates "conclusive" data are available. The Proposed Rule utterly fails this evidentiary standard. Second, the Proposed Rule violates both the ESA and judicial precedent that ESA listings cannot be made below the DPS level. Here, the Proposed Rule is based not on an analysis of the entire DPS but rather on a subset of the DPS, nesting females. While this one percent of the total loggerhead population is important, the Services cannot argue that this segment of the DPS population is the entire DPS population. The Services must examine the entire DPS.

The NAS concluded that "inadequate information" was available for the assessment of loggerhead population status and trends. The Services should heed this admonishment and gather sufficient data and to then correctly analyze it. The Proposed Rule should be issued in final form making no DPS designations and retaining the current listing status. To do otherwise will set in motion a process that will inflict significant and unmitigable harm on people whose lives and livelihoods will be impacted by the DPS and endangered designations if the Proposed Rule become final.

APPENDIX

Scientific Analysis Of The
Diffusion Approximation Analysis

The first assessment model used by the Biological Review Team ("BRT") was a diffusion approximation model ("Diffusion Model"). The Diffusion Model is a method used to assess the probability of a population going extinct based solely on the time series surveys of the number of loggerhead nests. As discussed below, there are numerous problems with how the model was applied.

What Was Done In The Diffusion Model?

The purpose of this section is to clarify how the nest survey data were analyzed by the BRT such that the important assumptions and shortcomings are understood. The first step taken by the BRT in using nest survey data was to convert the number of nests into the number of breeding females (nesters) by dividing the total number of nests by the number of nests produced per female (clutch frequency). Next, the BRT adjusted that data using a three-year running total to reduce "year-to-year fluctuations in the number of nests." BRT Report at 37. Although not discussed by the BRT, the purpose of this exercise was to mitigate measurement and observation errors (*e.g.*, nests missed in the count on the beach) that can corrupt the yearly variation and consequently the risk of extinction calculations. These data were assumed, *id.,* to comply with the following density independent exponential model:

$$R_{t+1} = R_t e^{u} ,$$

where R_t is the three-year running sum for year t (e.g., if the data series started in 1989, year 1 would be the 1989-91 sum, year 2 the 1990-92 sum, etc.) and μ is the log population growth rate (a parameter defining the trend in growth). In words, the equation states that next year's three-year running sum is equal to this year's three-year sum times a constant fraction (e^{μ}). Solving the equation for μ yields:

$$\mu = \ln\left(\frac{R_{t+1}}{R_t}\right).$$

In words, the natural logarithm of the ratio of two consecutive sums over time provides an estimate for the log population growth rate (*i.e.,* the growth trend). The mean of all the growth rates and variance between those growth rates of all the log growth rates in the sample.

$\ln\left(\frac{R_2}{R_1}\right), \ln\left(\frac{R_3}{R_2}\right), \ln\left(\frac{R_4}{R_3}\right),$ …, was then calculated.[12] These two population parameter estimates (the mean and variance) are the basis for all extinction inferences made by the BRT. For projections into the future, the first equation above can be rearranged as:

$$\ln(R_{t+1}) = \ln(R_0) + \mu t$$

where R_0 is the initial population size and was set to the sum of the last three years in the data series. Time into the future (now *t*) can go up to 100 years, Given a log growth rate, the associated variance, the initial population size (R_0), a threshold population size (termed quasi-extinction by the BRT) and the time into the future, the probability of obtaining the prescribed threshold within the prescribed time can be calculated.

For an accurate result to occur, the model must comply with the following: the population parameters do not change over time, no catastrophes or bonanzas in population size (outliers), no autocorrelation and no observation error. Failure by the BRT to review and evaluate these assumptions is a major failure in scientific protocol. For example, the Morris and Doak (2002) text, from which the BRT extracted substantial program code for their analysis, goes to considerable trouble to articulate the assumptions and to suggest procedures to test for compliance.

For loggerhead sea turtles, the first condition in the preceding paragraph cannot be met. Specifically, population parameters do change over time. A simple graphical check on how well the estimated mean of the log growth rates fits the data can be obtained by plotting the log of the three-year sums by year and overlaying the predicted trend, the last equation above where (R_0) is now the population size the year before collection of the data series started. When plotted, the predicted trend will be a straight line. As discussed above in Part II(A) of this Comment, the population parameter estimates, the fundamental starting point for analysis, are flawed because they do not use the most current data showing nesting population increases. Had this data been used, a different trend line would exist. From the outset, the results from the model are condemned to inaccuracy and failure. These issues are discussed more fully below using the Peninsular Florida Recovery Unit and the North Pacific DPS as examples.

As noted above, the cumulative probability of reaching a prescribed population size (quasi-extinction size, say) 100 years into the future is a straightforward calculation given a

[12] These estimates assume nesting surveys have been made for all years in the data series.

particular set of population parameters. However, uncertainty in the estimates of the population parameters (*i.e.,* the mean of the growth rate and the variance in that rate) must be addressed by computing the uncertainty in the probability of the population change. A computer-based method known as "parametric bootstrap" was used by the BRT to approximate the confidence intervals for the quasi-extinction probability. Because the accepted standard probability distributions that govern the population parameter estimates are well known, we can have the computer draw values of the parameters from the appropriate distributions. If both of these drawn values lie within their 95% confidence interval they are used to calculate the quasi-extinction probability throughout the 100 year horizon. By repeating the process many times, a range of quasi-extinction probabilities are generated that lie within the 95% confidence interval of the population parameter estimates. The extreme quasi-extinction probabilities define the upper and lower boundaries of probability over the 100 year horizon. The BRT chose to focus only on the cumulative quasi-extinction probability at 100 years and computed a metric introduced by Snover and Heppell (2009) named the susceptibility to quasi-extinction (SQE). The SQE was computed as the proportion of the bootstrap simulations with a cumulative probability greater than 0.9 at 100 years. In other words, the SQE is the probability that the cumulative probability of quasi-extinction 100 years in the future will be greater than 0.9.

<u>When Is Extinction?</u>

The intuitively obvious thing required to determine the probability of extinction is to know the population level below which the species cannot fall without being doomed to extinction. Since the whole point of the analysis is to determine if and when the population might face extinction, one would expect the Services to specify a population threshold, or range, that is the critical viability point. That point would be used to judge when the population is approaching, or at, the level at which it cannot survive. If relative thresholds (*e.g.*, percentage of current population size) are used, then the extinction threshold must also be provided by DPS. A very large distinct population (say in the hundreds of thousands of adult females) that is projected to experience a 90% decline over 100 years is at less risk of immediate extinction than a small distinct population (say in the hundreds of adult females) with the same projection and life history parameters.

The Services never provide that number. Thus, the Proposed Rule, based on the Diffusion Model, somehow concludes that the current loggerhead population in Peninsular Florida with a surveyed population of 19, 887 nesting females (2007-2009) faces immediate extinction, without telling us how many of the 19,887 observed nesters must cease to exist before we reach the critical threshold below which the species cannot survive.

The Modeled Extinction Level

Having failed to actually determine a population level below which the species is viable, the Diffusion Model measured when the existing nesting population (an incorrect number to begin with) would fall to 97.5% to 2.5% of its current level. The Diffusion Model also measured points in between, *i.e.,* 90%, 80%, 70%, etc.

Using each of these end points (90%, 80%, 70%, etc. of current population), the Diffusion Model conducts a probability analysis to determine the probability of the loggerhead population reaching that level within a 100 years. However, the BRT only reported the Susceptibility to Quasi Extinction ("SQE") as described above. Of course, this newly minted term and concept have nothing to do with actual extinction since the Services neglected to actually develop that number. Rather SQE is the probability that the cumulative probability of various population levels below current levels 100 years in the future are greater than 0.9. Indeed, as the BRT admits, "SQE values are not indicative of a true probability of quasi extinction...." BRT Report at 36.

Nevertheless, the originating paper, Snover & Heppell (2009), recommends a critical SQE of 40% based on simulations of loggerhead turtles using a stochastic model similar in structure to the threat matrix model. At that critical SQE value and with a cut-off cumulative probability of 0.9, the chance that the true population size is greater or less than the specified threshold is approximately the same (based on the Snover and Heppell simulation study). That is, population reductions with a SQE greater than 40% are "at risk" of reaching the specified reduction within 100 years. The SQE critical value of 40% in association with the cut-off cumulative probability of 0.9 are the only numbers published for loggerhead turtles. The BRT, however, arbitrarily used 30% as the critical SQE number despite the fact that the creators of the SQE statistic said the model should use 40%. BRT Report at 38.

Recognized Problems With Diffusion Models

Diffusion modeling applied to viability population analysis has drawn heavy criticism in the published literature (*e.g.,* Ludwig 1996, Ludwig 1999, Fieberg and Ellner 2000, Ellner *et al.* 2002). Most of the criticisms pertain to the high uncertainty and inaccuracy of predicting extinction probability far into the future. Proponents of diffusion modeling (*e.g.,* Morris and Doak 2002*,* Holmes *et al.* 2007) provide guidelines in response to the criticisms. Of course, even following these guidelines cannot remedy the data input and analytical weaknesses discussed above that infect the Diffusion Model.

The first and most important guideline is that the model assumptions must be evaluated with respect to the data inputs. The lack of such evaluation by the BRT is a major failure in scientific protocol because there was no effort to determine if the assumptions used in the model were accurate. Numerous diagnostic procedures are available for assessing the adequacy of the diffusion approximation. For example, Dennis *et al.* (1991), Morris and Doak (2002) and Holmes and Fagin (2002) provide suggestions specific to the Diffusion Model applied by the BRT. Draper and Smith (1981) is another classic statistical reference relevant to diagnostic procedures for the Diffusion Model. In the following section, we illustrate that the Peninsular Florida and North Pacific population parameter estimates change over time. This is a highly significant problem for the credibility of 100 year projections.

Another guideline for properly using diffusion modeling is that future projections of population growth or decline should not extend more than 2.5 times the length of the initial data time series (Holmes *et al.* 2007). Some authors (*e.g.*, Ellner *et al.* 2002*)* assert that projections should be limited to only 20% (0.2 times) of the initial data time series. In other words, if the initial time series is 20 years, then the population projection should not run beyond 50 years at the outside. Instead, the Diffusion Model makes projections to 100 years – and the Services use those 100 year projections as the basis for the Proposed Rule.

Finally, the data inputs for establishing the starting population level and trend should cover at least 20 previous years (Holmes *et al.* 2007). For long lived species such as loggerheads where a generation is greater than 30 years, a longer time series may be required to remove error (Sabo *et al.* 2004). The point is that the length of the data series should reflect the true environmental variation of the turtles. With the possible exception of mainland Australia, all the series are less than one generation. Under perfect compliance of the Diffusion Model assumptions through simulation, Snover and Heppell (2009) recommend at least 15 years. Examination of the actual series in hand demonstrates that the Diffusion Model assumptions do not hold.

<u>Application of the Diffusion Model to the Peninsular Florida and North Pacific Populations</u>

In this section, we apply the Diffusion Model to quantify the impact of using all the data, apply a simple graphical check that the Diffusion Model assumptions are consistent with the data and display the cumulative quasi-extinction probabilities for all years using the computer program and data supplied to SAA by the Services. Peninsular Florida was chosen because it is the largest extant population within the main Atlantic/Indian/South Pacific Ocean range for which data are available. The North Pacific was chosen for comparison.

Figure 1 plots the number of nesting females in Peninsular Florida (upper panel) and the North Pacific (lower panel). The open symbols (red lines) are new data and the filled symbols were used by the BRT. The trend lines with and without the updated series along with the three-year running sum values are plotted in the upper panel of Figures 2 and 3 for Florida and the North Pacific, respectively. Here, the trend lines that are to be used for the 100 year projections are superimposed on the data. If the data were consistent with the Diffusion Model then they should follow the trend line. Scatter-plots of the residuals (the data value minus the predicted value) versus the predicted value are provided in the lower panels of Figures 2 and 3. Specifically, the predicted value is zero line in each Figure. If the data and Diffusion model were consistent, the residuals should be randomly distributed about the zero line. Instead, the plots suggest that the Florida series can be partitioned into three segments: 1989-1997, 1998-2003, and 2004-2009 and the North Pacific into two segments: 1990-1996 and 1997-2008. It appears that these segments would fit the data much better than a single trend line.

The graphical diagnostics presented above are only approximate and do not provide statistical rigor. We followed the procedures provided by Morris and Doak (2002, pp. 76-79) to test for changes in population parameters in the series segments defined above. The results were significantly different mean log growth rates (*i.e.*, trend lines) in the segments at a 95% confidence level for both the Florida and North Pacific populations. Therefore, an underlying assumption (the population parameters do not change over time) of the Diffusion Model has been violated. In our opinion, there are three analysis options.

1. Use the segment that is thought to be the better indicator of the future (*e.g.*, the most recent). The problem here is that we are partitioning an already short series.

2. Proceed with the analysis as done by the BRT. The assumption is that all temporal changes in population parameters that could occur in future projections are fully represented in the observed series. However, under this assumption the variance of the log growth rates is overestimated because it includes the variation associated with the change in trends over the data history. Therefore, subsequent assessments are pessimistic estimates of quasi-extinction risk (actual risk is smaller than estimated).

3. Do not proceed with the analysis. Either accept the inability to make reasonable risk of quasi-extinction projections, or construct a model that better

reflects the population dynamics and error structure. For loggerheads, constructing a new model that better reflects the population dynamics represents the best option.

For the North Pacific, the pronounced and statistically significant "V" in the log growth rate argues against using the full series. The same outcome is obtained if 2008 is treated as an outlier and deleted from the series. The choice is either using the most recent segment (1997-2008) or not conducting any analysis because the series is too short (12 years). Given that 1997-2008 series exhibits a marked upward trend in population there is little need to conduct an analysis.

For the Florida Peninsular Recovery Unit we proceed with the Diffusion Model (option 2 above) despite the short series, a demonstrated change in population parameters over time, an overestimate of the variance and without a definition (numerical or proportion) of when the population is no longer viable. We proceed with the analysis to illustrate the impact of using all the data and that a plot of the quasi-extinction probabilities over the 100 year time horizon adds useful information for the decision maker.

Figure 4 (upper panel) displays the log-probability plot using the 1997-2007 data series with a quasi-extinction threshold of 1,000 nesters (5.3% of the current population) which produces a SQE value of 30% (*i.e.*, reproduces the critical value used by the BRT). The lower panel uses the full data series of 1997-2009 with a quasi-extinction threshold of 2,610 nesters (13.1% of the current population) which again produces a SQE value of 30%. Note that adding two years of data (2008 and 2009) more than doubled the quasi-extinction threshold deemed to be critical by the BRT. If we use the critical SQE of 40% recommended by Snover and Heppell (2009), the only published value for loggerheads; the quasi-extinction threshold is 20.1% of current population size. Are we to believe that a population with 13.1% or 20.1% of the current population size 100 years from now faces immediate extinction today? Also, recall that the actual risk is smaller than estimated because of the inflated variance estimate. Figure 4 displays the cumulative probabilities in log space as recommended by Morris and Doak (2002). All the useful information (>1%) is located at the very top of the plot. Figure 5 plots the cumulative quasi-extinction probabilities (without transform) for a threshold of 1,000 and 2,610 nesters overlaid for the mean and upper 95% confidence interval. Note that the mean probability of observing quasi-extinction (>1%, say) a drop in the population to 1,000 or 2,610 nesters is far into the future (>40 years).

<u>Conclusion</u>

The BRT failed to use all available nesting survey data for the Diffusion Model. The BRT also failed to evaluate the assumptions underlying the Diffusion Model. Both the Peninsular Florida and the North Pacific population parameters change over time in violation of the key underlying assumption for the Diffusion Model. Projections for these populations require a better understanding and depiction (*i.e.,* model) of the underlying population dynamics. Calculation of pessimistic time to quasi-extinction cumulative probability functions using all available nesting survey data for Florida, for example, provides quantitative evidence that the population is not at immediate risk of extinction.

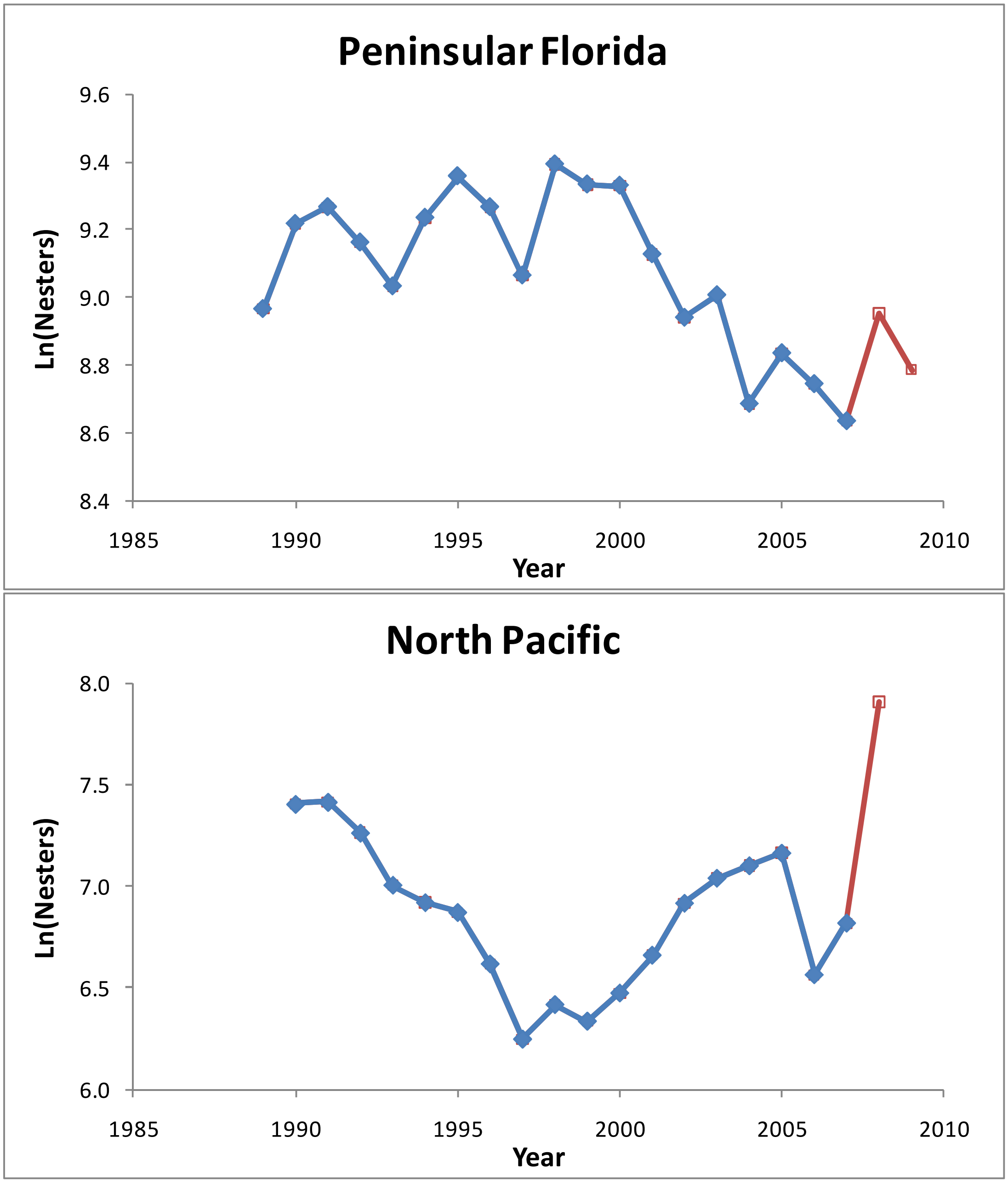


Figure 1. Number of nesting females at Peninsular Florida (upper panel) and the North Pacific (lower panel). Open symbols are new data and the filled symbols as used by the BRT.

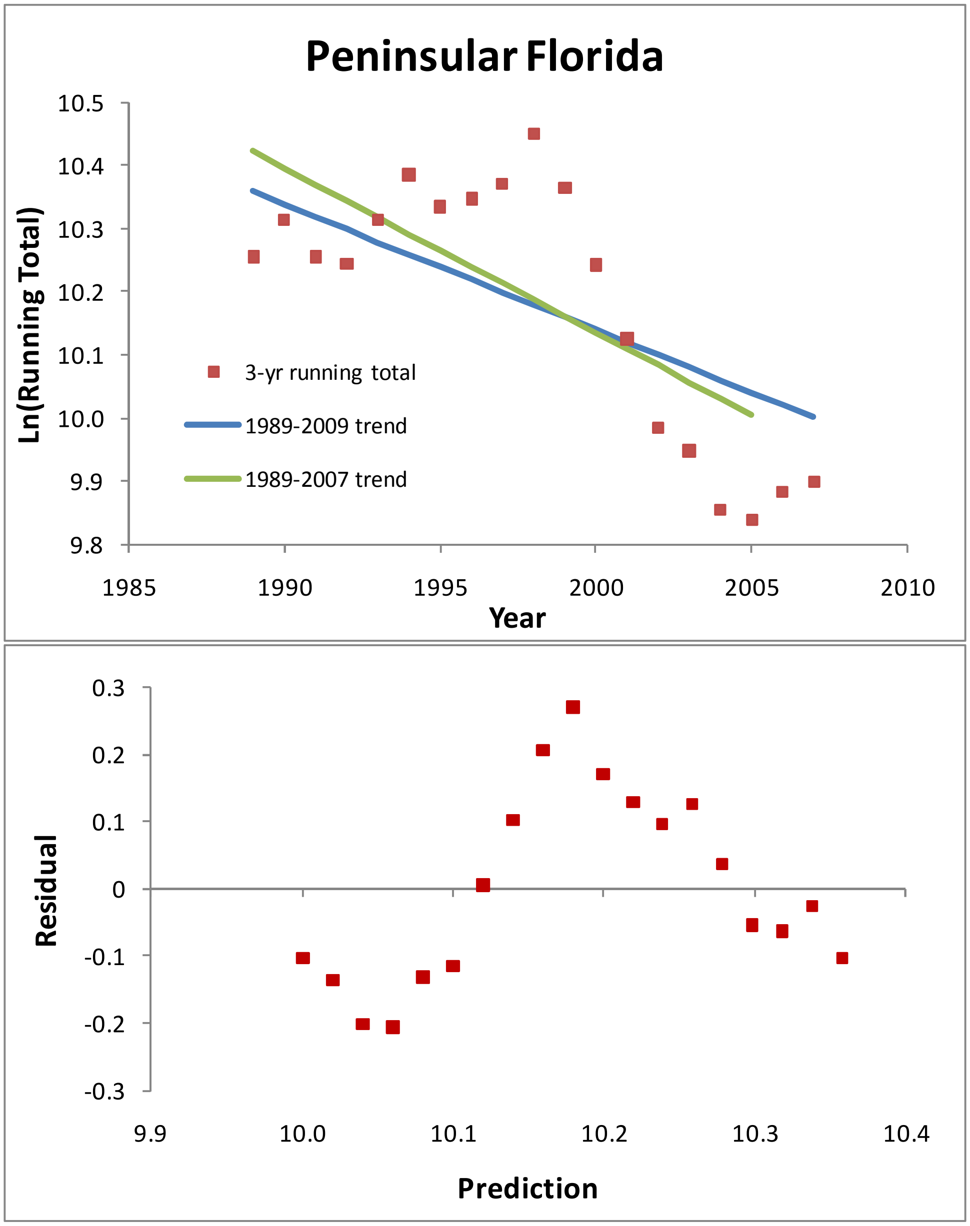


Figure 2. The logarithm of the three-year running sum of nesters and predicted trend lines using the 1989-2007 (shorter line) and 1989-2009 (longer line) periods (upper panel) and the associated residuals (data minus the predicted value) using the updated 1989-2009 series (lower panel) for the Peninsular Florida population.

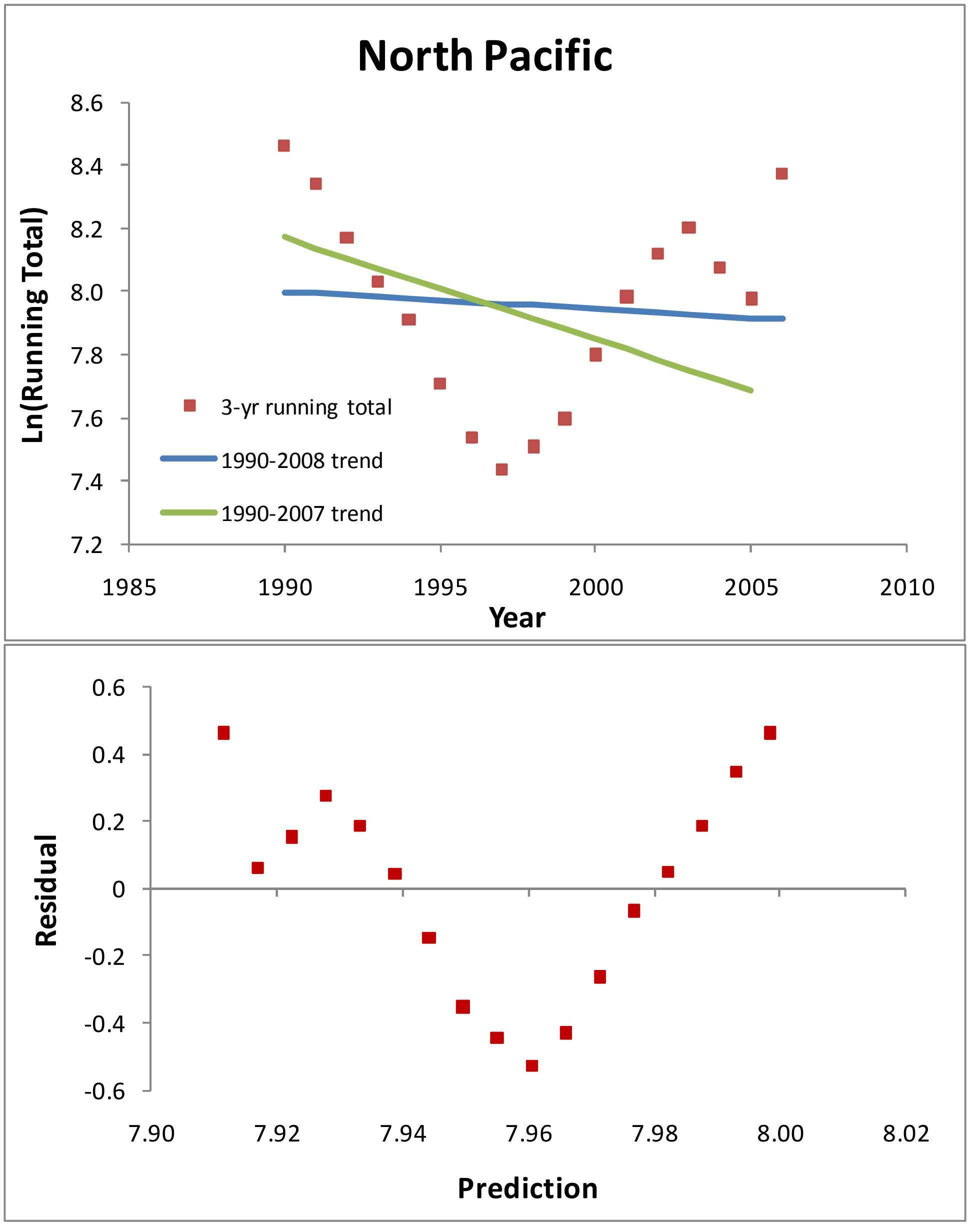


Figure 3. The logarithm of the three-year running sum of nesters and predicted trend lines using the 1990-2007 (shorter line) and 1990-2008 (longer line) periods (upper panel) and the associated residuals (data minus the predicted value) using the updated 1990-2008 series (lower panel) for the North Pacific Ocean population.

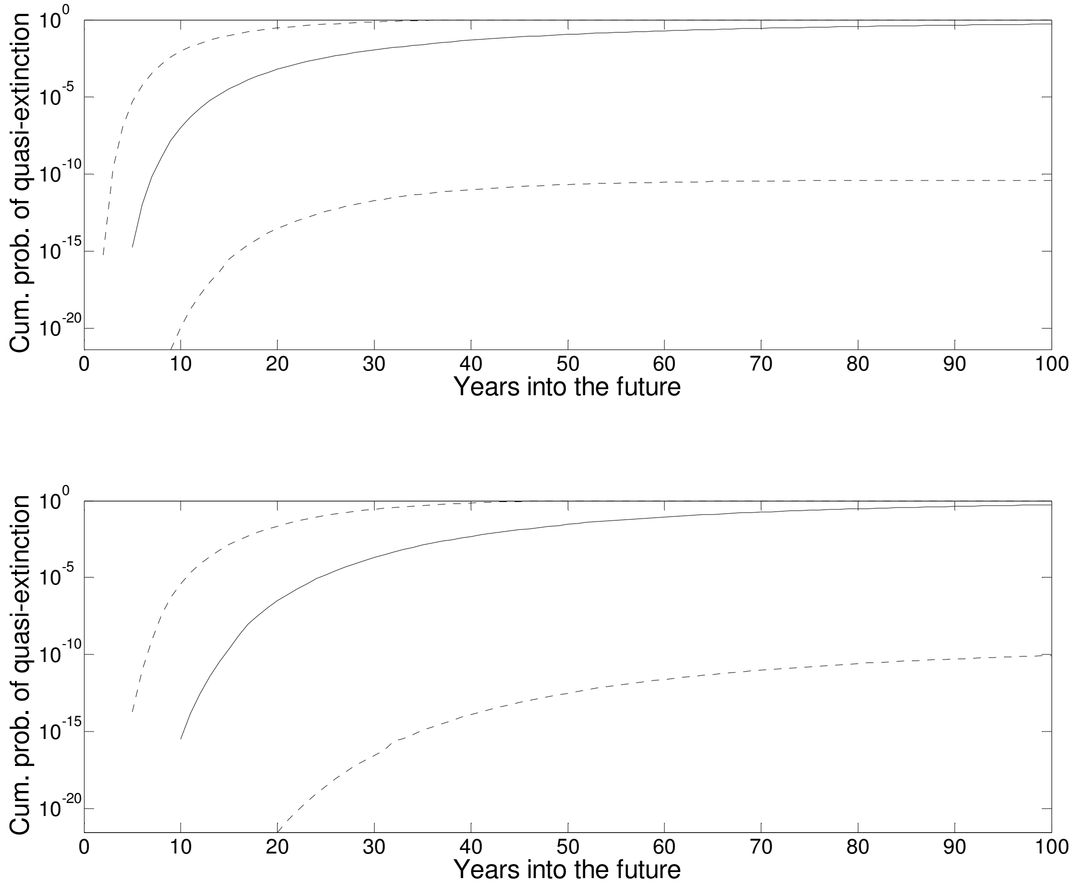
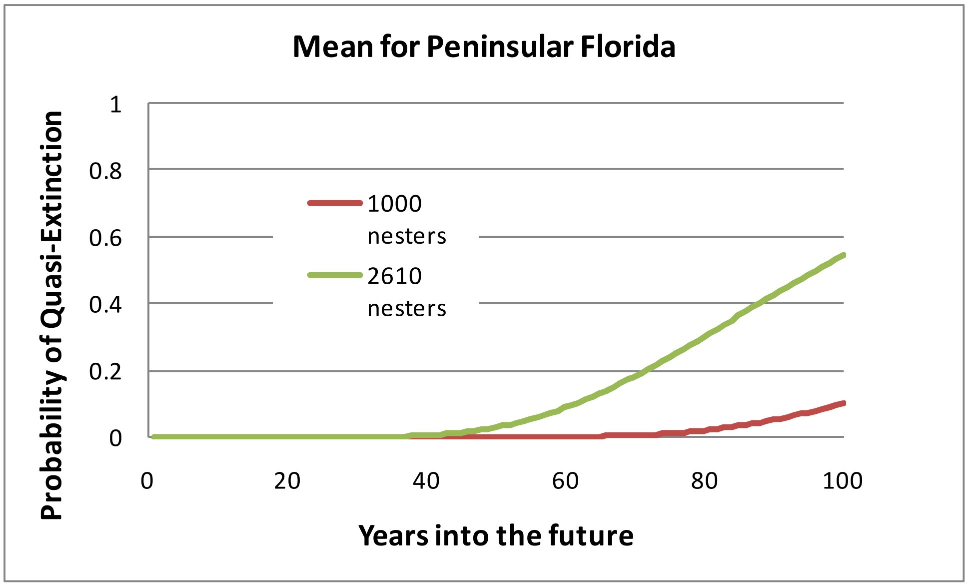


Figure 4. Cumulative extinction log-probability plot for the Peninsula Florida population using the 1989-2007 data series with a quasi-extinction threshold of 1,000 nesters (5.3% of current population) which produces SQE = 0.30 used by the BRT as the critical value (upper panel). Similarly, the lower panel uses the 1989-2009 period with quasi-extinction threshold of 2,610 nesters (13.1% of current population) which produces SQE = 0.30. The solid line is the mean and the dashed lines are the 95% confidence intervals.



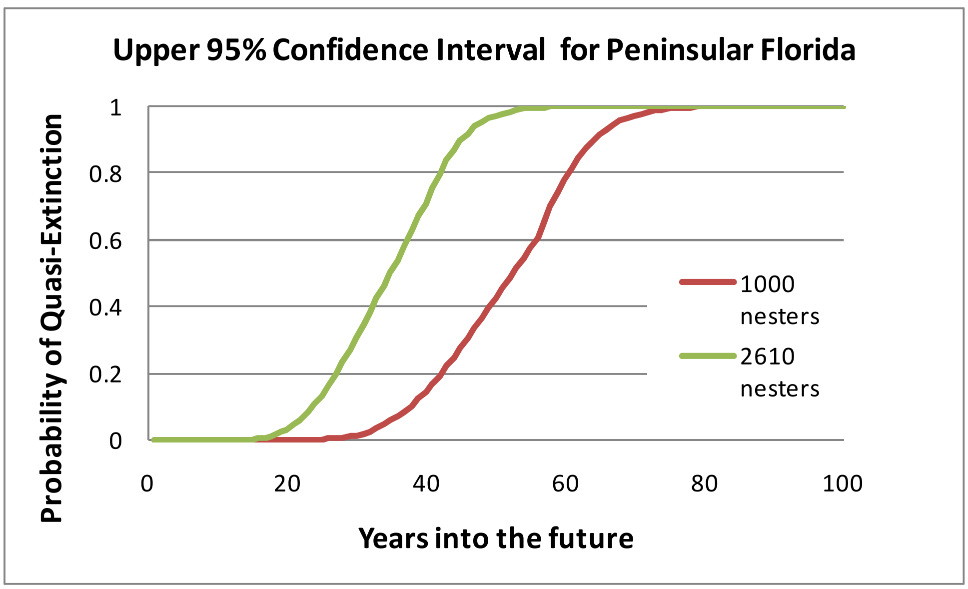


Figure 5. Cumulative probability of quasi-extinction for Peninsula Florida Unit using the 1989-2009 data series with quasi-extinction thresholds of 1,000 and 2,610 nesters. Upper panel is the mean and the lower panel is the 95% upper confidence interval.